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THE TELOTYPE;

A PRINTING ELECTRIC TELEGRAPH.

BY

FRANCIS GALTON, Esq., M.A.,

TRIN. COLL., CAMBRIDGE.

LONDON:
JOHN WEALE, 59, HIGH HOLBORN;
M^C MILLAN, CAMBRIDGE;
APPLETON AND CO., NEW YORK.

June, 1850.

Price, One Shilling.

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THE TELOTYPE:

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THERE is probably no branch of practical science in which a real improvement would be hailed with greater pleasure than that of Electro Telegraphy. A great deal has been done but very much more is wanted before Telegraphs can become the common, even household, instruments of communication that they probably are destined to be.

In all those now in use the art of transmitting and interpreting messages requires much time and practice for perfect acquirement. The superior advantages of a telegraph capable of printing messages in the ordinary alphabetical characters is obvious, but it is, in the first instance, a problem of some difficulty to govern machinery of the requisite power by the extremely feeble force of which we have to dispose.

To effect this is, in part, the object of the instrument described in the following pages. By the methods here explained it is hoped that a printing electric telegraph may be constructed, simple in its mechanism, easy to be worked, and equalling in its sureness and speed any of the forms of telegraph now in use. In the Telotype (as our instrument may be termed), by merely touching a key on which any letter is marked that letter is to be printed, almost instantaneously, at the opposite end of the line.

It is well known that there are two rival ways of applying Electricity so as to cause motion at a distance; by the one we have a greater variety of signals, less force and slower action; by the other, a less variety but greater motive power and a quicker action. The simple telegraph needle is light and well balanced on its axis, so that a breath of air would set it spinning, and is urged by a force just sufficient to turn it quickly; it can however make three signals, for it can turn to the right, to the left, or remain steady. The electro magnet, on the other hand, can only show two signals, raising its armature or letting it alone, but it works at a distance with a force, which though very inconsiderable in itself (about sufficient to free the hair trigger of a rifle), is yet far greater than that of the needle, and it was therefore with this for their governing power that the printing telegraphs hitherto attempted have been constructed.

Now by the machinery about to be explained not only the electro-magnet but even the needle can be made to govern the motion of the heaviest apparatus, and thus in spite of the extremely small motive power of the latter we are enabled to secure the advantages that its variety of signals affords.

Having thus caused that certain mechanical effects should follow the movements of each separate needle we have next to contrive an apparatus by which these effects should be compounded together and then by it translated into letters, so that if three wires and three needles be employed in combination (no needle exceeding one movement for each signal) the apparatus we require is such that for each of their different combined signals, twenty-seven in number, a different letter shall be printed. Now how all this can be done will shortly be explained. We shall see that in the Telotype the needles move as freely and as unencumbered by any restraint as if there were no machinery near them; but having once moved, and taken their several positions, slight rods are by an independent train of machinery allowed lightly to touch them. These touches last but for an instant, and the needles are free to begin another signal. There is no other connection whatever between the

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Fig. 6.

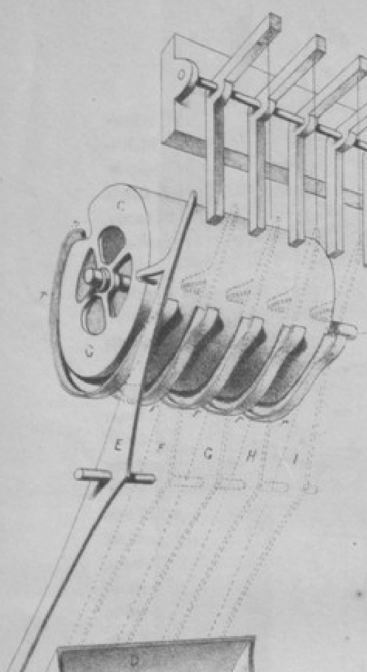
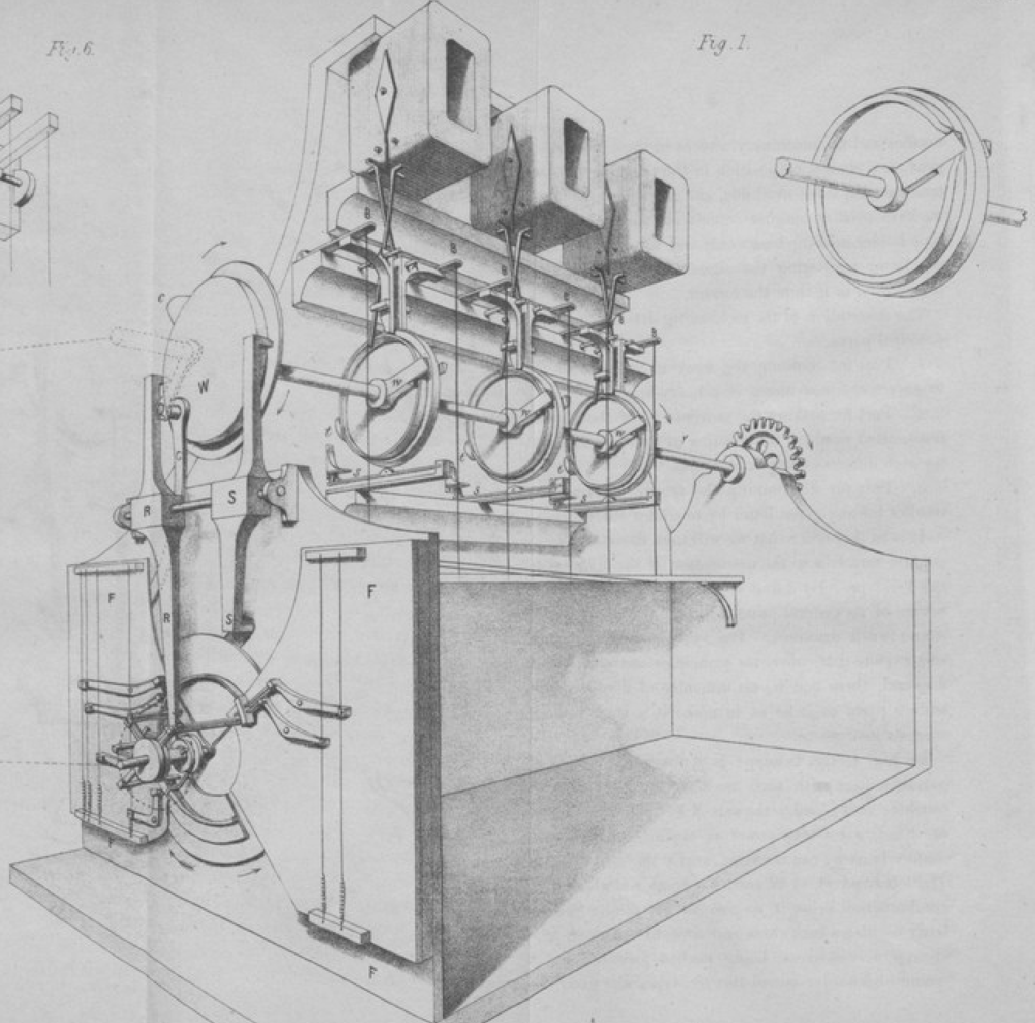


Fig. 1.



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needles and the machinery, except in these touches. They produce a change of position in the touching rods which is made mechanically available, and in the complete machine we have needles making signals on the one hand, and a type holder moving backwards and forwards on the other, the latter registering the signals combined together and transmitted to it from the former.

The description of the mechanism divides itself into three principal parts.

1. Part for enabling the weak movements of a needle to govern the movements of a heavy arm.

2. Part for making the movements of the needles thus transmitted result in the printing of letters, a different letter for each different compound signal.

3. Part for determining the proper movements of the needles for any given letter by touching a key.

It is in this order that we will take them, and we will first confine ourselves to the description of the simplest form of the Telotype. By doing so we shall gain a more concise notion of its general principles, and also avoid the evils of overcrowded drawings. The symmetry of the machine is so complete that, when its principles are once clearly understood, there can be no difficulty of detail whatever, in adding parts to it so as to make it a more powerful and efficient instrument.

In Fig. I. the Telotype is represented; above are the galvanometers with their needles; beneath them are the touching rods; and to the axle X Y, upon which four wheels are fixed, a rotatory power is applied, which, moving as uniformly as we can contrive, works the whole machinery. The left-hand wheel W carries a crank rod which influences another wheel below it, so that as the former revolves the latter oscillates backwards and forwards through an arc of about ninety degrees, being pushed forwards and left to spring backwards: round this the types are fixed; in front are a series of slides whose action determine the movements of the type-wheel; S is the stamp. At each signal that the needles make, corresponding slides are moved, and they cause the type-wheel to be so arrested in its backwards spring as to bring a proper type under S, which descending, impresses it on paper.

There is a degree of uncertainty which attaches itself to all telegraphy, in that the needles are not invariably commanded by their batteries. The insulation of the wires is sometimes imperfect, so that the current will not pass to the distant station, and we cannot make the needles move at all; sometimes, again, the atmosphere induces currents of its own through the wires, and makes the needles move whether we will it or not. These are causes of error which cannot be eliminated by any mechanism, and we do not profess to be able in any way to get rid of them; all that we can do, by means of the contrivances here explained, is to ensure that whatever signals the needles may have given at a distant station shall infallibly be recorded, interpreted into letters and printed upon paper. Fortunately for us the causes of error that are specified above are scarcely felt at short distances, and it is for these, such as communication from one part of a town to another, that we feel our instrument to be especially adapted. Having premised this we will commence our description with that part which enables the weak movements of a needle to govern the movements of a heavy arm.

Referring then, again, to Fig. I. The needles are balanced so as to hang vertically when at rest, and to their lower ends a strip of card or of some other light substance is attached, which projecting down below the galvanometer coils, comes in reach of the touching rods. There are two of these touching rods in correspondence with each needle, and each of them is capable of turning through a small arc round an axis; they, like the needle, are balanced so as to hang vertically when left to themselves, their lower ends rest on raised rings that are attached to the opposite surfaces of each wheel, and each ring has a notch in it. As the axle X Y and its wheels turn together, the rods continue steady and their upper ends apart until the moment when the notches come upwards, and then they all move, their lower ends falling into the notches, and their upper ends falling through the paths of the strips of card. As the notch is but a small one, the instant after the rods have been left free to fall forwards they are forced back again. Now, if during this short interval of time one or more of the needles lie out of a vertical position, corresponding rods will have fallen against the cards attached to them, and will have been checked so that their lower ends will not have sunk into the notches. Hence the arms with wedge-shaped extremities that are seen projecting through the notches, and which otherwise would have passed over, will now get beneath them, and from their peculiar shape will, as they move on, lever the rods up high above the ring and then drop them again upon it as soon as they have passed away from under them. Thus those touching rods that were unchecked fall into the notches, those that were checked get levered high up, and both, as soon as the arm and notch have together passed by, find themselves on the ring again. It is the levering up of the touching rods by these wedges that supplies the method of communicating between the needles and the machinery. The needle acts as an inert body, by its position entirely. The force by which it has been urged into its position has nothing to do with the effect it produces on the machinery, and in this lies a fundamental difference between the methods we adopt, and those that have been hitherto employed. In all previous contrivances of this nature the dynamical force of the current has had not only to move an armature, but also to drag away a catch from the teeth of a wheel that was held back by it, but the needle in the telotype has perfect freedom of motion ; it is during its motion entirely disengaged from all connections, except, of course, the point or axis on which it turns, but it is after it has moved that it produces its effect. Again, the only delay caused by the addition of machinery to the rapidity of signalling is that corresponding to the time occupied by the notch passing under the touching arm, one evidently of very short duration. The force with which the touching arm is levered up depends entirely upon that with which the axis and wheels are made to revolve, and if the touching arms were strong enough they might be made to communicate with the slides of the machinery at once. As, however, they are made extremely slight, so as not in any way to hazard injuring the needles or their delicate supports by dropping against them, we must make them first communicate with thicker bars on partly the same principle that the needles communicated with them, that is to say, we must make them influence the thicker bars by their position. These thicker bars are the bent ones shown in the drawing and marked A ; they rest not upon the raised ring as the touching rods do, but on the flat surface of the wheel. One bent bar lies to the side of and turns on the same axis as each touching rod. The distance between the lower ends of the rod and bar is a little less than the width of the wedge, and again the thickness of the wedge must be a little greater than its height above the wheel. Lastly, towards the lower end of the bent bar a hook is fixed so as to overhang the touching rod, and almost be in contact with it when that rod is resting upon the ring. Hence, if the touching rod be levered up higher than the ring, it lifts the bent bar off the wheel, and it follows from the proportions above assigned to the wedge, that immediately before the touching rod has travelled to its highest peak, the bent bar will have been lifted up on to a level just above that of the lower point of the wedge, which itself will at that moment have passed beneath it. The next instant the touching rod will be dropped and the bent bar alone will remain on the wedge, and it, in its turn, will be levered up still higher, and then dropped behind it, just as the touching rod had been before. Now, the bent bar may be very much heavier, and therefore stronger than the touching rod, and can in consequence be employed without danger to convey a much more powerful impulse from the wheels to the machinery than it could. All that has to be attended to is, that the touching rod should not be strained in being made the means of prising the bent bar on to the wedge. When once the touching rod is dropped it is out of the way of all injury, and the bent bar may immediately commence performing its duty. Thus, referring again to the drawing, we see levers B B pivoted at their further end, and resting upon a bracket; each lever lying across and just above the end of the horizontal part of the bent arms A A, its height above them being so adjusted that directly after the touching rods have been dropped by the wedges, and the lower ends of the bent bars have been levered a short way up by the wedge, the bar and lever shall come in contact, then the wedge passing further on, the bent arm moves higher, and the lever is lifted, and, lastly, the bent arm is dropped, and the lever drops also. By these means, as the wedge moves quickly by, a short impulsive movement is given to the levers, and by them, through the medium of the strings that are seen fastened at their ends, to the slides.

It therefore clearly results from all this that, there being two slides corresponding to each needle, according as any one of the needles has moved to the right, to the left, or has remained still, so shall one or the other or none of its corresponding slides be moved also.

It is evident that not only one, but a series of two or of any number of bars might be interposed between the needle and the lever, each bar increasing in massiveness and strength. One however is amply sufficient in the present instrument.

The touching rods are as light as they can be made consistently with anything like stiffness. The cross pieces that are attached to the top of them are, the one to fall against the strip of card, the other to prevent the card from ever, by any accident, getting behind them when they have fallen forwards; if they did so, the apparatus would of course be put out of order. The addition of the bent bars creates no delay whatever in the rapidity of signalling; each particular signal is delayed a short time by it, but signal follows signal as fast as if no bars at all were there. Exactly the same number of signals, for instance, can be made in a minute with them, or with any series of them, as without them; but in the latter case the whole communication is of course delayed, though for a perfectly immaterial space of time.

If we choose to employ a long series of arms on this principle, we see the signals made by the needles travel through them in a manner not unlike that in which messages do from Semaphore to Semaphore in the old system of telegraphs. The signal is taken up by each arm in its turn, and each having forwarded it on to the next, falls back to its place. The last arm moves as fast as the needle does; but at any one instant the movement of the last arm will have no reference to the signal which the needle at that time is employed about; it will correspond to one that had been made some time before: so the delay incurred by using a series of arms tells, once for all, upon the whole communication and is not repeated at each signal. And as the last arm does not begin to move till two or three seconds, perhaps, after the needle has begun signalling, so it will continue moving after the needle has stopped, making up its arrears.

It is very interesting to watch such a series in operation; how the delicate, scarcely perceptible touch of the first arm causes an influence that travels on, almost as if by instinct, through the whole series; how each arm hands it to the one beyond it, its available power increasing at each delivery.

With this we conclude the first part of the machine—namely, that whereby the needles are made to communicate with heavy mechanism; but before proceeding to the next division of our subject, we must explain the method whereby we inform ourselves of the exact time when in the distant machine the notch comes under the touching rods.

If the distant machine revolved with perfect uniformity, or if by any separate galvanic contrivance a wheel by the side of the operator was made to work in exact relation to the Telotype at a distance, the problem would be solved; but the last plan is cumbersome and the first impracticable, and therefore we are reduced to other methods. On the edge of each wheel we fix a tooth, *t*, and below each wheel, screwed to a bracket, is an arm holding a spring, *s*, bearing a metal point and pressing it against a metal plate. The circuit that passes round each galvanometer passes also between the metal point and plate, so when the tooth by means of the revolution of the wheels comes upon the spring it presses it down and breaks the circuit, and the operator at a distance, although still making contact with his battery, sees those needles before him that have moved swerve for an instant from their several positions. The eye readily detects this motion, although it be very slight and of short duration, and information is thus conveyed of its being time to commence another signal. The tooth is so adjusted as to press on the spring the instant the touching rod is above the notches.

We have now to avail ourselves of the movements of the slides, and to contrive that each different combination of them, not more than one slide out of each pair being moved, shall result in a different letter being printed. A crank rod

C is attached by a pivot to the face of the left-hand wheel, marked W in the figure ; the top part of it only is distinctly seen above the framework, and its further course is partly shown by dotted lines ; but in Fig. II. it is represented much more completely. Fig. II. represents the same part of the apparatus as that which is seen at the bottom of Fig. I., more or less covered with other mechanism that has reference to the movement of the slides. They are both of them supposed to be viewed from the same point of sight, but Fig. II. is magnified to double its size, in order to show more clearly its several parts. By referring then to Fig. II. we see that the lower end of the crank rod is pivoted on to an arm, R, the length of which is so chosen with reference to the radial distance of the first pivot from its centre, that when the upper wheel revolves the arm below oscillates backwards and forwards through an arc of ninety degrees, or thereabouts ; in front of this arm, and fixed to the same axle about which the arm itself moves freely, is a wheel, from behind which a point projects at the place marked *p* in Fig. II., and this wheel is urged by a spring which cannot be seen in the drawing, but the direction of whose action is indicated by the feathered arrows. Hence, as the arm oscillates up and down, the wheel alternately is pushed forwards in the direction of the plain arrows, and is left free to follow it by springing backwards in that of the feathered arrows. The axle to which this wheel is fixed is supported to the left by a bar that could not be shown in either of the drawings, and to the right by the main piece of framework marked F seen in the front of Fig. I., and which lies immediately behind the wheel and the arm that works it. The axle is prolonged just through this plate, and at its end a segment of a thin brass wheel is set of about one-third greater radius than the wheel of which we have just been speaking ; it is partly seen in Fig. II., is shaded and marked T ; this segment of a wheel T is divided into twenty-seven

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Fig. 2.

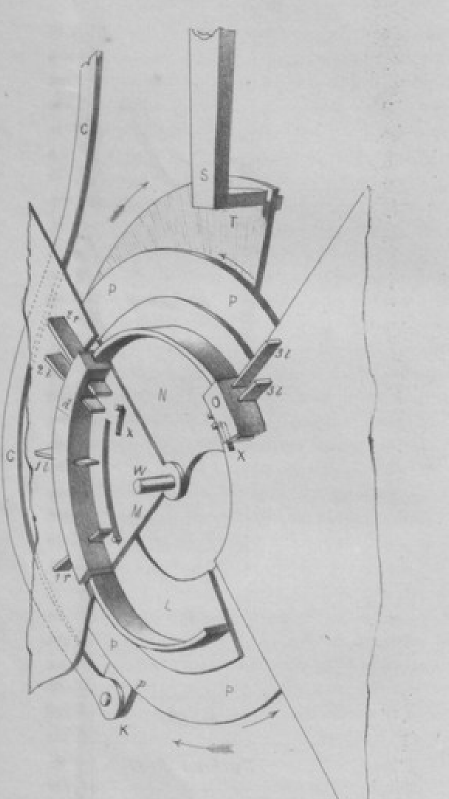

Fig. 3.

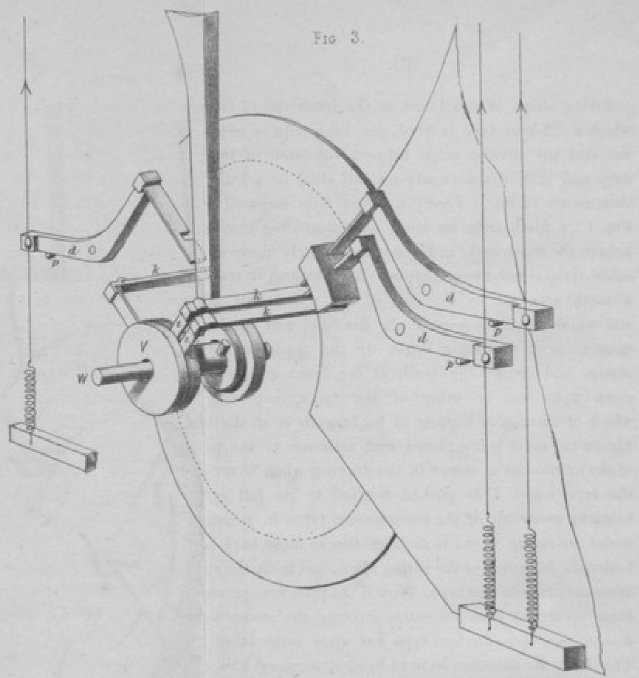

Fig. 5.

Needles		are taking their positions	are touched	are taking their positions	are touched	
Touching Rods		remain at rest	move	remain at rest	move	
Bent Arms B according to signal, and their corresponding slides	Remaining Slides Backwards	remain at rest	move	remain at rest	move	
Axle XV, with wheels, revolving uniformly move	Crank Rod and Arm together with L.M.N.	move up	move down	move up	move down	move up
	Stamp		remain at rest	print	remain at rest	print

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radiating strips of equal size, at the lower end of each of which a different type is fixed, and each strip is of such a size that the circular angle between the centre of the first strip and that of the twenty-seventh shall be a little less than an arc of 90°. Lastly, a stamp S is disposed as in Fig. I., a tooth from its lower end protruding through a hole in the framework, and lying immediately above one or other of the circular row of types. As the wheel W revolves, a tooth attached to the rim of the upper half of it, and which is partly seen in the drawing, and marked *c*, once in each revolution forces up the upper end of the stamp, and causes the tooth at the lower end to press down upon one or other of the types, according to which of them may happen to be beneath it at the time, Hence the tooth being placed with reference to the pivots of the crank arm as shown in the drawing, when W revolves the type wheel T is pushed forward to its full extent, bringing every one of the twenty-seven types in succession under the stamp S, and is then left free to move back again under the influence of the spring alone, and lastly the stamp descends upon its first type. Now if the plate were prevented from springing back its entire distance the stamp would descend not upon the first type but upon some other one. The object we therefore have in hand is to show how each different possible combination of movement of the slides, not more than one out of each pair acting at the same time, or in other words, how each different possible combination of simple signals that the three needles can give shall cause the stamp to descend upon a different type. This is effected on a principle of aggregate or compound movements. The slides in connection with the first needle cut off respectively one-third and two-thirds of the entire distance through which the type wheel is able to spring back, those in connection with the second needle, by an action quite independent of the first, cut off respectively one-ninth and two-ninths of the entire distance, and similarly those in connection with the third cut-off one twenty-seventh and two twenty-sevenths of the same. Now suppose no needle to act, the plate and type wheel fly back as far as they can and the stamp descends upon the first type ; next suppose the third needle to move to the right, the plate is stopped one twenty-seventh short and the stamp descends on the second type, if the third needle moves to the left for a similar reason the stamp descends on the third type. If again the second needle alone moves to the left one-ninth or three twenty-sevenths are cut off and the stamp falls on the fourth type, if the second needle moves to the left and the third also to the left then three twenty-sevenths are cut off by the second needle and one twenty-seventh by the third (four twenty-sevenths in all) and the stamp descends on the fifth type, and so on for all the remaining combinations taken in order; to make this clearer a table is annexed in which the figure *o* of the first three columns signifies that that needle under which it stands has not moved at all, *l* that it has moved to the left, and *r* that it has moved to the right: but the words at the foot of the columns refer to a subsequent page. These aggregate movements are obtained by the following methods. Out of a flat circular plate of metal on which is fixed a raised ring two segmentary pieces are cut, each containing an arc of rather more than ninety degrees, the

First Needle.	Second Needle.	Third Needle.	Distance cut off in twenty-sevenths.	Type on which the stamp descends.
o	*o*	*o*	*o*	1
o	*o*	*l*	1	2
o	*o*	*r*	2	3
o	*l*	*o*	3	4
o	*l*	*l*	4	5
o	*l*	*r*	5	6
o	*r*	*o*	6	7
o	*r*	*l*	7	8
o	*r*	*r*	8	9
l	*o*	*o*	9	10
l	*o*	*l*	10	11
l	*o*	*r*	11	12
l	*l*	*o*	12	13
l	*l*	*l*	13	14
l	*l*	*r*	14	15
l	*r*	*o*	15	16
l	*r*	*l*	16	17
l	*r*	*r*	17	18
r	*o*	*o*	18	19
r	*o*	*l*	19	20
r	*o*	*r*	20	21
r	*l*	*o*	21	22
r	*l*	*l*	22	23
r	*l*	*r*	23	24
r	*r*	*o*	24	25
r	*l*	*l*	25	26
r	*r*	*r*	26	27
First pair of Frames.	Second pair of Frames.	Third pair of Frames.		Key depressed.

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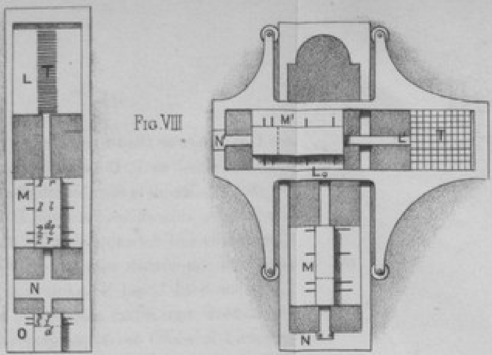


Fig. VIII

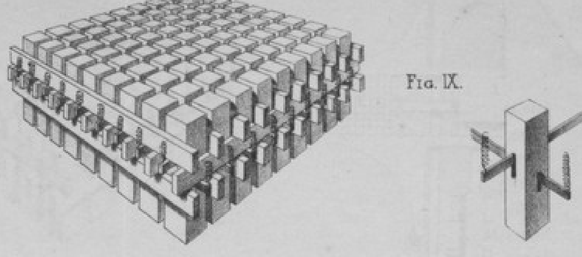


Fig. IX.

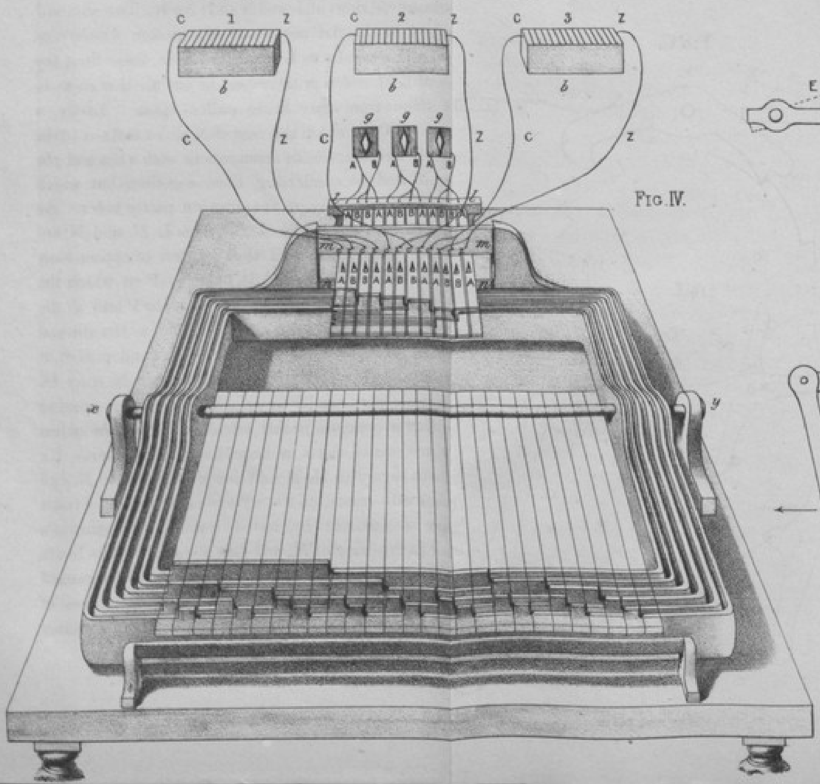


Fig. IV.

Fig. VII.

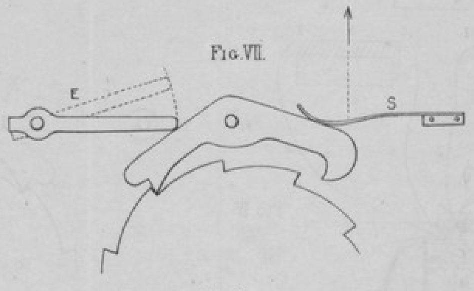
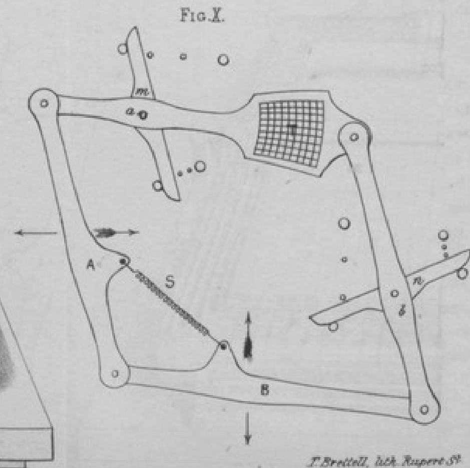


Fig. X.



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one piece L is little else than a piece of the rim with the ring upon it, and is fixed to P, the other piece N contains the centre of the circular plate from which it was cut and turns round the axis A while it lies flat upon P, hence N can be turned backwards and forwards about A until one or other of its edges come in contact with the corresponding ones of L. Lying upon both L and N and turning round the same axis A is another segmentary plate M, it has a broad ring running round it which is deeply grooved behind, in this groove the raised rings of L and N slide freely. Two slits will be observed in the flat portion of M, through which pins that are fixed severally to L and N protrude, these limit the motions of L M and N with respect to one another so as to prevent them from ever being pulled apart. Lastly, a grooved piece O in which the ring of N slides is fixed to the frame work, and there is an arrangement with a slit and pin between N and O connecting them together, but which however the perspective of the drawing partly hides. As the arm oscillates up and down the pieces L M and N are pulled out to their utmost and then left free to spring back again, L it will be recollected is fixed to P on which the spring acts, L therefore, connected as it is to P and to the type wheel, slides under and into M as far as the slit and pin allow it, then it carries M along with it and pushes it on to N, and then all three moving together N enter O. The action is very similar in principle to that of opening and shutting a common pocket telescope. Now the object of the three sets of slides is respectively to determine L's motion with regard to M, M's motion with regard to N, and N's motion with regard to O; this is easily done. First a diaphragm *d* indicated by dotted lines is fixed through the raised hollow ring of M, and also through O, this limits the action of L M and N, the slits being of course arranged so as to allow sufficient latitude of motion for the edges of the rings of L and M to reach the diaphragm; when therefore

R moves back P will follow it until L M and N have closed upon one another and upon O as far as these diaphragms permit, at this point P and the type wheel T stop, and the stamp S will descend upon a particular point of the latter, and that will indicate the position of the first type, it is the circular distance between this point and just behind that which lies beneath the stamp when M and N are pulled out to their furthest that has to be divided into twenty-seven parts. Across the raised rings of M and O run slides of the shape best shown in Fig. III., the notch in their bottom coincides with the section of the groove when the slides are pushed forwards so that the rings of L and M can run as freely as if no slides were there, but if any one of them be pushed backwards the groove is closed by it, and is blocked up as effectually as by the diaphragm *d*. This is the whole contrivance, the slides are numbered in Fig. II. according to the needles that severally act on them, thus 3 *l* means the third needle moving to the left. Hence from *d* to 3 *l* and from 3 *l* to 3 *r* is one twenty-seventh of the distance through which P moves; from *d* to 2 *l* and from 2 *l* to 2 *r* is three twenty-sevenths, or one-ninth; and from *d* to 1 *l* and from 1 *l* to 1 *r* is nine twenty-sevenths, or one-third. An arrangement of this sort works with extreme lightness and precision, the adjustments are very simple, for the face of any slide where the raised ring is liable to come against it can be filed until the distances are quite right; and lastly, the slides are very strong and simple, and cannot get out of order.

The method by which the slides are worked is shown most clearly by the diagram Fig. III.; the slides there represented are 3 *l* 3 *r* and 2 *r*; one end of each crank *d* is attached to a thread that reaches it from A; a spring at the lower part of this thread keeps the crank pressed against a limiting stop, *p;* when the thread is pulled the crank pushes the slide, when let go it springs back to its limiting stop. In order to replace it another crank, *i k*, works opposite; it turns round an axis W that moves in supports which are not shown, as they would hide the rest of the drawing. If now the arm of the crank, *i k*, be not rigid but a spring, then, if at the same moment an impulse is given both to it and to *d*, that spring will bend, and *d* will overmaster *i*, and the slide will be pushed forward. If an impulse be given only to *i*, then it will not bend, but *k* will push back the slide. Now a kind of reel, V, slides up and down the axis, in a groove round V all the arms *i*, *i*, lie. If then the reel V be depressed all the horizontal arms will also be depressed, and each crank will push back its opposite slide, unless an opposing crank *d*, pulled by its string, resists any of them, in which case these latter will not be pushed back but forwards, while all the others will be pushed backwards, the movement of the reel V is given by the rod R, which is worked by the same tooth, *c*, that works the stamp S. Now as R is on the opposite side of the wheel to S, then evidently inasmuch as S works at the moment that the arm K is at its highest, so R works when the arm K is at its lowest,—or in other words, when L, M, and N are pulled out to their utmost, and therefore when the slides are free to move. It must be remembered that at the commencement and completion of each movement there is a period of almost entire rest, quite enough so to allow of a quick impulsive movement, like that of the cranks, to act upon the slides. It will also be evident that all their mechanism offers no hindrance to the rapidity of making signals.

The needles have not to wait until the signal they have just made has produced its whole chain of effects before they can begin to work again, they continue their movements exactly as though no machinery were near them. There are two separate actions going on—the needle moving, and the mechanism working, the whole time taken up by each complete needle movement being the same as that of each complete mechanical movement, but they are contemporaneous, and not successive. The speed with which the axis is made to revolve certainly regulates that with which the needles may signal. But it can be made to revolve as slow or as quick within certain limits as we like. We, therefore, cause it to move as fast as the needles can with certainty and precision take their positions, the more uniformly it can be made to work the better, and we can of course choose any source of power we please to work it that we find convenient. The diagram Fig. V., gives a table of the movements of all these parts. No new method is here offered for the movement of the paper or the inking of the types; there are many plans well known to mechanicians of doing all this, and we have here no other suggestions to offer on the point, but at the same time we must express our conviction that these methods admit of very considerable improvement. We here close the description of the second division of the machine, namely, that whereby the movements of the needles, having been first transmitted to machinery result in the printing of letters, a different letter for each different compound signal, and it remains for us to show how we can determine the proper movements of the needles for any given letter by touching a key.

Fig. IV. represents this apparatus. On an axle x y, six frames, one within another, are capable of turning through a small angle; upon the same axle twenty-seven keys like those of a pianoforte also turn, and they each are intended to bear a different letter. In Fig. IV. their outlines alone are shown in order that the arrangement of the frame-work beneath them might be seen. Now each adjacent pair of these frames refer to a different needle, the first pair refer to the first needle, the second pair to the second needle, the third pair to the third needle; we contrive that the first of each pair on being depressed in front shall move the needle corresponding to it to the left, and we will, therefore, distinguish the first of each pair by the letter *l*, similarly the second of each pair moves the needle to the right, and we will call it *r*, while if neither one nor the other of the pair be moved, then the needle they correspond to remains still; so that by depressing not more than one out of each pair of frames, and by doing so in every possible manner, we command all the twenty-seven different needle signals. It will be seen in Fig. IV. how this is effected; properly arranged teeth stand up immediately beneath the keys and in contact with them, so that on any one key being touched only those frames are moved whose teeth are in contact with it. It will also be observed, by examining Fig. IV., that there the combinations are all gone through in order, so that on referring back to the table, page 14, and considering the contents of the columns to refer to what is written at the bottom of them, the effect of each key, as shown in Fig. IV., will be found there expressed; and by further inspection it will be manifest, that the result of depressing the first key, counting from the right hand, is that the first type shall be printed from, that of depressing the second the second, the third the third, and so on.

The way in which the necessary contacts are made is shown at the back of Fig. IV. Twelve metal springs, insulated from one another, are fixed to the bar *l l* and each spring has two metal points that are fixed opposite to one another, the one on the top of the spring and the other on the bottom of it, this latter is of course hidden by the drawing. A metal bar *n n* lies beneath these lower points, and a wooden bar *m m* which has metal tablets let into it lies above the upper ones, so that each spring when left to itself rests upon *n n*, but when pushed up about the eighth of an inch its point pushes against *m m*. Now *b b b* are supposed to indicate the batteries and *g g g* the galvanometers; wherever the letter C is found, the part it stands on is connected with the copper plate of its battery, and similarly Z with the zinc plate; in the same way A refers to the left-hand wire of the galvanometer, and B to the right-hand one. The first four springs refer to the first galvanometer and battery, the second four springs to the second, and the third to the third; considering then, any one of these sets of four springs each, if no one of the four be touched, then, lying as they all do in contact with the metal bar *n n*, A and B will obviously be in metallic contact. If however the left-hand pair be pressed against *m m*, and the right-hand pair be just lifted off *n n*, A will be put in contact with C, and B with Z; but if on the other hand the right-hand pair be pressed against *n n*, and the left hand pair be just lifted off *m m*, then A will be put in contact with Z, and B with C. All therefore that we have to do is to contrive that the springs in each set shall be so lifted when the frames corresponding to them are depressed in front by the keys, and this is done by filing away every portion of each frame that could possibly come in contact with the springs, leaving only a high tooth under those springs that it has to press against *m m*, and a low tooth under those which it has just to lift off *n n*. Hence, by depressing the first frame the first needle is moved to the left, by depressing the second frame the first needle is moved to the right, by depressing the third the second needle is moved to the left, and so on as it should be.

We have now sufficiently explained the essential parts of the Telotype, the description of them, like that of all other mechanism, is necessarily tedious, yet the conception of their action, when once clearly understood, is extremely simple. The instrument itself presents little or no difficulty of construction, there are no accurately fitted parts, no delicate adjustments in it, nothing in fact that requires superior workmanship; indeed the Telotype appears to belong peculiarly to that class of machines that are themselves readily made by machinery. Here then we end our description of its

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simplest form, it remains for us to explain certain additional parts which may or may not be used, but which if employed increase very considerably the efficacy of the instrument. They are as follows :—

Part for commanding a larger number of types than there are simple signals of the needles, so that on certain pairs of signals being made a third and a different letter shall be printed.

Part for determining that on making certain signals no letter shall be printed, but a mechanical effect shall be produced which may be applied as we will, to ring a bell, and so forth.

Part for regulating the movements of the paper carrier so that the printing may not be disconnected owing to the use of the above parts.

Part for enabling communications to be carried on in cypher, so that, two machines being adjusted according to the letters of a key word, messages may be signalled and printed as usual between them, but which shall appear in cypher at every other station.

Suggestions for enabling two needles to perform very nearly the work of the three in the manner they are employed above.

We have shown how by using three wires and three needles we can obtain command over twenty-seven different types, and this is certainly *sufficient* for most purposes, but it would vastly increase the perfection of the machine if we had at our disposal more of these. We want fair typography to convey our communications in, we want capital as well as common letters, we want commas, full stops, notes of interrogation, and so forth, we want the numerals, and, besides these, certain contractions would be very convenient. It is true that we might select say two signals out of the twenty-seven, which we will call respectively *u* and *v*, and agree that they should have no real meaning in them-

f. 15v

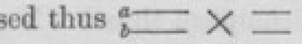
selves, but should change the meaning of the letter that was printed next after them ; so that, for instance, the two successive signals *u, a,* should stand for and represent A *u, b,* should represent B, and so on ; and, again, that *v, a,* should represent 1 ; *v, b,* 2, &c. ; but this would be a return to that symbolism which, in our instrument, we desire especially to avoid, and we will therefore explain a simple contrivance by which, when the two signals *u, a,* are made, A and nothing else but A shall be printed ; when *u, b,* are made B shall be printed, and so on. Of course, if we employed a fourth wire, and added on a fourth wheel with its rods to the axle X Y, and a fourth sliding segment of a circle to L M and N, we should gain exactly as much in variety of signals and types as we should by setting aside the two signals *u* and *v*, and using two successive signals to indicate each of the extra letters; indeed there are cases where, from having to communicate through very small distances, as from one part of a large building to another, such a plan might be a very advisable one to adopt, but generally speaking, each additional wire is a very serious inconvenience, and, therefore, one that we should use every endeavour to make ourselves independent of. The delay caused by the use of two signals for each of the extra letters would not tell much upon the whole communication, as those letters are much the less common ones, but, on the other hand, the convenience of having them is very great, and much confusion would constantly occur did we not possess them.

Fig. VI. represents the contrivance : a cylinder C turns on an axis, which is supposed to join on by means of a crank to X Y : a segment of a cylinder D is fixed below to the axis W, which, it will be recollected, is attached to and turns together with the type wheel. Certain rods, E F G H I, exactly similar to one another, and represented as five in number, lie between C and D, and owing to their balance their upper ends press against C, and their lower ends lie at a distance of about one-sixth of an inch apart from D. We have at present only to concern ourselves with the two left hand ones, E and F, the others, though acting just as they do, are used to fulfil another purpose. Along the cylinder C runs a notch *n*, and round the cylinder are rims *r r*, of the shape shown in the drawing. Hence the bent ends of the rods E F, when the notch comes round, would naturally fall into it, and then emerge out of it on the opposite side, on to the surface of the cylinder; but if either E or F be prevented from falling into the notch, then the rim *r* will pass beneath it, and the rod will lie upon the rim.

Now the motions of D being exactly the same as those of the type wheel, let there be two studs fixed upon D, so that when *u* comes under the stamp S, one of them comes under the arm E, and when *v* comes under the stamp S, the other stud comes under the arm F. Again, let the notch be so placed as to come under the upper ends of E and F, at exactly the same moment that the stamp falls. Hence E and F fall at the same time that the stamp does; and if either *u* or *v* have been signalled, then E or F, as the case may be, falls upon the stud and is checked by it, and in consequence gets forced up on to the rim. If any other signal has been made, both E and F will fall freely, and emerge from the notch, not upon the rim, but on the surface of the cylinder. Now the rims are continued concentric with the cylinder, about half-way round its circumference, and they end by turning upwards abruptly, forming a kind of a cam tooth, so that the instant that R is jerked by the tooth *c*, that same instant E or F, if either of them be upon the rim, will be jerked up also, and will act upon slides that move across an additional sliding segment of a circle to L M and N, and which we will call K, on exactly the same principles that the bent arms A A do, and of course at precisely the same time as they. The slides in K must lie apart from one another, at a distance one-third of that at which those of O lie, and by these means we obtain command over eighty-one different types, the extra fifty-four lying in pairs between each adjacent two of the original twenty-seven. To admit of this arrangement, the type-wheel and all the sliding segments should be increased exactly threefold of what they were obliged to be before, so as to give adequate room for the row of types and for the working of the slides: *u* and *v* are of course blank; no letter is impressed on the paper when they come under the stamp, their office is simply to cause the slides of K to work, and thus to influence the next letter. If the types lie thus in order: *a* A 1, *b* B 2, *c* C 3, and so on; and if *u* influences the slide nearest the diaphragm of K, and *v* influences the other, then evidently if we make the successive signals *u a*, A and A only will be printed; if *v*, *a*, 1 will be printed; if *u*, *v*, B; and so on.

We have now to show how that on making certain signals different mechanical effects can be produced, which may be applied to what purposes we will, and this is a power that we shall find to be very convenient, and to a degree even necessary. The rods G H I, a greater or less number of which might be employed, fulfil this end; for let studs which we will call respectively *g*, *h*, *i*, be fixed in such a manner upon D, that when the signals, say *v x*, *v y*, and *v z* are made, *g*, *h* and *i* shall severally come beneath G, H and I, which will fall upon and be checked by them on precisely the same principles that we have employed to work E and F; the strings that are attached to the arms above G, H and I, will therefore be pulled, and these will produce the required mechanical effects. By altering the shape of the rims we can obviously determine when and for how long, within certain limits, these actions shall occur and last so as to answer the desired ends. This contrivance can be employed to regulate the movements of the paper when compound signals are employed, for without some express regulator the paper, always moving one step for each signal, would cause in these cases a defect in the typography, as each new letter would be preceded by a space. We therefore fix beneath G not one but a row of studs, one corresponding to each new letter; hence, whenever *u* or *v* is signalled, the string in correspondence with G gets pulled, and by altering the shape of the rim from that shown in the drawing to one in which it shall rise to its full height immediately after quitting the notch, and then continue concentrically with the cylinder almost round to the notch again, the string will be pulled directly after the signal *u* or *v* has been made, and will continue stretched during the whole space, or nearly so, of one complete revolution of C, or that of one complete signal. Now, whatever plan may be employed for governing, generally, the movement of the paper carrier, let the method by which the power is applied be one which will admit of that movement being stopped without stopping the whole machine, and also let the catch by which it is permitted to move on step by step be like the one represented in Fig. VII. In this drawing Q is a rigid arm moving up and down once during each signal, and S is a spring that presses upon the detent; when then Q is lifted S urges the detent to follow it, and when Q descends it forces the detent back again, thus permitting the wheel that works the paper carrier to advance one step. Now if S be removed from off the detent, then when Q is raised the detent will not follow it, as there will be no spring acting to compel it to do so, and the paper carrier will accordingly remain still. A string is therefore attached at one end to S, and at the other to the rod above G, so when *u* or *v* is signalled the paper will advance one step, but when the next letter is signalled the detent refuses to work, and it will not advance another, and so the type descends where it ought to do, namely, on the space next adjacent to the letter that was previously printed. There are certain exceptions where it is advisable that the new letters should be preceded by a space, as in the case of capital letters, which beginning a word, require a space before them, and therefore in such instances no provision of this sort need be made.

The signal corresponding to *o, o, o,* should stop the paper carrier, so that when no message is passing the paper shall remain still, and we further require a blank type to put in the blanks between the words. These two, added to *u* and *v*, will together subtract four out of our twenty-seven simple signals, leaving us twenty-three for the immediate purposes of printing.

If the wires on leaving the contact keys were, any of them, crossed thus  it is obvious that signals passing through them would appear totally different at the distant station to what they were at the near one; but if the wires were again crossed (re-versed) at the distant station, then the signals would be put right again and become intelligible. In this way two telegraphs might evidently correspond freely with one another, while an interposed telegraph could not understand the messages that were conveyed through it. Now, supposing we use three galvanometers, and if the state of the currents passing through the uncrossed wires be indicated respectively by the letters $a\ b\ c$, and if when passing through crossed wires by the letters $a'\ b'\ c'$, then the different possible ways in which the currents sent through them could be modified by crossing, would be as follows, seven in number, or eight possible states in all—

$$a\,b\,c \quad a\,b'c \quad a'b\,c \quad a'b'c$$
$$a\,b\,c' \quad a\,b'c' \quad a'b\,c' \quad a'b'c'$$

and with reference to these we must make our apparatus. Now, if we have a table like that represented in Fig. XI.,

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with strips of metal so fixed into it that in each pair of lines those strips that are shaded are in metallic communication with each other, but otherwise insulated, and likewise those that are left white; and if each wire be divided in two and their ends attached to metal points, which press upon the strips along the lines, *m m* and *n n*, that is to say the six ends of the wires on the left, A_1 B_1 A_2 B_2 A_3 B_3 pressing along the line *m m*, and those to the right A'_1 B'_1 A'_2 B'_2 A'_3 B'_3 along *n n*, then obviously by pushing the slide backwards and forwards we can variously modify the connections between each of these pairs of wires and ensure any one of the eight different states, mentioned above, being assumed. Thus if we push the row of letters *a b c* up to the index none of the wires are crossed if the row *d e f*, then the connection of the wires, are those represented by the letters *a b c'* and so on; so suppose *k* to be chosen as a key letter, then the cypher-slides are pushed up till that letter comes beneath the index point at each of the communicating stations, the instruments then become *en rapport* and the signalling can proceed as usual, though unintelligibly to intermediate stations. Eight changes are, however, far too few for effectual secrecy, and we must, therefore, complicate our machine by using not a single "cypher-slide," but four or five, placed respectively along the slides of a prism, with an equal number of sides. These cypher-slides are, of course, to be differently adjusted. We take any word as a key word, say "Telotype." Then the first slide must be pushed up to the row of letters in which *t* lies, the second up to that in which *e* lies, and so on till the cypher-slides are all exhausted. This prism is urged to revolve round its axis by a force, but is held back by a detent, which lets it escape at the moment which elapses between the close of one signal and the commencement of the next, in fact, during the time that the tooth on the side of the wheel *w* breaks contact. This

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catch is, of course, worked by the Telotype machinery at the distant station, but by the keys at the near one. One cypher-prism is sufficient at each station, as the detent can readily be placed so as to be worked at pleasure either by the keys or by the machinery. The contact points are raised off the slides during the time that the prism is actually in motion by any simple contrivance as that in Fig. XIII.

We are compelled to combine with this a second apparatus so as to obviate the possibility of the communicating machines getting *hors de rapport* with each other. We must have a simple signal which, when made, shall readjust both cypher-prisms by bringing them round to their starting points whenever we please. This is easily done by the apparatus, Fig. XII. W is the catch wheel, t an arm fixed on to the face of it; d the detent, p p_1 p_2 are limiting stops standing out from a frame work, c c_1 c_2 are centres of motion round which the arm that works the detent, W, and also a large V shaped arm, that itself carries the detent, severally turn; this latter is pressed by a spring against p. If Q be not raised the detent works exactly as if Q were fixed, but if it be raised up to p_2 then the detent clears W and the tooth on the lower arm of Q checks t as it and the wheel run round; on lowering Q again the detent acts as before, but the prism has been brought back to its starting point. Now the signal that produces the mechanical effect of raising Q (by means, of course, of the mechanism described p. 24) should be blank, so that if we use it occasionally instead of the ordinary signal for putting in blanks between words, no time whatever is lost by employing it. Lastly, as to the best number of cypher-slides to use. If we have two, there are 8 × 8 or 64 different ways of adjusting the two instruments; if three, 64 × 8 or 512; if four, 4,096, or very near 5,000; and therefore, if five, 40,000. We should prefer this latter number; it is to all practicable purposes *perfectly* secure, while four slides would hardly be so. We have

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Fig. XI

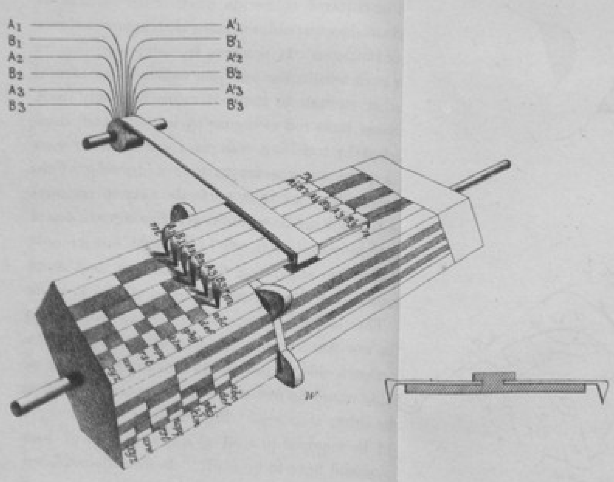


Fig. XIII

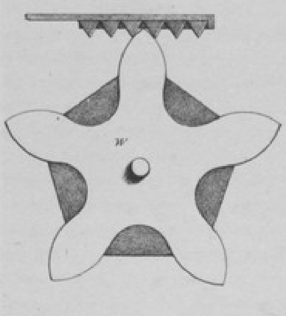
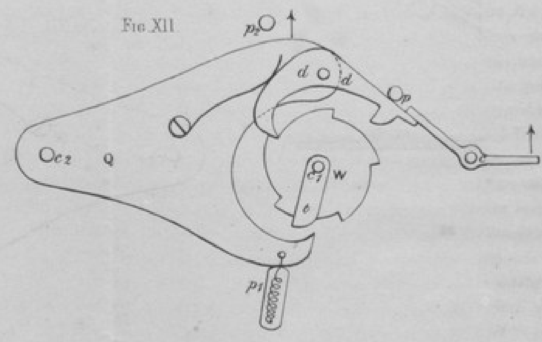


Fig. XII



T. Brettell lith. Rupert St.

f. 20r

therefore represented Fig. XI. as working a prism with five sides.

A very important question remains to be considered, it is whether in actual telegraphy two needles could not be made to do the work of the three in the manner they are employed above. The method we propose is one that appears to answer extremely well, but considerable experience would be required to decide whether the chances of error that it introduces would not more than counterbalance its obvious advantages. It consists in using two battery strengths for each needle, the one just sufficient to turn it freely, the other enough to make it move not only itself, but also a second light rod swinging by its side, and which itself is touched by touching rods that are made to work slides. It is quite unnecessary to give a drawing of the Telotype thus modified, as it is perfectly easy to conceive the instrument. Each needle will thus give five signals, one to the left, one strongly to the left, one to the right, one strongly to the right, and one still; and thus with two needles we obtain command over twenty-five signals, which is quite sufficient. The sliding segments would be reduced in number, O is not wanted, and N is fixed, there would be four slides on each side of the diaphragm in M; and those on the one side would be five times the distance apart that those on the other side are. For making contact eight frames would be required instead of six, and other obvious modifications would have to be made. Before telegraphing, we should find out by trial and then set apart the number of plates that would just be sufficient for the weaker battery, and use the whole of the battery plates for the stronger one. We sincerely trust that experiments will be made upon this point, which, though but shortly treated of here, is yet one of the most important.

With this our description of the Telotype ends, it remains only for us to offer a few additional remarks on the subject of compounding signals. There are many methods besides that described in Fig. IV. of doing this, it was given not as the one necessarily best in itself, but rather as that whose accompanying mechanism seemed most calculated to be represented in a drawing. Other methods are certainly simpler in themselves, but the machinery that works them is in those we have constructed too much involved to admit of the whole apparatus being exhibited in one single drawing. Now the sliding parts L, M, and N, instead of moving circularly round an axis, may be made to move as in Fig. VIII., between grooved rods in a straight line. And again to L another pair of grooved rods may be fixed cross-ways, between which a second system of sliding-parts L^1, M^1, and N^1, may also work. Hence a rectangular type-holder whose length is equal to the play of L^1, and whose breadth to that of L can have any part of its surface brought beneath a stamp that has been properly adjusted. In this way a great number of types can be packed in a small compass, and the play of the machine will be considerably diminished; thus, supposing each type to take up the sixth of an inch, then a row of an inch-and-a-half long would contain nine of them, and, therefore, eighty-one types could be packed into a frame only one inch and-a-half square. A good way of packing types is shown in Fig. IX. Each type is there fixed to the bottom of a separate rod, and the rods themselves fit into the square holes that are formed by thin bars crossing one another in opposite directions, these are ultimately secured by passing through the sides of a frame, but which for clearness sake is not represented in the drawing. By these means the types are restricted to movements in a vertical direction; and to keep them suspended slits are made crossways in each rod through which thin bars pass loosely, one bar through each row of rods looking in one direction, and also one bar through each row looking in the other, and the bars themselves are supported by springs. Hence, each type is doubly suspended, so that if the support of only one bar be taken away, the type rod will, nevertheless, be kept in place. Now if the stamp falls on one of these type-rods, it of course depresses it, and together with it the two bars by which it is supported, but obviously for the reasons just stated, no other type will be put out of place.

Such a type-holder is of course heavy, but it is very strong, and could hardly ever get out of order. There is however another method of impressing letters upon paper, namely, by punching the letters out of a thin sheet of metal, and inking the stamp, which is far lighter and more simple than the last one, but is difficult to be used without smearing the paper. If by any method this smearing could be avoided, a remarkably simple method of compounding signals might be employed, namely, that shown in Fig. X. It is only suitable to govern a light type bearer, as, from the number of joints in it, it would get strained by working a heavy weight; and, again, the type bearer being far from square, almost the only way of ensuring the right position of the types would be to punch them out after the rest of the machine had been completed, placing the punch in exactly the position that the stamp should itself occupy, and this is readily enough done when we have to treat with a thin sheet of metal. There is another advantage in the machine Fig. X., from its slides moving on fixed supports, it also works with the greatest precision, and its simplicity and lightness are great recommendations: A and B are two arms moving freely round the same axis C, to these other arms a and b are jointed, and again a and b are jointed together. To a, the type holder T is fixed. Hence the action of A is to move T backwards and forwards, that of B to move it up and down. We further compound the movements of the two ends of the bar m upon a, and that of n upon b, so that by using in all eight slides, and four diaphragms, T can be placed in nine different positions reckoning sideways, and also in nine different positions reckoning lengthways, or in eighty-one positions altogether. There are other methods of compounding signals, but they do not appear to be so useful as those we have mentioned above, and, therefore, do not require further notice here.

If telegraphs, that worked and printed satisfactorily, were once found practicable, most large houses, public and private, would soon become supplied with them. The communication being so immediate, answer following question as fast as it is put, affords much more nearly the advantages of a personal communication than the best regulated post office ever could. Any scheme to introduce telegraphs generally, would probably be first confined to London. There would be central offices, and from these bundles of wires would radiate to numerous branch offices; from the branch offices again wires would pass along the adjacent streets, and supply houses as they passed. The expense of distributing wires in this way could not be extreme, for, if the branch offices were as numerous as the branch post offices now are, the distance that the wires to each private house would have to traverse would never be great.

At this point we will conclude these pages, the result of many experiments, and we offer them to the public in a firm hope and belief that the contrivances here explained will be found fully capable of subduing that subtle agent—Electricity, to the better service of man.

FINIS.

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THE FIRST STEPS

TOWARDS

THE DOMESTICATION OF ANIMALS.

BY FRANCIS GALTON, F.R.S.

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THE domestication of animals is one of the few relics of the past whence we may reasonably speculate on man's social condition in very ancient times. We know that the domestication of every important member of our existing stock originated in pre-historic ages, and, therefore, that our remote ancestors accomplished in a variety of cases, what we have been unable to effect in any single instance.

The object of my paper is to discuss the character of ancient civilisation, as indicated by so great an achievement. Was there a golden age of advanced enlightenment? Have extraordinary geniuses arisen who severally taught their contemporaries to tame and domesticate the dog, the ox, the sheep, the hog, the fowl, the camel, the llama, the reindeer, and the rest? Or again, Is it possible that the ordinary habits of rude races, combined with the qualities of the animals in question, have sufficed to originate every instance of established domestication?

The conclusion to which I have arrived, is entirely in favour of the last hypothesis. My arguments are contained in the following paper; but I will commence by stating their drift, lest the details I introduce should seem trifling or inconsequent. It will be this:—All savages maintain *pet* animals, many tribes have *sacred* ones, and kings of ancient states have *imported* captive animals, on a vast scale, from their barbarian neighbours. I infer that every animal, of any pretensions, has been tamed over and over again, and has had numerous opportunities of becoming domesticated. But the cases are rare in which these opportunities can lead to any result. No animal is fitted for domestication unless it fulfils certain *stringent conditions*, which I will endeavour to state and to discuss. My conclusion is, that all domesticable animals of any note, have long ago fallen under the yoke of man. In short, that the animal creation has been pretty thoroughly, though half unconsciously, explored, by the every-day habits of rude races and simple civilisations.

Pets.—It is a fact familiar to all travellers, that savages frequently capture young animals of various kinds, and rear them as favourites, and sell or present them as curiosities. Human nature is generally akin: savages may be brutal, but they are not on that account devoid of our taste for taming and caressing young animals; nay, it is not improbable that some races may possess it in a more marked degree than ourselves, because it is a childish taste with us; and the motives of an adult barbarian are very similar to those of a civilised child.

In proving this assertion, I feel embarrassed with the multiplicity of my facts. I have only space to submit a few typical instances, and must, therefore, beg it will be borne in mind that the following list could be largely re-inforced. Yet even if I inserted all I have thus far been able to collect, I believe insufficient justice would be done to the real truth of the case. Captive animals do not commonly fall within the observation of travellers, who mostly confine themselves to their own encampments, and abstain from entering the dirty dwellings of the natives; neither do the majority of travellers think tamed animals worthy of detailed mention. Consequently the anecdotes of their existence are scattered sparingly among a large number of volumes. It is when those travellers are questioned, who have lived long and intimately with savage tribes, that the plenitude of available instances becomes most apparent.

I proceed to give anecdotes of animals being tamed in various parts of the world, at dates when they were severally beyond the reach of civilised influences, and where, therefore, the pleasure taken by the natives in taming them must be ascribed to their unassisted mother-wit.

I will, then, leave it to be inferred that the same rude races who were capable of great fondness towards animals in particular instances, would not unfrequently show a little of it in others.

North America.—The traveller Hearne, who wrote towards the end of the last century, relates the following story of moose or elks in the more northern parts of North America. He says, "I have repeatedly seen moose at Churchill as tame as sheep and even more so. . . . The same Indian that brought them to the Factory had, in the year 1770, two others so tame, that when on his passage to Prince of Wales's Fort in a canoe, the moose always followed him along the bank of the river; and at night, or on any other occasion when the Indians landed, the young moose generally came and fondled on them, as the most domestic animal would have done, and never offered to stray from the tents."

Sir John Richardson, in an obliging answer to my inquiries about the Indians of North America, after mentioning the bison calves, wolves, and other animals that they frequently capture and keep, says, "It is not unusual, I have heard, for the Indians to bring up young bears, the women giving them milk from their own breasts." He mentions that he himself purchased a young bear, and adds, "The red races are fond of pets and treat them kindly; and in purchasing them there is always the unwillingness of the women and children to overcome, rather than any dispute about price. My young bear used to rob the women of the berries they had gathered, but the loss was borne with good nature."

I will again quote Hearne, who is unsurpassed for his minute and accurate narratives of social scenes among the Indians and Esquimaux. In speaking of wolves, he says, "They always burrow underground to bring forth their young, and though it is natural to suppose them very fierce at those times, yet I have frequently seen the Indians go to their dens, and take out the young ones and play with them. I never knew a Northern Indian hurt one of them; on the contrary, they always put them carefully into the den again; and I have sometimes seen them paint the faces of the young wolves with vermilion or red ochre."

South America.—Ulloa, an ancient traveller, says, "Though the Indian women breed fowl and other domestic animals in their cottages, they never eat them: and even conceive such a fondness for them, that they will not sell them, much less kill them with their own hands. So that if a stranger who is obliged to pass the night in one of their cottages, offers ever so much money for a fowl, they refuse to part with it, and he finds himself under the necessity of killing the fowl himself. At this his landlady shrieks, dissolves into tears, and wrings her hands, as if it had been an only son; till seeing the mischief past mending, she wipes her eyes and quietly takes what the traveller offers her."

The care of the South American Indians, as Quiloa truly states, is by no means confined to fowls. Mr. Bates, the distinguished traveller and naturalist of the Amazons, has favoured me with a list of twenty-two species of quadrupeds that he has found tame in the encampments of the tribes of that valley. It includes the tapir, the agouti, the guinea-pig, and the peccari. He has also noted five species of quadrupeds that were in captivity, but not tamed. These include the jaguar, the great ant-eater, and the armadillo. His list of tamed birds is still more extensive.

North Africa.—The ancient Egyptians had a positive passion for tamed animals such as antelopes, monkeys, crocodiles, panthers, and hyenas. Mr. Goodwin, the eminent Egyptologist, informs me that "They anticipated our zoological tastes completely," and that some of the pictures referring to tamed animals are among their very earliest monuments, viz., 2000 or 3000 years B.C. Mr. Mansfield Parkyns, who passed many years in Abyssinia and the countries of the Upper Nile, writes me word, in answer to my inquiries, "I am sure that Negroes often capture and keep alive wild animals. I have bought them and received them as presents —wild cats, jackals, panthers, the wild dog, the two best lions now in the Zoological Gardens, monkeys innumerable and of all sorts, and mongoos. I cannot say that I distinctly recollect any pets among the *lowest* orders of men that I met with, such as the Denkas, but I am sure they exist, and in this way. When I was on the White Nile and at Khartoum, very few merchants went up the White Nile; none had stations. They were little known to the natives; but none returned without some live animal or bird which they had procured from them. While I was at Khartoum, there came an Italian wild beast showman, after the Wombwell style. He made a tour of the towns up to Doul and Fazogly, Kordofan and the peninsula, and collected a large number of animals. Thus my opinion distinctly is, that Negroes do keep wild animals alive. *I am sure of it;* though I can only vaguely recollect them in one or two cases. I remember some chief in Abyssinia who had a pet lion which he used to tease, and I have often seen monkeys about huts."

The most remarkable instance I have met with in modern Africa, is the account of a menagerie that existed up to the beginning of the reign of the present boy king of the Wahumas, on the shores of Lake Nyanza. Suna, the great despot of that country, reigned till 1857. Captains Burton and Speke were in the neighbourhood in the following year, and Captain Burton thus describes (*Journal R. G. Soc.*, xxix, 282) the report he received of Suna's collection. "He had a large menagerie of lions, elephants, leopards, and similar beasts of disport; he also kept for amusement fifteen or sixteen albinos; and so greedy was he of novelty that even a cock of peculiar form or colour would have been forwarded by its owner to feed his eyes." Captain Speke, in his subsequent journey to the Nile, passed many months at

Uganda, as the guest of Suna's youthful successor, M'tese. The fame of the old menagerie was fresh when Captain Speke was there. He writes to me, as follows, concerning it. "I was told Suna kept buffaloes, antelopes, and animals of all 'colours' (meaning 'sorts'), and in equal quantities. M'tese, his son, no sooner came to the throne, than he indulged in shooting them down before his admiring wives, and now he has only one buffalo and a few parrots left."

In Kouka, near Lake Tchad, antelopes and ostriches are both kept tame, as I am informed by Dr. Barth.

South Africa. The instances are very numerous in South Africa, where the Boers and half-castes amuse themselves with rearing zebras, antelopes, and the like; but I have not found many instances among the native races. Those that are best known to us are mostly nomad and in a chronic state of hunger, and therefore disinclined to nurture captured animals as pets; nevertheless, some instances can be adduced. Livingstone alludes to an extreme fondness for small tame singing birds (pp. 324 and 453). Dr. Kirk, who accompanied him in later years, mentions guinea-fowl,—that do not breed in confinement and are merely kept as pets,—in the Shiré valley, and Mr. Oswell has furnished me with one similar anecdote. I feel, however, satisfied that abundant instances could be found, if properly sought for. It was the frequency with which I recollect to have heard of tamed animals when I myself was in South Africa, though I never witnessed any instance, that first suggested to me the arguments of the present paper. Dr. Kirk informs me that, "As you approach the coast or Portuguese settlements, pets of all kinds become very common; but then the opportunity of occasionally selling them to advantage, may help to increase the number; still, the more settled life has much to do with it."

In confirmation of this view, I will quote an early writer, Pigafetta (Hakluyt Coll., ii, 562), on the South African kingdom of Congo, who found a strange medley of animals in captivity, long before the demands of semi-civilisation had begun to prompt their collection. The king of Congo on being Christianised by the Jesuit missionaries in the sixteenth century, "signified that whoever had any idols should deliver them to the lieutenants of the country. And within less than a month all the idols which they worshipped were brought into court, and certainly the number of these toys was infinite, for every man adored what he liked without any measure or reason at all. Some kept serpents of horrible figures; some worshipped the greatest goats they could get; some leopards, and others monstrous creatures. Some held in veneration certain unclean fowls, etc. Neither did they content themselves with worshipping the said creatures when alive, but also adored the very skins of them, when they were dead and stuffed with straw."

In Australia, where the natives rank as the lowest race upon the earth, Mr. Woodfield records the following touching anecdote, occurring in an unsettled part of West Australia, in a paper communicated to the Ethnological Society. "During the summer of 1858-9, the Murchison river was visited by great numbers of kites, the native country of these birds being Shark's bay. As other birds were scarce, we shot many of these kites, merely for the sake of practice, the natives eagerly devouring them as fast as they were killed. One day a man and woman, natives of Shark's bay, came to the Murchison, and the woman immediately recognising the birds as coming from her country, assured us that the natives there never kill them, and that they are so tame that they will perch on the shoulders of the women and eat from their hands. On seeing one shot, she wept bitterly, and not even the offer of the bird could assuage her grief, for she absolutely refused to eat it. No more kites were shot while she remained among us."

The Australian women habitually feed the puppies they intend to rear, from their own breasts, and show an affection to them equal, if not exceeding, that to their own infants. Sir Charles Nicholson informs me that he has known an extraordinary passion for cats to be demonstrated by Australian women at Fort Phillip.

New Guinea Group. Captain Develyn is reported (Bennett, *Naturalist in Australia*, p. 244) to say of the island of New Britain, near Australia, that the natives consider cassowaries "to a certain degree sacred, and rear them as pets. They carry them in their arms and entertain a great affection for them."

Professor Huxley informs me that he has seen sucking pigs nursed at the breasts of women, apparently as pets, in islands of the New Guinea group.

Polynesia.—The savage and cannibal Fijians are no exceptions to the general rule, for Dr. Seemann writes me word that they make pets of the flying fox (bat), the lizard, and parroquet. Captain Wilkes, in his exploring expedition (ii, 122), says the pigeon in the Samoon islands, "is commonly kept as a plaything, and particularly by the chiefs. One of our officers unfortunately on one occasion shot a pigeon, which caused great commotion, for the bird was a king pigeon, and to kill it was thought as great a crime as to take the life of a man."

Mr. Ellis, writing of these islands (*Polynesian Researches*, ii, 285), says, "Eels are great favourites, and are tamed and fed till they attain an enormous size. Taoarii had several in different parts of the island. These pets were kept in large holes, two or three feet deep, partially filled with water. I have been several times with the young chief, when he has sat down by the side of the hole, and by giving a shrill sort of whistle, has brought out an enormous eel, which has moved about the surface of the water and eaten with confidence out of his master's hand."

Syria. I will conclude this branch of my argument by quoting the most ancient allusion to a pet that I can discover in writing, though some of the Egyptian pictured representations are considerably older. It is the parable spoken by the Prophet Samuel to King David, that is expressed in the following words, "The poor man had nothing save one little ewe lamb, which he had bought and nourished up: and it grew up together with him and with his children; it did eat of his own meat, and drank of his own cup, and lay in his bosom and was to him as a daughter."

Sacred Animals.—We will now turn to the next stage of our argument. Not only do savages rear animals as pets, but communities maintain them as sacred. The ox of India and the brute gods of Egypt occur to us at once; the same superstition prevails widely. The quotation already given from Pigafetta, is in point; the fact is too well known to readers of travel, to make it necessary to devote space to its proof. I will, therefore, simply give a graphic account, written by M. Jules Gérard at Whydah, in West Africa. "I visited the Temple of Serpents in this town, where thirty of these monstrous deities were asleep in various attitudes. Each day at sunset, a priest brings them a certain number of sheep, goats, fowls, etc., which are slaughtered in the temple and then divided among the 'gods.' Subsequently during the night they (? the priests) spread themselves about the town, entering the houses in various quarters in search of further offerings. It is forbidden under penalty of death to kill, wound, or even strike one of these sacred serpents, or any other of the same species, and only the priests possess the privilege of taking hold of them, for the purpose of reinstating them in the temple should they be found elsewhere."

It would be tedious and unnecessary to adduce more instances of wild animals being nurtured in the encampments of savages, either as pets or as sacred animals. It will be found on inquiry that few travellers have failed altogether to observe them. If we consider the small number of encampments they severally visited in their line of march, compared with the vast number that are spread over the whole area, which is or has been inhabited by rude races, we may obtain some idea of the thousands of places at which half unconscious attempts at domestication are being made in each year. These thousands must themselves be multiplied many thousand-fold, if we endeavour to calculate the number of similar attempts that have been made since men like ourselves began to inhabit the world.

My argument, strong as it is, admits of being considerably strengthened by the following consideration:—

Menageries.—The natural inclination of barbarians is often powerfully reinforced by an enormous demand for captured live animals on the part of their more civilised neighbours. A desire to create vast hunting-grounds and menageries and amphitheatrical shows, seems naturally to occur to the monarchs who preside over early civilisations, and travellers continually remark that, whenever there is a market for live animals, savages will supply them in any quantities. The means they employ to catch game for their daily food, readily admits of their taking it alive. Pit-falls, stake-nets, and springes do not kill. If the savage captures an animal unhurt, and can make more by selling it alive than dead, he will doubtless do so. He is well fitted by education to keep a wild animal in captivity. His mode of pursuing game requires a more intimate knowledge of the habits of beasts than is ever acquired by sportsmen who use more perfect weapons. A savage is obliged to steal upon his game, and to watch like a jackall for the leavings of large beasts of prey. His own mode of life is akin to that of the creatures he hunts. Consequently, the savage is a good game-keeper: captured animals thrive in his charge, and he finds it remunerative to take them a long way to market. The demands of ancient Rome appear to have penetrated Northern Africa as far or further than the steps of our modern explorers. The chief centres of import of wild animals were Egypt, Assyria (and other eastern monarchies), Rome, Mexico, and Peru. I have not yet been able to learn what were the habits of Hindostan or China. The modern menagerie of Lucknow is the only considerable native effort in those parts with which I am acquainted.

Egypt.—The mutilated statistical tablet of Karnak (*Trans. R. Soc. Lit.*, 1847, p. 369, and 1863, p. 65) refers to an armed invasion of Armenia by Thothmes III, and the payment of a large tribute of antelopes and birds. When Ptolemy Philadelphus fêted the Alexandrians (*Athenæus*, v), the Ethiopians brought dogs, buffaloes, bears, leopards, lynxes, a giraffe, and a rhinoceros. Doubtless this description of gifts was common. Live beasts are the one article of curiosity and amusement, that barbarians can offer to civilised nations.

Assyria.—Mr. Fox Talbot thus translates (*Journal Asiatic Soc.*, xix, 124) part of the inscription on the black obelisk of Ashurakbal found at Nineveh and now in the British Museum. "He caught in hunter's toils (a blank number) of armi, turakhi, nali, and yadi. Every one of these animals he placed in separate enclosures. He brought up their young ones and counted them as carefully as young lambs. As to the creatures called burkish, utrati (dromedaries?), tishani, and dagari, he wrote for them and they came. The dromedaries he kept in enclosures, where he brought up their young ones. He entrusted each kind of animal to men of their own country to tend them. There were also curious animals of the Mediterranean Sea, which the King of Egypt sent as a gift and entrusted to the care of men of their own land. The very choicest animals were there in abundance, and birds of Heaven with beautiful wings. It was a splendid menagerie, and all the work of his own hands. The names of the animals were placed beside them."

Rome.—The extravagant demands for the amphitheatre of ancient Rome must have stimulated the capture of wild animals in Asia, Africa, and the then wild parts of Europe, to an extraordinary extent. I will quote one instance from Gibbon. "By the order of Probus, a vast quantity of large trees torn up by the roots were transplanted into the midst of the circus. The spacious and shady forest was immediately filled with a thousand ostriches, a thousand stags, a thousand fallow-deer, and a thousand wild boars, and all this variety of game was abandoned to the riotous impetuosity of the multitude. The tragedy of the succeeding day consisted in the massacre of a hundred lions, an equal number of lionesses, two hundred leopards, and three hundred bears." Further on, we read of a spectacle by the younger Gordian of "twenty zebras, ten elks, ten giraffes, thirty African hyenas, ten Indian tigers, a rhinoceros, an hippopotamus, and thirty-two elephants.

Mexico.—Gomara, the friend and executor of Herman Cortes, states, "There were here also many cages, made of stout beams, in some of which, there were lions (pumas); in others, tigers (jaguars); in others, ounces; in others, wolves; nor was there any animal on four legs, that was not there. They had for their rations, deer and other animals of the chase. There were also kept in large jars or tanks, snakes, alligators, and lizards. In another court, there were cages containing every kind of birds of prey, such as vultures, a dozen sorts of falcons and hawks, eagles, and owls. The large eagles received turkeys for their food. Our Spaniards were astonished at seeing such a diversity of birds and beasts; nor did they find it pleasant to hear the hissing of the poisonous snakes, the roaring of the lions, the shrill cries of the wolves, nor the groans of the other animals given to them for food."

Peru.—Garcilasso de la Vega (*Commentarios Reales*, v, 10), the son of a Spanish conqueror by an Indian princess, born and bred in Peru, writes, "All the strange birds and beasts which the chiefs presented to the Inca, were kept at court, both for grandeur and also to please the Indians who presented them. When

I came to Cuzco, I remember there were some remains of places where they kept these creatures. One was the serpent conservatory, and another where they kept the pumas, jaguars, and bears."

Syria and Greece. I could have said something on Solomon's apes and peacocks, and could have quoted at length the magnificent order given by Alexander the Great (Pliny, *Nat. Hist.*, viii, 16) towards supplying material for Aristotle's studies in natural history; but enough has been said to prove what I maintained, namely, that numerous cases occur, year after year, and age after age, in which every animal of note is captured and its capabilities of domestication unconsciously tested.

I would accept in a more stringent sense than it was probably intended to bear, the text of St. James, who wrote at a time when a vast variety and multitude of animals were constantly being forwarded to Rome and to Antioch for amphitheatrical shows. He says (James iii, 3), "Every kind of beasts, and of birds, and of serpents, and of things in the sea, is tamed and has been tamed by mankind."

Conditions of Domestication.—I conclude from what I have stated that there is no animal worthy of domestication that has not frequently been captured, and might ages ago have established itself as a domestic breed, had it not been deficient in certain necessary particulars which I shall proceed to discuss. These are so numerous and so stringent as to leave no ground for wonder that out of the vast abundance of the animal creation, only a few varieties of a few species should have become the companions of man.

It by no means follows that because a savage cares to take home a young fawn to amuse himself, his family, and his friends, that he will always continue to feed or to look after it. Such attention would require a steadiness of purpose foreign to the ordinary character of a savage. But herein lie two shrewd tests of the eventual destiny of the animal as a domestic species.

Hardiness.—It must be able to shift for itself and to thrive, although it is neglected; since, if it wanted much care, it would never be worth its keep.

The hardiness of our domestic animals is shewn by the rapidity with which they establish themselves in new lands. The goats and hogs left on islands by the earlier navigators, throve excellently on the whole. The horse has taken possession of the Pampas, and the sheep and ox of Australia. The dog is hardly repressible in the streets of an oriental town.

Fondness for Man.—Secondly, it must cling to man, notwithstanding occasional hard usage and frequent neglect. If the animal had no natural attachment to our species, it would fret itself to death, or escape and revert to wildness. It is easy to find cases where the partial or total non-fulfilment of this condition is a corresponding obstacle to domestication. Some kinds of cattle are too precious to be discarded, but very troublesome to look after. Such are the reindeer to the Lapps. Mr. Campbell of Islay informs me that the tamest of certain herds of them, look as if they were wild: they have to be caught with a lasso to be milked. If they take fright, they are off to the hills; consequently the Lapps are forced to accommodate themselves to the habits of their beasts, and to follow them from snow to sea and from sea to snow at different seasons. The North American reindeer has never been domesticated, owing, I presume, to this cause. The Peruvian herdsmen would have had great trouble to endure had the llama and alpaca not existed, for their cogeners, the huanacu, and the vicuna, are hardly to be domesticated.

Zebras, speaking broadly, are unmanageable. The Dutch Boers constantly endeavour to break them to harness, and though they occasionally succeed to a degree, the wild mulish nature of the animal is always breaking out, and liable to baulk them.

It is certain that some animals have naturally a greater fondness for man than others; and as a proof of this, I will again quote Hearne about the moose, who are considered by him to be the easiest to tame and domesticate of any of the deer tribe. Formerly the closely allied European elks were domesticated in Sweden, and used to draw sledges, as they are now occasionally in Canada; but they have been obsolete for many years. Hearne says, "The young ones are so simple that I remember to have seen an Indian paddle his canoe up to one of them, and take it by the poll, without experiencing the least opposition, the poor harmless animal seeming at the same time as contented alongside the canoe as if swimming by the side of its dam, and looking up in our faces with the same fearless innocence that a house lamb would." On the other hand, a young bison will try to dash out its brains against the tree to which it is tied, in terror and hatred of its captors.

It is interesting to note the causes that conduce to a decided attachment of certain animals to man, or between one kind of animal and another. It is notorious that attachments and aversions exist in nature. Swallows, rooks, and storks frequent dwelling houses; ostriches and zebras herd together; so do bisons and elks. On the other hand, deer and sheep, which are both gregarious, and both eat the same food and graze within the same enclosure, avoid one another. The spotted Danish dog, the Spitz dog and the cat have all a strong attachment to horses, and horses seem pleased with their company; but dogs and cats are proverbially discordant. I presume that two species of animals do not consider one another companionable, or clubable, unless their behaviour and their persons are reciprocally agreeable. A phlegmatic animal would be exceedingly disquieted by the close companionship of an excitable one. The movements of one beast may have a character that is unpleasing to the eyes of another; his cries may sound discordant; his smell may be repulsive. Two herds of animals would hardly intermingle, unless their respective languages of action and of voice were mutually intelligible. The animal which above all others is a companion to man is the dog, and we observe how readily their proceedings are intelligible to each other. Every whine or bark of the dog, each of his fawning, savage, or timorous movements is the exact counterpart of what would have been the man's behaviour, had he felt similar emotions. As the man understands the thoughts of the dog, so the dog understands the thoughts of the man, by attending to his natural voice, his countenance, and his actions. A man irritates a dog by an ordinary laugh, he frightens him by an angry look, or he calms him by a kindly bearing; but he has less spontaneous hold over an ox or a sheep. He must study their ways and tutor his behaviour before he can either understand the feelings of those animals or make his own intelligible to them. He has no natural power at all over many other creatures. Who, for instance, ever succeeded in frowning away a musquito, or in pacifying an angry wasp by a smile?

Desire of Comfort.—There is a motive which strongly attaches certain animals to human habitations, even though they are unwelcome: it is a motive which few persons who have not had an opportunity of studying animals in savage lands, are likely to estimate at its true value. The life of all beasts in their wild state is an exceedingly anxious one. From my own recollection, I believe that every antelope in South Africa has to run for its life every one or two days upon an average, and that he starts or gallops under the influence of a false alarm many times in a day. Those who have crouched at night by the side of pools in the desert, in order to have a shot at the beasts that frequent them, see strange scenes of animal life; how the creatures gambol at one moment and fight at another; how a herd suddenly halts in strained attention, and then breaks into a maddened rush, as one of them becomes conscious of the stealthy movements or rank scent of a beast of prey. Now this hourly life and death excitement is a keen delight to most wild creatures, but must be peculiarly distracting to the comfort-loving temperament of others. The latter are alone suited to endure the crass habits and dull routine of domesticated life. Suppose that an animal which has been captured and half-tamed, received ill-usage from his captors, either as punishment or through mere brutality, and that he

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rushed indignantly into the forest with his ribs aching from blows and stones. If a comfort-loving animal, he will probably be no gainer by the change, more serious alarms and no less ill-usage awaits him: he hears the roar of the wild beasts, and the headlong gallop of the frightened herds, and he finds the buttings and the kicks of other animals harder to endure than the blows from which he fled: he has peculiar disadvantages from being a stranger; the herds of his own species which he seeks for companionship constitute so many cliques, into which he can only find admission by more fighting with their strongest members than he has spirit to undergo. As a set-off against these miseries, the freedom of savage life has no charms for his temperament; so the end of it is, that with a heavy heart he turns back to the habitation he had quitted. When animals thoroughly enjoy the excitement of wild life, I presume, they cannot be domesticated, they could only be tamed, for they would never return from the joys of the wilderness after they had once tasted them through some accidental wandering.

Gallinas have so little care for comfort, or indeed for man, that they fall but a short way within the frontier of domestication. It is only in inclement seasons that they take contentedly to the poultry yards.

Elephants, from their size and power, are not dependent on man for protection; hence, those that have been reared from calves, and have never learnt to dread and obey the orders of a man, are peculiarly apt to revert to wildness if they once are allowed to wander and escape to the woods. I believe this tendency, together with the cost of maintenance and the comparative uselessness of the beasts, are among the chief causes why Africans never tame them now; though they have not wholly lost the practice of capturing them when full-grown, and of keeping them imprisoned for some days alive. Mr. Winwood Reade's recent account of captured elephants, seen by himself near Glass Town in Equatorial Western Africa, is very curious.

Usefulness to Man.—To proceed with the list of requirements which a captured animal must satisfy before it is possible he could be permanently domesticated: there is the very obvious condition that he should be useful to man; otherwise, in growing to maturity, and losing the pleasing youthful ways which had first attracted his captors and caused them to make a pet of him, he would be repelled. As an instance in point, I will mention seals. Many years ago, I used to visit Shetland, when those animals were still common, and I heard many stories of their being tamed: one will suffice:—A fisherman caught a young seal; it was very affectionate, and frequented his hut, fishing for itself in the sea. At length it grew self-willed and unwieldy; it used to push the

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children and snap at strangers, and it was voted a nuisance, but the people could not bear to kill it on account of its human ways. One day the fisherman took it with him in his boat, and dropped it in a stormy sea, far from home; the stratagem was unsuccessful; in a day or two the well-known scuffling sound of the seal, as it floundered up to the hut, was again heard; the animal had found its way home. Some days after, the poor creature was shot by a sporting stranger, who saw it basking, and did not know it was tame. Now had the seal been a useful animal and not troublesome, the fisherman would doubtless have caught others, and set a watch over them to protect them; and then, if they bred freely and were easy to tend, it is likely enough he would have produced a domestic breed.

The utility of the animals as a store of future food, is undoubtedly the most durable reason for maintaining them; but I think it was probably not so early a motive as the chief's *pleasure in possessing* them. That was the feeling under which the menageries, described above, were established. Whatever the despot of savage tribes is pleased with, becomes invested with a sort of sacredness. His tame animals would be the care of all his people, who would become skilful herdsmen under the pressure of fear. It would be as much as their lives were worth if one of the creatures were injured through their neglect. I believe that the keeping of a herd of beasts, with the sole motive of using them as a reserve for food, or as a means of barter, is a late idea in the history of civilisation. It has now become established among the pastoral races of South Africa, owing to the traffickings of the cattle traders, but it was by no means prevalent in Damara-Land when I travelled there twelve years ago. I then was surprised to observe the considerations that induced the chiefs to take pleasure in their vast herds of cattle. They were valued for their stateliness and colour, far more than for their beef. They were as the deer of an English squire, or as the stud of a man who has many more horses than he can ride. An ox was almost a sacred beast in Damara-Land, not to be killed except on momentous occasions, and then as a sort of sacrificial feast, in which all bystanders shared. The payment of two oxen was hush money for the life of a man. I was considerably embarrassed by finding that I had the greatest trouble in buying oxen for my own use, with the ordinary articles of barter. The possessors would hardly part with them for any remuneration; they would never sell their handsomest beasts.

One of the ways in which the value of tamed beasts would be soon appreciated, would be that of giving milk to children. It is marvellous how soon goats find out children and tempt them to suckle. I have had the milk of my goats, when encamping for the night in African travels, drained dry by small black children, who had not the strength to do more than crawl about, but nevertheless came to some secret understanding with the goats and fed themselves. The records of many nations have legends like that of Romulus and Remus, who are stated to have been suckled by wild beasts. These are surprisingly confirmed by Gen. Sleeman's narrative of six cases where children were nurtured for many years by wolves in Oude. (*Journey through Oude in* 1849-50, i, 206.)

Breeding freely.—Domestic animals must breed freely under confinement. This necessity limits very narrowly the number of species which might otherwise have been domesticated. It is one of the most important of all the conditions that have to be satisfied. The North American turkey, reared from the eggs of the wild bird, is stated to be unknown in the third generation, in captivity. Our turkey comes from Mexico, and was abundantly domesticated by the ancient Mexicans.

The Indians of the Upper Amazon took turtle and placed them in lagoons for use in seasons of scarcity. The Spaniards who first saw them, called these turtle "Indian cattle". They would certainly have become domesticated like cattle, if they had been able to breed in captivity.

Easy to tend.—They must be tended easily. When animals reared in the house are suffered to run about in the companionship of others like themselves, they naturally revert to much of their original wildness. It is therefore essential to domestication that they should possess some quality by which large numbers of them may be controlled by a few herdsmen. The instinct of gregariousness is such a quality. The herdsman of a vast troop of oxen grazing in a forest, if he sees one of them, knows pretty surely that they are all in reach. If they are frightened and gallop off, they do not scatter, but are manageable as a single body. When animals are not gregarious, they are to the herdsman like a falling necklace of beads whose string is broken, or as a handful of water escaping between the fingers.

The cat is the only non-gregarious domestic animal. It is retained by its extraordinary adhesion to the comforts of the house in which it is reared.

An animal may be perfectly fitted to be a domestic animal, and be peculiarly easy to tend in a general way, and yet the circumstances in which the savages are living may make it too troublesome for them to maintain a breed. The following account, taken from Mr. Scott Nind's paper on the Natives of King George's Sound, in Australia, and printed in the first volume of the *Journal of the Geographical Society*, is particularly to the point. He says: "In the chase the hunters are assisted by dogs, which they take when young and domesticate; but they take little pains to train them to any particular mode of hunting, After finding a litter of young, the natives generally carry away one or two to rear; in this case, it often occurs that the mother will trace and attack them; and, being large and very strong, she is rather formidable. At some periods, food is so scanty as to compel the dog to leave his master and provide for himself; but in a few days he generally returns." I have also evidence that this custom is common to the wild natives of all parts of Australia.

The gregariousness of all our domestic species is, I think, the primary reason why some of them are extinct in a wild state. The wild herds would intermingle with the tame ones, some would become absorbed, the others would be killed by hunters, who used the tame cattle as a shelter to approach the wild. Besides this, comfort-loving animals would be less suited to fight the battle of life with the rest of the brute creation; and it is therefore to be expected that those varieties which are best fitted for domestication, would be the soonest extinguished in a wild state. For instance, we could hardly fancy the camel to endure in a land where there were large wild beasts.

Recapitulation.—I will shortly recapitulate what appear to be the conditions under which wild animals may become domesticated:—1, they should be hardy; 2, they should have an inborn liking for man; 3, they should be comfort-loving; 4, they should be found useful to the savages; 5, they should breed freely; 6, they should be gregarious.

I believe that nearly every animal has had its chance of being domesticated, and that almost all of those which fulfilled the above conditions, were domesticated long ago. It would follow as a corollary to this, that the animal creation possesses few, if any, more animals worthy of domestication, at least for such purposes as savages care for.

Selection.—The irreclaimably wild members of every flock would escape and be utterly lost; the wilder of those that remained would assuredly be selected for slaughter, whenever it was necessary that one of the flock should be killed. The tamest cattle—those that seldom ran away, that kept the flock together and led them homewards—would be preserved alive longer than any of the others. It is therefore these that chiefly become the parents of stock, and bequeath their domestic aptitudes to the future herd. I have constantly witnessed this process of selection among the pastoral savages of South Africa. I believe it to be a very important one, on account of its rigour and its regularity. It must have existed from the earliest times, and have been in continuous operation, generation after generation, down to the present day.

Exceptions.—I have already mentioned the African elephant, the North American reindeer, and the apparent, but not real, exception of the North American Turkey. To these must be added the South African eland, which inhabits an area occupied by those very races whom I have shown to be remarkable for the absence of the habit of keeping animals alive. It is not, however, proved as yet that the eland is truly domesticable. I should also mention the ducks and geese of North America, but I cannot consider them in the light of a very strong case, for a savage who constantly changes his home is not likely to carry aquatic birds along with him. Beyond these few, I know of no notable exceptions to my theory.

To conclude. I see no reason to suppose that the first domestication of any animal, except the elephant, implies a high civilisation among the people who established it. I cannot believe it to have been the result of a preconceived intention, followed by elaborate trials, to administer to the comfort of man. Neither can I think it arose from one successful effort made by an individual, who might thereby justly claim the title of benefactor to his race; but, on the contrary, that a vast number of half-unconscious attempts have been made throughout the course of ages, and that ultimately, by slow degrees, after many relapses, and continued selection, our several domestic breeds became firmly established.

CATALOGUE

OF

MODELS

ILLUSTRATIVE OF

THE ARTS OF CAMP LIFE.

Westminster:

PRINTED BY THOMAS BRETTELL, RUPERT STREET, HAYMARKET.

1858.

J f. 2

CATALOGUE

OF

MODELS AND SPECIMENS.

FIRE :—

Lucifer Matches,........................	of wax. of wood.
Burning Glasses,........................	telescope lens. eye glass, (convex). pair of spectacles, (convex).
Sulphur Matches, or bits of stick with their ends dipped in sulphur.	sulphur melted on a hot pebble. do. in a bent up piece of tin. do. in a piece of ordinary paper.
Fire Sticks,........................	drill bow and string. drill stick of oak fire block of mulberry. do. of ivy.
Flints and their substitutes,.........	flints. quartz. rock crystal. granite. other siliceous stones. porcelain, various specimens.
Steels and their substitutes, ...	steels. pyrites. case hardened iron. clay or loam mould for making it. animal charcoal, do.
Tinders :—1st. Those which are not convenient to hold, and which therefore require a tinder box.	grass, &c., wetted in the mouth, and rubbed with gunpowder, and having grains of gunpowder scattered about it. cotton wool ; (when charred all the better). tinder of burnt rag, and how to make it in the open field. touchwood.

f. 3

Tinders :—2nd. Those that require no tinder box, but admit of being held between the finger and thumb together with the flint,	amadou. roll of cotton rag, with frayed ends. cotton wick. tin tube, convenient for holding a cotton wick and for protecting its charred end. roll of touch paper. pith, sewed in a sheath, with tube, &c., as sold in the shops.
Tinder Boxes, ...	North American, made of wood, and barrel shaped to give a good grasp to the flint. South African, brass and cylindrical, with a loose moveable bottom to push up the tinder. common leather case for holding a supply of amadou.
Saltpetre for improving tinder, ...	pure Saltpetre, and touchpaper made by rubbing it upon paper. ashes of tobacco, do.
Making a spark into a blaze by whirling, blowing, or holding up against the wind,	a nest ready for use, with a representation of the piece of lighted tinder inside it. materials for the nest, bark rubbed small between finger and thumb. do. grass. do. moss. do. lichen. do. a bit of string picked into oakum. do. shavings and wood scrapings. do. paper cut into very fine shreds. peat, one piece propped up nearly touching another, with the lighted tinder between. (To be blown upon, not whirled.) dry cattle dung, do. do.
Art of fire making, shews the different sizes of sticks that must be collected or prepared, before attempting to make a fire under ordinary bivouac circumstances.	sticks, the size of thin lucifer matches. do. of common lead pencils. do. of the finger. do. of an inch in diameter. logs.
Bivouac Fires, ...	Ovampo plan, useful where only dry grass and brushwood is to be met with. Swedish plan, where fir trees abound. ordinary bivouac fire. boat fire place.

WATER :—

Rain water, to collect, ...	a cloth stretched by its corners with a stone to weight it down in the middle, and to direct the droppings into a cup placed below.

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Dew water, to collect,	a sponge tied to a stick for gathering the drops off leaves, &c. a wisp of grass to whisk them down.
Saltwater, to distil,	a still made with a kettle and a bottle. do. with a pot and a gunbarrel.
Turbid water, to purify	Namaqua filter of a wisp of grass. handkerchief thrown over a dirty puddle, to be sucked through. two tubs, one of which is perforated and packed inside the other with moss, &c., between them. a bottle with a hole knocked in its bottom and loosely plugged up with sponge, &c. a flower pot, do. (These three last are to be plunged in the dirty water). a piece of alum, amply sufficient for a a bucket full of very turbid water. a bottle of dirty water that has been purified by alum. do. in which the mud has at length subsided of itself.
Filtering materials,	sand. charcoal. moss. grass, &c. sponge. flannel.
Digging wells,	digging stick used with the hand, no other instrument being employed.
Watering cattle from wells,	a trough made by laying a piece of any kind of cloth over a trench scraped in the ground. raising water with a pole and bucket.
Carrying water,	in an oil cloth laid in a basket. in a bladder carried by help of two skewers. in an intestine with a handkerchief rolled about it. way of tying the ends of do. so that the string shall not cut them. mending a torn water bag by plugging the hole. do. by tying up the hole. do., at leisure, by patching it. wax for waxing over the seams of leaky water bags; tallow.

FOOD AND COOKING :—

Cooking tents,	Gipsy fashion, 4 wands stuck in the ground, and blanketing skewered round them.

f. 5

Cooking places,........................	oven of rough stones plaistered over. loam and grass dome shaped; built in concentric rigs over a fire. meat buried under a camp fire. broiling slices between 2 large heated stones propped one above the other. soldiers cooking trench.
Portable food,........................	meat-biscuit. pemmican. Chollet's vegetables.
Tea and Coffee,........................	making tea in a muslin bag when there is a kettle but no teapot. metal gauze box, commonly sold for the same purpose. grinding coffee between two stones.
Copper pots to tin, ...	specimens of tinned copper. materials used, viz.: Sal Ammoniac, Resin, and Tin.

RAFTS AND BOATS:—

Rafts, ...	way of securing logs together with a small supply of cord. do. with saplings, and without cords, when there are means of boring holes. reed float.
Boats,..................................	way of burning out a tree. making a boat of reeds, or of the inside bark of trees, when no wood of sufficient buoyancy, or no tools are at hand. frame work for a commodious hide boat. canoes made of a single sheet of stout bark turned inside out.
Caulking seams,........................	materials used—old canvas. do. inner bark of trees. do. cord picked into oakum. do. grass, moss, &c.

CATTLE AND HARNESS:—

Enclosures, &c.,........................	a kraal of bushes for sheep or oxen. (for picketing horses, *see* ways of securing tent ropes).
Breaking in, ...	a nose stick and bridle for an ox. an ox packed, and with his nose bridle properly put on and secured.
Harness, &c.,...........................	pack-saddles—the "trees" made by lashing straight sticks crosswise. do. do. by sticks naturally curved and lashed lengthwise. a loop of tough wood as a substitute for iron packing rings. model shewing one half filled sack properly packed, and another one badly packed.

Vehicles,	sledges made of a forked branch. Indian horse trail. Indian dog trail.

WAYSIDE ARTS & DEVICES:—

Lifting Heavy Weights	alternate levering of ends and building beneath. combined force of boughs bent down. rude windlass of ropes or thongs, and 3 short poles.
Hides, &c.,	way of smoking small dressed skins, to make them resist wet.
Sinews and Intestines,	sinews made into thread. way of making catgut. do. of thread from inner membrane of intestine. bone awl for cobbling with.
Horns, Feathers, Hair,	horn that has been shaped after softening it in hot earth, as in that beneath a camp fire, or in hot ashes. pens made from raw quills, by dipping in hot sand as above. horse hair, its preparation for stuffing saddles, beds, &c.
Candles,	a strip of rag dipped into melted fat and wound round a cleft stick; it burns an inch a minute.
Soap making,	materials used, viz.:—ashes and fat. soap half made in its pot.
Lead castings by means of paper moulds placed in the ground,	metallic lead pencils. tablets of lead for writing or punching notices and inscriptions. slugs and shot (tables minced into squares, or rods into segments). lamps, inkstands, or other hollow vessels.
Blacksmith Work,	bellows made out of a goat-skin.
Carpentry,	felling trees, and shaping them with fire. seasoning green wood in a single night.
Guns,	to set a common gun as a spring gun. a very useful gun pricker and nipple charger, made with two quills, a stick, and a needle. guns, to sleep with when danger is urgent, (see Bivouac).

Marking the Track, ……………
- blazing trees to show the way.
- knotted twigs.
- small piles of stones.
- gipsy paterans, viz. :—handsfull of grass.
- do., a cross on the road with the long arm as a pointer.
- do., a twig laid in the cleft of a stick, planted against the hedge on the left hand of the way side, to be felt for on dark nights.

Signals, ……………
- a looking glass capable of flashing sun signals to a distance of ten miles, and prepared for use by having a hole cut through the back of its case, and an eye-hole scraped in its silvering.

Knots, ……………
- clove-hitch for a firm hold, very difficult to loosen.
- timber-hitch, for a secure hold while the strain is on, but which can be instantly thrown off on slackening.
- bowline, for tying loops, or for tying two loose ends together.
- knotting leather thongs together.

ENCAMPMENTS :—

Bivouac, ……………
- between blankets in a heap of cut heather.
- sleeping bag reaching to armpits, stuffed with dry grass, &c.
- ordinary sleeping bag.
- way of making a mattrass.
- sleeping with gun by side when danger is urgent.

Screens, ……………
- common screen of leafy boughs, &c.
- turf turned up on edge.
- cloth spread as an awning.
- substitute for uprights, a faggot of sticks, a musket.
- boughs laid on a rude prop of poles.
- wigwam.
- gipsy tent.

Huts, ……………
- framework of a common hut.
- inner bark used as string in making it.
- half hut, plaistered over.
- whole hut, do.
- do. do., raised on walls.
- reed hut.
- common underground hut.
- Kamstchatchan jourta.
- log hut.

f. 8

Different kinds of walls for huts,....
- wattle and dab.
- straw and reed walls.
- tarpaulin
- painted cloth.
- skins.
- boards hitched together, Malay fashion.
- reeds, do. do.
- do. double hitch.
- mat of reeds; the simplest form of bush loom.
- palisading.
- turf walls.
- sundried bricks.
- rough stone walls.
- bricks of peat.

Roofing, ...
- shingles of wood.
- bark.
- rude thatching.

Floors,..................................
- clay.
- wood-ashes and cowdung.
- paving.

Windows, ...
- shaping glass with a key.
- waxed cloth.
- intestines.

Tent ropes to secure in bad holding ground,..............................
- bushing tents.
- tying tent ropes to trees.
- weighting with stones, &c.
- backing tent pegs.
- objects to be buried a foot deep, viz. sticks.
- do. faggot of brush wood.
- do. a moderate sized stone.
- do. a handkerchief or cloth filled with sand.

Pitching Tents,........................
- regimental tent pitched in inclement weather, viz., sunken floor, low wall round, guy ropes, and double drain.
- regimental tent raised on walls; useful in hot climates.

Tent Pole, to mend, replace or lengthen ....
- way of securing two poles together by means of a "wolding stick."
- three poles tripod fashion, instead of one centre one.
- rope carried down from the overhanging limb of a tree.

f. 1.

ON

STEREOSCOPIC MAPS,

TAKEN

FROM MODELS OF MOUNTAINOUS COUNTRIES.

BY

FRANCIS GALTON, Esq., F.R.S.

ILLUSTRATED BY SPECIMENS PHOTOGRAPHED BY
ROBERT CAMERON GALTON, ESQ.

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AND CHARING CROSS.

f. 3r

STEREOSCOPIC MAPS.

A LARGE amount of theory and practical skill has been directed to the art of mapping mountainous countries, on an accurate and pictorial system; but the results are far from satisfying the every-day requirements of mountaineers and other travellers. The idea obtained from the best of these maps is considerably inferior to the knowledge gained by seeing a model.

There are serious obstacles to the complete success of the mapmaker in representing mountainous countries. Simple shading is too feeble an instrument to express gradations of relief, and the insertion of names interferes with the regularity of the shading. Contour maps are complete failures whenever crags and cliffs have to be represented, for the lines then become so superimposed as to be wholly unintelligible.

I have often had disagreeable experience of the inadequacy of maps to express the configuration of Alpine districts; and, on thinking how it could be remedied, the idea occurred to me of testing the effect of stereographs. I accordingly borrowed a few of the smaller and less delicate models from the collection of the Royal Geographical Society, and placed them in the hands of my cousin, Mr. R. Cameron Galton, who is an excellent amateur photographer, and who had kindly offered to assist me in carrying my object into effect. The result has been the production of the instructive specimens which we have exhibited to the Society.

It was not our aim to go to greater labour and expense than was necessary to show the complete feasibility of the idea. If larger models had been attacked, it would have been necessary to photograph them *in situ*, by erecting a stage above them, on which a camera could traverse in a vertical position. It would also have been necessary to have recourse to some special means of illumination. All this would have created an amount of labour and inconvenience which would, I believe, be henceforth well justified on the part of professional photographers, making stereoscopic maps for the purpose of sale, but which was in no way requisite to prove what I wished to maintain, namely, the effectiveness of this method of chartography.

It is not by any means necessary that these maps should be limited to the size of ordinary stereoscopic slides. A specimen is exhibited of the Island of St. Paul, taken in four quarters, in which the four pair of stereoscopic prints have been brought pretty closely together, both laterally and longitudinally, with good effect. If we call the upper quarters A 1, and A 2, and the lower quarters B 1, and B 2, and if we distinguish the left and right-hand halves of each stereoscope by the letters *l* and *r*, then the photographs have been pasted side by side, as in the upper part of the following diagram.

A 1 *l*	A 1 *r*	A 2 *l*	A 2 *r*
B 1 *l*	B 1 *r*	B 2 *l*	B 2 *r*
C 1 *l*	C 1 *r*	C 2 *l*	C 2 *r*
&c.		&c.	

The four middle squares forming an almost continuous photographic map, as shown in the small diagram to the side; of which either the left side, by itself; or the right side, by itself; may be viewed stereoscopically. For convenience of carriage, the right and left wings of the specimen I exhibit, have been made to fold over the middle part.

A 1 *r*	A 2 *l*
B 1 *r*	B 2 *l*

Though, theoretically, the eye-glasses of the stereoscope ought to be held exactly above the centres of each stereograph, yet, I find, that no such accuracy is needed in practice. The glasses may even be held over the line that divides one stereograph from that which lies next below it; for instance, over the line that separates the A's from the B.'s We might, therefore, prolong the map to any extent downwards, by annexing rows of C's below the B's; and of D's below the C's; and so on.

I also find that the glasses may be held somewhat out of their proper place, to *one side;* including, for example, a portion of A 2 *l*, and excluding a corresponding portion of A 1 *l*. It is now easy to apply the eyes to the stereoscope, in such a way (partly by withdrawing them to a trifling distance from it, and partly by not looking through the centre of the lenses) so as to limit the field of view, sufficiently to prevent the portion of A 2 that is seen by the right eye, being overlapped by anything seen through the lenses, by the left. There need be no conflict of images between A 1 and A 2. This operation is difficult to describe, but is very easy to recognise and also to effect in practice. Of the whole picture then in view, it is of course only a part that is seen by binocular vision, and therefore stereoscopically; nevertheless a stereoscopic *illusion* is insensibly conveyed to the remainder. This is exactly what occurs in ordinary vision. Only the middle belt of our ordinary field of view is seen by both eyes at the same time; as is instantly to be proved, by shutting first one eye and then the other. It will then be found that fully a sixth part of the field, on either side, has been seen by one eye alone; and that only four-sixths of the total view, have fallen within the range of binocular vision. Nevertheless, we are not conscious of any break in the stereoscopic effect. The stereoscopic illusion is carried on insensibly, principally through the medium of the perspective and shading, which remain unchanged. We are also quite unconscious of the presence of the object that limits the completeness of the true stereoscopic effect. This object is the nose, in ordinary vision; and the woodwork of the stereoscope, in the case we were describing. In either instance, the intervening object is thoroughly out of focus with the images on which our eyes are intent; and therefore its presence is the more easily to be ignored.

Owing to these properties, we are able to deal with models of very considerable dimensions both laterally and longitudinally. When such a model has been stereoscoped in separate squares, and the prints have been carefully united, it becomes possible to view *any* part of the large map with stereoscopic effect.

Two of the models—that of the Orteles Spitze and of the Island of St. Paul—are Austrian. They are accompanied by maps, prepared with signal success by Austrian artists, that may fairly be considered to represent the most advanced stage of map-making at the present day. A comparison of the stereographs, photographed from the same models that the map-maker endeavoured to represent, cannot fail to show the infinite superiority of the stereographs over the engravings. They belong to quite another order of representation. The delicacy of their detail is far superior to the workmanship of any engraver, and the vividness of their relief is absolutely startling.

The insertion of names necessarily obliterates so much of the surface as is occupied by the strokes of the letters, but it is no hindrance to stereoscopic effect. On the contrary, it is advantageous to it, and for the following reason:—When we look at a model tinted in a perfectly uniform manner, or in purely white plaster-of-Paris, so equally illuminated as to be affected by no shadow whatever, it appears to be flat and featureless. The eyes can select no points on which to converge or to focus themselves, and therefore the stereoscopic effect is *nil.* Under circumstances of ordinary illumination there are always some spots, peaks, or ridges, picked out by the lights and shadows, and therefore there is usually some appearance of stereoscopic relief. The total effect is, however, due to the shading, rather than to the true stereoscopic effect, as is evident from the fact that, whether we look at a purely white model with two eyes or with only one, there is little difference. But as soon as names, discolorations, or marks of any kind, however delicate, are made upon its surface, the case is altered. The eyes find numerous definite points to lay hold of, and the features of the model start into saliency. In illustration of this, I may mention it is a common remark, that the height of a small room appears notably diminished, when its ceiling is painted in a pattern. The fact being, that when the ceiling is of a uniform tint, no stereoscopic data exist, to enable us to estimate its distance from our eyes. Consequently the distance is indefinite, and we think nothing about it. But as soon as the ceiling has been painted in patterns, there can be no possibility of error, nor of forgetfulness of the real height of the room.

Contour lines may be drawn on the model, and will appear on the stereograph with good effect.

The size of an ordinary stereoscopic slide is very suitable for District Maps, such as are commonly inserted in guide-books, where they occupy a single octavo page. As the stereoscopic lenses usually magnify an object twofold, the apparent scale of an ordinary stereograph and of the map that fills an octavo page, are nearly identical. The stereograph of the Orteles Spitze well represents the character of such a district map, as may be found in the Swiss guides of Baedecker and Berlepsch.

Travellers who may hereafter use stereoscopic maps, need by no means burden themselves with box stereoscopes for the purpose of viewing them. The cheap and common little instrument, used like a double eye-glass, proves a perfect substitute in a skilful hand. It consists of two stereoscopic lenses, set into a thin strip of wood, and it will go with perfect ease into the waistcoat-pocket, if the handle be shortened or made to fold.

Numerous models of the more frequented mountain districts are already in existence, on a suitable scale for photography.

Many of them are large and heavy, much more important than those from which these stereographs have been taken. They are to be found in the collection of the Royal Geographical Society, in the Geological Museum in Jermyn-street, and in the South Kensington Museum, as well as in numerous other museums both in England and on the Continent. There are, in addition, a few models on a yet greater scale, that have been the labour of years to construct, and form sights that travellers delight to visit, such as that of the English lakes at Keswick, those of Switzerland at Berne, Zurich, Lucerne, and Geneva; and of the Pyrenees at Luchon. Unfortunately for the photographer, the majority of models are too highly coloured, and are placed in far too dark rooms for their convenience. But even these difficulties may be overcome when desired. So far as the models are painted in oil, they can be temporarily tinted with water-colour, to be afterwards sponged away, and the camera could be brought to bear upon them in the following manner:—A stage might be built round the models, like that erected by builders above the large works they are employed upon. A framework, holding the camera in a vertical position, looking downwards, would run laterally on a stage that itself moved longitudinally. This is precisely the same principle as that on which the builder's crane is constructed, by which it is enabled to be brought over any point that may be desired. Lines would next be drawn upon the model, dividing it into squares of a suitable size, and the camera would be brought over the centre of each of these squares in succession. The necessary illumination would be easily obtained by the magnesium light. When stereographs had been made and printed off, and had to be united, they would be cut with a free hand, following the lines which now, being represented in perspective, would cease to be straight. It is impossible that the adjacent squares, photographed from different points of perspective, should fit against each other with absolute accuracy, but the misfit is inconsiderable.

If the merit of Stereoscopic Maps should be generally recognised, we may expect that models will hereafter be made for the especial purpose of affording photographic copies; and that stereographs of all the frequented mountain districts and passes will become easily obtainable, to the great convenience of the annual ten thousands of summer tourists.

The Photographs exhibited are:—

1. Island of St. Paul, in four parts united together, from a bronze Austrian model.

2. Island of St. Paul, in a single slide, from a bronze Austrian model.

3, 4, 5. Orteles Spitze, differently marked, for names; from an Austrian model.

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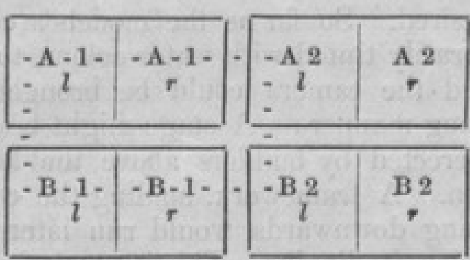

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SOME months back my cousin, Mr. Francis Galton, suggested the possibility of obtaining all the advantages of maps in relief, and other Geographical models, by reproducing them stereoscopically, and proposed to me to carry out this idea. He has obtained for me several models from the Royal Geographical Society, from which I have made the accompanying stereographs. They show all the sharp reliefs of the originals, while they are as portable as could be desired, and I believe this invention is calculated to be of immense value to travellers and others.

Ordinary maps, the very best, give but a very imperfect notion of the physical aspect of a country, while these give every variation of surface with the greatest distinctness. To travellers in mountainous lands it is of the last importance to be acquainted with the nature of the surface, and not less important is such knowledge for strategic purposes. These stereoscopic maps, from their extreme portability and their truthfulness, will, I am sure, open out a new field in the department of maps. I have, of course, met with some difficulties in producing these stereographs. Perhaps the most important has been that of inserting legible names. This difficulty applies chiefly to extremely mountainous countries, where there is but little level ground. In other cases it is scarcely felt. Several methods have been tried to evade the difficulty, and some have been represented in the different specimens. For instance, in the stereograph of the Orteles Spitze, No. 1, I have inserted numbers, and the Orteles Spitze, No. 2, letters of the alphabet, capital, and small, with a printed explanatory index on the back of each stereograph. In the Orteles Spitze, No. 3, I have stretched threads across the model at right angles to each other, like lines of latitude and longitude, with letters and numbers to designate the several squares, after a well-known method, as A 1, A 2, B 1, B 2, &c.; while, instead of an index of names, I have prepared a blank sheet of paper with similar and corresponding squares, letters, and numbers, and have inserted the names of the chief points of interest in their proper places, as shown by the squares; this sheet I have photographed and affixed to the back of the stereograph. In the stereograph of the Isle of Wight I have simply written the names on the surface.

In photographing these portable models, with an ordinary single-lensed camera, working on a stereoscopic slide, I have found it convenient to affix them to a vertical board propped up like an easel, but with a leg in front as well as behind. This position of the model is more manageable than any other, and the light can be arranged as well as, or better than, if the model were horizontal. I have used one of Ross's No. 1 carte de visite lens. In order to have the marginal definition clear, I have used a small diaphragm, No. 3, with an opening about half-an-inch in diameter, and consequently the time of exposure has been somewhat long, varying from one and a half to three minutes. Some of the models are a good deal discoloured by age and rough treatment; and in order to obtain sufficient contrast in the different parts, I have had either to recolour them with ordinary water-colours, or to coat them over entirely with white. For the latter purpose I have used kaolin, mixed with gum and water, with the best results; it gives a very perfect dead-white surface, and can, moreover, be removed with the greatest ease.

ROBERT CAMERON GALTON.

Feb. 14, 1865.

London: Printed by W. CLOWES and SONS, Stamford Street, and Charing Cross.

ON

STEREOSCOPIC MAPS,

TAKEN

FROM MODELS OF MOUNTAINOUS COUNTRIES.

BY

FRANCIS GALTON, Esq., F.R.S.

ILLUSTRATED BY SPECIMENS PHOTOGRAPHED BY
ROBERT CAMERON GALTON, ESQ.

[*Read before the Royal Geographical Society of London, on the 13th March*, 1865.]

LONDON: PRINTED BY WILLIAM CLOWES AND SONS, STAMFORD STREET,
AND CHARING CROSS.

STEREOSCOPIC MAPS.

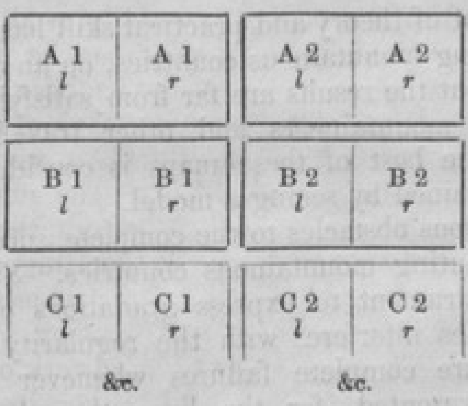
A LARGE amount of theory and practical skill has been directed to the art of mapping mountainous countries, on an accurate and pictorial system; but the results are far from satisfying the every-day requirements of mountaineers and other travellers. The idea obtained from the best of these maps is considerably inferior to the knowledge gained by seeing a model.

There are serious obstacles to the complete success of the map-maker in representing mountainous countries. Simple shading is too feeble an instrument to express gradations of relief, and the insertion of names interferes with the regularity of the shading. Contour maps are complete failures whenever crags and cliffs have to be represented, for the lines then become so superimposed as to be wholly unintelligible.

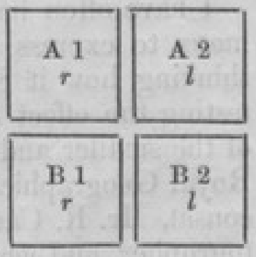
I have often had disagreeable experience of the inadequacy of maps to express the configuration of Alpine districts; and, on thinking how it could be remedied, the idea occurred to me of testing the effect of stereographs. I accordingly borrowed a few of the smaller and less delicate models from the collection of the Royal Geographical Society, and placed them in the hands of my cousin, Mr. R. Cameron Galton, who is an excellent amateur photographer, and who had kindly offered to assist me in carrying my object into effect. The result has been the production of the instructive specimens which we have exhibited to the Society.

It was not our aim to go to greater labour and expense than was necessary to show the complete feasibility of the idea. If larger models had been attacked, it would have been necessary to photograph them *in situ*, by erecting a stage above them, on which a camera could traverse in a vertical position. It would also have been necessary to have recourse to some special means of illumination. All this would have created an amount of labour and inconvenience which would, I believe, be henceforth well justified on the part of professional photographers, making stereoscopic maps for the purpose of sale, but which was in no way requisite to prove what I wished to maintain, namely, the effectiveness of this method of chartography.

It is not by any means necessary that these maps should be limited to the size of ordinary stereoscopic slides. A specimen is exhibited of the Island of St. Paul, taken in four quarters, in which the four pair of stereoscopic prints have been brought pretty closely together, both laterally and longitudinally, with good effect. If we call the upper quarters A 1, and A 2, and the lower quarters B 1, and B 2, and if we distinguish the left and right-hand halves of each stereoscope by the letters *l* and *r*, then the photographs have been pasted side by side, as in the upper part of the following diagram.



The four middle squares forming an almost continuous photographic map, as shown in the small diagram to the side; of which either the left side, by itself; or the right side, by itself; may be viewed stereoscopically. For convenience of carriage, the right and left wings of the specimen I exhibit, have been made to fold over the middle part.



Though, theoretically, the eye-glasses of the stereoscope ought to be held exactly above the centres of each stereograph, yet, I find, that no such accuracy is needed in practice. The glasses may even be held over the line that divides one stereograph from that which lies next below it; for instance, over the line that separates the A's from the B.'s We might, therefore, prolong the map to any extent downwards, by annexing rows of C's below the B's; and of D's below the C's; and so on.

I also find that the glasses may be held somewhat out of their proper place, to *one side;* including, for example, a portion of A 2 *l*, and excluding a corresponding portion of A 1 *l*. It is now easy to apply the eyes to the stereoscope, in such a way (partly by withdrawing them to a trifling distance from it, and partly by not looking through the centre of the lenses) so as to limit the field of view, sufficiently to prevent the portion of A 2 that is seen by the right eye, being overlapped by anything seen through the lenses, by the left. There need be no conflict of images between A 1 and A 2. This operation is difficult to describe, but is very easy to recognise and also to effect in practice. Of the whole picture then in view, it is of course only a part that is seen by binocular vision, and therefore stereoscopically; nevertheless a stereoscopic *illusion* is insensibly conveyed to the remainder. This is exactly what occurs in ordinary vision. Only the middle belt of our ordinary field of view is seen by both eyes at the same time; as is instantly to be proved, by shutting first one eye and then the other. It will then be found that fully a sixth part of the field, on either side, has been seen by one eye alone; and that only four-sixths of the total view, have fallen within the range of binocular vision. Nevertheless, we are not conscious of any break in the stereoscopic effect. The stereoscopic illusion is carried on insensibly, principally through the medium of the perspective and shading, which remain unchanged. We are also quite unconscious of the presence of the object that limits the completeness of the true stereoscopic effect. This object is the nose, in ordinary vision; and the woodwork of the stereoscope, in the case we were describing. In either instance, the intervening object is thoroughly out of focus with the images on which our eyes are intent; and therefore its presence is the more easily to be ignored.

Owing to these properties, we are able to deal with models of very considerable dimensions both laterally and longitudinally. When such a model has been stereoscoped in separate squares, and the prints have been carefully united, it becomes possible to view *any* part of the large map with stereoscopic effect.

Two of the models—that of the Orteles Spitze and of the Island of St. Paul—are Austrian. They are accompanied by maps, prepared with signal success by Austrian artists, that may fairly be considered to represent the most advanced stage of map-making at the present day. A comparison of the stereographs, photographed from the same models that the map-maker endeavoured to represent, cannot fail to show the infinite superiority of the stereographs over the engravings. They belong to quite another order of representation. The delicacy of their detail is far superior to the workmanship of any engraver, and the vividness of their relief is absolutely startling.

The insertion of names necessarily obliterates so much of the surface as is occupied by the strokes of the letters, but it is no hindrance to stereoscopic effect. On the contrary, it is advantageous to it, and for the following reason:—When we look at a model tinted in a perfectly uniform manner, or in purely white plaster-of-Paris, so equally illuminated as to be affected by no shadow whatever, it appears to be flat and featureless. The eyes can select no points on which to converge or to focus themselves, and therefore the stereoscopic effect is *nil.* Under circumstances of ordinary illumination there are always some spots, peaks, or ridges, picked out by the lights and shadows, and therefore there is usually some appearance of stereoscopic relief. The total effect is, however, due to the shading, rather than to the true stereoscopic effect, as is evident from the fact that, whether we look at a purely white model with two eyes or with only one, there is little difference. But as soon as names, discolorations, or marks of any kind, however delicate, are made upon its surface, the case is altered. The eyes find numerous definite points to lay hold of, and the features of the model start into saliency. In illustration of this, I may mention it is a common remark, that the height of a small room appears notably diminished, when its ceiling is painted in a pattern. The fact being, that when the ceiling is of a uniform tint, no stereoscopic data exist, to enable us to estimate its distance from our eyes. Consequently the distance is indefinite, and we think nothing about it. But as soon as the ceiling has been painted in patterns, there can be no possibility of error, nor of forgetfulness of the real height of the room.

Contour lines may be drawn on the model, and will appear on the stereograph with good effect.

The size of an ordinary stereoscopic slide is very suitable for District Maps, such as are commonly inserted in guide-books, where they occupy a single octavo page. As the stereoscopic lenses usually magnify an object twofold, the apparent scale of an ordinary stereograph and of the map that fills an octavo page, are nearly identical. The stereograph of the Orteles Spitze well represents the character of such a district map, as may be found in the Swiss guides of Baedecker and Berlepsch.

Travellers who may hereafter use stereoscopic maps, need by no means burden themselves with box stereoscopes for the purpose of viewing them. The cheap and common little instrument, used like a double eye-glass, proves a perfect substitute in a skilful hand. It consists of two stereoscopic lenses, set into a thin strip of wood, and it will go with perfect ease into the waistcoat-pocket, if the handle be shortened or made to fold.

Numerous models of the more frequented mountain districts are already in existence, on a suitable scale for photography.

Many of them are large and heavy, much more important than those from which these stereographs have been taken. They are to be found in the collection of the Royal Geographical Society, in the Geological Museum in Jermyn-street, and in the South Kensington Museum, as well as in numerous other museums both in England and on the Continent. There are, in addition, a few models on a yet greater scale, that have been the labour of years to construct, and form sights that travellers delight to visit, such as that of the English lakes at Keswick, those of Switzerland at Berne, Zurich, Lucerne, and Geneva; and of the Pyrenees at Luchon. Unfortunately for the photographer, the majority of models are too highly coloured, and are placed in far too dark rooms for their convenience. But even these difficulties may be overcome when desired. So far as the models are painted in oil, they can be temporarily tinted with water-colour, to be afterwards sponged away, and the camera could be brought to bear upon them in the following manner:—A stage might be built round the models, like that erected by builders above the large works they are employed upon. A framework, holding the camera in a vertical position, looking downwards, would run laterally on a stage that itself moved longitudinally. This is precisely the same principle as that on which the builder's crane is constructed, by which it is enabled to be brought over any point that may be desired. Lines would next be drawn upon the model, dividing it into squares of a suitable size, and the camera would be brought over the centre of each of these squares in succession. The necessary illumination would be easily obtained by the magnesium light. When stereographs had been made and printed off, and had to be united, they would be cut with a free hand, following the lines which now, being represented in perspective, would cease to be straight. It is impossible that the adjacent squares, photographed from different points of perspective, should fit against each other with absolute accuracy, but the misfit is inconsiderable.

If the merit of Stereoscopic Maps should be generally recognised, we may expect that models will hereafter be made for the especial purpose of affording photographic copies; and that stereographs of all the frequented mountain districts and passes will become easily obtainable, to the great convenience of the annual ten thousands of summer tourists.

The Photographs exhibited are:—

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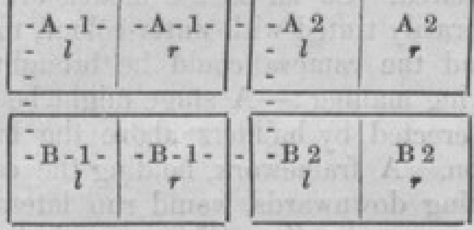

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ARTS OF CAMPAIGNING:

AN INAUGURAL LECTURE, DELIVERED

AT

ALDERSHOT,

ON THE OPENING OF HIS MUSEUM AND LABORATORY
IN THE SOUTH CAMP, V, Nos. 18 AND 20,

BY FRANCIS GALTON, ESQ.

LONDON:
JOHN MURRAY, ALBEMARLE STREET.
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BY THE SAME AUTHOR.

THE ART of TRAVEL; or, Hints on the Shifts and Contrivances available in Wild Countries. With Woodcuts, post 8vo, 6s.

Woodfall and Kinder, Printers, Angel Court, Skinner Street, London.

ARTS OF CAMPAIGNING.

GENTLEMEN, — I have invited your attendance this afternoon, that I might have an opportunity of explaining in a public manner the objects for which I have come here, aided and cordially recognised by Lords Panmure and Hardinge, and by the General commanding this camp. I do not presume to instruct you in military matters. I am a civilian; and it is only in that part of a soldier's life where his wants and methods of meeting them are necessarily identical with those used by civilians, that I claim any right to interfere. I shall keep aloof from all matters of drill and regimental discipline, and of that complex

system of organisation by which the members of a vast army are animated, every one of them, by the spirit of one commander, and confine myself entirely to civil matters. I only profess to teach those Arts and Contrivances which stand the soldier in stead when military organisation fails to help him. I wish to show how he may be self-sufficing and self-sustaining, as well as that efficient part of a great military machine, into which you, gentlemen, as officers, take care that he is fashioned. We all acknowledge how justly proverbial are the chances of war, and we know that every soldier who enters the field risks each day of his life a chance of being thrown, in some degree, on his own resources. There is not a chapter in the history of war, whether it be in modern or ancient times, where we are not met with that dread of all Generals, "deficiency of supplies;" at one time it is a deficiency of food, at another, of tools and utensils. At another time, it is of skilled

f. 4r

labour; at another, of raw materials to work upon. Under these circumstances, the soldier must either sit still and suffer, or else he must bestir himself to meet the difficulty. No military organisations will help him; the existence of any want at all proves that organised systems have failed in supplying that want; and therefore if the soldier is without skill to shift for himself, and to supply with his own brain and hands what is wanting, he must go without it. Now, so far as he is liable to be thrown upon his own resources, to exactly the same degree does he want that kind of knowledge and dexterity which has nothing to do with matters of drill and discipline, but which is precisely what persons who are the very opposites of soldiers—I mean travellers in rude countries, emigrants, missionaries, and so forth—have to make an actual profession of. The soldier, as such, depends upon a system; he is part of a great machine, that does its work efficiently, just as all other

machines do, by each part of it having some appointed and special work: a traveller or an emigrant is not part of a system; he is all in all to himself, and cannot make sure of indulging in a single comfort which his own hands are unable to procure.

The soldier, I say, is part of a machine that does its work admirably so long as its pieces are whole and in gear, but which is liable to give way under pressure. The emigrant, though he gets through his work in a rough sort of way, and often with great waste of labour, depends solely upon himself and not upon others; and, therefore, when organisation breaks down, it is then that qualities such as he possesses are able to come in and shield the soldier from disaster.

Now there is nothing in the professional education of military men to teach them this knowledge, which is the life-stay of travellers and emigrants. And yet it is an art most highly prized by all campaigners. All soldiers

who spend years in the field learn much of it, but they do so under the occasional teachings of hard necessity, which most likely have proved fatal to an equal number of their comrades, whose constitutions had sunk under the pressure of hardships which these had survived, but which neither of them had skill to evade, or knowledge to resist. Now, what I want, is, to do away with the necessity of this cruel apprenticeship, and by varied instruction in this camp, to enable the young soldier to take the field with no small portion of the knowledge of the oldest campaigner. It is your science, gentlemen, that explains the art of handling men in masses, of organising, and of commanding. My science is a humbler one, and must be classed at a very distant point on the scale of knowledge; but, gentlemen, I protest against its being disregarded; and I am sure you will agree with me that an army of soldiers, each man of whom can take care of himself, is in a far more efficient

state than another composed of men no better drilled, while individually they are almost helpless. In easy-going times there may be little apparent difference between the two; but it is when supplies fail, and the army is harassed, and the weather is cruel, and calamities fall thick, that the knowledge I profess to teach, and which I earnestly urge you to learn, is the raft that saves. Nay, I may compare the army that can depend, not on its organisation alone, but also on the self-sufficiency of every unit that forms it, to a life-boat: she will sail and row as well as another, and yet you may water-log her, but you cannot sink her.

I do not profess to explain the complicated processes of manufacture used in civilised countries, but I aim at showing all those ways of obtaining necessaries and what are well called "necessary comforts," that a man may practise when encamped out in the field. And I may remark that this art is more pe-

culiarly practised by a traveller than by a settler, for the reason that a settler has plenty of time, and can accumulate materials and stock ; but a traveller is always on the move, and is able to carry little or nothing with him ; the ingenious combination by which he may have supplied any want of yesterday has been left behind him, and he must re-make it to-day if the want recurs. In stating the case broadly, I might say that the settler makes what he may want durably, and once for all ; the traveller meets each want as it arises. The methods of the latter are much simpler and more generally available than those of the former, who, in a rude way, imitates the processes of civilisation. The settler who has a bench to carpenter upon, a forge to smithy with, a log shanty over his head, a fire-place and pots to cook with—rude though all these things may severally be, looks upon the arts of life, and on the ways of satisfying his wants, in quite a different light to a traveller who un-

packs a load of some two hundred weight from his horse's back, in which all his available possessions in the world are contained, and on which he had been living for months, and with which, as a nucleus, he is everywhere able to make himself perfectly at home, and to surround himself with improvised and ample comforts.

I will now attempt to state, in general terms, the methods of instruction that I shall endeavour to pursue.

You must not think, gentlemen, that any person who has once roughed it becomes a proficient in this art of campaigning. It has struck me forcibly during the years in which I have made a study of it, how ignorant the inhabitants and bushrangers of each rude country are of the shifts and contrivances used by those of others, and with what great advantage persons who have roughed it in different lands could combine their information. In reading works of travel, which I

have long done with a view to extracting hints that might help me in systematising a science of campaigning, I cannot tell you how often it has struck me that the narrators have been baffled by difficulties under circumstances which, though unusual to them, are very common to people in other countries, who have their own ingenious ways of surmounting them. And still more frequent is it to read of travellers recording, with the utmost delight, and for the good of others, ingenious discoveries which they had made, after months of inconvenience to themselves, and which not only one or two, but more, previous travellers had equally puzzled out and similarly recorded with a natural satisfaction as their own original inventions. Now it was after travels long since undertaken by me, that I was so much impressed with the advantage of collecting the experiences of all rude nations, and of as many travellers as I could, that when I was carrying on my ex-

plorations of South Africa in 1850–2, I began to plan the making of such a collection: and there as I rode along, I used to jot down all that I could think of, of African and other experiences. This formed a nucleus, to which, since my return in 1852, I have steadily added, reading and inquiring in all directions, and lastly, publishing a little book called the "Art of Travel," which I know that some of you have seen, in which was inserted what I had then to say, so far as I could arrange it in some sort of order, and at no great length. It has since done me good service in making my objects more widely known, and in inducing persons to kindly forward to me for further publication such of their original experiences as I had been ignorant of, and had therefore left unrecorded. I mention this, gentlemen, to show what kind of method I have been pursuing for my own instruction, and for the service of those who, as I have done, may be required to rough it,

f. 8r

whether they be soldiers, or whether they be civilians. I wish to show to you how I have made a special study of the matters which I profess to teach. Would that I were better informed upon them, to do you and the subject more justice. But as it is a new thing to endeavour to unite and systematise the numerous and widely-dispersed fragments of the information in question, I trust you will bear with me, and also make allowance for the confessedly incomplete method in which I may handle the subject, on the score of the really earnest way in which I have striven to improve my knowledge, and make it of effect.

Now, as to the means through which I propose to work. I desire to offer opportunities to all officers who choose to accept them, by which they may learn these things, and acquire skill to practise them. I wish to convey knowledge and manual dexterity, for neither without the other will ever be of much avail, and I propose to do it in this

f. 8v

way: I shall rapidly increase the collection of sketches now begun (and I beg you to bear in mind, with regard to its embryo condition, that it is only a short time since Lords Panmure and Hardinge have been pleased to accept my proffered services), and shall continue adding to them so long as the present season lasts; they will be arranged in my hut, with explanatory foot-notes and references, and form as complete a museum of the shifts and contrivances of camp life, as the time and my abilities and the nature of the case will allow me to make; I shall also have a small collection of books of reference on matters kindred to these things, and I invite officers to visit this museum and make what use of it they like, as copying the pictures, applying for information on them, and reading the books. It will, in the first instance, be open from half-past one to half-past six o'clock every day. It is my sincere endeavour to afford every facility and comfort to persons earnestly study-

f. 9r

ing these things. Next as regards teaching the *hand*. I am collecting a motley stock of very simple tools and raw material, planks, logs, twigs, canvas, cloths, and every single thing necessary for making with the hand those very things that you will see pictured in the museum; I urge you to come and make use of them. In the palisadoed plot of ground, between the huts, you can sit and work just as roughly as you would in the Crimea, and you will from time to time have intelligent workmen to assist you in your difficulties, and explain the use of the tools you work with. I particularly hope that those who have any mechanical aptitude will give some of their leisure hours to these occupations, and avail themselves of the present opportunity. I beg you to come and make the most of it; I am sure there is plenty of interesting and rational occupation to be found by doing so; believe me, it will often stand you in good stead, whether in the field or in

f. 9v

houses. We ought all to know these simple matters of handicraft; nearly all of us have wished that at some time or other an opportunity had been offered us of learning them. The expense will be a mere nothing; I only require that injured tools be replaced, and record, but seldom expect to enforce, the principle that all raw materials used be paid for. I mean, as to this latter proviso, that three-fourths of the material used by each person will be of too little worth to be valued at all, but when more expensive things are wanted, as canvas, leather, &c. (if I have them, and if you come to me to furnish you with them), I shall expect them to be paid for before they are served out. I must beg you to recollect, that although these huts and some of the furniture in them has been provided by Government, yet that I possess no guarantee that any expenses I have, or shall have, incurred in setting this undertaking on foot, or in keeping it in action, will ever

be allowed me. And, therefore, although you will always find me ready to supply every accommodation that may prove needful in giving effect to this scheme that I have gratuitously undertaken, you must not be surprised if I do not cater for wants before they arise, by bringing down at once a complete establishment of artificers, with expensive supplies of tools. Indeed, the more that you make serviceable makeshifts out of worthless raw materials and with common tools, the more thoroughly shall I feel that you sympathise with the spirit in which I myself undertake this matter.

And, gentlemen, let me remark how advantageous it must prove, if such of you as are destined to active service, and who having happened to have learnt any particular art, as drawing, turning, carpentering, and so forth, will also practise here, at Aldershot, how to make for yourselves all the tools and appliances required for that art. In this way,

when you are far from shops and without the materials you have been accustomed to use, you will know how to procure them for yourselves, and how to furnish yourselves with those very articles for the want of which the knowledge you had at much pains acquired, would lead to no result. I wish to induce you to supply yourselves with that wanting link which is able to connect the arts and handicrafts, learnt in civilised life, with those available in a temporary and ill-provided encampment. Thus, in drawing, I have little doubt that I am now addressing many gentlemen, highly accomplished in that art, who yet have no knowledge how to *size* common paper, and make it fit to take the sharp strokes of a pen; whose paper being greasy and useless for want of *ox-gall*, they do not know how to procure it; whose stray leaves of paper they have never practised making into a handy and efficient *block*. Nay, who cannot make a moderately good *paint-*

f. 11r

brush for themselves, nor a good substitute for writing or Indian *Ink*, nor a good writing *pen* out of a plucked quill feather. Yet all these things, and many more besides, referring to the same art, can be made at all times, with a simplicity and a readiness astonishing to those who are used to think that manufactories are necessary for producing every manufacture they see. There is no habitable country so wild and so inhospitable as not frequently to afford ample materials for making each thing that I have mentioned. But unless we learn to draw our supplies direct from nature, and not through the medium of manufactories, we may sit with our hands folded in unwilling idleness, and complaining of want when we are really in the midst of abundance, and surrounded by opportunities of using them. So with the Carpenter: he may be an excellent workman in London, but useless in the field; for he may have nothing but growing trees at hand, and yet not know how to season green wood to a sufficient degree for working, in a

single night; he may not be enough of a blacksmith to repair or make his awls and other small tools, when he has nowhere to buy them; he may become disheartened because he has no nails, and is unpractised in using substitutes for them; he may be without proper tools at all, and be unable to teach others how to fell and rudely to fashion trees for his use by means of fire, directing the encroachments of the flame by judicious scrapings and quenchings; it is possible he may not be able to soften the temper of his axe when he finds it shiver against the hard wood which alone he can procure to work with, or do other of the many matters which are quite necessary that he should be an adept in, before he is fitted to take the field, but which I should only weary you now by recounting more at length. Again as to the Turner, the last of those I mentioned: he is helpless without a lathe; and that simple form of one which may be joinered up in two hours at the foot of any tree, which was, I believe, generally used

up to the last half-century, which is still used everywhere in Italy, has been quite forgotten by us, though by forgetting it we are made dependent on manufactories, and lose the power of making a lathe for ourselves, quite good enough to give useful results wherever and whenever we please.

Now, gentlemen, I say that those among you who have already been at the pains to acquire any art, should take the next easy step further, and acquaint themselves with the rough and ready way that they will have here an opportunity of practising, of making their tools and other appliances. Whatever a soldier has learnt, let him also learn what little more is necessary to make his knowledge available whenever he may be called upon to leave the regions of shops and houses, and to take up his quarters in the open field. When any of you require to learn what neither I nor the workmen then with me can show you how to perform, do not be disheartened, but make experiments, have full

confidence in yourselves, determined not to be beaten by difficulties, consult what books may be at hand, and the chances of success will be strong in your favour. I hope that these huts may be looked upon more as a laboratory where learners may teach themselves, which is the best kind of learning,—rather than as a place where they are formally taught. I wish to make it a kind of head-quarters of the knowledge of those shifts, contrivances, and handicrafts that are available in camp life ; and I call upon you to help me with your assistance. Write to your friends from the Crimea, or from the bush, who take an interest in these things, get hints of original experiences from them, and communicate them to me ; they will not lie idle, but will at once be turned to account in increasing a store already large, and will remain recorded in pictures or in models for the good of ourselves and all who follow us. In so far as I may feel called upon to instruct, otherwise than in showing readiness to explain and converse about whatever is shown in

this little museum and laboratory, you must allow me to feel my way gradually, and not to offer you an exact programme at the outset; much will depend on the number of officers whom I may succeed in attracting to a somewhat earnest and steady pursuit of the Art of camp life. Possibly occasional lectures may be convenient, especially if combined with field excursions. Those who wish to work can keep any bags or boxes in my second hut. The tools that I shall furnish them with will be of the simplest description; but if any of you know how to use others, and choose to provide yourselves with them, you can bring them here, and keep them locked up in safety. I shall endeavour to form classes, and to have a clever workman here for the half-day to teach each class. I fear there is hardly an officer in camp who, if all his cooking utensils were broken, or had been dropped in a rapid march, would know how to teach his men to make others of clay, and glaze them with salt. But why should you not learn both this and many

other equally elementary matters of handicraft? For a man who has once made a thing, however roughly, is like a bather who has once swum a few strokes; he never utterly forgets how to do it, and always retains his self-confidence. When matters fall into shape, I should be very glad to accompany large parties to a distance on the heath, and there in the free moorland to go through much of what had been practised by the side of the huts. I think if I can find a sufficient number of officers to enter thoroughly into the spirit of the thing, I have little doubt but that I shall be able to attract many highly-experienced travellers and campaigners, and we might carry out most pleasant and instructive expeditions, each learning from the special knowledge of the others. And I shall endeavour, as far as possible, to give a thoroughly practical and earnest character to such meetings, practising those matters which are of most important and common use, and avoiding scrupulously whatever is simply fanciful.


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f. 15v

Mrs. E. Wheler

f. 16

ARTS OF CAMPAIGNING:

Duplicate

AN INAUGURAL LECTURE, DELIVERED

AT

ALDERSHOT,

ON THE OPENING OF HIS MUSEUM AND LABORATORY
IN THE SOUTH CAMP, V, Nos. 18 AND 20,

BY FRANCIS GALTON, ESQ.

LONDON:
JOHN MURRAY, ALBEMARLE STREET.
1855.

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Woodfall and Kinder, Printers, Angel Court, Skinner Street, London.

ARTS OF CAMPAIGNING.

GENTLEMEN, — I have invited your attendance this afternoon, that I might have an opportunity of explaining in a public manner the objects for which I have come here, aided and cordially recognised by Lords Panmure and Hardinge, and by the General commanding this camp. I do not presume to instruct you in military matters. I am a civilian; and it is only in that part of a soldier's life where his wants and methods of meeting them are necessarily identical with those used by civilians, that I claim any right to interfere. I shall keep aloof from all matters of drill and regimental discipline, and of that complex

system of organisation by which the members of a vast army are animated, every one of them, by the spirit of one commander, and confine myself entirely to civil matters. I only profess to teach those Arts and Contrivances which stand the soldier in stead when military organisation fails to help him. I wish to show how he may be self-sufficing and self-sustaining, as well as that efficient part of a great military machine, into which you, gentlemen, as officers, take care that he is fashioned. We all acknowledge how justly proverbial are the chances of war, and we know that every soldier who enters the field risks each day of his life a chance of being thrown, in some degree, on his own resources. There is not a chapter in the history of war, whether it be in modern or ancient times, where we are not met with that dread of all Generals, "deficiency of supplies;" at one time it is a deficiency of food, at another, of tools and utensils. At another time, it is of skilled

f. 19r

labour; at another, of raw materials to work upon. Under these circumstances, the soldier must either sit still and suffer, or else he must bestir himself to meet the difficulty. No military organisations will help him; the existence of any want at all proves that organised systems have failed in supplying that want; and therefore if the soldier is without skill to shift for himself, and to supply with his own brain and hands what is wanting, he must go without it. Now, so far as he is liable to be thrown upon his own resources, to exactly the same degree does he want that kind of knowledge and dexterity which has nothing to do with matters of drill and discipline, but which is precisely what persons who are the very opposites of soldiers—I mean travellers in rude countries, emigrants, missionaries, and so forth—have to make an actual profession of. The soldier, as such, depends upon a system; he is part of a great machine, that does its work efficiently, just as all other

f. 19v

machines do, by each part of it having some appointed and special work: a traveller or an emigrant is not part of a system ; he is all in all to himself, and cannot make sure of indulging in a single comfort which his own hands are unable to procure.

The soldier, I say, is part of a machine that does its work admirably so long as its pieces are whole and in gear, but which is liable to give way under pressure. The emigrant, though he gets through his work in a rough sort of way, and often with great waste of labour, depends solely upon himself and not upon others ; and, therefore, when organisation breaks down, it is then that qualities such as he possesses are able to come in and shield the soldier from disaster.

Now there is nothing in the professional education of military men to teach them this knowledge, which is the life-stay of travellers and emigrants. And yet it is an art most highly prized by all campaigners. All soldiers

who spend years in the field learn much of it, but they do so under the occasional teachings of hard necessity, which most likely have proved fatal to an equal number of their comrades, whose constitutions had sunk under the pressure of hardships which these had survived, but which neither of them had skill to evade, or knowledge to resist. Now, what I want, is, to do away with the necessity of this cruel apprenticeship, and by varied instruction in this camp, to enable the young soldier to take the field with no small portion of the knowledge of the oldest campaigner. It is your science, gentlemen, that explains the art of handling men in masses, of organising, and of commanding. My science is a humbler one, and must be classed at a very distant point on the scale of knowledge; but, gentlemen, I protest against its being disregarded; and I am sure you will agree with me that an army of soldiers, each man of whom can take care of himself, is in a far more efficient

state than another composed of men no better drilled, while individually they are almost helpless. In easy-going times there may be little apparent difference between the two; but it is when supplies fail, and the army is harassed, and the weather is cruel, and calamities fall thick, that the knowledge I profess to teach, and which I earnestly urge you to learn, is the raft that saves. Nay, I may compare the army that can depend, not on its organisation alone, but also on the self-sufficiency of every unit that forms it, to a life-boat: she will sail and row as well as another, and yet you may water-log her, but you cannot sink her.

I do not profess to explain the complicated processes of manufacture used in civilised countries, but I aim at showing all those ways of obtaining necessaries and what are well called "necessary comforts," that a man may practise when encamped out in the field. And I may remark that this art is more pe-

culiarly practised by a traveller than by a settler, for the reason that a settler has plenty of time, and can accumulate materials and stock; but a traveller is always on the move, and is able to carry little or nothing with him; the ingenious combination by which he may have supplied any want of yesterday has been left behind him, and he must re-make it to-day if the want recurs. In stating the case broadly, I might say that the settler makes what he may want durably, and once for all; the traveller meets each want as it arises. The methods of the latter are much simpler and more generally available than those of the former, who, in a rude way, imitates the processes of civilisation. The settler who has a bench to carpenter upon, a forge to smithy with, a log shanty over his head, a fire-place and pots to cook with—rude though all these things may severally be, looks upon the arts of life, and on the ways of satisfying his wants, in quite a different light to a traveller who un-

packs a load of some two hundred weight from his horse's back, in which all his available possessions in the world are contained, and on which he had been living for months, and with which, as a nucleus, he is everywhere able to make himself perfectly at home, and to surround himself with improvised and ample comforts.

I will now attempt to state, in general terms, the methods of instruction that I shall endeavour to pursue.

You must not think, gentlemen, that any person who has once roughed it becomes a proficient in this art of campaigning. It has struck me forcibly during the years in which I have made a study of it, how ignorant the inhabitants and bushrangers of each rude country are of the shifts and contrivances used by those of others, and with what great advantage persons who have roughed it in different lands could combine their information. In reading works of travel, which I

have long done with a view to extracting hints that might help me in systematising a science of campaigning, I cannot tell you how often it has struck me that the narrators have been baffled by difficulties under circumstances which, though unusual to them, are very common to people in other countries, who have their own ingenious ways of surmounting them. And still more frequent is it to read of travellers recording, with the utmost delight, and for the good of others, ingenious discoveries which they had made, after months of inconvenience to themselves, and which not only one or two, but more, previous travellers had equally puzzled out and similarly recorded with a natural satisfaction as their own original inventions. Now it was after travels long since undertaken by me, that I was so much impressed with the advantage of collecting the experiences of all rude nations, and of as many travellers as I could, that when I was carrying on my ex-

plorations of South Africa in 1850–2, I began to plan the making of such a collection: and there as I rode along, I used to jot down all that I could think of, of African and other experiences. This formed a nucleus, to which, since my return in 1852, I have steadily added, reading and inquiring in all directions, and lastly, publishing a little book called the "Art of Travel," which I know that some of you have seen, in which was inserted what I had then to say, so far as I could arrange it in some sort of order, and at no great length. It has since done me good service in making my objects more widely known, and in inducing persons to kindly forward to me for further publication such of their original experiences as I had been ignorant of, and had therefore left unrecorded. I mention this, gentlemen, to show what kind of method I have been pursuing for my own instruction, and for the service of those who, as I have done, may be required to rough it,

whether they be soldiers, or whether they be civilians. I wish to show to you how I have made a special study of the matters which I profess to teach. Would that I were better informed upon them, to do you and the subject more justice. But as it is a new thing to endeavour to unite and systematise the numerous and widely-dispersed fragments of the information in question, I trust you will bear with me, and also make allowance for the confessedly incomplete method in which I may handle the subject, on the score of the really earnest way in which I have striven to improve my knowledge, and make it of effect.

Now, as to the means through which I propose to work. I desire to offer opportunities to all officers who choose to accept them, by which they may learn these things, and acquire skill to practise them. I wish to convey knowledge and manual dexterity, for neither without the other will ever be of much avail, and I propose to do it in this

f. 23v

way: I shall rapidly increase the collection of sketches now begun (and I beg you to bear in mind, with regard to its embryo condition, that it is only a short time since Lords Panmure and Hardinge have been pleased to accept my proffered services), and shall continue adding to them so long as the present season lasts; they will be arranged in my hut, with explanatory foot-notes and references, and form as complete a museum of the shifts and contrivances of camp life, as the time and my abilities and the nature of the case will allow me to make; I shall also have a small collection of books of reference on matters kindred to these things, and I invite officers to visit this museum and make what use of it they like, as copying the pictures, applying for information on them, and reading the books. It will, in the first instance, be open from half-past one to half-past six o'clock every day. It is my sincere endeavour to afford every facility and comfort to persons earnestly study-

f. 24r

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f. 28v

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system of organisation by which the members of a vast army are animated, every one of them, by the spirit of one commander, and confine myself entirely to civil matters. I only profess to teach those Arts and Contrivances which stand the soldier in stead when military organisation fails to help him. I wish to show how he may be self-sufficing and self-sustaining, as well as that efficient part of a great military machine, into which you, gentlemen, as officers, take care that he is fashioned. We all acknowledge how justly proverbial are the chances of war, and we know that every soldier who enters the field risks each day of his life a chance of being thrown, in some degree, on his own resources. There is not a chapter in the history of war, whether it be in modern or ancient times, where we are not met with that dread of all Generals, "deficiency of supplies;" at one time it is a deficiency of food, at another, of tools and utensils. At another time, it is of skilled

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labour; at another, of raw materials to work upon. Under these circumstances, the soldier must either sit still and suffer, or else he must bestir himself to meet the difficulty. No military organisations will help him; the existence of any want at all proves that organised systems have failed in supplying that want; and therefore if the soldier is without skill to shift for himself, and to supply with his own brain and hands what is wanting, he must go without it. Now, so far as he is liable to be thrown upon his own resources, to exactly the same degree does he want that kind of knowledge and dexterity which has nothing to do with matters of drill and discipline, but which is precisely what persons who are the very opposites of soldiers—I mean travellers in rude countries, emigrants, missionaries, and so forth—have to make an actual profession of. The soldier, as such, depends upon a system; he is part of a great machine, that does its work efficiently, just as all other

machines do, by each part of it having some appointed and special work: a traveller or an emigrant is not part of a system; he is all in all to himself, and cannot make sure of indulging in a single comfort which his own hands are unable to procure.

The soldier, I say, is part of a machine that does its work admirably so long as its pieces are whole and in gear, but which is liable to give way under pressure. The emigrant, though he gets through his work in a rough sort of way, and often with great waste of labour, depends solely upon himself and not upon others; and, therefore, when organisation breaks down, it is then that qualities such as he possesses are able to come in and shield the soldier from disaster.

Now there is nothing in the professional education of military men to teach them this knowledge, which is the life-stay of travellers and emigrants. And yet it is an art most highly prized by all campaigners. All soldiers

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who spend years in the field learn much of it, but they do so under the occasional teachings of hard necessity, which most likely have proved fatal to an equal number of their comrades, whose constitutions had sunk under the pressure of hardships which these had survived, but which neither of them had skill to evade, or knowledge to resist. Now, what I want, is, to do away with the necessity of this cruel apprenticeship, and by varied instruction in this camp, to enable the young soldier to take the field with no small portion of the knowledge of the oldest campaigner. It is your science, gentlemen, that explains the art of handling men in masses, of organising, and of commanding. My science is a humbler one, and must be classed at a very distant point on the scale of knowledge; but, gentlemen, I protest against its being disregarded; and I am sure you will agree with me that an army of soldiers, each man of whom can take care of himself, is in a far more efficient

state than another composed of men no better drilled, while individually they are almost helpless. In easy-going times there may be little apparent difference between the two; but it is when supplies fail, and the army is harassed, and the weather is cruel, and calamities fall thick, that the knowledge I profess to teach, and which I earnestly urge you to learn, is the raft that saves. Nay, I may compare the army that can depend, not on its organisation alone, but also on the self-sufficiency of every unit that forms it, to a life-boat: she will sail and row as well as another, and yet you may water-log her, but you cannot sink her.

I do not profess to explain the complicated processes of manufacture used in civilised countries, but I aim at showing all those ways of obtaining necessaries and what are well called "necessary comforts," that a man may practise when encamped out in the field. And I may remark that this art is more pe-

culiarly practised by a traveller than by a settler, for the reason that a settler has plenty of time, and can accumulate materials and stock ; but a traveller is always on the move, and is able to carry little or nothing with him ; the ingenious combination by which he may have supplied any want of yesterday has been left behind him, and he must re-make it to-day if the want recurs. In stating the case broadly, I might say that the settler makes what he may want durably, and once for all ; the traveller meets each want as it arises. The methods of the latter are much simpler and more generally available than those of the former, who, in a rude way, imitates the processes of civilisation. The settler who has a bench to carpenter upon, a forge to smithy with, a log shanty over his head, a fire-place and pots to cook with—rude though all these things may severally be, looks upon the arts of life, and on the ways of satisfying his wants, in quite a different light to a traveller who un-

packs a load of some two hundred weight from his horse's back, in which all his available possessions in the world are contained, and on which he had been living for months, and with which, as a nucleus, he is everywhere able to make himself perfectly at home, and to surround himself with improvised and ample comforts.

I will now attempt to state, in general terms, the methods of instruction that I shall endeavour to pursue.

You must not think, gentlemen, that any person who has once roughed it becomes a proficient in this art of campaigning. It has struck me forcibly during the years in which I have made a study of it, how ignorant the inhabitants and bushrangers of each rude country are of the shifts and contrivances used by those of others, and with what great advantage persons who have roughed it in different lands could combine their information. In reading works of travel, which I

have long done with a view to extracting hints that might help me in systematising a science of campaigning, I cannot tell you how often it has struck me that the narrators have been baffled by difficulties under circumstances which, though unusual to them, are very common to people in other countries, who have their own ingenious ways of surmounting them. And still more frequent is it to read of travellers recording, with the utmost delight, and for the good of others, ingenious discoveries which they had made, after months of inconvenience to themselves, and which not only one or two, but more, previous travellers had equally puzzled out and similarly recorded with a natural satisfaction as their own original inventions. Now it was after travels long since undertaken by me, that I was so much impressed with the advantage of collecting the experiences of all rude nations, and of as many travellers as I could, that when I was carrying on my ex-

plorations of South Africa in 1850–2, I began to plan the making of such a collection: and there as I rode along, I used to jot down all that I could think of, of African and other experiences. This formed a nucleus, to which, since my return in 1852, I have steadily added, reading and inquiring in all directions, and lastly, publishing a little book called the "Art of Travel," which I know that some of you have seen, in which was inserted what I had then to say, so far as I could arrange it in some sort of order, and at no great length. It has since done me good service in making my objects more widely known, and in inducing persons to kindly forward to me for further publication such of their original experiences as I had been ignorant of, and had therefore left unrecorded. I mention this, gentlemen, to show what kind of method I have been pursuing for my own instruction, and for the service of those who, as I have done, may be required to rough it,

whether they be soldiers, or whether they be civilians. I wish to show to you how I have made a special study of the matters which I profess to teach. Would that I were better informed upon them, to do you and the subject more justice. But as it is a new thing to endeavour to unite and systematise the numerous and widely-dispersed fragments of the information in question, I trust you will bear with me, and also make allowance for the confessedly incomplete method in which I may handle the subject, on the score of the really earnest way in which I have striven to improve my knowledge, and make it of effect.

Now, as to the means through which I propose to work. I desire to offer opportunities to all officers who choose to accept them, by which they may learn these things, and acquire skill to practise them. I wish to convey knowledge and manual dexterity, for neither without the other will ever be of much avail, and I propose to do it in this

way: I shall rapidly increase the collection of sketches now begun (and I beg you to bear in mind, with regard to its embryo condition, that it is only a short time since Lords Panmure and Hardinge have been pleased to accept my proffered services), and shall continue adding to them so long as the present season lasts; they will be arranged in my hut, with explanatory foot-notes and references, and form as complete a museum of the shifts and contrivances of camp life, as the time and my abilities and the nature of the case will allow me to make; I shall also have a small collection of books of reference on matters kindred to these things, and I invite officers to visit this museum and make what use of it they like, as copying the pictures, applying for information on them, and reading the books. It will, in the first instance, be open from half-past one to half-past six o'clock every day. It is my sincere endeavour to afford every facility and comfort to persons earnestly study-

ing these things. Next as regards teaching the *hand*. I am collecting a motley stock of very simple tools and raw material, planks, logs, twigs, canvas, cloths, and every single thing necessary for making with the hand those very things that you will see pictured in the museum ; I urge you to come and make use of them. In the palisadoed plot of ground, between the huts, you can sit and work just as roughly as you would in the Crimea, and you will from time to time have intelligent workmen to assist you in your difficulties, and explain the use of the tools you work with. I particularly hope that those who have any mechanical aptitude will give some of their leisure hours to these occupations, and avail themselves of the present opportunity. I beg you to come and make the most of it ; I am sure there is plenty of interesting and rational occupation to be found by doing so ; believe me, it will often stand you in good stead, whether in the field or in

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houses. We ought all to know these simple matters of handicraft; nearly all of us have wished that at some time or other an opportunity had been offered us of learning them. The expense will be a mere nothing; I only require that injured tools be replaced, and record, but seldom expect to enforce, the principle that all raw materials used be paid for. I mean, as to this latter proviso, that three-fourths of the material used by each person will be of too little worth to be valued at all, but when more expensive things are wanted, as canvas, leather, &c. (if I have them, and if you come to me to furnish you with them), I shall expect them to be paid for before they are served out. I must beg you to recollect, that although these huts and some of the furniture in them has been provided by Government, yet that I possess no guarantee that any expenses I have, or shall have, incurred in setting this undertaking on foot, or in keeping it in action, will ever

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be allowed me. And, therefore, although you will always find me ready to supply every accommodation that may prove needful in giving effect to this scheme that I have gratuitously undertaken, you must not be surprised if I do not cater for wants before they arise, by bringing down at once a complete establishment of artificers, with expensive supplies of tools. Indeed, the more that you make serviceable makeshifts out of worthless raw materials and with common tools, the more thoroughly shall I feel that you sympathise with the spirit in which I myself undertake this matter.

And, gentlemen, let me remark how advantageous it must prove, if such of you as are destined to active service, and who having happened to have learnt any particular art, as drawing, turning, carpentering, and so forth, will also practise here, at Aldershot, how to make for yourselves all the tools and appliances required for that art. In this way,

when you are far from shops and without the materials you have been accustomed to use, you will know how to procure them for yourselves, and how to furnish yourselves with those very articles for the want of which the knowledge you had at much pains acquired, would lead to no result. I wish to induce you to supply yourselves with that wanting link which is able to connect the arts and handicrafts, learnt in civilised life, with those available in a temporary and ill-provided encampment. Thus, in drawing, I have little doubt that I am now addressing many gentlemen, highly accomplished in that art, who yet have no knowledge how to *size* common paper, and make it fit to take the sharp strokes of a pen; whose paper being greasy and useless for want of *ox-gall*, they do not know how to procure it; whose stray leaves of paper they have never practised making into a handy and efficient *block*. Nay, who cannot make a moderately good *paint-*

brush for themselves, nor a good substitute for writing or Indian *Ink*, nor a good writing *pen* out of a plucked quill feather. Yet all these things, and many more besides, referring to the same art, can be made at all times, with a simplicity and a readiness astonishing to those who are used to think that manufactories are necessary for producing every manufacture they see. There is no habitable country so wild and so inhospitable as not frequently to afford ample materials for making each thing that I have mentioned. But unless we learn to draw our supplies direct from nature, and not through the medium of manufactories, we may sit with our hands folded in unwilling idleness, and complaining of want when we are really in the midst of abundance, and surrounded by opportunities of using them. So with the Carpenter: he may be an excellent workman in London, but useless in the field; for he may have nothing but growing trees at hand, and yet not know how to season green wood to a sufficient degree for working, in a

single night; he may not be enough of a blacksmith to repair or make his awls and other small tools, when he has nowhere to buy them; he may become disheartened because he has no nails, and is unpractised in using substitutes for them; he may be without proper tools at all, and be unable to teach others how to fell and rudely to fashion trees for his use by means of fire, directing the encroachments of the flame by judicious scrapings and quenchings; it is possible he may not be able to soften the temper of his axe when he finds it shiver against the hard wood which alone he can procure to work with, or do other of the many matters which are quite necessary that he should be an adept in, before he is fitted to take the field, but which I should only weary you now by recounting more at length. Again as to the Turner, the last of those I mentioned: he is helpless without a lathe; and that simple form of one which may be joinered up in two hours at the foot of any tree, which was, I believe, generally used

up to the last half-century, which is still used everywhere in Italy, has been quite forgotten by us, though by forgetting it we are made dependent on manufactories, and lose the power of making a lathe for ourselves, quite good enough to give useful results wherever and whenever we please.

Now, gentlemen, I say that those among you who have already been at the pains to acquire any art, should take the next easy step further, and acquaint themselves with the rough and ready way that they will have here an opportunity of practising, of making their tools and other appliances. Whatever a soldier has learnt, let him also learn what little more is necessary to make his knowledge available whenever he may be called upon to leave the regions of shops and houses, and to take up his quarters in the open field. When any of you require to learn what neither I nor the workmen then with me can show you how to perform, do not be disheartened, but make experiments, have full

confidence in yourselves, determined not to be beaten by difficulties, consult what books may be at hand, and the chances of success will be strong in your favour. I hope that these huts may be looked upon more as a laboratory where learners may teach themselves, which is the best kind of learning,—rather than as a place where they are formally taught. I wish to make it a kind of head-quarters of the knowledge of those shifts, contrivances, and handicrafts that are available in camp life ; and I call upon you to help me with your assistance. Write to your friends from the Crimea, or from the bush, who take an interest in these things, get hints of original experiences from them, and communicate them to me ; they will not lie idle, but will at once be turned to account in increasing a store already large, and will remain recorded in pictures or in models for the good of ourselves and all who follow us. In so far as I may feel called upon to instruct, otherwise than in showing readiness to explain and converse about whatever is shown in

this little museum and laboratory, you must allow me to feel my way gradually, and not to offer you an exact programme at the outset; much will depend on the number of officers whom I may succeed in attracting to a somewhat earnest and steady pursuit of the Art of camp life. Possibly occasional lectures may be convenient, especially if combined with field excursions. Those who wish to work can keep any bags or boxes in my second hut. The tools that I shall furnish them with will be of the simplest description; but if any of you know how to use others, and choose to provide yourselves with them, you can bring them here, and keep them locked up in safety. I shall endeavour to form classes, and to have a clever workman here for the half-day to teach each class. I fear there is hardly an officer in camp who, if all his cooking utensils were broken, or had been dropped in a rapid march, would know how to teach his men to make others of clay, and glaze them with salt. But why should you not learn both this and many

other equally elementary matters of handicraft? For a man who has once made a thing, however roughly, is like a bather who has once swum a few strokes; he never utterly forgets how to do it, and always retains his self-confidence. When matters fall into shape, I should be very glad to accompany large parties to a distance on the heath, and there in the free moorland to go through much of what had been practised by the side of the huts. I think if I can find a sufficient number of officers to enter thoroughly into the spirit of the thing, I have little doubt but that I shall be able to attract many highly-experienced travellers and campaigners, and we might carry out most pleasant and instructive expeditions, each learning from the special knowledge of the others. And I shall endeavour, as far as possible, to give a thoroughly practical and earnest character to such meetings, practising those matters which are of most important and common use, and avoiding scrupulously whatever is simply fanciful.

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Duplicate

The barometers, thermometers, and hygrometers were placed on wooden shelves, painted white, in the centre of one of the windows (6 feet 8 inches by 4 feet) on the N. side of the house, outside the window but inside the green-painted Venetian blinds, removed from the walls, and beyond the effects of reflection or other improper influences. The air could circulate freely betwixt the window and the Venetian blind, but the instruments were protected by the latter from violent blasts or currents of wind, and from the rays of the sun, during his *most* northernly declination, by a projecting lath and plaster cornice which encircles the top part of the house wall. From the position of the house the sun's rays could merely interfere with the 9 A.M. observations, and only for a few weeks; but when the cornice, already described, did not afford sufficient protection, *at that observation* the instruments were previously removed to, and placed in a similar manner in, a window on the W. side of the house, having an open street 44½ feet wide in front of it. The ground-floor of the house is 3 feet English above the level of the sea.

1. The *Pluviometer* employed was Crosley's self-registering one, made by Watkins and Hill, of Charing-cross. Its position was on the terrace of the house, exposed in every direction, and without shelter from any adjacent objects. Height above the level of the sea, 64 ft. 6¾ in. English.

2. The *Barometer* was one of Newman's Mountain Barometers, the error of which, as compared with the standard of the Royal Society, is minus 0·012 inch—that is to say, it would be required to add the above error to the readings of my barometer in order to obtain the indications of *that* of the Royal Society. The observations in my register are simply corrected for capacity and capillarity, and for nothing besides, and are not reduced to the usual standard of comparison, the freezing-point; nor has the error of the instrument been calculated in my corrections. The height above the level of the sea of the cistern was 41 ft. 3 in. English.

3. The *Thermometers* were made by Troughton and Simms, and compared with one of their *standards,* which I possess. Height above the level of the sea 43 ft. 4¾ in. English, or 40 ft. 4¾ in. English above the level of the great square.

4. The *Radiating*, or *Black Bulb, Thermometer*, made by Newman, and also compared with Troughton and Simms's standard, was placed on the S. side of the house, at a distance of 2 ft. 2 in. English from the wall, and 3 ft. 8 in. English from the ground, with an open space behind, free from building, of about 120 feet.

5. The *Hygrometers* in use were made by Newman, of Regent-

street, with the usual cistern for distilled water, and wet and dry bulbs, and with the scales divided to tenths of a degree of Fahrenheit. The dew-points were calculated according to the formula given by Dr. Mason, and the degree of dryness found by deducting the dew-points from the temperature of the circumambient atmosphere. Height above the level of the sea 43 ft. 4¾ in.

6. The *Anemometer* used was Lind's, made by Newman, and placed at an elevation of 68 ft. 4 in. English above the level of the sea. Surrounding space perfectly open and unsheltered.

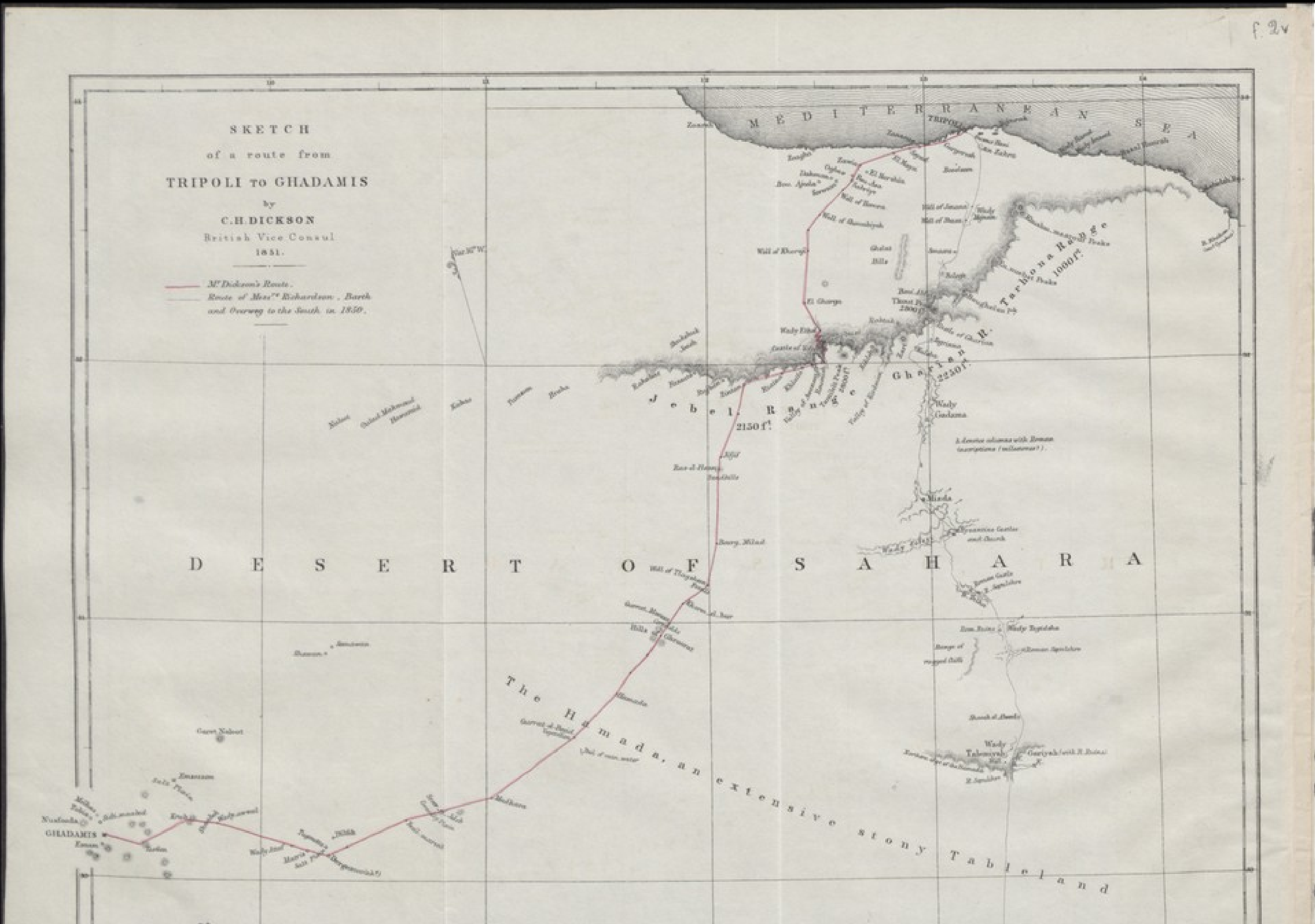
7. The weathercock, or vane, was likewise fixed on the terrace of the house, at a distance of 69 ft. 4 in. above the level of the sea, and equally exposed, and unsheltered by neighbouring objects, with the anemometer and pluviometer. The true N. was found by astronomical calculation.

8. Alexandria *mean* time was used for the hours of observation, and was obtained from a Dent's dipleidoscope, which I placed by double altitudes of the sun, frequently repeated and checked, in the plane of the meridian; and then permanently fixed it *there*. Correct *true* time was thus converted into *mean* time, and transferred to a Barraud's chronometer, which was again transferred to a common clock, set 2 minutes fast, and which, in striking the hours, was heard in every corner of and even beyond the dwellinghouse, thus giving loud and sufficient warning of the approaching hour of observation. In Egypt, as in most Mussulman countries, public clocks are unknown; and the only *reference* of time, at the command of the uninitiated, is the *not* unerring voice of the Muezzin, from the minaret of his mosque, summoning the faithful to declare the unity of God and the apostleship of Mohammed.

The titles of the columns of the Journal are as follows:—(1) Date; (2) Pluviometer at 9 A.M.; (3) Barometer corrected at 9 A.M.; (4) 3 P.M., and (5) 11 P.M.; (6) Thermometer attached at 9 A.M., (7) 3 P.M., and (8) 11 P.M.; (9) Temperature, maximum, and (10) minimum; (11) Dew-points at 9 A.M.; (12) 3 P.M., and (13) 11 P.M.; (14) Absolute dryness at 9 A.M.; (15) 3 A.M., and (16) 11 P.M.; (17) Mortality, males, and (18) females; (19) Winds and weather at 9 A.M.; (20) 3 P.M., and (21) 11 P.M.; (22) Number of days in the month during which each wind prevails. There are also hourly observations of most of the above-named instruments on the 21st of March, 21st of June, 21st of September, and 21st of December.

The original MS. Journal is preserved in the archives of the Royal Geographical Society.—Ed.

The following table, showing the mean temperature of the seasons and the quantity of rain at Alexandria, as obtained from Mr. Thurburn's three years' observations, was prepared by Colonel Ph. Yorke, and exhibited to the Society at the meeting.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SKETCH
of a route from
TRIPOLI to GHADAMIS
by
C.H. DICKSON
British Vice Consul
1851.
Mr Dickson's Route.
Route of Messrs Richardson, Barth and Overweg to the South in 1850.
MEDITERRANEAN SEA
TRIPOLI
Zoarah
Tarhona Range
1000 ft.
Gharian
2250 ft.
Jebel Range
2150 ft.
Wady Gadama
Mizda
DESERT OF SAHARA
The Hamada, an extensive stony Tableland
Wady Tabouniyah
Gariyah (with R. Ruins)
Garet Nabort
GHADAMIS
Derj
Longitude East from Greenwich
Published for the Journal of the Royal Geographical Society, by John Murray, Albemarle St. London 1852.

—	Winter.	Spring.	Summer.	Autumn.	Mean.
Temperature, Fahr. .	58°·54	66°·46	78°·34	73°·81	69°·29
Rain in inches . .	6·247	0·278	0·008	0·974	Total. 7·507

TABLE of Mean Monthly Temperature at Alexandria, from Observations by Hugh THURBURN, Esq.

—	1847.	1848.	1849.	Mean.
January	58·28	56·87	56·94	57·36
February . . .	60·06	58·73	54·70	57·83
March	63·32	61·67	61·49	62·16
April	67·10	65·66	68·19	66·98
May	70·81	69·98	69·99	70·26
June	76·72	76·53	75·46	76·25
July	78·28	78·17	79·05	78·50
August	80·29	80·53	80·04	80·28
September . . .	78·57	77·43	78·40	78·13
October	73·87	76·48	74·27	74·84
November . . .	66·57	69·86	69·09	68·47
December . . .	59·27	61·58	60·47	60·44
Mean . . .	69·43	69·46	69·01	69·29

IX.—*Extract from* Vice-Consul C. H. DICKSON's *Report of his Journey from Tripoli to Ghadamís.*

Communicated by the Foreign Office.

Read March 8, 1852.

THE accompanying map shows the route from Tripoli to Ghadamís usually taken by caravans, and which I followed in the year 1849. It is constructed on a spherical projection, from bearings taken with a Kater's pocket-compass; and in order to determine the distance from one stage to another, I employed a string 1000 yards long, held at each extremity by a man on foot, who proceeded along with the caravan, marking every length of the string, while I at the same time noted them down, and marked also the hour of departure and arrival.

As the present route has never before been travelled over by Europeans (Major Laing and Mr. Richardson, the only two Christians who visited Ghadamís before me, having proceeded, the former through Wady Shiaty, in Fezzan, and the latter *viâ* Seenawan), I shall add a short description of the most remarkable stages, beginning at *Zanzour*. This is reckoned one of the finest districts on the coast of Tripoli for richness of soil and good water, and enjoys, moreover, a healthy and temperate climate. It contains a population of about 4000 Arabs, besides a few Jews. The village, from whose centre rises an old Moorish castle, is closely hemmed in with gardens, surrounded on every side by hedges of the cactus-opuntia, or prickly-pear bush. Its productions are olive-oil, melons, corn, dates, and onions, which are particularly esteemed throughout the regency. Starting from this village, a fine broad road leads through the small oases of Seyad and El Maya to *Zawia*, which is considered one stage from Tripoli, distant 25 miles. This is a large district, and the residence of a Raid, who has also under his jurisdiction the oases of El-Harshia, Bou-Asa, Ogba, Sorman, Dahman, Bou-Ajeela, and Zoagha, containing an aggregate population of about 20,000 souls, belonging chiefly to the tribes called Orshefana, El-asà, and Juari; there are, moreover, some 500 resident Jews. I may also observe that the natives have claim to sanctity, on account of their descent from a noted Marocquine marabout, by name "Sidi El-lijeh," who, according to tradition, was the first propounder of the Mahomedan faith in these parts, about the twelfth century. The quality of its soil is much inferior to that of Zanzour, yet Zawia yields the same productions. To these, however, must be added extensive plantations of tobacco, of an indifferent sort, which is monopolized by the Government. The climate in some localities is unhealthy, particularly in the oasis of Soagha, where ague is very prevalent in autumn. A fact worthy of notice is the recent introduction, under the auspices of Government, of silkworms, the rearing of which, owing to the number of mulberry-trees they possess, promises to extend rapidly throughout these villages. A fair is held at Zawia on Mondays and Thursdays, to which most of the neighbouring tribes resort. From Zawia caravans are directed to a well called Homra, distant 7 miles, passing first through the gardens of El-Harshia; the water being superior to that of Shauabiyeh and El-khoreji, a sufficient quantity is here collected to last till they reach the Jebel Mountains, a journey of 2½ days, being all together about 100 miles from Tripoli. There are no villages between Zawia and the Jebel, except the oases of Shukshuk and Joosh, at the foot of these mountains, which I shall refrain from describing on account of their lying out of the present route. On quitting Zawia, the ground keeps gradually rising, and presents but few sand-hills; the whole tract consists of a prairie covered with brushwood, with occasional patches of corn-fields scattered over it, the latter the property of the Bel-arà and Jedouri tribes. The spot marked El-Gharga is a halting-place in a hollow on the plain; and

Wady Ethel is not a running stream, but merely a narrow torrent-bed, containing a little rain-water in winter, and on the borders of which grow clusters of wild tamarisk-trees, whence it derives its name. After quitting Wady Ethel, and as you gradually approach the mountains, this lofty range, with its tapering peaks and bold elevations, becomes more fully displayed, and, as the ground rises, beds, consisting of sharp flints and pebbles, occasionally interrupted the solemn but sure pace of the camel; in other places the hills are covered over with wild thyme, and a variety of aromatic herbs peculiar to these elevated regions. Caravans now proceed either to Yefren, to Zintan, or to Rujban, but more commonly to the two latter, from the circumstance of the camel-drivers being natives of these places. Yefren is the chief district, and the residence of a kaïmakan, or lieutenant-governor, whose authority extends over Ghadamís, and over all the Jebel and Gharian range. Since the subjugation of the Jebel in 1844, the Turks have erected a fort at Yefren, in which are stationed 500 troops, being the total amount of garrison force in these mountains. A military road has lately been constructed leading to it from the foot of the mountains. The Jebel is evidently a continuation of the Atlas chain, and commences at the Tunisian frontier, and extends in a north-easterly course as far as the Valley of Kirdmeen, where it joins with the Gharian; it is remarkable for the ruggedness of its surface, and presents a striking contrast to the Gharian and Tarhona ranges, by displaying everywhere massive rocks of flint and hard limestone, intersected by innumerable ravines and precipices. Perched on the summit of these immense blocks are seen the villages of the natives; while in the valleys below, corn-fields and clumps of date and fig trees present an agreeable relief to the eye. By observations made by Dr. Overweg, the African traveller (attached to the late Mr. J. Richardson's Mission), the highest peak in this range, which rises in the neighbourhood of Yefren, is 2800 feet above the level of the sea. The houses have merely one story, and are of very rude construction, being low and irregularly shaped; they are built of stones cemented with gypsum, but without plaster on the outside. The district of Zintan, however, forms an exception to this rule; the natives, like those of the Gharian, living in subterranean cells. In those places which possess no wells or springs, tanks are constructed for collecting rain-water, which is then reserved only for domestic purposes, no irrigation being allowed to the soil in summer. Fassato and Yefren contain the purest water, supplied from living streams.

The population of the Jebel may be roughly estimated at

60,000, comprising about 1000 Jews, which latter reside chiefly at Kikleh and Yefren. It is divided into 16 districts, subdivided into villages, and each possessing a sheikh, a kadi, a mosque, and a kasbah, or blockhouse, erected in feudal times for purposes of defence. The largest and most populous district is Fassato, divided into 11 villages, and containing altogether about 6000 souls. The other districts are in succession from E. to W. as follows:—Rabtah, Kikleh, Yefren (*Jedaret*), Khlaïfa, Riaina, Zintan, Rujban, Raheibat, Hraba, Tumzeen, Kabao, Hawamid, Oulad Mahmoud, and Naloot.

The natives form three distinct classes: one which claims to be aboriginal, and must have descended from the Berbers, whose language it partly retains, having some affinity with that spoken by the Tuarics and natives of Ghadamís. This class does not belong to any of the four orthodox sects of Islam, but in common with the Wahabites it differs on various dogmas, thereby embodying itself into a fifth order of Mohammedans, hence termed Khoamsa. The second class consists of nomadic tribes of Arabs, some of whom also reside in villages. Though living in promiscuous communities, these two classes seldom or never intermarry; and, in former times, they continually retaliated upon each other their implacable feuds. The number of Khoamsa amounts to about 32,000, who reside principally at Fassato and Yefren. Besides these, there is another tribe, amounting to 9000, called Siaan, which inhabits the plains below, and the oases of Joosh and Shukshuk. These are noted for possessing numerous flocks of sheep and camels, to the amount of about 100,000 of each. The Jebel produces an abundance of olive-oil and corn; besides dates, figs, grapes, melons, a few vegetables, wool, and butter. Fassato supplies the greatest quantity of oil, having altogether about 12,000 olive-trees. The quantity of barley usually sown exceeds that of wheat by one-half, and gives an average of sixty-fold. There are also extensive manufactures of goats'-hair sacks and woollen blankets, but all of very coarse texture. The amount of taxes exacted from this portion of the regency, exclusive of Ghadamís, is 24,000 mahboubs (4000*l.*). This sum is rated according to an estimate of the number of sheep and cattle each village possesses. Government claims, moreover, a tenth part of the annual produce of corn and oil. These taxes are paid in quarterly and sometimes half-yearly instalments, and are a source of endless complaint on the part of the natives, owing to the inequitable mode in which they are assessed.

Among the objects deserving of notice in the Jebel are numerous ruins of ancient Roman towers, scattered over the whole of its surface, some bearing inscriptions. The following epitaph was found in the Valley of Roumieh, during my journey from Yefren to Zintan:

DMS
IVLIA FAVS
TINA VIXIT
RPAA NNXXVIII
MXIDXVIV.......ION
RAIVS.....LIB.

Having quitted Zintan, the caravan now entered on the desert of Sahara, the greater portion of which, to within a few miles of the oasis of Derge, is not entirely destitute of vegetation, but affords abundance of brushwood and shrubs, serving as food for camels; while, on the other hand, this immense expanse teems with herds of antelopes, ostriches, wadan (Ovis tragelaphus), and bagarr-el-wahsh, or wild ox.

There are no villages between the Jebel and Derge. Tlagsheen is a solitary well of indifferent water, 2 days' journey from Zintan; and the other points of my route marked on the map are mere halting-places and landmarks, the whole line presenting, at intervals, ridges of hills composed of sandstone, more or less blackened by the influence of the atmosphere, and occasionally, though seldom, a few sand-hills. In some parts the soil is remarkably fertile, yielding a rich pasture in spring, when it is visited by wandering Arabs with their flocks.

The Hamada (table-land) consists of an extensive stony plain, stretching across the Sahara as far as Fezzan; and Sreer-ej-jeleb is a wilderness, so named from its being often fatal to sheep while traversing it, owing to its utter sterility.

After a journey of 6 days, the caravan reached Derge, a distance of nearly 180 miles from the mountains. It consists of 4 oases—Derge proper, Tugoutta, Matris, and Ifilfelt—situated at no great distance from one another, and separated by ridges of stony hills. Matris is more elevated than the rest; a salt plain intervenes between it and Derge, which probably accounts for the unhealthiness of its climate. The water, likewise, of these oases, except that of Matris, is deemed unwholesome. The population may be computed at 2000, and, unlike that of Ghadamís, is essentially rural, yet speaking the same language. The staple produce is dates, of a superior quality, corn and gussob-grapes; apples and vegetables are also reared, but in small quantities.

These oases pay a tax of 1050 mahboubs (175*l.*) a-year to the Pasha of Tripoli, besides the other of a tenth part of the annual produce of corn.

As you proceed from Derge to Ghadamís, the road is at first rugged and difficult, leading through a deep labyrinth called Wady Attaf, then over high sand-hills to another hollow, the Shaabeh, whence it emerges in a north-westerly direction, bearing towards two high prominences, called the Krub, at the base of which lies the track that conducts to Ghadamís. The Shaabeh and Krub are noted haunts of the Shaanbah robbers. From the Krub the palms of Ghadamís are just perceptible, displaying a dark streak in the horizon; and, as you gradually approach this oasis, the soil undulates over thick layers of gypsum.

Concluding Remarks.—The distance from Tripoli to Ghadamís, as computed according to my measurements, is 320 geographical miles, the whole journey having occupied 17 days, exclusive of stoppages. This route is not the shortest, but, from the fact of its being less exposed to the incursions of the Algerian robbers, is preferred by caravans to the more direct one of Seenawan and Hraba, an easy journey of 10 days, and which I suppose to be some 250 miles long, at an average rate of 25 miles per diem. My journey may be deemed a fair rate of caravan-travelling in winter, giving as daily average 18 miles in 8 hours, or 2¼ miles per hour.

A remarkable feature in the geological structures of this part of the Sahara is the numerous shells and other organic remains with which it abounds. These are observable in the vicinity of Tlagsheen and in the neighbourhood of Derge; the localities in which they exist presenting more or less marks of sterility.

X.—*Notes from a Journal kept during a Hunting Tour in South Africa.* By HENRY S. GASSIOTT, Esq.

Communicated by Colonel SYKES.

Read March 22, 1852.

THE few remarks I now offer to the Royal Geographical Society will, I trust, be viewed with some indulgence, as my recent visit to Southern Africa was never intended in my own mind to be one of geographical research. The few observations which I have now to present to the Society may, however, be of some service in the guidance of future travellers.

On a recent map, kindly given to me by Mr. Arrowsmith previous to my departure from England, I marked my course by compass, taking 3 miles per hour as the speed of a regular bullock-waggon. This was entered in a rough journal which I kept, from which I have made extracts, comprising the particulars contained in the present paper.

I left London in the 'Agincourt,' accompanied by two friends, on the 19th of July, 1850. We arrived at Cape Town on the 20th of September, when we had the mortification of being informed by a gentleman who had just returned from the interior, that, in consequence of the disturbances with the Boers, we could not proceed on our intended route. Fearing to lose the season, we then chartered a small vessel to Angra Peguina. We landed here in a large bay surrounded by sandhills, extending, as I afterwards ascertained, for some miles into the interior. Here we were detained about 5 weeks, during which we rode to a place called Bethany, in order to procure the necessary oxen.

The country in the vicinity of the sea consists of a series of sandhills, and is entirely devoid of water, with which, during the short time traders remain, they are supplied from the Cape. The beds which are marked as rivers in the maps are dry at all seasons of the year. The nearest water is about 20 miles from the bay, but very brackish, at a fountain called Viow Viowsep, from whence the country continues sandy, with a grassy defile of some miles in length, named Teiras Flat, but without water. The next fountain we meet is Quebes, so called from its stony nature (Quep signifying stone in the Namaqua language). From Quebes the country to Bethany is barren, until you arrive within about 2 miles, where some camel-thorns and willows grow. At Bethany there is a fountain of fine water, and a missionary, named Kreutzen, resides there.

Circumstances prevented our continuing the intended journey, which otherwise might have proved interesting; but I was happy, on my arrival in England, to learn that Mr. Galton had succeeded in pursuing his course in this direction.

We remained at Bethany 3 weeks. The first water on the road from thence to the southward has to be dug for about 2 feet, when it flows comparatively freely. From this place to Kardop no water can be procured, the country being rugged and full of the mimosa, as far as Hudap, a large Bushman kraal, and Hoons, which is already marked on the maps. Here we found water, but the heat was fearful, 105° in the shade. From thence we proceeded to Kaidorp Grotpoort, where we obtained an abundance of water; and 2 days' journey brought us to the Orange river, which we crossed at a part between 200 and 300 yards wide. The country from thence to Cape Town has been often truly described as miserable, with a scarcity of water and scarcely any vegetation, excepting the euphorbias and ice-plant.

On our return to Cape Town my companions separated; one returned to England, and the other is still in the colony, prosecuting his researches in the interior.* From some information I obtained I was now induced to attempt another journey by way of Natal.

* Mr. Dolman, since murdered by the natives.—ED.

I once more left Cape Town, and after a long passage of 33 days (usually done in 10) I arrived at D'Urban. The bar is bad, not allowing large vessels to cross; but the coast abounds with fine timber. I only remained at D'Urban 4 days, proceeding with my bullock-waggon, which I brought from Cape Town, 14 oxen, 1 horse, 1 servant, a driver, and 2 Kaffirs, to Pieter Maritzburg, a nice-looking village, and well watered.

From this place the ground ascends by the Bushman and Tugala rivers until reaching the Drakenberg or Quathlamba mountains. These latter are of considerable elevation, taking about 4 hours for the waggon to ascend to the summit, and after about 10 miles of undulating ground we arrived at Nelson's Head, the highest point of the range.

The country now is undulating, but quite devoid of bush, for a distance of about 140 miles to the Vaal river, which, at the drift where I crossed, is about 100 to 120 yards broad. From hence the road proceeds by Suikerbosch Rand, an inconsiderable stream, flowing into the Vaal, at which place is found the first settlement of the Boers. The country is covered with grass, but is fertile in corn and well provided with cattle. The bush is scarce, but sufficient for supplying the people with fire-wood.

About here I found many farms within a few miles of each other, but afterwards for three days I met with no inhabitants until I fell in with a Boer named Erasmus, at whose place, "marked on the map," there was at that time a "lager" or Dutch camp. This consists of a circle of waggons, the interstices of which are filled with strong thorn-bushes, affording thus a good defence against attacks from the natives. Passing onwards through Darepoort I arrived at Pinner's River, flowing, I believe, into Eland River. From this I proceeded to a beautiful spot called Boukenhouts Kloof, and soon afterwards saw the Eland River. The country in this part is covered with the mimosa, and is very rugged. A herb, poisonous to cattle from the end of October to the end of November, is found here, but when fully grown in December it is quite harmless. After leaving the farm of Cobus-uys, on the Eland River, I proceeded to the station, or rather outspan, of Inkle Doorn, or "single thorn-tree," and thence to Kameelpoort, which is marked in Mr. Arrowsmith's last map; a branch of Eland River runs here. Kameelpoort is so named from the numbers of giraffes which are found here. From Kameelpoort to the N.W. I had a fine view of an extensive table-land called Macapan's Hill, distant about 50 miles. Between Kameelpoort and Macapan's Hill, to the S. of the latter, there is a place called the Bad, or Warm-bath, which is correctly given in Arrowsmith's new map. I was informed that with this exception there was no water to be found in that direction.

For the sportsman this is a most interesting spot, and every kind of animal common to Southern Africa, the elephant and hippopotamus excepted, is to be found here. From this place I proceeded through mountain passes to a farm kept by a Boer named Van Dyck, a Veldt-Cornet, with whom I remained some days. A few miles eastward of this is Moose River. With the intention of proceeding towards Delagoa Bay by the Masouasi country, I crossed Melon River—so called from the bitter water-melon which grows very luxuriantly on its banks. The river winds through a chain of mountains and discharges itself into Elephant River. Continuing my route in a S.E. direction I came to a farm belonging to a Boer named Andreas Peice, to whom I had a letter of introduction; he was unfortunately from home on a mission to Delagoa Bay, and here I was stopped, being refused permission by the Veldt-Cornet to proceed through the Masouasi country. I had now no other resource but to retrace my steps and proceed by a somewhat different route to the northward to Leidenburg, a place containing about twenty houses, a fort, and chapel; here I applied to the landrost for permission to proceed, which was again peremptorily refused. Through the kindness of a Boer I was enabled to rest my oxen, and proceed with him in his waggon to Origstadt, situated in a fertile but unhealthy spot, surrounded by mountains. I was here attacked by fever and ague, and returned to Leidenburg, whence I proceeded through Steelpoort and Magnetshoek, on an inclined plain about 3 miles long. I found the ground thickly covered with magnetic ore, of which I brought home a specimen. At Steelpoort there are numbers of shells, two of which I also brought to England.

From Magnetshoek I proceeded to Soquati's kraal. This, the principal chief of the Mantatees, furnished me with a number of his tribe to attend and assist me in hunting. The water here is detestable, and a large kind of cactus grows in great abundance. Crossing the Elephant River several times, I left my waggon and oxen under the charge of my servant, and proceeded on horseback with the driver and pack-oxen, and accompanied by eighty to one hundred Mantatees. Avoiding Zout-pans-berg, the furthest settlement of the Dutch Boers, who would in all probability have detained me, I arrived at the Limpopo, which I crossed, and which was my furthest point. The river, at this part, was about 200 yards wide, very shallow, in some parts not over the horses' knees, and alligators were abundant. Before I arrived at the Limpopo I found the bush very thick and abounding with the flies (setse) so destructive to cattle and horses, and which render travelling in this part of Africa so very difficult.

Previous to my leaving England Mr. Arrowsmith had urged upon my companions the desirability of endeavouring to trace the course of the Limpopo to the sea, and I had not forgotten their conversations with this gentleman, but I regret that it was out of my power to collect any certain information on this point for the Society. I may, however, add that I made every inquiry in my power of the Boers, several of whom informed me that they had penetrated far into the interior; one, named Trechart, had been as far even as Sofala. They all affirmed that the Limpopo and Elephant River join each other and then flow into the ocean at Inhambane, a Portuguese settlement on the coast. At the junction the river is said to be over a mile in breadth. The Elephant River is in places very rapid, full of falls and drifts. In conclusion I can only say that much self-denial, untiring energy, and dogged perseverance are indispensable before geographical discoveries can be made in this part of Africa; and even these qualifications will be of little avail, unless assisted by subordinates possessing local knowledge of the country and of the habits of the Boers, as well as of the natives. The prejudices of the Dutch Boer are great in the extreme; he views every stranger with suspicion, and, contented with his own uncontrolled sphere of existence, he aspires to nothing beyond. His hatred to the English name, however, I found more intense even than I had been led to suppose.

XI.—*Recent Expedition into the Interior of South-Western Africa.* By Francis Galton, Esq., F.R.G.S.

Read Feb. 23 and April 26, 1852.

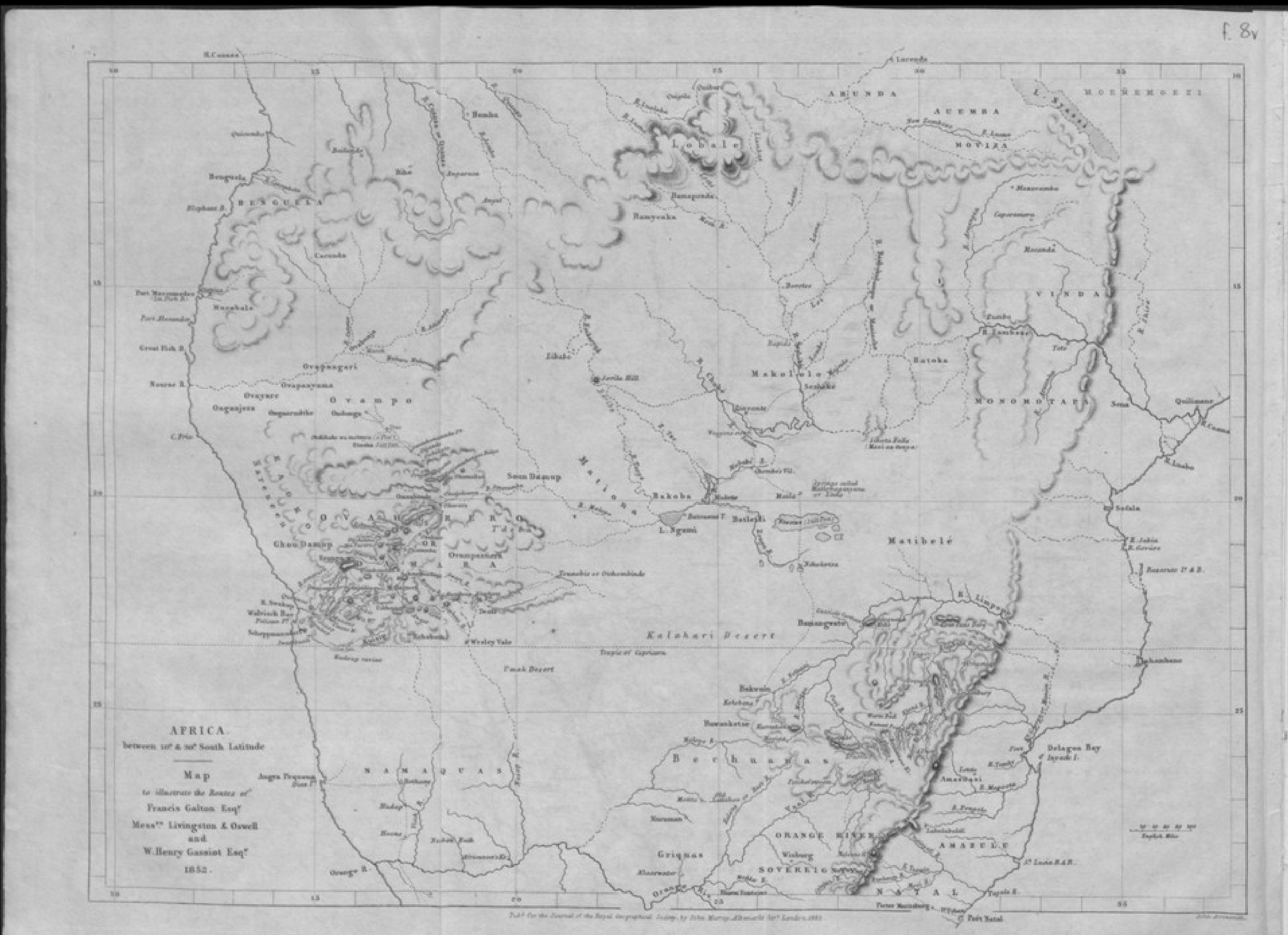
Mr. President,

A little more than two years ago, urged by an excessive fondness for a wild life, I determined to travel for a second time in Africa. I then became a Fellow of your Society, and through the active kindness of Dr. Shaw your Secretary, of Mr. Arrowsmith, and of others, I was thoroughly advised as to those geographical points which more immediately awaited inquiry, and, guided by their views, chose South Africa as the field of my travels.

I left England in April 1850, accompanied by Mr. Andersson, a Swede, to whose most active and cheerful co-operation throughout a tedious and harassing journey, I am in the greatest degree indebted. He still remains in Africa, principally with a view of investigating the natural history of the lake district, and of thence bringing home a complete collection of specimens.

At the Cape, upon the strong recommendation of Sir Harry Smith, I freighted a vessel for Walfisch Bay, instead of travelling the usual route from Port Elizabeth. The emigrant Boers had at

f. 8v



AFRICA.
between 10° & 30° South Latitude
Map
to illustrate the Routes of
Francis Galton Esqr
Messrs Livingston & Oswell
and
W. Henry Gassiot Esqr
1852.
Benguela
Lobale
Makololo
Monomotapa
Matibelé
Bechuanas
Namaquas
Kalahari Desert
Tropic of Capricorn
L. Ngami
Walvisch Bay
Delagoa Bay
Natal
Port Natal
Quilimane
Orange River
Griquas
Amazulu
Ovampo

that time assumed a menacing attitude, and it was currently believed that they intended taking immediate possession of the lake country, and of refusing passage to all travellers from the Cape. Two parties had already been turned back; and as on the one hand there was every reason to believe that the same course might be adopted towards me, and cause a fruitless result to my journey, so on the other, the country to the north of Walfisch Bay was an entirely open field for exploring, and I proceeded thence.

At Cape Town I could obtain but little information about even those parts, in which missionaries had already formed stations, and what I there learnt was also much exaggerated, as the country was believed to be extremely fertile and very populous. The Damaras, into the heart of whose country no white man had ever penetrated, were described as a most powerful, numerous, and interesting nation; and the fact that some traders had settled at Walfisch Bay, whence large droves of Damara cattle were dispatched south, shipped to St. Helena, or sold to the, at one time, numerous guano and whaling vessels, seemed to warrant the opinion of the fertility of the country. This view was again confirmed by the great jealousy shown to the attempted expedition of our late member, Mr. Ruxton, afterwards so well known by his travels in America, who, when he landed, experienced such determined opposition and obstructions, that he was compelled to set sail without having penetrated more than 20 miles into the country.

Warned by his failure I took mules with me, besides my waggons and a cart, in order that I should be, to a certain degree, independent of assistance, and be able, at least, to carry my things across the barren desert, which intervenes between the coast and the more habitable parts.

I was also requested by Sir Harry Smith to establish, if possible, friendly relations on the part of the Colonial government with such tribes as were liable to be exposed to the attack of the emigrant Boers, and to disavow strongly all sympathy on its part with them. Indeed a mere expression of good will, without holding out the least prospect of direct aid, is a custom much valued and well understood by South African tribes generally.

I landed in Walfisch Bay, the estuary of the Kuisip, in August, and was very hospitably received by Mr. Bam, the Rhenish missionary. Some time and great trouble were required to drag all my heavy things with my few mules across the sandy desert, already mentioned, to his station, where they could remain in security whilst I went up the country to buy and to break in oxen for my onward journey. It would be out of place here to allude particularly to the extreme difficulty I experienced before all this

f. 9v

could be done and I could make my final start in February from Barmen. The country was in the utmost confusion; the Namaquas were robbing and murdering the Damaras in every direction, and had indeed, just on my landing, attacked and destroyed the Schmelen's Hope Station, that of Mr. Kolbe. I was fortunate in having good interpreters, and a black servant whom I had taken from the Cape, and who was born in some central part of Africa, found the language so much like his own, that by the time all was ready for a start he had become quite fluent in it; besides whom, I had a man, Damara born, but bred among the Hottentots, and to communicate with him, an excellent Dutch and Hottentot interpreter; so that I got on from the first extremely well.

The knowledge that Europeans possess of these parts, dates originally from Sir James Alexander, who, chiefly in company with Swartboys (a Hottentot chief), travelled from the south to Bethany, Rehoboth, and thence to Walfisch Bay; on his return he wrote to two missionary societies, recommending this country as a favourable field for missionary enterprise, in consequence of which some German Protestants and English Wesleyans were sent there. Their head-quarters were at and near Eikhams, the present home of Jonker Africaner, who was at first a warm supporter, but latterly a bitter opponent of them. Subsequently the Wesleyans extended their missions towards the interior, and the Germans along the coast. Their present stations are marked on my map. When I arrived no European foot had ever penetrated 20 miles to the northward of the 22nd parallel of latitude, or 20 miles to the eastward of Elephant Fountain.*

My course from Barmen, as marked on my map, led me through the middle and most populous part of Damara land, at the further frontier of which one of my waggons broke down. I thence rode on across a bushman tract to the Ovampo, and returning, joined my then mended waggon, and came back to Barmen, which I reached in August 1851. I thence sent a messenger overland to the Cape to make arrangements for forwarding a vessel to meet me at Walfisch Bay in December or January, and lightening one waggon as far as I could, went with all my available oxen on a quick journey eastwards. At Elephant Fountain I joined Amiral, a Hottentot chief, and leaving my waggon there, rode on with him and explored to Otchombindé (called by the Hottentots T'ounobis), and thence returning, arrived with utterly exhausted oxen on the coast in December 1851. From Walfisch Bay I sailed to St. Helena, where I waited some weeks

* In Sir J. Alexander's, and in the missionary maps, the positions of the more distant parts explored by them are laid down very erroneously. In one map Elephant Fountain is placed one hundred miles too far towards the interior.

in vain for an opportunity of making a short excursion to Little Fish Bay and of obtaining some information from thence. I consequently set sail, and arrived in England at the end of last month, after exactly two years' absence.

To avoid misconception I must give some explanation concerning the names which I have placed on my map, in the selection of which I had some difficulty. In all the border country, and where the missionary stations now exist, most places are known by two, three, or even four names. The Damaras have one, the Hottentots another—this latter, translated into Dutch, forms a third, which is used very generally—and the missionaries add a fourth; thus the place marked Scheppmansdorf, which is called Aban'hous in Hottentot, is always known as Roëbank by the traders and as Scheppmansdorf by the missionaries. It would have created great confusion to have attached all these different names to each place on the map, and I have therefore adopted the missionary names for their own stations, Damara names for all places that have them, and used Hottentot words as little as possible, for no orthography can possibly express their sound, except in rare instances, such as T'was and T'ounobis, which are capable of being pronounced. With perhaps less reason I have adhered to the Dutch word "Damara" to express the Ovaherero and Ovampantieru tribes, as it is a convenient name and one that has been long established, and which has as much right to pass current as the word "Caffre." The Hottentot name for that people is Damup in the plural, or Daman in the singular, and this is the root of the name "Damara," which it is needless to state is utterly unknown to the people themselves.

The country over which I travelled, proved to be the broadly developed end of that chain of hills and high land which runs parallel and near to the western coast from the Cape colony upwards, and separates the Fish River from the sea. Though this country is dotted over with hills and even groups of hills, and is very deeply scored on its western face with watercourses, yet in its general aspect it consists simply of a plain sloping steadily away on all sides from a small district of the greatest elevation, which is situated about the sites of the mountains Omatako, Diambotodthu, and thence to Awass, and which (from boiling-water observation) lies some 6000 feet above the sea-level. From this district, the watershed *eastwards* falls with a very gentle inclination to the cup-shaped basin of Central South Africa—to its lake, its flooded lands, and interlacing rivers; *northwards*, with still less incline, to a large river, of which the Cunene is a tributary, and which appears partly to drain that basin; *southwards* from Awass, Fish River begins its long and peculiar course towards the colony; and the comparatively steep *western* slope is ploughed up by the Kuisip, the Swakop, and five other more northerly river courses, which run into the Atlantic.

The sea-face of this broad belt is, except along the water-courses, uninhabitable, as during half the year there is no water and scarcely any pasturage. A strip of desert sand, 40 miles wide, follows the coast line, beyond which lies, north of Walfisch Bay, the barren Kaoko, and to the south of it the arid Namaqua land. The summit of the belt is a dense impracticable thorn coppice, though affording grass and a few scanty springs; but as we descend westward, and at about 220 miles from the coast, the thorns almost cease, and the land assumes the appearance of those broad plains, covered with grass and timber-trees, that have so often been described as lying between the Orange River and the Limpopo. Again, in the far north, at the latitude of Ondonga, the country becomes one of most striking and peculiar fertility.

Over all these parts the rains are periodical, and, from the nine years' experience that the Rev. Mr. Hahn has had at Eikhams and at Barmen, very variable. From the middle of May to November rain is scarcely ever known to fall, thence to January occasional and sometimes very heavy showers occur, but the true rainy season may be considered to be between the first of January and the last of April; the showers are extremely violent, but partial, and are always accompanied by thunder. The ground is seldom saturated till February, and then pools of rain-water (Vleys) are to be found everywhere; but, by June, all but the largest of these are dried up again. As a general rule, the rains fall most heavily on the summit, and on the northern and eastern slopes of the country, and, at Ondonga, they were described as being much heavier than in Damara land. The rivers are all *periodical*, and run to very different extents in different years. The Kuisip had been seven years without reaching the sea, and then almost, if not quite, reached it three times in six years. Of late, the Swakop has flowed three or four times every rainy season; yet, when it was first seen by Europeans, about ten years ago, the whole of the lower part of its course was choked with sand-hills, bushes, and trees; these the first inundation swept entirely away, since which most violent torrents have passed down it. On the other hand, it was a constant complaint of the Damaras, that less rain falls now in their country than some twenty or thirty years back; and even their extensive migration from the Kaoko, to which I shall have occasion hereafter to refer, has been ascribed by the Damaras to the water failing them for their cattle.

It may, perhaps, give a more accurate notion of the country I visited if I describe in some detail a route through it. Leaving the excellent, but perfectly desert, harbour of Walfisch Bay—a journey of 16 hours across sand, soft and sinking at first and covered with shifting dunes, but afterwards hard and pebbly and crackling like frozen snow under the feet, takes us to Oosop. Ten miles before reaching the river, the plain shelves steadily and rapidly downwards to its bed, to which we descend at last through an imposing gorge about 300 feet wide and 4 miles long. The river-bed here is 100 yards broad, and consists of heavy sand, overgrown with patches of grass, and fringed on either side with a dense row of high reeds, beyond which, where the rocks leave sufficient space, are some fine groups of Unna trees. From the middle of the bed, a small streamlet springs out in all but the very driest seasons of the year, and after running some distance loses itself again in the sand. Notwithstanding a general appearance of drought, marks of violent torrents are everywhere visible,—trees lie uprooted, heaps of dead sticks and reeds and mud are washed high on the ledges of the rocks where they confine the river-bed, and also on the lower branches of the trees that still remain standing. The cliffs on each side are precipitous and magnificent. From Oosop to Davieep there is, perhaps, no one single place where an expert mountain-climber could get out of the bed on the north or right bank of the river, and only two places where cattle can be driven up from the left bank. The rocks are so bold and so broken, especially on the right bank, that a traveller can hardly realise the idea that they are not independent mountains, but only the face of a deep cutting which the river has made for itself, and that the general level of the country is from 800 to 1000 feet above his head. I ascertained this elevation as well as I could, by climbing up a hill on the left bank, the height of which I measured carefully to be a little more than 600 feet; then from the top of it I levelled across to the opposite cliffs, from the top of which the plain began, and, with my sextant, guessed at the remaining height. The plain north of the Swakop appears at this place to be quite level and barren, but not sandy; and it is almost, if not quite, uninhabited. I had good views of it from many different heights. The Canna river cuts its way through it in exactly the same manner as the Swakop, though its cliffs are described as being even higher. I could trace its course for a distance, from a hill near Hycomkap; and an appearance along the plain, as if the ground were broken up, indicated, for 20 or 30 miles, the gorge through which it ran. On leaving Davieep, the cutting through which the river flows loses the character of a gorge, and the mountainous sides open out more, continuing still to bank up the plains. Those of Onanis, 20 miles east from the left bank, give excellent pasturage, and the desert sand ceases about Tincas. Passing up the Tsobis river to avoid the deep sand of the Swakop, after 7 hours' up-hill travelling, through gorges nearly as striking as those of Oosop, we emerge on the plain, which we now find everywhere covered with thin grass, and studded over with stunted thorn trees. The "Hakis," or Fish-hook thorn, as the Dutch call it, begins to grow at Tsobis, but the land more to the westward is too barren to give sustenance even to that, and from this point to the borders of Ovampo-land the traveller has to tear his way through its cruel and tangled branches. Not a tree grows that does not bear thorns, and very few in which the thorns are not hooked; and their sharpness and strength are such as to throw a most serious difficulty in the way of exploring, especially as when travelling with a waggon, the oxen will not face them, and in difficult parts it is often quite impossible to get through the bushes, round to the struggling and fighting oxen. Cruel as the Hakis thorn is, there is yet another and much more severe opponent in a smaller, but sharper and stronger thorn. I have frequently tried the strength of all these with a spring weighing machine, by tying a loop of string to one end of it, which I hooked round the thorn, and then steadily pulled at the other end till the thorn gave way, marking the number of pounds resistance that the scale indicated at the moment the thorn broke; the Hakis thorn stood a pull of 4 or 5 lbs., and the other one of about 7, but often much more, and on one occasion I registered a strain of 24 lbs. Now, as several of these thorns generally lay hold of the traveller's clothes or person at once, it may easily be conceived what cruel laceration they cause. Continuing our route we descend to the Swakop again, near Otjimbingue, having, when at Tsobis, just caught sight of Erongo, a mountain 3000 feet above its base, but rising from the deep hollow of the Camna. From Otjimbingue to Barmen the river passes again through a broken, confused series of gorges, and among mountains; and it is not until we are far past Schmelen's Hope that we arrive at the source of the Swakop, and entirely clear of its valley. At the time of my visit to these countries, Schmelen's Hope, and a very few miles north of it, was the furthest point known to the missionaries, and other Europeans. As I travelled northwards, ascending the plateau, I saw the tops of the hills by the river, that had appeared so prominent when among them, slowly sink down below my level, and disappear among the trees. Diambotodthu no longer bounded the prospect in front, but on a sudden the two magnificent, almost faultless cones of Omatako burst full into sight, each appearing like a Teneriffe, beyond which was the broken ground of Otjihinna ma Parero, and the long wall of Koniati, that bound the arid Kaoko. I had but just left a tributary of the Swakop, still a broad river bed, when, to my surprise, I came upon another water-course of considerable size, running N.E., which I followed some distance, and which I found went towards the Omoramba. It seemed incredible that a water-course 30 or 40 yards broad, with steep banks, could have an origin in the open plain within a mile, but I found afterwards that this sudden commencement of broad river beds was the rule, and not the exception, in Damaraland. I had also constantly noticed that the breadth of the river beds was often out of all proportion to the quantity of water that they could ever carry: thus the Erora, which has not a course of more than 20 miles and is by no means an important drain to the country, is about 500 yards across, but I found that the same cause influenced both the length and the breadth of the river bed. It must be recollected that the ground is entirely sand, but well fixed on its surface by the long running roots of the grass that covers it. The wet in the rainy season drains through the sand into the river bed, and, of course, constantly washes away some with it; but the subsoil yields before the surface, and thus the banks get gradually undermined, and are always falling in, so that the river has a constant tendency to grow broader, and to push its apparent source higher up towards the water-shed. It is very curious to see the head of one of these river courses, where the ground seems to have fallen in suddenly, leaving a place like a gravel pit, whence the bed begins at once some 12 or 20 yards wide, and perhaps 10 feet deep.

In the case of the Omoramba K'omatako, whose course lies alternately over districts of sand and over hard ground, it is very curious to observe how, what in the first case is a fine magnificent river bed with high banks, suddenly, as the ground becomes hard, loses itself in the open plain, where there is not a vestige of its course; and a few miles further on, the ground becoming sandy, the river bed re-appears again, just as unexpectedly as it had been lost, and altogether as large as before.

I had made a considerable détour to avoid a very hostile tribe of Damaras, who were then encamped on the Omoramba, and through whose neighbourhood my men refused to attempt a passage. I, therefore, guided only by such vague information as I could then occasionally procure from the savages, went under the escarped sides of Omuvereoom, at the termination of which the reported lake Omanbondé was said to lie. Through the whole of this road I had to trust to chance in finding water, and in also finding a practicable road for waggons. At this time my men were undisciplined, and in no way to be depended upon. My oxen were only half broken. There was fighting going on between two powerful tribes immediately behind us, and a dense jungle of thorns surrounded us on all sides. Of game there was none, so that it was impossible to depend on anything else but my oxen for food. The waters were drying up on all sides, and the prospects of the expedition became gloomy enough. I chanced, however, to fall upon a curious watercourse, that we named the River Vley. It was a narrow strip of green, not much depressed below the country on either side, which contained frequent shallow pools. It was simply a succession of Vleys or pools, and varied from thirty yards to much more in width, and here and there stretched out into broad plains; the thickest of thorn jungle, and one perfectly impassable to a waggon, pressed close upon it, so closely that the waggons were frequently taken through the water where there was not room for them between the Vleys and the thorns.

It is wonderful how little inhabitable country there is in this part of Africa. Either the thorns occupy the ground to the exclusion of everything else, or drought makes it unfit for cattle. In fact, Damara-land is made by the watercourses and a very limited number of springs; take away these, and no pastoral people could inhabit the country; whilst agriculture, except to the most limited extent, is in all cases out of the question. The watercourses, though utterly arid to all appearance, are really to a great extent reservoirs of water, which is checked in its evaporation by the great depth of sand that overlies it. There are places known to the natives in most of these river beds, and which probably correspond to the lowest parts of the longer reaches, where water can be got by digging; but it is useless, as I am well aware by long experience, to dig deeper than the sand, for no water exists in the hard ground below it. It must not, therefore, be in any way supposed that, because I have dotted out in the map a large space and called it Damara-land, the whole of that area is occupied by these people. The case is far different. The number of pasturages is extremely small; and I am sure that I myself have seen and know quite half of them. It will be easily understood also that the boundaries of a people like the Damaras are exceedingly arbitrary. It rains perhaps heavily one season, and there is abundant Vley water at a distant place, where the pasturage is also good; the neighbouring tribes, of course, flock there, and spare the grass nearer their usual haunts and more certain waters until the dry season. The boundaries are not definite and natural, excepting so far as the long range of Omuvereoom is concerned; but they are decided to a certain degree by custom, and I have endeavoured, under this view only, to represent them on the map. One point bearing on this subject must not be forgotten—that two African tribes never live close up to a common frontier. They are always fighting and robbing, and therefore a broad border-land is essential; and in these border-lands, so far as I have seen, the Bushmen and other outcasts live. As regards the water that these get, it will

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easily be understood that many places are found which yield enough for a small family, but which would scarcely support two or three oxen. The water oozing slowly into a well from the damp sand surrounding its bottom, at the rate say of a gallon in a day, is a case often observed, and then the tracks of Bushmen are pretty sure to be seen about it also. But to proceed with my itinerary. Just where the Vley River began to be lost the bushes became more open, and Omuvereoom sloped down into the plain, and I here met with guides who took me straight to Omanbondé. This had been constantly described to me as a large lake. I thought it might have been the Demboa of early maps, with whose position it fairly coincides, and to whose name Omanbondé bears a certain resemblance.

The occasional existence of hippopotami in it was also thoroughly substantiated, and yet, when I saw it, it was *perfectly dry*. The place is a remarkable one enough, for it is the long reach of a watercourse, closed up at both ends by a dam, which, together with its sides, slope upwards till they attain an elevation of about 100 feet above the bed. The breadth of Omanbondé is trifling, but its length is some 8 miles. Going downwards, and passing over a broad dam, we come into another reach also dammed up, and then again into another. N. of Omanbondé there are also two other places just like it. The water evidently filters through these dams in the rainy season; and I was assured that, even in the driest times of usual years, Omanbondé was a reservoir of water: I, however, to my great misfortune in many ways, chanced to travel during a year of great and almost unprecedented drought. The course of the Omoramba downwards was so clogged with thorns as to be quite impracticable for a waggon, and it was indeed with great difficulty that we even crossed it.

On leaving Omanbondé, and getting out of the valley of the Omoramba as well as I could, we came to a far more open plain than any I had hitherto met with; and suddenly, at lat. 19° 50′, found ourselves among palms—the harbingers of a better land. A little further on they flourished to the exclusion of almost every other tree; but before we came to Okamabuti had ceased almost as suddenly as they began, though during the whole road to Ovampo-land one or two were every day to be seen. We had now arrived at a much more luxuriant country, and water was plentiful: a long limestone ridge at Kutjianashongué yielded springs in numerous places, around which large herds of cattle and numbers of Damaras were collected. Timber-trees began to appear, growing in clumps, with long open grassy savannahs between them; and to me it was a constant wonder to observe the straight and perfectly defined borders that these belts of wood presented. Here and elsewhere I have seen them look, not

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as one would conceive that Nature could have planted them, but presenting exactly the appearance of the work of an ornamental gardener. I am in no way able to account for this striking peculiarity, as there is no perceptible difference in the soil on which the trees grow, and in that where they are absent. I cannot explain the fact, but simply state it. Okamabuti may be considered as the northern boundary of Damara-land, though in the rainy season the natives sometimes go further. The country is said to be quite impassable to the N.E. It appears to be entirely uninhabited, and is thickly wooded. I made an excursion to a hill in that direction, about 8 hours off; but, so far as I could see from the top of it, one level forest extended far away.

The masses of hills that lie to the N.W. of Okamabuti are all limestone. I saw a good deal of them from the guide having lost his way more than once when he first took us there, which ended in compulsory and anxious wanderings for more than a week about them. A great many Bushmen live among these hills. I saw there a most curious freak of nature, which I afterwards witnessed on a far more magnificent scale at Otchikoto. Wherever a piece of bare rock is to be seen (which is nearly everywhere between Ootui and Otchikoto), it is pierced with holes perfectly circular, and of all sizes, and like round smoothed tubes. Thousands of them would just admit the thumb, and are quite shallow; numbers are about the diameter of a bucket, and from 3 to 5 feet deep, forming most dangerous pitfalls: in many of them we find trees growing, some not quite filling the hole, others just fitting it, and, again, others so constricted that the trunk swells over and entirely hides the sides of the hole. I saw a few holes about 8 feet across, but I do not recollect observing any intermediate size between that and Orujo, a perfectly circular hollow in the midst of chalk about 30 feet deep and 90 feet across. The sides of this were certainly not smooth, but they formed an exact circle, like a gigantic pan, the floor of which was level, with a small well in the middle. Otchikoto was still more astonishing. Equally circular, and its sides equally steep, it measured 400 feet across, and was almost filled with the clearest of water, the level of which stood at 33 feet below the banks, with the extraordinary depth of from 170 to 180 feet, which I plumbed in five places. I heard there was another, if not two more of these places, somewhere among the Soun Damup. The water-level of Otchikoto was, as I was told, and I could myself gather from appearances, not increased in the rainy season.

I was fortunately not encumbered here with my waggons, for I do not think it would have been possible to have taken them on through the thick forest. Here there is not a single landmark to catch the eye, and nothing but the most skilful tracking could find the road when the rain had obliterated the spoors of the preceding year. We got water at Otchando, and came to the first Ovampo cattle-post at Omutchamatunda. Travelling on, we arrived suddenly at the large salt-pan of Etosha, which is about 9 miles across from N. to S., and extends a long way to the W. The mirage was too strong to admit of my measuring the distance of the high banks that there bound it, and which I could just make out both as I went and as I returned.

This lake is impassable in the rainy season, but was perfectly dry when I saw it, and its surface was covered over in many parts with very good salt. A little further on we come to the remarkable Otchihako-wa Motenya, a perfectly flat, grassy, but treeless extent of country, stretching like an estuary between high and thickly wooded banks. It is said to extend a very considerable distance W.; indeed, I cannot help thinking even down to the sea-coast. I passed it near its head, where it was only 12 miles across; but where the Ondonga and Omaruru route crosses it, it is a long day's journey from side to side, and all the Damaras who had been that route assured me that it extended as far as they knew to the W. Again, the Omaruru and Onganjera route crosses a flat of three days' extent, but in which there is some water, and which is asserted, and indeed appears, to be identical with it. It is looked upon with great horror by the Damaras from the bitter coldness of a night passed upon it, as there is of course no fuel and no shelter.

It is difficult for me to express the delight that we all felt when in the evening of the next day we suddenly emerged out of the dense and thorny coppice in which we had so long been journeying, and the charming corn country of Ondonga lay stretched like a sea before us. The agricultural wealth of the land, so far exceeding our most sanguine expectations,—the beautifully grouped groves of palms,—the dense, magnificent, park-like trees,—the broad, level fields of corn interspersed with pasturage, and the orderly villages on every side, gave an appearance of diffused opulence and content, with which I know no other country that I could refer to for a parallel.

I arrived ultimately at Nangoro, the king's werft, where I spent three weeks most pleasantly. But my oxen had fallen lame and sadly out of condition, and I felt some misgivings as to whether they could even take me back, and there was no grass for them at Nangoro's to eat. All his cattle were sent far away to the cattle posts. Half my party were left scarcely in a fit state to protect themselves among the Damaras, and I had often anxious thoughts for their safety. My provisions were getting very low, and unless more cattle could be bought in Damara-land we had not sufficient to take us back even to Barmen, where we had left the missionaries in too great want to be able to help so large a party as ourselves. The country too was fast drying up, and the road southward might become impassable; still the *great river* was only four long, or five comparatively easy, days ahead; but this and the return journey, together with the rest necessary for my oxen, I was aware would be at least a three weeks affair, and I hardly knew what course to take in case Nangoro would give me permission to proceed. It was certainly with regret, yet still with a feeling of relief, as putting an end to my indecision, that a message was at length received from Nangoro, prohibiting me from proceeding farther. If I had been in a condition to temporize, I have no doubt that I could have persuaded him to let me proceed, but that was now out of the question, and I therefore took leave and returned. Fortune now favoured me in many ways. I found my waggon mended, a sufficiency of cattle bought, and obtaining a guide, returned by a good road up the Omoramba without much difficulty, except in having nearly every day to dig or to clear out wells, which fully employed my whole party, now consisting of thirty-four people.

Returning to our starting point, Barmen, I will next describe the route which I followed to the eastwards, and which is very interesting, as it presents a peculiarly easy and open highway to the interior, and one practicable at almost all times for waggons, though indeed I—travelling at the driest time of an unusually dry year, one in which many of the Damara cattle perished of thirst, even at their own cattle posts—failed in reaching Lake 'Ngami. Proceeding up a small, but frequently running, river-course, a tributary of the Swakop, to Eikhams, and thence by a well-made Hottentot waggon road, over a very broken and arid country, we ascend out of the valley, keeping the high ridge of Awass to the right hand. We are now upon an elevated open plain, presenting no difficulties whatever to waggons if we follow the course of the Kuyip, but the ground that borders the upper part of the Noosop, by which I went, is very rugged and thorny. There is far more water to be got all about here than in Damara-land, but this being at present the border country between the Hottentots and Damaras, the wells were not generally opened. From Kurrikoop eastwards no anxiety need be felt for food, as there is plenty of game, though the animals are exceedingly shy. The ground is sandy and undulating. Proceeding on, we get to Elephant Fountain, beyond which there are no peaked hills, nor landmarks, in fact, that could be laid down in the map, and thence recognised by a future traveller. Elephant Fountain had been a Wesleyan missionary station, but was abandoned for the double reason of being subject to fever from April to June, as well as from its vicinity to some warlike Damara tribes. There is nothing in the appearance of Elephant Fountain that would suggest an idea of unhealthiness; it possesses, indeed, no peculiar feature, but it stands well on a barren and thorny hill; the—here contracted—bed of the Swart River is below, and there is a small, clear spring, which supplies water. Most fearful attacks of fever have year after year been experienced at the place, but not, so far as I could learn, anywhere else in the immediate neighbourhood. At Elephant Fountain I left my waggon, and rode on with a Hottentot chief, Amiral, and about forty of his men, to the eastwards. They had lately explored a long limestone ridge of hills that extends some 50 miles from T'was, and which is greatly intersected by water-courses, headed by springs, and along which we went. It appears to be of about 20 miles breadth, and attains a height of at least 1000 feet above the general level of the country. I consider it quite as a natural boundary between the thorny country of Damara-land and the broad, sandy, and wooded tracts of Central Africa. I contrived to get Bushmen guides to take us and about half of Amiral's party to T'ounobis, which we reached after a journey most trying to the oxen. The road passed by many large but dried up vleys; the ground was sufficiently hard, and would at ordinary seasons afford an excellent road for waggons, which after leaving T'was should pass not on the top of the ridge, as I did, but skirt it in the plain. T'ounobis is a fountain in a river-course, sufficient to supply any quantity of cattle, where I remained a week recruiting my oxen, of which I had barely sufficient to carry me back. I found a large village of Bushmen there, from whom I received much information concerning the lake and the country ahead. The land in front, up to its very borders, was described as being exactly the same as that we had now traversed. Hard sand, with plenty of trees, but not so thickly overgrown as to form any obstacle to a waggon, and growing but very few thorns,—indeed, I had great difficulty in getting thorn-bushes, of which to make my sheep kraals. So far then as T'ounobis I can guarantee the road from Walfisch Bay towards the interior to be perfectly open at any season of the year, and, except in the driest of times, from T'ounobis onwards. T'ounobis was passed by the Kubabees Hottentots in 1850. They had come upwards along the Umak Desert on a plundering and shooting excursion, with horses and oxen in great numbers to ride on. They had also built shooting-huts by the waterside, which I used, and had left other tokens of their passage. At T'ounobis they obtained a guide, whom I saw, and from whom I received much information, and under his escort they reached the lake.

A perfectly marvellous quantity of game congregated here;

deep pools of water that were supplied by a fountain were drunk dry every night, and I therefore more readily believed in the constant assertions of the Bushmen, that there was then no water whatever for a distance twice as great as that over which we had travelled ahead.

Having now described briefly the geography of those parts that I visited, I will next state what I learnt from various natives respecting the countries ahead of me.

At T'ounobis I received the fullest description from the Bushmen natives of a lake called by them Il' Annee, which they often visited, and which I now feel assured to be Lake 'Ngami. It had been reached thence from T'ounobis by the party of Kubabees Hottentots, that I mentioned above, in 7 days in August 1850, and the direction of its nearest point was uniformly stated to lie thence N. 50 E. true (N. 75 E. compass). The chiefs of the black tribes at that point were Maharaquè and Tworrathabè, names which are identified by Mr. Oswell as being those of the chiefs of the Maclumma on the S.W. of the lake. The distance represented by 7 days' journey in these parts would be pretty nearly 120 geographical miles measured straight, certainly between the wide limits of 100 and 140 miles.

I also heard much of a large river whose "lay" was N.N.W., and which joined the lake. This evidently is the Tso, but it was described to me as being called the Beribè (in Sichuana), and as the T'guain Tl' Obo (in Hottentot). As regards the course of this river I was assured that it ran out of, and not into, the lake, but my information was not such as to withstand the more immediate testimony of Mr. Oswell, corroborated strongly as it is by the general features of the country.

This river, the Beribè (or Tso), was stated to pass entirely through the country of the adjacent tribes, and far in a N.N.W. direction to the other side of them. A much smaller stream S. of the Beribè, and having the same general course, was described as joining it just where it met the lake. This last streamlet, the Malopo (in Hottentot the T'kains), on which there are no boats, is separated during its course from the Beribè by a range of hilly country, called by the Hottentots the T'dèba. The Omoramba, at a distance of about 90 miles easterly from Omanbondé, meets another dry river-bed, and the two together ultimately reach some large water, but which I do not think to be the Malopo. It appears to be stagnant or nearly so, but I received very contradictory information about it; the large river (the Beribè) is beyond it. The Omoramba at about 60 miles from Omanbondé passes through a very hilly country, which, as far as I could make out, was continuous with the T'dèba. I have mentioned that hippopotami have constantly made their appearance at Omanbondé when there was water there; this is a sure proof that the Omoramba cannot be entirely lost in the plain, but must join a large water, some such as I have mentioned. About the lower part of the Omoramba a peculiar race of negroes (the Soun Damup) live, and extend very far to the northward. I shall refer to them again later; they were described as living N. 15 E. true from T'ounobis.

My Ovampo information refers to a large river that runs from E. to W., and which is 4 quick or 5 easier days' march (say 100 miles due N.) from Nangoro's werft. It is a broad, swift-flowing stream, to the border of which Portuguese traders come and traffic. The ferry, which is chiefly used by the Ovampo, lies N. 19 E. by compass, or N. 7 W. true, from Nangoro's werft in Ondonga, and is near the junction of the two streams which principally form this river. One of these, the larger, comes from the very far E., the other from the S.E. and from the Mationa country; Mationa being the name given both by the Ovampo and Damaras to the tribes living on the Beribè, including those belonging to the chiefs whose names I have already mentioned. The Cunene was said to run into this river, but of its point of confluence I am not satisfied. Mr. Oswell informs me that he had always conceived an idea, from what the natives told him, that the Tso was in some way connected in the far N. with a large river running to the W. The Mationa river, mentioned above, may be this link. Where the embouchure of the Ovampo river may be I have no idea, but I have many reasons for thinking it not to be the Nourse. The captains of coasting-traders in those parts assured me that the Nourse is a periodical water-course, while I learnt from the same and other authorities that a constant river of considerable size, though small at its actual mouth, flows into Little Fish Bay (Mosammedes). There is now a thriving settlement there, where a Frenchman has long resided, who is said to make distant trading journeys into the interior. It would be very desirable for any officers of the slave squadron, or others who might land at that port, to make inquiries about the lower part of this stream, which must be perfectly well known there. The Ovampo told me that it seldom ran quite into the sea, but ended in a large deep pool close by the coast, beyond which the sand was dangerous to walk over, as it was a quicksand.

There is also a Portuguese trading station on the river opposite the country of the Onganjèra; this cannot be far from the coast, for the caravan from Damara-land to that nation leaves Omaruru and travels northwards for a long way over some very high land frequently in view of the sea. From the mouth of the river a kind of sea-shell, much prized, and called by the natives Ombou, is frequently brought. As regards the size of this river it is said to be such, that when a man calls across it his voice can be heard, but not his words. Opposite to the Ovampo it is extremely swift (boats cannot paddle up it) and very deep. It appears to be a most interesting river, and well worth exploring. I can say nothing as regards its salubrity, except that Ovampo-land appeared a remarkably healthy country, and Damara-land I know is such. Corn land extends the whole way S. of it from Ovampo-land to very near the sea. Between the two confluents of the river the Ovabuntja live. Their country is described as very marshy, and many of their houses are built on poles: of course fever is to be dreaded there.

Ethnology.—I will now pass on to the distribution of tribes in this part of South Africa. Their history is not a little involved; but they may be enumerated thus:—1. The Ovampo are corn-growing tribes to the north, who, considered as blacks, are a highly civilized people, and one with strong local attachments, well ordered, honest, laborious, and neat, yet still with much of the negro in them. 2. The Damaras are a vagabond, lazy, thieving, pastoral race. 3. The Hottentots to the south are too well known to require further comment. 4. The Mationa Caffres to the east; and lastly, 5, the Bushman Hottentots and others, who lead a Bushman's life in the barren tracts, that separate these larger nations.

The Namaqua Hottentot is an invader of the last few years, but the Bushmen have not even a tradition of another home. Living with them are outcast Damaras, and also a very peculiar race of negroes speaking the Hottentot tongue, and that only. These have no traditions indicating their descent, and are found as far south as Bethamy. They live peculiarly on the hills, and have puzzled ethnologists ever since they were first described. They call themselves Ghou Damup, and in Sir James Alexander's work and in missionary publications, are described as the Damaras of the hills. With the Damaras, however, they have nothing in common. Their features, shape, customs, and aptitudes indicate an entirely different origin, and it will be seen that an enquiry into their earlier history throws great light upon the former state of this country. The Mationa are Bechuanas, among whom, partly as slaves and partly independent, live the Soun Damup, a tribe kindred to the Ghou Damup in every respect, language, appearance, and superstitions.

To make the matter clearer I will state the results of frequent enquiries from many independent sources, the agreement in which is very striking.

About 70 years ago (certainly between 65 and 75 years), and when, from uniform testimony, water was much more abundant than it is now, the Damaras lived in the Kaoko alone. The Ovampo were within their present frontier, but the Mationa extended to Ovampantieru-land, certainly far to the westward of Otchombindé, and all between these and down to the Orange River, lived Hottentots of various tribes. The Nareneen lived by the sea, and the Ounip (called by the Dutch Toppners) about the parts of which we are now speaking, and south of these were the Keikouka, now represented by the red people, by Swartboy, the Kubabees, and Blondel Swartz. Near to the Orange River the tribes were more numerous and more civilized, from their neighbourhood to the Dutch. They had a few guns, sometimes waggons and so forth, and these were the ancestors of Jonker, Amirals, Jan Boys, and other smaller tribes, as Buchess' and Fransman's. There was also a certain admixture of bastard blood in these last, who came to be designated Oerlams (a term of half reproach) by the Dutch, and to be disavowed by the Keikouka as partly aliens. Hence a jealousy arose, and still exists, between the two great divisions of the more southern Hottentots, the Keikouka and the Oerlams, who together are usually called in the aggregate " Namaquas," in contradistinction to the northerly tribes of Bushmen.

Interspersed among the Hottentots from the north to the south were the Ghou Damup, who were invariably considered as slaves and a good deal ill-used; they lived, when in communities, in the hills, or table-mountains, of which there are many, such as Omuvereoom, Konati, Ketjo, Erongo, and many others, of which I have often heard, more to the south and west. Two movements now began to take place; first the Damaras, pressed for room or for some other cause, made an irruption to the eastwards, and spread over the country as far as Otchombindé, almost exterminating the Hottentots in their way and driving back the Mationa, while the Ghou Damup were pretty safe in their mountain-fortresses and received but little harm. The Toppners, however, not being at that time accustomed to the mountain-passes with which the Ghou Damup were familiar, were, as I said, greatly cut off. And it is curious, that within very late times (about eight years ago), exactly the same thing occurred to the Nareneen living west of the Kaoko.

The more northerly Toppners were thus quite cut off from all communication with those about Walfisch Bay, and remain so to the present time. There exists, however, the greatest fondness for traditional stories among these people, and I found the liveliest interest expressed on my return from the north relative to the well-being of those Hottentots whom I met among the Ovampo, and of whom scanty information only had been received from time to time. In Sir James Alexander's work mention will be found of the Navees, or Nabees, as he spells it, on information

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received among the Hottentots. These are the Ovampo; Navees being the Hottentot name for them.

We have seen thus how the Damaras drove the Toppners to the same places as the Ghou Damup. Community of misfortune is gradually destroying the feeling of difference of race between them, so that intermarriage, which would have been quite unheard of in former years, is now becoming common. The Hottentots told me that 10 years ago it was quite unknown; and I have never seen any but children of the mixed race.

The Mationa made at various times reprisals on the Damaras; the last being about 20 years ago, when the Mationa came up the Epukiro River, while on a previous occasion they had passed up the Omoramba.

From the Damara invasion we now come to that of the Namaquas, which dates at a much later period, and in which Jonker Africaner played the principal part. Of all the particulars of this I have the fullest information; but I cannot expect that an interest which depends chiefly on persons and parties in South Africa, will be felt here; suffice it, therefore, to say, that by gradual encroachment the tribes, whose names you see here mentioned, strengthened and formed themselves, and plundered all before them. Sometimes they went on a professed national feeling to aid the Toppners, sometimes on none at all. In every case, however, the Toppners were thoroughly victimised; and it is only of late, when the Nareneen had obtained so many guns and so much ammunition from whalers and guano ships, that they acquired sufficient strength to be recognised as others than simply as Bushmen by the Namaquas.

The moment that I saw the Ovampo I was most strongly impressed with the national identity of the Ghou Damup; it is true that the latter are most squalid and thievish, very strikingly opposite characteristics to those of the Ovampo, but on the other hand we cannot forget that they must have been an outcast race for ages, to have so completely lost, not only their own language, but all traditions of it. They dig and plant, which neither the Hottentots nor the Damaras do; and on the other hand I was assured that the Soun Damup, who lived to the north, were the field labourers of the Mationa (the Hottentots call bread "soun" from them), and were exactly the same as the Ovampo, except in some trivial difference of dress, and that there, some spoke Ovampo, some Mationa, some Hottentot, and some all of these tongues.

I conclude, then, that the Ghou Damup were the real aborigines of the country S. of the Ovampo, that very long since the Hottentots invaded and entirely conquered them, and that they both together settled down into the condition in which I described them to be at the beginning of this account.

I may add that exactly the same process is now going on between the Namaquas and the Damaras, and probably one-half of the whole Damara population has already been enslaved or murdered by the Namaquas. Those that are made slaves are used as cattle-watchers; their children, as they grow up, learn Hottentot, and readily identify themselves with the habits of their masters, so that few generations will probably have passed before the Damara language will be obsolete among them, and they will have become a race affording an exact parallel to that of the Ghou Damup. The Namaquas are still pressing on with the peculiar restlessness and obstinacy of the race, a belief in their destiny, a scorn of blacks, and a fondness for plunder, which has already led them from the Orange river, and which now seems to be more marked than ever. As unarmed savages can never resist their guns, which number between 3000 and 4000, my belief is that not many years will have elapsed before they will have utterly destroyed the Damaras, and will come into direct conflict both with the Ovampo and the Mationa.

On the habits of the Damaras I have little to say. Physically speaking they are a striking race, with an appearance of strength, lightness, and daring that is highly imposing. They are tall, upright, and often remarkably handsome men, models for sculptors. They have a fair facial angle of about 70°; fine, manly, open countenances, and often beautifully chiselled features; but morally they are the most worthless, thieving, and murderous of vagabonds, and at the least irritation their usually placid countenance changes into one of the most diabolical expression. Much struck as I was with them at first, I came ultimately to the conclusion that, except their general good humour, there was not a single good point in their character. Their very personal strength is wonderfully small considering their immense muscular development. Often as I have had trials in lifting weights and so forth among them, I never found one who was anything like a match for the average of my own men. Idea of a Supreme Being they have none; but ceremonies and superstitions innumerable; none of which have anything poetical in their character. They are chiefly shown in smearing with fat and with cow-dung and in abstaining from eating cattle with certain marks, different according to the family they descend from: of the fetisch superstition there is no trace. A tree is supposed to be the universal progenitor, two of which divide the honour, one at Omaruru, the other on the road to the Ovampo. All the men are circumcised.

They have no government; any man with 20 cows calls himself an independent captain. They are devoid of all national or social ties to a perfectly marvellous degree. If one werft is plundered, the adjacent ones rarely rise to defend it, and thus the Namaquas have destroyed or enslaved piecemeal about one-half of the whole Damara population. As to the language, a very complete grammar and dictionary has been compiled by the Rhenish Missionaries, and sent last year to the Professor of Philology at Bonn, who will, I believe, shortly publish it.

Very different from these in every respect are the Ovampo, who are orderly, centralised, hard-working, neat, and scrupulously honest. Ondonga is plotted out into small, well-farmed holdings of corn and pasturage, each occupied by a family, generally the grandfather, son, and children. Every one here has the appearance of plenty, and none of the squalid, wretched, uncared-for, old people, so painfully common among the Damaras, are to be found amongst them. The King, Nangoro, is despotic, and seems to rule with a patriarchal sway. Laws against theft are peculiarly severe. The tribute to the King is small, and paid by a per centage on the tobacco grown, and *not on the corn.* The marriage tie is extremely lax. The Ovampo possess the entire carrying trade between the Damaras and the Portuguese.

My map is for all practical purposes, and so far as it professes to go, very fairly accurate. I am not aware that any isolated hill is left out, though I do not profess to give the peaks in each group. I should have been involved in endless confusion, had I attempted so much. The limits however of all hills and all groups of hills are taken. I triangulated chiefly with an azimuth compass, from Walfisch Bay onwards as far as there were mountains to triangulate by, that is to Otchikoto on the N., and to Elephant Fountain on the E. I have so great a number of bearings, that I have had no difficulty in making many independent series of triangles, and checking one by the other. I thus pretty easily found out such errors, either of reading off observations, of mistaking hills, or of writing them wrongly down, which I saw in spite of all my care would occasionally occur. I then selected the series of triangles that I thought would give the most trustworthy result, guided by the size of the angles, and more particularly by the definiteness of the mountain peaks that I observed, and then protracted them. Having done this, and registered the longitudes which this triangulation gave for my three main stations, Barmen, Okamabuti, and Elephant Fountain (assuming the longitude of Pelican Point, Walfisch Bay, at 14° 27′ 5″, and determining the scale of the map by differences of latitude), I compared these longitudes with those deduced astronomically, and I am glad to say that the agreement is very satisfactory. There is an abstract of all this at the end of the paper. Some grave error had affected my instrument, so that although the observations in each group agree extremely well together, yet there is a wide difference between the longitudes derived from these several groups. I had, however, done my best when taking lunars, say E., to take others W. under as nearly as possible the same circumstances, both of altitudes and of distances, as I could, and of the same bodies also. With sun observations I used one coloured glass, and always the same one, toning the instrument of course as required. I also examined the adjustments of my sextant with all the care I could, previously to beginning to observe; and it is solely from having taken these precautions with great pains that I can account for the excellent agreement of the mean longitudes (deduced as they are from such wide extremes) with that obtained by triangulation. As regards T'ounobis and Ondonga the lunars were taken with a good, though small, and not clearly divided circle, which had to be read off by firelight; still the results of the former are very fair, and those of the latter, being checked by the position of Otchikoto, will answer sufficiently well. I am thus particular upon these matters, as it is of course a satisfactory thing to have well determined the geography of a new country, even though only in outline, for it may save much trouble and doubts to future travellers. I have altogether determined astronomically the longitudes of 6, and the latitudes of 53 stations, and I had no object in taking more.

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TABLE OF LATITUDES OBSERVED.

Places of Observations.	The Number of Bodies observed. N.	S.	Latitudes. °	′	″
Sand Fountain	1	1	22	57	57
Scheppmansdorf			23	10	
Hycomkap	2	..	22	41	45
Oosop	3	2	22	45	38
Davieep	1	..	22	48	0
Annaas	1	..	22	43	0
Mouth of Tsobis River	1	..	22	26	3
Tsobis	1	..	22	30	55
Kurrikoop (on Swakop)	2	1	22	23	2
Otjimbinguè	2	..	22	21	50
Barmen	2	1	22	7	7
Schmelen's Hope	1	1	21	59	34
Okandu	1	..	21	56	30
Kutjiamakompè	2	..			
Okanjoè	1	..	21	27	9
On leaving the Omoramba	1	..	21	13	29
Otjikururumè	1	1	21	5	54
Ontekeremba	1	..	21	1	49
Ozukaro	1	..	20	49	57
Near Ja Kabaca	3	..	20	38	50
Otjironjuba	1	..	20	32	25
July 16th, Omoramba	1	1	20	29	45
April 1st, Werft	2	..	20	22	15
July 14th, Omoramba	1	..	20	18	43
Okavarè	2	1	20	6	37
Omanbondè	1	1	20	2	30
On Flat, April 12th	..	1	20	1	5
Okatjokeama S. Vley	1	2	19	57	15
,, N. Vley	1	2	19	55	35
Okapukua	..	1	19	47	4
Okamabuti	many		19	30	48
Omutirakanè	1	1	19	25	50
At foot of hills, April 30th	1	..	19	20	13
Otchikoto	..	2	19	10	0
Otjando	..	1	18	58	15
Small well	..	1	18	54	37
Omutchamatunda	..	2	18	47	32
South border of Flat	..	1	18	31	51
Two miles within Ondonga	1	..	18	6	41
Nangoro's Werft	..	1	17	58	40
			°	″	′
Barmen Cattle post	1	..	22	15	55
Due E. of High Peak	2	1	22	14	36
Katjimasha's Kraal	1	..	22	26	0
Eikhams	3	..	22	34	40
On plain, Sept. 3rd	1	..	22	27	4
On Noosop R., Sept. 5th	1	..	22	16	11
Elephant Fountain	2	..	22	27	15
T'was	2	..	22	36	18
Kurrikoop (on Noosop)	1	..	22	25	21
Occultation-place	..	1	22	27	35
Near Okomavaka	1	1	22	15	20
On road to T'ounobis	1	1	22	7	35
T'ounobis	2	1	21	54	40

Lunars taken with Sextant.—Calculations by Mr. Burdwood, Hydr. Off.

—	Distinguishing Letter in Calculations.	Body observed.	East or West of Moon.	Proximate Altitude of Sun or Star.	Proximate Altitude of Moon.	Proximate Distance	Longitudes deduced.	Distinguishing Letter of Groups.	Means.	Longitude by means of East and West.	Longitude by Triangulation alone, using 14° 27′ 5″, as longitude of Pelican Point.	Difference.	Longitude used in Map.	Sets omitted as being evidently bad ones.
						°	° ′		° ′					
Elephant Fountain.	a	Saturn	W.	55	42	19	19 13·2	A						a*
	a**			57	46		19 21·7							
	b			54	52		19 17·2		19 16·2					
	c			51	53	20	19 16·7	B						b*
	c*			51	52		19 15·5							
	c*			50	52		19 12·7			° ′	° ′	′	° ′	
	d_1	Sun .	E.	25	28		18 26·5			*18 53.7	18 – 59	+5·2	18 59	
	d_2			26	28		18 30·2			but if calculated according to method mentioned in the note we have				
	e			29	26	106	18 30·2	C						
	f			32	23		18 36·5							
	g			27	34		18 31·2		18 31·5	18 59.5	18 59	–′·5	18 59	
	h			30	32		18 30·2							
	i			46	19	93	18 32	D						
	j			48	17		18 36·5							
Schmelen's Hope.	k_1	Sun .	E.	37	20		16 28							
	k_2			38	19	122	16 27·7	E	16 25					
	l_1			40	17		16 17·5							l_1*
	l_2			40	16		16 26·2							l_2*
	m_1	Sun .	W	34	20		17 23·5			16 53·6	16 56·5	+1′	16 56·5	
	m_2			33	21		17 17·7							
	m_1*			32	22	119	17 19·7	F						
	m_2*			31	23		17 18 2							
	n			44	41		17 37 2		17 26·2					
	o	Jupiter		39	46	94	17 33	G						
	o*			30	55		17 29							
	o*			28	57		17 35·5							
Okamabuti	p	Sun .	E	25	40		17 58·7							
	q			31	33	109	18 3·5	H	18					s*
	r			34	28		18 1·7							
	s			38	33		17 55·2							
	t_1	Sun .	W.	26	39		18 44·5			18 25	18 20	–5	18 20	
	s			25	40	98	18 53·7	I	18 50					
	t_1			24	41		18 52·2							
	u_2			23	42		18 49							
Occultation, 57 m. W. of Elephant Fountain.	..	..	..	..	..	..	..	..	..	18 7·7	18 2	–5·7	18 2	

* It is evident that the mean of these E. and W. observations cannot be expected to give the true longitude of Elephant Fountain; because the circumstances under which they were severally taken, both as regards the altitudes of the bodies and their distances, in no way match together. If we choose we can reject them entirely, and trust only to the accordance of the calculated result of the occultation with that obtained by triangulating, to corroborate the correctness of the latter method as regards the positions of places in *this* part of the journey. Or else, as the observations C and D were taken under very similar circumstances to the whole of those at Schmelen's Hope and Okamabuti, we can find the error which, whatever its causes may be, was found in practice to affect the results of those observations; and then, by applying this error to the longitude as obtained from C and D, we ought to obtain a much more trustworthy approximation than before to the true longitude of Elephant Fountain. Thus—

	Okamabuti.	Schmelen's Hope.
	° ′	° ′
East Observations	18 0	16 25
West ,, ...	18 50	17 26
Difference	50	1 1
Error	25	30.5

or, 28′ = the mean error. Now, the mean of C and D is 18° 31′·5; and this + 28′ = 18° 59′·5, which differs only 0′·5 from 18° 59′, which was the longitude obtained by triangulation from Walfisch Bay.

Lunars taken with a small Circle.—Calculations by Mr. Burdwood, Hydr. Off.

—	Distinguishing Letter in Calculations.	Body observed.	E. or W. of Moon.	Proximate Altitude of Sun or Star.	Proximate Altitude of Moon.	Proximate Distance.	Longitudes deduced.	Means.	Longitude by Means of E. and W. Distances.	Sets not used.
				°	°	°	° ′ ″	° ′ ″	° ′	
T'ounobis . . .	a	Sun .	..	14	58	107	20 51·2			b
	d	Antares	W.	18	68	51	21 7	21 5·3		c
	e						21 7·5			h
	f	Sun .		46	16	117	21 9			j
	g						21 12		21 1	
	k						21 2·2			l
	m						20 55·7			
	n	Saturn	E.	24	68	45	20 55·5	20 57		
	o						21			
	p						20 52·5			
Ondonga, Nangoro's Werft.	t	Regulus	W.	38	72	46	16 17·5	..	16 14	q
	u	Antares	E.	44	72	54	16 10·5			r s

XII.—*Latest Explorations into Central Africa beyond Lake 'Ngami, by the Rev. D. Livingston and William Cotton Oswell, Esq.*

Communicated through the London Missionary Society and Lieut.-Col. Steele, F.R.G.S.

Read February 9th and June 14th, 1852.

In our late journey to the country of Sebitoané, or the region situated between 200 and 300 miles beyond Lake 'Ngami, we followed our former route until we came to 'Nchokotsa. From thence, with our Bamangwato guides, our course became nearly due N., crossing the dry bed of the Zouga at a point where a few small stone dykes for catching fish still remain, when we entered a country abounding in what are termed "salt-pans," one of which, named 'Ntwétwé, was at least 15 miles broad and perhaps 100 long; another we found covered with an incrustation of salt about two inches thick, but in general they had only a thin efflorescence of salt and lime. Large numbers of several varieties of recent shells strewed their surfaces, and each salt-pan had a spring of brackish water on one of its banks. In speculating on these curious features in the physical appearance of this country we have sometimes thought that the continual deposit for centuries, resulting from the evaporation of the water of these springs, may have been the chief agent in their formation. But the presence of recent shells shows that the formerly more extended

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flow of the Zouga may have had something to do in the matter. Beyond the salt-pans the country is perfectly level and hard, and covered with Mopané and Baobob trees, the underlying rock being white tufa, in which a great number of springs of good water are found. These, from their number and proximity, are called 'Matlomaganyana,' or the 'links,' as of a chain. A considerable population of Bushmen live in their vicinity, under the sway of Sekhami. They are remarkably unlike their more southern brethren, though speaking a dialect of the same language and bearing the same name. They are fine, tall, strapping fellows, and nearly as black as the Caffres, and are also the most daring Bushmen in the country, frequently killing the elephant both by day and during moonlight nights. The entire Bushman nation, as scattered over the Kalahari desert around and westwards of the Lake, and likewise in the regions to the N. of that, must be very large. The theory that Bushmen are Hottentots, driven to their present position and habits by the encroachments of the whites, receives no confirmation from any tradition existing among themselves, nor from the actual and immemorial condition of the more distant hordes.

Around the wells, and indeed through the whole region upon which we had now entered, a tree called *Mopané* abounds. The leaf is peculiar in shape, and affords shelter to myriads of a little insect not larger than the head of a common pin. The dwellings of these little colonists are in shape like a limpet-shell, and though each is only large enough to hold its little owner, so many exist on one leaf that, being of a sweet, gummy substance, the natives collect them in large quantities for food. The country between the Matlomaganyana and the Mababi was the worst we had seen in Africa. The drought was excessive; not a bird or an insect disturbed the stillness of death which reigned over the scene. All around was one level of low scrub, Mohonono bush, and Mopané trees. Our Bushman guide seemed to wander, or only follow the tracks of elephants made when going from one Mohonono bush to another, which, however, on the morning of the fourth day disappeared altogether. Having come at length upon a rhinoceros trail, we allowed the cattle, which were nearly worn out by the deep dry sands through which we had passed, to run along it until their instinct led them to the water, unfortunately, however, at a part of the Mababi infested by the 'Tsetse' fly. We were unfeignedly thankful to find ourselves on the banks of this river, as it is a branch of the Tsō flowing to the E.N.E. Bakoba-Bushmen and Banajoa villages we found on its banks, the inhabitants deriving subsistence from a root called 'tsitla' when their crops fail. The Banajoa huts are built with a kind of second story, the upper being the sleeping place. A fire is placed under it in order that the occupants may get rid of their troublesome tormentors, the mosquitoes, by means of the smoke. Chombo, the head man of the Banajoa village, became our guide to Sebitoané, and led us through the reedy swamp, about 10 miles broad, on which his village is situated, and across the river Sonta by night, on account of it being infested by the 'tsetse,' and early on the morning of the 19th of June we found ourselves on the banks of the Chobé. As circumstances led to our remaining on this spot, in lat. 18° 20′ S., and long. 26° E., for more than two months, and the hopes which Mr. Oswell and I then entertained are not entirely extinguished, I may be excused entering a little into detail.

Sebitoané, the great chief of this large territory, was one of a horde of Mantatees, which in 1824 threatened to destroy Lattakoo and invade the colony. Driven back by the Griquas he fled towards the north, and fighting his way through the countries of the Bawanketsé, Bakwain, and Bamangueato, he followed nearly the same route as our own to the Zouga. Having plundered the Bakoba and Botlétli, living on that river, he next crossed over the desert between Lake 'Ngami and the Dámaras, to the west. Scarcity of water and the bravery of the Dámaras obliged him, however, to retrace his steps much poorer in cattle than upon his arrival. Proceeding up the Tamunaklé, he conquered the black races inhabiting the rivers Chobé, Seshéké, &c., and at last established himself on an island, said to have been formed artificially by Seunturu, a chief of the Borotse. Having been informed of the efforts made in vain by Mr. Oswell and myself to penetrate into his country, he evinced great anxiety to open up a path for us, and not only sent men to search for us along the Zouga, but made considerable presents of cattle to different chiefs on the way, with the request that they would render us every assistance in their power, and furnish us with guides. He even came nearly 300 miles southwards, and would have come further, in order to be near to us and to English traders. When we met he seemed overjoyed, and having remarked that our cattle had been bitten by the fly and would certainly die, said, "Never mind; I have plenty, and will give you as many as you need." He then appeared anxious to remove us to the N., or the safe side of the Chobé, and also to be near his town of Linyanti; but when he saw that our waggons were too large for his canoes, he ordered the people of the town to remove to our resting-place. A few days afterwards, however, and when the new village was quickly springing up, he became ill, and, after lingering a fortnight, to our great regret died. His people received our condolence and advice in a friendly manner, and requested us not to leave them, but treat his children as we should have done had Sebitoané been still alive. His daughter, Mamochishane, who succeeded him in the chieftainship, being still at the head town of the Borotse, a distance of 12 days, or nearly 200 miles, from the waggons, double that time was required to open up a communication with her. She was reported to be in childbed at the time, but sent the chief next in authority to herself to visit us on her behalf. His instructions were to treat us as kindly as if her father were still alive, and full permission was granted for us to proceed wherever we wished to go. We then rode about 100 miles on horseback to the N.E., and were well rewarded by a sight of the great river called Seshéké, in lat. 17° 28′ S. A variety of considerations, all of which we had previously weighed, induced us on our return to the waggons to decide on starting for the Cape. On our return we went along the Mababi and Tamunakle, and after crossing the Zouga fell in with our old road to the Lake.*

Before detailing the information which our two months' sojourn on the Chobé enabled us to collect, I may remark that the opening up of a path from either the E. or the W. coast to the centre of the continent—a prominent feature in all our plans—would be a worthy subject for the consideration of the Royal Geographical Society; and for such an undertaking I know no one better suited than my friend Mr. Oswell. He has courage and prudence equal to any emergency, and possesses, moreover, that qualification, so indispensable in a traveller, of gaining the confidence of the natives, while maintaining the dignity of a gentleman.

The following information, derived partly from the natives, who came in great numbers to see the white strangers, and partly from our own observation, will prove of service to any one who may wish to visit the country. The extensive region to the N., N.E., and N.W. of the Chobé and Seshéké rivers, which owned the sway of the late Sebitoané, and is now governed by the Makololo (Quilolo = Captain ?), in name of his daughter, is for hundreds of miles nearly a dead level. In passing along 100 miles from the part where our waggons stood on the Chobé to the river Seshéké, we saw no rise higher than an ant-hill. The country is intersected by numerous deep rivers, and, adjacent to each of these, immense reedy bogs or swamps stretch away in almost every direction. Oxen cannot pass through these swamps, but sink in; and, on looking down into the holes thus made, the parts immediately under the surface are seen to be filled with water. These rivers are not like many which bear the

* Mr. Oswell informs me that "the country which owned Sebitoané's sway (now his daughter's) is very large. Eighty-two tribes or principal towns paid him tribute, and his influence may be said to have extended over a circle of 800 to 1000 miles." —ED.

name of such in South Africa, mere "nullahs," containing nothing but sand and stones: on the contrary, all those which came under our observation here contained large volumes of water, and that too flowing with considerable rapidity at the end of an extraordinarily dry season. Yet on sounding the Chobé we found it to have a regular depth of 15 feet on the side to which the water swung, and of 12 feet on the calm side. The banks below the lowest water-mark were nearly perpendicular, and the water was as deep, a foot from the bank, as in the middle of the stream; the roots of the reeds and coarse grass seeming to prevent it from wearing away the banks, which, however, in many parts are undermined and overhang the water. The lands in this region are raised only a few feet above the prevailing level, on which the people pasture their cattle, make their gardens, and build their towns. The rivers overflow their banks annually. The unusual drought of the season preceding our visit seems to have extended even to the sources of the waters. The Chobé ought to have overflowed in July, and the people ascribed the non-appearance of the waters to the death of their chief. But when the rivers do fill, the whole country is inundated, and must present the appearance of a vast lake, with numerous islands scattered over its surface. The numerous branches given off by each of the rivers, and the annual overflow of the country, explain the reports we had heard of "Li-noka-noka" (rivers on rivers), and "large waters" with many islands in them. The Chobé must rise 10 feet in perpendicular height before it can reach the dykes made for catching fish, which we observed about a mile from its banks; and the Seshéké must rise 16 to 20 feet perpendicularly before it overflows its banks; yet we saw unmistakeable evidences of its flood extending 15 miles out. The natives traverse the country in every direction in their canoes, and even visit their gardens in them, so that a boat will be indispensable in the equipments of future travellers.

The soil seemed fruitful, and is generally covered with rank, coarse grass; but many large and beautiful trees, most of which were to us entirely new, adorned the landscape. We claimed acquaintance, however, with the gigantic Baobob, which raises its enormous arms high above its companions, and makes them in contrast appear as mere bushes. We recognised also date-trees in large numbers, and also many palmyras. The date-trees were in blossom on the road to Seshéké at the time of our visit, and the seeds were observed below them. Of the new trees some were splendid evergreens, bearing edible fruits; and, in addition to parasitical plants of great size, we observed two of the Orchidean family. One beautiful fruit-bearing tree particularly attracted our attention, but unfortunately the seeds, about the size of peach-stones, were all broken by some animal. The natives raise, besides their usual grains, considerable quantities of a bean which bears its pods underground. They are called "motu o hatsi" (earthman), and are sweet when roasted. They grow well at Kuruman, and may succeed at the Cape. The people of the Borotse tribe cultivate the sugar-cane and sweet potato. Wheat, maize, peach and apricot stones, and other garden seeds, have been left with the Makololo, as they spontaneously offered to make a garden for our use. The moisture from the rivers permeates the soil, rendering constant irrigation unnecessary; and some of the seeds left may vegetate and increase the food of the inhabitants, but of this indeed their stout and healthy appearance indicates no want.

We ascertained that the hilly parts beyond were without defence against the Matibele, and Mosilekatse constantly makes excursions against them, but their own deep, reedy rivers are safe against inroads.

Respecting the sources of these rivers we are quite in the dark. The Makololo have ascended the Seshéké, or, as it is otherwise called, the Borotse River, at least 400 miles above the town to which our journey extended. Its general course may be inferred from their statement, that, when ascending it, "the sun rose on one cheek and set on the other." But some, in drawing maps for us, gave it a little Westing. In lat. 17° 21′ S. it contains a volume of water of from 400 to 500 yards broad, and though we saw white banks of sand in it, the depth was evidently considerable. The name Seshéké refers to these banks of white sand. Alligators and hippopotami abound in it. A series of rapids, situated above the town of Seshéké, compels the boatmen to drag their canoes some distance along the shore, and at about 80 miles below the same point a waterfall is reported, the spray and noise arising from which are so considerable as to have led to the name "Mosi oa tunya" (smoke sounding). The spray is said to be visible 10 miles off. This waterfall may have prevented the slave-dealer from sailing up this river, which we believe to be the main branch of the Zambesi, for it is a fact to which all the natives bore uniform testimony, "*that the slave-trade only began on the Seshéké in* 1850." At the falls the river is narrowed by means of rocks. It soon, however, spreads out, and becomes placid again. The natives, who have been to the eastward, know of the Seshéké being joined by another river at about a month's distance from the town, which, beyond the junction, assumes the name Zambésa or Zambesi. The large river referred to is called the Bashukulompo or Maninche, but it is only 80 or 100 yards wide. A great many branches, as the Loi Lombé, 'Njoko, Majeele, &c., connect the Maninche with the Seshéké. These are inserted in the map as given by the natives, but even beyond the point where these streams empty themselves into it the Seshéké must be a large river, for Seunturu, the Borotse chief expelled by Sebitoané, built a large boat of planks sewn together, which was roofed in with white cloth, so that his people might see it at a great distance, and it required twenty men to paddle it. The best informed natives can give no information as to whence the supply of water comes which these rivers convey with considerable velocity to the E. The usual answer to our inquiries was, "The water comes from Lobale." And what is Lobale?—"The source of the waters." Lobal*a* in Sichuana means a large plain without trees, but whether Lobal*e* means an expanse (lake), or a province,* we could not determine. The Loena, Liambae, and Lonta are said to be large rivers, and the inhabitants possess many canoes. The Louta contains light-coloured water, the Liambae clear water, and both flow for some distance in the same channel after their junction, side by side and unmixed. The water of the Seshéké is hard but clear, and does not readily form a lather with soap. The water of the Chobé is clear, and as soft as that of the Zouga. The Tamunakle and the Teogé had their annual rise in June and July, while, as we had an opportunity of observing, the Chobé was unaffected, or rather fell slightly 1½ inches during the same period. The sources of the Chobé and Zouga, &c., would therefore appear to be distinct. A good highway into a large section of this continent is now opened to our view, and any one may travel for hundreds of miles unmolested. The country around Libabe is reported very swampy, or rather boggy, for people not unfrequently slip through the crust of earth which covers the underlying waters, and perish. Near to the hill Sorila on the Embarrah a waterfall is reported, but the natives of the country oppose any path being made in that direction.

The people of these regions are black, totally distinct from the Bechuanas. Those of Sebitoané are called Makololo. The black race which he found inhabiting the numerous islands consists of tribes of different names, as Borotse, Manyeti, Batoka, Bashukulompo, &c. Being the victorious party, the Sichuana is the language of the court. The Bible is being translated into that language, and Providence has prepared the way for it. Besides Sichuana there are the different dialects of the black race inhabiting the country, and though some of the radicles show them to be of the same family of languages as the Sichuana, none of the Bechuana could understand it. To judge from a comparison between 300 words of the Bayeiyé or Bakoba, and about an equal

* Mr. Oswell thinks that there may be some connexion between the names of the R. Lobali and of the Portuguese province of Loval or Louval to the N.W. of the Borotse.—Ed.

number from each of the following tribes, viz. the Bashubea, Borotse, Batoka, Bamyenka, Bamapanda, and Balajaze, with the Sichuana, the former bear about the same relation to the latter that provincial English does to broad Scotch. We found everywhere people who could understand us. These blacks designate the Supreme Being by the name of Nyampi or Reza, which latter is identical with the Oreeja of the Bayayé. The Borotse are very ingenious in basket-making and wood-work generally. The Banyeti are excellent smiths, making ox and sheep bells, spears, knives, needles, and hoes of superior workmanship. Iron of excellent quality abounds in their country, which they extract from the ore. They are famed as canoe-builders also, and the abundance of a light, strong kind of wood, called Molompi, enables them to excel in this branch of industry. Other tribes are famed for their skill in pottery; their country yields abundance of native produce; but though they are stoutly made, especially their upper extremities, they seem never to have been addicted to war, but appear to have trusted to the defences which their deep, reedy rivers afford to their numerous populations. In constructing the rough sketch of their country only the largest towns are given. Scores of the people were employed by Mr. Oswell and myself at different times, and as their accounts generally agree, we consider that we give an approximation to the truth. The size of the towns, as indicated in their drawings, derives additional confirmation from the fact that in our ride to Seshéké we saw several villages of from 300 to 500 inhabitants each, which were not enumerated by our informants, being in their estimation too small to mention. The Batoka and Bashukulompo follow the curious custom of knocking out the upper front teeth at the age of puberty; the lower incisors, being relieved from the pressure of the upper, become long and press out the lower lip, while the upper lip falling in, gives to the countenance a sort of gaberlunzie appearance. European manufactures in considerable quantities find their way from both E. and W. coasts to the centre of the continent. We were amused soon after our arrival at the Chobé by seeing a black gentleman walk towards us in a gaudily-coloured dressing-gown, and many of the Makololo possessed cloaks of blue, green, and red baize, or of different coloured prints. On inquiry we found that most of these had been obtained in exchange for slaves, and that the traffic began on the Seshéké only in 1850. A party of an African tribe called Mambari visited Sebitoané during that year with considerable quantities of English manufactured cloth, and a few old Portuguese guns, marked "Legitimo de braga," and they refused everything in exchange except boys of about fourteen years of age. The Makololo viewed the traffic with dislike, but having great numbers of the black race living in subjection to them, they were too easily persuaded to give these for the guns. Eight of these old, useless instruments were purchased by Sebitoané for as many boys. They then incited the Makololo to go on a razzia against the Bashukolompo, stipulating beforehand that, in consideration for the use to be made of their guns in the attack on the tribe, they should receive all the captured slaves, while they, the Makololo, should have all the cattle. While on this expedition the Makololo met some Portuguese, or bastard Portuguese, slave-dealers on the Bashukulompo or Maninche river. They were said to be as white as we were (our complexion being then a shade or two deeper than wash-leather), and they had straight but short hair. These traders presented three English muskets to the Makololo in exchange for about thirty captives. The Mambari went off to the N.W. with about 200 slaves, the other party towards the E. coast, but both were so well pleased with their new customers as to promise to return in 1851. The Mambari are said to live in the vicinity of the sea to the W. The other party came up to the Zambesi from the sea on the E. If traders from Europe would come up the Zambesi, the slave-dealer might soon be driven out of the market.

We were informed of the existence of a large water or lake called Sebolemokoa, in the direction usually given as that of Lake Maravi (Tanganna?). Many slaves come from that quarter, and the goods employed in the traffic probably go up the Zambesi. Can English traders not equal slave-dealers in enterprise? Any party possessing sufficient energy would reap abundantly, for since we opened up a way to the river Zouga, considerable profits have been made. We know of 900 elephants having been killed on the Zouga in the space of three years, and previous to our arrival no use whatever was made of the ivory; but on the rivers indicated in my map armlets are made of the ivory, half an inch of which is lost in the process of making, the saw used being a quarter of an inch in diameter, and armlets of brass wire would be preferred by the natives. Tusks (called by the people "Marapshela" or bones) may even now be seen, completely spoiled by sun and rain, lying with the other bones of the animal. More than 10,000*l.* worth of ivory has been brought down to the colony from that river alone, and if the discovery of only one river has added so materially to the commerce of the colony, what may we not expect from the numerous populations which are now brought to light?* But should European traders proceed into the new region, the blacks will be supplied with fire-arms, and give the

* Mr. Moffatt writes to the Rev. Dr. Tidman, "Of course Livingston has written to you fully, and you will see what an immense field for missions presents itself on the banks of the Zambesi *among a teeming population speaking Sichuana.*"—Ed.

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colonists much trouble afterwards. Can the trade in fire-arms be prevented? So long as, according to Cumming's account, 3000 per cent. can be made by the trade in arms, it is in vain to attempt to stop it. The result, however, of all our observations in this matter is, that the introduction of fire-arms among the blacks has produced the same effect as it did among the whites. It puts an end to most of their petty wars, and renders such as do occur much less bloody than they formerly were. Should any one be disposed to establish a trade on the Zambesi, let it be particularly borne in mind that June, July, and August are the only safe months we at present know. We arrived on the Chobé in July, and had frost, but the winter is very short. We observed swallows on the Seshéké in the beginning of August, and the trees, generally speaking, never lose their leaves.

The bite of the tsetse (fly) is fatal to nearly all domestic animals, yet, when allowed to settle on the hand, all it is observed to do is to insert its proboscis a little farther in than seems necessary to reach the blood. It then withdraws it a little, the proboscis assumes a crimson colour, the mandibles appear in operation, the shrunken body swells, and in a few seconds the animal becomes quite full and quietly leaves. Its size is almost that of the common blue fly which settles on meat, but the wings are longer. In the ox the following symptoms are produced by the bite of the insect:—The eye runs, the glands under the jaw swell, the coat loses its gloss, there is a peculiar flaccidity of the muscles generally, and emaciation commences, which proceeds unchecked until, perhaps months after the bite, purging supervenes, and the animal perishes of extreme exhaustion. Some die soon after the bite is inflicted, especially if they are in good condition or should rain fall, but in general the process of emaciation goes on for months. I had a horse which perished five months after being exposed to the insect.

When the animal is destroyed, in consequence of not being able to rise, the following appearances may be observed. The cellular tissue under the skin is injected with air, and the surface of the body presents the appearance of a number of soap-bubbles strewed over the carcase. The fat is of a greenish-yellow colour, and of oily consistence. The muscles are flabby and the heart frequently pale and softened. The lungs have diseased patches on their surface of a pink or grey colour, the liver is frequently diseased, and the gall bladder always distended with bile. The stomach presents no particular appearance, but the small intestines are pale and generally empty. The blood is remarkably small in quantity, and so devoid of colouring matter that it scarcely stains the hands. The poison seems to be of the nature of a ferment, capable of propagating itself, and acts principally on the brain, heart, lungs, and liver. The brain seemed affected in several by the circulation of the morbid fluid, for the animal became unsteady in its gait and sometimes even blind. The tsetse is fatal only to domestic animals, as the wild feed in parts infested by it quite undisturbed. There are large tribes which cannot keep either cattle or sheep because the tsetse abounds in their country, yet it bites man and no danger follows. Our children lived for two months among the tsetse, and were frequently bitten, but suffered no harm, while we lost most of our best oxen after having been in contact with the fly on only one or two occasions. We have seen zebras, buffaloes, and antelopes feeding undisturbed in the vicinity of our waggons on the Chobé, quite unmolested by the tsetse which buzzed around them. Oxen and buffaloes, horses and zebras, antelopes and goats, jackals and dogs, possess somewhat of the same nature. What is there in domestication which renders domestic animals obnoxious to the poison? Is man not as much a domestic animal as a dog? Is it the tsetse at all which kills the animals? Captain Vardon, of the Indian army, decided this point, for he rode a horse up to a hill infested by tsetse, and in ten days his doubts were removed by the death of his horse. A curious feature in the case is, that dogs, though reared on milk, die if bitten, while calves are safe so long as they suck the cow. A dog, reared on the meat of game, may be hunted in tsetse districts in safety. The tsetse only inhabits particular localities well known to the natives. Is there any antiseptic in the fluids of game and man which does not exist in the fluids of tame animals, or in those of dogs reared on milk?

EXTRACT from a letter received by Mr. Oswell from Mr. Livingston, dated Cape Town, April 27, 1852.

"Captain Tuckey gives a vocabulary of the dialects on the River Zaire, and among many other words which we know, there stands staring at us Mokanju's favourite 'Mabotabota,' with very little alteration; and many of the 'Prize men' here know the parts mentioned in the Map in the fork of the Bashukulompo and Seshéké rivers. They are also well acquainted with the Maninche or Bashukulompo."

Mrs. Livingston has since arrived in England with her children, her enterprising husband having proceeded again northwards upon two years' leave from the London Missionary Society, to explore still further the central portions of Africa.—ED.

From Captain Parish, 45th Regt., at Winburg in the Sovereignty, March 6, 1852.

"I omitted in my map the lat. of Blomfontein, which I can get correctly from the Surveyor-General, Mr. Ford. I have just seen a map of the Bassuto (Mospah's territory), published in Paris by the French mission, from the Notes of M. Dyke, a French missionary—it seems very well got up, and as far as I can judge, correctly drawn. Although the Missionary Maps are principally to show the boundaries of their Chiefs, they are the only ones on which many of the principal points are mentioned.

"Mr. Green is now staying with us in camp, having returned from an expedition to the Great Lake with Messrs. Shelley and Bushe. He is just starting again with some of my brother officers to shoot lions. He proposes returning towards the lake in April. All travelling in that part of the country is just now stopped by a disease which attacks the horses; even here they are dying of it every day. It seems epidemic, and carries them off very quickly. An animal, quite well in the morning, is dead before night: sometimes an hour or two after taken with it. It has the appearance of inflammation of the lungs, and is dreaded by the Boers and farmers as one of the worst scourges they are subject to."

XIII.—*Proposal for a Rapid Communication with the Pacific and the East, viâ British North America.* By Capt. M. H. Synge, R.E., F.R.G.S.

Read Jan. 12 and 26, 1852.

The proposed communication consists of component parts, each of which is in itself complete and independent, opening a new and distinct feature of the country, and forming separately a profitable and reproductive work. Each part is characterised by these distinctive features, and by marks of superiority over competing routes, similar to those which distinguish the entire proposed inter-oceanic communication. Every part of the chain may, therefore, rely on its intrinsic merits, and is capable of separate execution. That execution would, however, be the most profitable, and for every reason the most desirable, which would most speedily open the country and effect the communication the whole way to the Pacific.

An examination of the globe shows that the entire route, as connecting Europe with the Pacific and the East, is shorter in proportion as it is *northerly*. Thus one through the United States is shorter than one through Central America; and one through British America, shorter than one through the United States. Equal facilities existing for crossing the respective transcontinental portions of these routes, it necessarily follows that the shortest can also be most quickly traversed. These and other important advantages belong equally to the several parts which form the route through British America. The comparison presents the same result through every link and feature; but the detailed examination of vast tracts of country which it would require, involves so many points of physical, special, and political geography, that to be at all adequately dealt with they must be treated as separate, though subordinate and related subjects.*

* It must suffice briefly to remark that no route can be carried out within the United States by similar natural advantages. That which is universally allowed to be the best that could be formed there, is longer by the inferior position of the Atlantic seaboard within their territories, and extends to the same termination of

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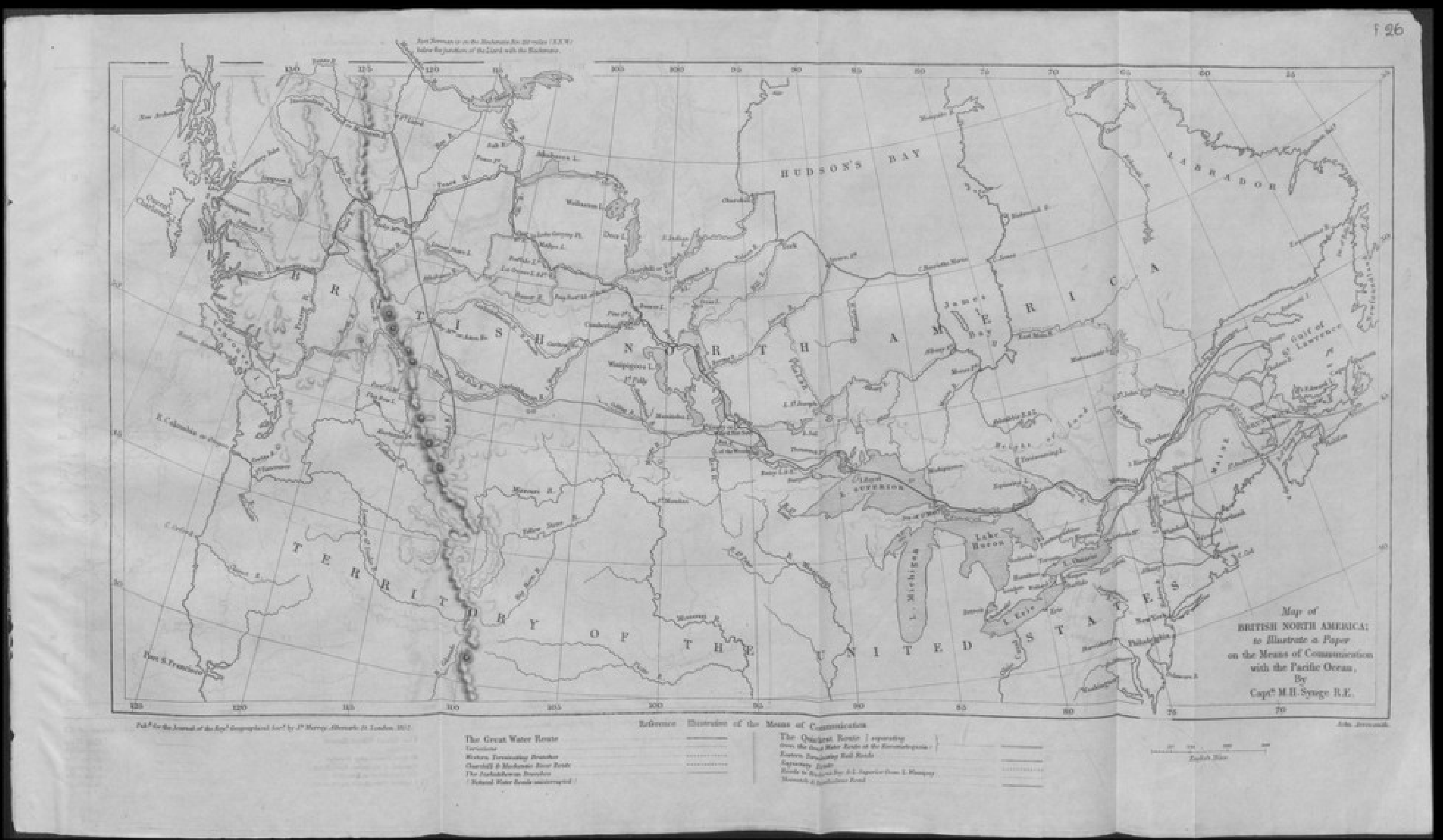
HUDSON'S BAY
LABRADOR
James Bay
L. SUPERIOR
Lake Huron
L. Michigan
L. Erie
Winnipegon L.
Philadelphia
Port S. Francisco
B R I T I S H   N O R T H   A M E R I C A
T E R R I T O R Y   O F   T H E   U N I T E D   S T A T E S
Map of
BRITISH NORTH AMERICA;
to Illustrate a Paper
on the Means of Communication
with the Pacific Ocean,
By
Captn. M.H. Synge R.E.
Reference Illustrative of the Means of Communication
The Great Water Route
The Quickest Route
English Miles

With regard to the British route, beginning at the East, railroads throughout the provinces of Nova Scotia and New Brunswick, to connect the ports of the seaboard with the interior, are essential to the success of the new portion of the route: they would be the means of turning the tide of emigration, labour, and commerce, and would at once demonstrate the superior position of the British territory. The present high development of the Canadian frontier has already led to the planning or actual execution of a line of railroads extending from Amherstburgh to Quebec. Thus the whole country from Lake Huron to the ocean would possess both land and water routes.

The Welland and the St. Lawrence canals, and the Caughnawaga canal, constructing between Lake Champlain and the St. Lawrence, have decided advantages over their competitors in the United States both in speed and economy; and the opening of a communication by land, by water, or both, between Lake Huron and the St. Lawrence, *viâ* French River, Lake Nipissing, and the Ottawa, would effect a farther abbreviation of 400 miles over the Canadian frontier route.

The head of Lake Huron is the farthest point to which the unobstructed navigation at present extends, and to which railroads are immediately contemplated. It forms a splendid reach of 1510 miles from the ocean, and is the most magnificent inland navigation in the world.

This great chain of waters has formed the basis of the whole existing Canadian development, and has laid the foundation for yet more brilliant prosperity. Improved communications have followed, and railroads will speedily exist, all resulting from the industrial activity and wealth which this great trunk communication of Nature's grand designing has called into existence.

The physical characteristics of the central portion of the Continent being similar, the adoption of the same means may therefore be followed by the same results.

The great river system which falls into Lake Winnipeg, and has its outlet by Port Nelson River into Hudson Bay, rivals the St. Lawrence in grandeur and extent, and opens the country to the very foot of the Rocky Mountains.

A third system, with an almost equal extent of navigable water, penetrates nearly to the shores of the Pacific, and indicates the approximate position of the most favourable passes through the mountains. The width and elevation of the land of the dividing

Puget Sound, on the Pacific. It is consequently more circuitous throughout, it has to encounter a greater amount of mountainous, and a large extent of barren territory. It does not consist of separate and complete links, and is assisted by no similar great waterpaths. If practicable it must be confined to a trunk railway, and would be neither aided nor accompanied by any main or tributary development irrespective of actual construction.

ridges are so slight, that in seasons of flood *the waters of these different systems commingle at their sources.*

The climate of the upper regions of Lake Superior, and of the country between it and Lake Winnipeg, is less genial, and the soil less productive than the balmy and fertile peninsula of South-Western Canada. It bears a nearer resemblance to the sterner and more rugged lower province; but the season of vegetation, though brief, is extremely rapid, and grain and fruits come to full maturity. Farther to the west, the mildness of the climate again increases, and the waters of the west central portion, in even the 58th parallel of latitude, are clear of ice, as early and as late, if not earlier and later, than those of Canada. In Vancouver Island the apple and pear trees bud in March, the wild gooseberry appears in full leaf, strawberries are in bloom, and the swallow and humming-bird return. Between these two the climate of the intermediate country varies, approaching, according to its situation, nearer to the one or to the other. The isothermal line, which traverses the centre of England, passes midway between the southern extremity of James' Bay and the northern point of Lake Superior, then rapidly rising towards the west, runs finally nearly parallel to the Russian boundary considerably within the British territory.

The more quickly the communication is carried out to the Pacific the sooner will the results of that connection be added to those of the several independent component links, and the advantages of both be secured. A certain measure of inhabitation of the intervening country is, moreover, essential not only to the success, but to the very construction of the route. By making the utmost use of the natural facilities afforded by the great water-courses, minimum of construction will be accompanied by maximum of advance, and inhabitation carried out to the fullest practicable extent, both along the principal rivers and their numerous and noble tributaries.

1. Upon this principle of reaching the Pacific as speedily as possible, the first new link of construction would be at the Straits of St. Mary, between Lake Huron and Lake Superior. The removal of an obstacle of from only 18 to 22 feet, would add a length of 400 miles to the uninterrupted navigation. It would place this region of immense, if not unrivalled, mineral wealth in direct communication with the seaboard. Facilities of transport alone are required to lead to the highest development of the mines whence huge masses of pure copper are continually being extracted, and where mountains of iron-ore exist. The mines extend for a distance of 140 miles along the coast. It would also render the valley of the Kamenis Toquoih accessible, whence Sir George Simpson, the Governor-in-Chief of the Hudson's Bay

RECENT EXPEDITION

INTO THE

INTERIOR OF SOUTH-WESTERN AFRICA.

By FRANCIS GALTON, Esq., F.R.G.S.

[*Read before the Royal Geographical Society of London on February* 23 *and April* 26, 1852.]

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F. 2v
AFRICA.
between 10° & 30° South Latitude
Map
to illustrate the Routes of
Francis Galton Esq.r
Mess.rs Livingston & Oswell
and
W. Henry Gassiot Esq.r
1852.
ARUNDA
AUEMBA
MOVIZA
Lobale
BENGUELA
Benguela
Bihe
Bomba
Port Mossamedes
Great Fish B.
Ovampo
Ondonga
Makololo
Sesheke
Matiguna
Bakoba
L. Ngami
Soun Damup
Ghou Damup
Walvisch Bay
Kalahari Desert
Tropic of Capricorn
Matibelé
VINDA
MONOMOTAPA
Tete
Sena
Quilimane
Sofala
Inhambane
Delagoa Bay
Bechuanas
NAMAQUAS
Griquas
ORANGE RIVER SOVEREIGNTY
NATAL
AMAZULU
Port Natal
Orange R.
Wesley Vale
Rehoboth

f. 3r

E. Wheler

RECENT EXPEDITION

INTO THE

INTERIOR OF SOUTH-WESTERN AFRICA.

Mr. President,

A little more than two years ago, urged by an excessive fondness for a wild life, I determined to travel for a second time in Africa. I then became a Fellow of your Society, and through the active kindness of Dr. Shaw your Secretary, of Mr. Arrowsmith, and of others, I was thoroughly advised as to those geographical points which more immediately awaited inquiry, and, guided by their views, chose South Africa as the field of my travels.

I left England in April 1850, accompanied by Mr. Andersson, a Swede, to whose most active and cheerful co-operation throughout a tedious and harassing journey, I am in the greatest degree indebted. He still remains in Africa, principally with a view of investigating the natural history of the lake district, and of thence bringing home a complete collection of specimens.

At the Cape, upon the strong recommendation of Sir Harry Smith, I freighted a vessel for Walfisch Bay, instead of travelling the usual route from Port Elizabeth. The emigrant Boers had at that time assumed a menacing attitude, and it was currently believed that they intended taking immediate possession of the lake country, and of refusing passage to all travellers from the Cape. Two parties had already been turned back; and as on the one hand there was every reason to believe that the same course might be adopted towards me, and cause a fruitless result to my journey, so on the other, the country to the north of Walfisch Bay was an entirely open field for exploring, and I proceeded thence.

At Cape Town I could obtain but little information about even those parts, in which missionaries had already formed stations, and what I there learnt was also much exaggerated, as the country was believed to be extremely fertile and very populous.

The Damaras, into the heart of whose country no white man had ever penetrated, were described as a most powerful, numerous, and interesting nation; and the fact that some traders had settled at Walfisch Bay, whence large droves of Damara cattle were dispatched south, shipped to St. Helena, or sold to the, at one time, numerous guano and whaling vessels, seemed to warrant the opinion of the fertility of the country. This view was again confirmed by the great jealousy shown to the attempted expedition of our late member, Mr. Ruxton, afterwards so well known by his travels in America, who, when he landed, experienced such determined opposition and obstructions, that he was compelled to set sail without having penetrated more than 20 miles into the country.

Warned by his failure I took mules with me, besides my waggons and a cart, in order that I should be, to a certain degree, independent of assistance, and be able, at least, to carry my things across the barren desert, which intervenes between the coast and the more habitable parts.

I was also requested by Sir Harry Smith to establish, if possible, friendly relations on the part of the Colonial government with such tribes as were liable to be exposed to the attack of the emigrant Boers, and to disavow strongly all sympathy on its part with them. Indeed a mere expression of good will, without holding out the least prospect of direct aid, is a custom much valued and well understood by South African tribes generally.

I landed in Walfisch Bay, the estuary of the Kuisip, in August, and was very hospitably received by Mr. Bam, the Rhenish missionary. Some time and great trouble were required to drag all my heavy things with my few mules across the sandy desert, already mentioned, to his station, where they could remain in security whilst I went up the country to buy and to break in oxen for my onward journey. It would be out of place here to allude particularly to the extreme difficulty I experienced before all this could be done and I could make my final start in February from Barmen. The country was in the utmost confusion; the Namaquas were robbing and murdering the Damaras in every direction, and had indeed, just on my landing, attacked and destroyed the Schmelen's Hope Station, that of Mr. Kolbe. I was fortunate in having good interpreters, and a black servant whom I had taken from the Cape, and who was born in some central part of Africa, found the language so much like his own, that by the time all was ready for a start he had become quite fluent in it; besides whom, I had a man, Damara born, but bred among the Hottentots, and to communicate with him, an excellent Dutch and Hottentot interpreter; so that I got on from the first extremely well.

The knowledge that Europeans possess of these parts, dates originally from Sir James Alexander, who, chiefly in company with Swartboys (a Hottentot chief), travelled from the south to Bethany, Rehoboth, and thence to Walfisch Bay; on his return he wrote to two missionary societies, recommending this country as a favourable field for missionary enterprise, in consequence of which some German Protestants and English Wesleyans were sent there. Their head-quarters were at and near Eikhams, the present home of Jonker Africaner, who was at first a warm supporter, but latterly a bitter opponent of them. Subsequently the Wesleyans extended their missions towards the interior, and the Germans along the coast. Their present stations are marked on my map. When I arrived no European foot had ever penetrated 20 miles to the northward of the 22nd parallel of latitude, or 20 miles to the eastward of Elephant Fountain.*

My course from Barmen, as marked on my map, led me through the middle and most populous part of Damara land, at the further frontier of which one of my waggons broke down. I thence rode on across a bushman tract to the Ovampo, and returning, joined my then mended waggon, and came back to Barmen, which I reached in August 1851. I thence sent a messenger overland to the Cape to make arrangements for forwarding a vessel to meet me at Walfisch Bay in December or January, and lightening one waggon as far as I could, went with all my available oxen on a quick journey eastwards. At Elephant Fountain I joined Amiral, a Hottentot chief, and leaving my waggon there, rode on with him and explored to Otchombindé (called by the Hottentots T'ounobis), and thence returning, arrived with utterly exhausted oxen on the coast in December 1851. From Walfisch Bay I sailed to St. Helena, where I waited some weeks in vain for an opportunity of making a short excursion to Little Fish Bay and of obtaining some information from thence. I consequently set sail, and arrived in England at the end of last month, after exactly two years' absence.

To avoid misconception I must give some explanation concerning the names which I have placed on my map, in the selection of which I had some difficulty. In all the border country, and where the missionary stations now exist, most places are known by two, three, or even four names. The Damaras have one, the Hottentots another—this latter, translated into Dutch, forms a third, which is used very generally—and the missionaries add a

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* In Sir J. Alexander's, and in the missionary maps, the positions of the more distant parts explored by them are laid down very erroneously. In one map Elephant Fountain is placed one hundred miles too far towards the interior.

fourth; thus the place marked Scheppmansdorf, which is called Aban'hous in Hottentot, is always known as Roëbank by the traders and as Scheppmansdorf by the missionaries. It would have created great confusion to have attached all these different names to each place on the map, and I have therefore adopted the missionary names for their own stations, Damara names for all places that have them, and used Hottentot words as little as possible, for no orthography can possibly express their sound, except in rare instances, such as T'was and T'ounobis, which are capable of being pronounced. With perhaps less reason I have adhered to the Dutch word "Damara" to express the Ovaherero and Ovampantieru tribes, as it is a convenient name and one that has been long established, and which has as much right to pass current as the word "Caffre." The Hottentot name for that people is Damup in the plural, or Daman in the singular, and this is the root of the name "Damara," which it is needless to state is utterly unknown to the people themselves.

The country over which I travelled, proved to be the broadly developed end of that chain of hills and high land which runs parallel and near to the western coast from the Cape colony upwards, and separates the Fish River from the sea. Though this country is dotted over with hills and even groups of hills, and is very deeply scored on its western face with watercourses, yet in its general aspect it consists simply of a plain sloping steadily away on all sides from a small district of the greatest elevation, which is situated about the sites of the mountains Omatako, Diambotodthu, and thence to Awass, and which (from boiling-water observation) lies some 6000 feet above the sea-level. From this district, the watershed *eastwards* falls with a very gentle inclination to the cup-shaped basin of Central South Africa—to its lake, its flooded lands, and interlacing rivers; *northwards*, with still less incline, to a large river, of which the Cunene is a tributary, and which appears partly to drain that basin; *southwards* from Awass, Fish River begins its long and peculiar course towards the colony; and the comparatively steep *western* slope is ploughed up by the Kuisip, the Swakop, and five other more northerly river courses, which run into the Atlantic.

The sea-face of this broad belt is, except along the watercourses, uninhabitable, as during half the year there is no water and scarcely any pasturage. A strip of desert sand, 40 miles wide, follows the coast line, beyond which lies, north of Walfisch Bay, the barren Kaoko, and to the south of it the arid Namaqua land. The summit of the belt is a dense impracticable thorn coppice, though affording grass and a few scanty springs; but as we descend westward, and at about 220 miles from the coast, the thorns almost cease, and the land assumes the appearance of those broad plains, covered with grass and timber-trees, that have so often been described as lying between the Orange River and the Limpopo. Again, in the far north, at the latitude of Ondonga, the country becomes one of most striking and peculiar fertility.

Over all these parts the rains are periodical, and, from the nine years' experience that the Rev. Mr. Hahn has had at Eikhams and at Barmen, very variable. From the middle of May to November rain is scarcely ever known to fall, thence to January occasional and sometimes very heavy showers occur, but the true rainy season may be considered to be between the first of January and the last of April; the showers are extremely violent, but partial, and are always accompanied by thunder. The ground is seldom saturated till February, and then pools of rain-water (Vleys) are to be found everywhere; but, by June, all but the largest of these are dried up again. As a general rule, the rains fall most heavily on the summit, and on the northern and eastern slopes of the country, and, at Ondonga, they were described as being much heavier than in Damara land. The rivers are all *periodical*, and run to very different extents in different years. The Kuisip had been seven years without reaching the sea, and then almost, if not quite, reached it three times in six years. Of late, the Swakop has flowed three or four times every rainy season; yet, when it was first seen by Europeans, about ten years ago, the whole of the lower part of its course was choked with sandhills, bushes, and trees; these the first inundation swept entirely away, since which most violent torrents have passed down it. On the other hand, it was a constant complaint of the Damaras, that less rain falls now in their country than some twenty or thirty years back; and even their extensive migration from the Kaoko, to which I shall have occasion hereafter to refer, has been ascribed by the Damaras to the water failing them for their cattle.

It may, perhaps, give a more accurate notion of the country I visited if I describe in some detail a route through it. Leaving the excellent, but perfectly desert, harbour of Walfisch Bay—a journey of 16 hours across sand, soft and sinking at first and covered with shifting dunes, but afterwards hard and pebbly and crackling like frozen snow under the feet, takes us to Oosop. Ten miles before reaching the river, the plain shelves steadily and rapidly downwards to its bed, to which we descend at last through an imposing gorge about 300 feet wide and 4 miles long. The river-bed here is 100 yards broad, and consists of heavy sand, overgrown with patches of grass, and fringed on either side with a dense row of high reeds, beyond which, where the rocks leave sufficient space, are some fine groups of Unna trees. From the middle of the bed, a small streamlet springs out in all but the very driest seasons of the year, and after running some distance loses itself again in the sand. Notwithstanding a general appearance of drought, marks of violent torrents are everywhere visible,—trees lie uprooted, heaps of dead sticks and reeds and mud are washed high on the ledges of the rocks where they confine the river-bed, and also on the lower branches of the trees that still remain standing. The cliffs on each side are precipitous and magnificent. From Oosop to Davieep there is, perhaps, no one single place where an expert mountain-climber could get out of the bed on the north or right bank of the river, and only two places where cattle can be driven up from the left bank. The rocks are so bold and so broken, especially on the right bank, that a traveller can hardly realise the idea that they are not independent mountains, but only the face of a deep cutting which the river has made for itself, and that the general level of the country is from 800 to 1000 feet above his head. I ascertained this elevation as well as I could, by climbing up a hill on the left bank, the height of which I measured carefully to be a little more than 600 feet; then from the top of it I levelled across to the opposite cliffs, from the top of which the plain began, and, with my sextant, guessed at the remaining height. The plain north of the Swakop appears at this place to be quite level and barren, but not sandy; and it is almost, if not quite, uninhabited. I had good views of it from many different heights. The Canna river cuts its way through it in exactly the same manner as the Swakop, though its cliffs are described as being even higher. I could trace its course for a distance, from a hill near Hycomkap; and an appearance along the plain, as if the ground were broken up, indicated, for 20 or 30 miles, the gorge through which it ran. On leaving Davieep, the cutting through which the river flows loses the character of a gorge, and the mountainous sides open out more, continuing still to bank up the plains. Those of Onanis, 20 miles east from the left bank, give excellent pasturage, and the desert sand ceases about Tincas. Passing up the Tsobis river to avoid the deep sand of the Swakop, after 7 hours' up-hill travelling, through gorges nearly as striking as those of Oosop, we emerge on the plain, which we now find everywhere covered with thin grass, and studded over with stunted thorn trees. The "Hakis," or Fish-hook thorn, as the Dutch call it, begins to grow at Tsobis, but the land more to the westward is too barren to give sustenance even to that, and from this point to the borders of Ovampo-land the traveller has to tear his way through its cruel and tangled branches. Not a tree grows that does not bear thorns, and very few in which the thorns are not hooked; and their sharpness and strength are such as to throw a most serious difficulty in the way of exploring, especially as when travelling with a waggon, the oxen will not face them, and in difficult parts it is often quite impossible to get through the bushes, round to the struggling and fighting oxen. Cruel as the Hakis thorn is, there is yet another and much more severe opponent in a smaller, but sharper and stronger thorn. I have frequently tried the strength of all these with a spring weighing machine, by tying a loop of string to one end of it, which I hooked round the thorn, and then steadily pulled at the other end till the thorn gave way, marking the number of pounds resistance that the scale indicated at the moment the thorn broke; the Hakis thorn stood a pull of 4 or 5 lbs., and the other one of about 7, but often much more, and on one occasion I registered a strain of 24 lbs. Now, as several of these thorns generally lay hold of the traveller's clothes or person at once, it may easily be conceived what cruel laceration they cause. Continuing our route we descend to the Swakop again, near Otjimbingue, having, when at Tsobis, just caught sight of Erongo, a mountain 3000 feet above its base, but rising from the deep hollow of the Canna. From Otjimbingue to Barmen the river passes again through a broken, confused series of gorges, and among mountains; and it is not until we are far past Schmelen's Hope that we arrive at the source of the Swakop, and entirely clear of its valley. At the time of my visit to these countries, Schmelen's Hope, and a very few miles north of it, was the furthest point known to the missionaries, and other Europeans. As I travelled northwards, ascending the plateau, I saw the tops of the hills by the river, that had appeared so prominent when among them, slowly sink down below my level, and disappear among the trees. Diambotodthu no longer bounded the prospect in front, but on a sudden the two magnificent, almost faultless cones of Omatako burst full into sight, each appearing like a Teneriffe, beyond which was the broken ground of Otjihinna ma Parero, and the long wall of Koniati, that bound the arid Kaoko. I had but just left a tributary of the Swakop, still a broad river bed, when, to my surprise, I came upon another water-course of considerable size, running N.E., which I followed some distance, and which I found went towards the Omoramba. It seemed incredible that a water-course 30 or 40 yards broad, with steep banks, could have an origin in the open plain within a mile, but I found afterwards that this sudden commencement of broad river beds was the rule, and not the exception, in Damaraland. I had also constantly noticed that the breadth of the river beds was often out of all proportion to the quantity of water that they could ever carry: thus the Erora, which has not a course of more than 20 miles and is by no means an important drain to the country, is about 500 yards across, but I found that the same cause influenced both the length and the breadth of the river bed. It must be recollected that the ground is entirely sand, but well fixed on its surface by the long running roots of the grass that covers it. The wet in the rainy season drains through the sand into the river bed, and, of course, constantly washes away some with it; but the subsoil yields before the surface, and thus the banks get gradually undermined, and are always falling in, so that the river has a constant tendency to grow broader, and to push its apparent source higher up towards the water-shed. It is very curious to see the head of one of these river courses, where the ground seems to have fallen in suddenly, leaving a place like a gravel pit, whence the bed begins at once some 12 or 20 yards wide, and perhaps 10 feet deep.

In the case of the Omoramba K'omatako, whose course lies alternately over districts of sand and over hard ground, it is very curious to observe how, what in the first case is a fine magnificent river bed with high banks, suddenly, as the ground becomes hard, loses itself in the open plain, where there is not a vestige of its course; and a few miles further on, the ground becoming sandy, the river bed re-appears again, just as unexpectedly as it had been lost, and altogether as large as before.

I had made a considerable détour to avoid a very hostile tribe of Damaras, who were then encamped on the Omoramba, and through whose neighbourhood my men refused to attempt a passage. I, therefore, guided only by such vague information as I could then occasionally procure from the savages, went under the escarped sides of Omuvereoom, at the termination of which the reported lake Omanbondé was said to lie. Through the whole of this road I had to trust to chance in finding water, and in also finding a practicable road for waggons. At this time my men were undisciplined, and in no way to be depended upon. My oxen were only half broken. There was fighting going on between two powerful tribes immediately behind us, and a dense jungle of thorns surrounded us on all sides. Of game there was none, so that it was impossible to depend on anything else but my oxen for food. The waters were drying up on all sides, and the prospects of the expedition became gloomy enough. I chanced, however, to fall upon a curious watercourse, that we named the River Vley. It was a narrow strip of green, not much depressed below the country on either side, which contained frequent shallow pools. It was simply a succession of Vleys or pools, and varied from thirty yards to much more in width, and here and there stretched out into broad plains; the thickest of thorn jungle, and one perfectly impassable to a waggon, pressed close upon it, so closely that the waggons were frequently taken through the water where there was not room for them between the Vleys and the thorns.

It is wonderful how little inhabitable country there is in this part of Africa. Either the thorns occupy the ground to the exclusion of everything else, or drought makes it unfit for cattle. In fact, Damara-land is made by the watercourses and a very limited number of springs; take away these, and no pastoral people could inhabit the country; whilst agriculture, except to the most limited extent, is in all cases out of the question. The watercourses, though utterly arid to all appearance, are really to a great extent reservoirs of water, which is checked in its evaporation by the great depth of sand that overlies it. There are places known to the natives in most of these river beds, and which probably correspond to the lowest parts of the longer reaches, where water can be got by digging; but it is useless, as I am well aware by long experience, to dig deeper than the sand, for no water exists in the hard ground below it. It must not, therefore, be in any way supposed that, because I have dotted out in the map a large space and called it Damara-land, the whole of that area is occupied by these people. The case is far different. The number of pasturages is extremely small; and I am sure that I myself have seen and know quite half of them. It will be easily understood also that the boundaries of a people like the Damaras are exceedingly arbitrary. It rains perhaps heavily one season, and there is abundant Vley water at a distant place, where the pasturage is also good; the neighbouring tribes, of course, flock there, and spare the grass nearer their usual haunts and more certain waters until the dry season. The boundaries are not definite and natural, excepting so far as the long range of Omuvereoom is concerned; but they are decided to a certain degree by custom, and I have endeavoured, under this view only, to represent them on the map. One point bearing on this subject must not be forgotten—that two African tribes never live close up to a common frontier. They are always fighting and robbing, and therefore a broad border-land is essential; and in these border-lands, so far as I have seen, the Bushmen and other outcasts live. As regards the water that these get, it will easily be understood that many places are found which yield enough for a small family, but which would scarcely support two or three oxen. The water oozing slowly into a well from the damp sand surrounding its bottom, at the rate say of a gallon in a day, is a case often observed, and then the tracks of Bushmen are pretty sure to be seen about it also. But to proceed with my itinerary. Just where the Vley River began to be lost the bushes became more open, and Omuvereoom sloped down into the plain, and I here met with guides who took me straight to Omanbondé. This had been constantly described to me as a large lake. I thought it might have been the Demboa of early maps, with whose position it fairly coincides, and to whose name Omanbondé bears a certain resemblance.

The occasional existence of hippopotami in it was also thoroughly substantiated, and yet, when I saw it, it was *perfectly dry*. The place is a remarkable one enough, for it is the long reach of a watercourse, closed up at both ends by a dam, which, together with its sides, slope upwards till they attain an elevation of about 100 feet above the bed. The breadth of Omanbondé is trifling, but its length is some 8 miles. Going downwards, and passing over a broad dam, we come into another reach also dammed up, and then again into another. N. of Omanbondé there are also two other places just like it. The water evidently filters through these dams in the rainy season; and I was assured that, even in the driest times of usual years, Omanbondé was a reservoir of water: I, however, to my great misfortune in many ways, chanced to travel during a year of great and almost unprecedented drought. The course of the Omoramba downwards was so clogged with thorns as to be quite impracticable for a waggon, and it was indeed with great difficulty that we even crossed it.

On leaving Omanbondé, and getting out of the valley of the Omoramba as well as I could, we came to a far more open plain than any I had hitherto met with; and suddenly, at lat. 19° 50′, found ourselves among palms—the harbingers of a better land. A little further on they flourished to the exclusion of almost every other tree; but before we came to Okamabuti had ceased almost as suddenly as they began, though during the whole road to Ovampo-land one or two were every day to be seen. We had now arrived at a much more luxuriant country, and water was plentiful: a long limestone ridge at Kutjianashongué yielded springs in numerous places, around which large herds of cattle and numbers of Damaras were collected. Timber-trees began to appear, growing in clumps, with long open grassy savannahs between them; and to me it was a constant wonder to observe the straight and perfectly defined borders that these belts of wood presented. Here and elsewhere I have seen them look, not as one would conceive that Nature could have planted them, but presenting exactly the appearance of the work of an ornamental gardener. I am in no way able to account for this striking peculiarity, as there is no perceptible difference in the soil on which the trees grow, and in that where they are absent. I cannot explain the fact, but simply state it. Okamabuti may be considered as the northern boundary of Damara-land, though in the rainy season the natives sometimes go further. The country is said to be quite impassable to the N.E. It appears to be entirely uninhabited, and is thickly wooded. I made an excursion to a hill in that direction, about 8 hours off; but, so far as I could see from the top of it, one level forest extended far away.

The masses of hills that lie to the N.W. of Okamabuti are all limestone. I saw a good deal of them from the guide having lost his way more than once when he first took us there, which ended in compulsory and anxious wanderings for more than a week about them. A great many Bushmen live among these hills. I saw there a most curious freak of nature, which I afterwards witnessed on a far more magnificent scale at Otchikoto. Wherever a piece of bare rock is to be seen (which is nearly everywhere between Ootui and Otchikoto), it is pierced with holes perfectly circular, and of all sizes, and like round smoothed tubes. Thousands of them would just admit the thumb, and are quite shallow; numbers are about the diameter of a bucket, and from 3 to 5 feet deep, forming most dangerous pitfalls: in many of them we find trees growing, some not quite filling the hole, others just fitting it, and, again, others so constricted that the trunk swells over and entirely hides the sides of the hole. I saw a few holes about 8 feet across, but I do not recollect observing any intermediate size between that and Orujo, a perfectly circular hollow in the midst of chalk about 30 feet deep and 90 feet across. The sides of this were certainly not smooth, but they formed an exact circle, like a gigantic pan, the floor of which was level, with a small well in the middle. Otchikoto was still more astonishing. Equally circular, and its sides equally steep, it measured 400 feet across, and was almost filled with the clearest of water, the level of which stood at 33 feet below the banks, with the extraordinary depth of from 170 to 180 feet, which I plumbed in five places. I heard there was another, if not two more of these places, somewhere among the Soun Damup. The water-level of Otchikoto was, as I was told, and I could myself gather from appearances, not increased in the rainy season.

I was fortunately not encumbered here with my waggons, for I do not think it would have been possible to have taken them on through the thick forest. Here there is not a single landmark to catch the eye, and nothing but the most skilful tracking could find the road when the rain had obliterated the spoors of the preceding year. We got water at Otchando, and came to the first Ovampo cattle-post at Omutchamatunda. Travelling on, we arrived suddenly at the large salt-pan of Etosha, which is about

9 miles across from N. to S., and extends a long way to the W. The mirage was too strong to admit of my measuring the distance of the high banks that there bound it, and which I could just make out both as I went and as I returned.

This lake is impassable in the rainy season, but was perfectly dry when I saw it, and its surface was covered over in many parts with very good salt. A little further on we come to the remarkable Otchihako-wa Motenya, a perfectly flat, grassy, but treeless extent of country, stretching like an estuary between high and thickly wooded banks. It is said to extend a very considerable distance W.; indeed, I cannot help thinking even down to the sea-coast. I passed it near its head, where it was only 12 miles across; but where the Ondonga and Omaruru route crosses it, it is a long day's journey from side to side, and all the Damaras who had been that route assured me that it extended as far as they knew to the W. Again, the Omaruru and Onganjera route crosses a flat of three days' extent, but in which there is some water, and which is asserted, and indeed appears, to be identical with it. It is looked upon with great horror by the Damaras from the bitter coldness of a night passed upon it, as there is of course no fuel and no shelter.

It is difficult for me to express the delight that we all felt when in the evening of the next day we suddenly emerged out of the dense and thorny coppice in which we had so long been journeying, and the charming corn country of Ondonga lay stretched like a sea before us. The agricultural wealth of the land, so far exceeding our most sanguine expectations,—the beautifully grouped groves of palms,—the dense, magnificent, park-like trees,—the broad, level fields of corn interspersed with pasturage, and the orderly villages on every side, gave an appearance of diffused opulence and content, with which I know no other country that I could refer to for a parallel.

I arrived ultimately at Nangoro, the king's werft, where I spent three weeks most pleasantly. But my oxen had fallen lame and sadly out of condition, and I felt some misgivings as to whether they could even take me back, and there was no grass for them at Nangoro's to eat. All his cattle were sent far away to the cattle posts. Half my party were left scarcely in a fit state to protect themselves among the Damaras, and I had often anxious thoughts for their safety. My provisions were getting very low, and unless more cattle could be bought in Damara-land we had not sufficient to take us back even to Barmen, where we had left the missionaries in too great want to be able to help so large a party as ourselves. The country too was fast drying up, and the road southward might become impassable; still the *great river* was only four long, or five comparatively easy, days ahead; but this and the return journey, together with the rest necessary for my oxen, I was aware would be at least a three weeks affair, and I hardly knew what course to take in case Nangoro would give me permission to proceed. It was certainly with regret, yet still with a feeling of relief, as putting an end to my indecision, that a message was at length received from Nangoro, prohibiting me from proceeding farther. If I had been in a condition to temporize, I have no doubt that I could have persuaded him to let me proceed, but that was now out of the question, and I therefore took leave and returned. Fortune now favoured me in many ways. I found my waggon mended, a sufficiency of cattle bought, and obtaining a guide, returned by a good road up the Omoramba without much difficulty, except in having nearly every day to dig or to clear out wells, which fully employed my whole party, now consisting of thirty-four people.

Returning to our starting point, Barmen, I will next describe the route which I followed to the eastwards, and which is very interesting, as it presents a peculiarly easy and open highway to the interior, and one practicable at almost all times for waggons, though indeed I—travelling at the driest time of an unusually dry year, one in which many of the Damara cattle perished of thirst, even at their own cattle posts—failed in reaching Lake 'Ngami. Proceeding up a small, but frequently running, river-course, a tributary of the Swakop, to Eikhams, and thence by a well-made Hottentot waggon road, over a very broken and arid country, we ascend out of the valley, keeping the high ridge of Awass to the right hand. We are now upon an elevated open plain, presenting no difficulties whatever to waggons if we follow the course of the Kuyip, but the ground that borders the upper part of the Noosop, by which I went, is very rugged and thorny. There is far more water to be got all about here than in Damara-land, but this being at present the border country between the Hottentots and Damaras, the wells were not generally opened. From Kurrikoop eastwards no anxiety need be felt for food, as there is plenty of game, though the animals are exceedingly shy. The ground is sandy and undulating. Proceeding on, we get to Elephant Fountain, beyond which there are no peaked hills, nor landmarks, in fact, that could be laid down in the map, and thence recognised by a future traveller. Elephant Fountain had been a Wesleyan missionary station, but was abandoned for the double reason of being subject to fever from April to June, as well as from its vicinity to some warlike Damara tribes. There is nothing in the appearance of Elephant Fountain that would suggest an idea of unhealthiness; it possesses, indeed, no peculiar feature, but it stands well on a barren and thorny hill; the—here contracted—bed of the Swart River is below, and there is a small, clear spring, which supplies water. Most fearful attacks of fever have year after year been experienced at the place, but not, so far as I could learn, anywhere else in the immediate neighbourhood. At Elephant Fountain I left my waggon, and rode on with a Hottentot chief, Amiral, and about forty of his men, to the eastwards. They had lately explored a long limestone ridge of hills that extends some 50 miles from T'was, and which is greatly intersected by water-courses, headed by springs, and along which we went. It appears to be of about 20 miles breadth, and attains a height of at least 1000 feet above the general level of the country. I consider it quite as a natural boundary between the thorny country of Damara-land and the broad, sandy, and wooded tracts of Central Africa. I contrived to get Bushmen guides to take us and about half of Amiral's party to T'ounobis, which we reached after a journey most trying to the oxen. The road passed by many large but dried up vleys; the ground was sufficiently hard, and would at ordinary seasons afford an excellent road for waggons, which after leaving T'was should pass not on the top of the ridge, as I did, but skirt it in the plain. T'ounobis is a fountain in a river-course, sufficient to supply any quantity of cattle, where I remained a week recruiting my oxen, of which I had barely sufficient to carry me back. I found a large village of Bushmen there, from whom I received much information concerning the lake and the country ahead. The land in front, up to its very borders, was described as being exactly the same as that we had now traversed. Hard sand, with plenty of trees, but not so thickly overgrown as to form any obstacle to a waggon, and growing but very few thorns,—indeed, I had great difficulty in getting thorn-bushes, of which to make my sheep kraals. So far then as T'ounobis I can guarantee the road from Walfisch Bay towards the interior to be perfectly open at any season of the year, and, except in the driest of times, from T'ounobis onwards. T'ounobis was passed by the Kubabees Hottentots in 1850. They had come upwards along the Umak Desert on a plundering and shooting excursion, with horses and oxen in great numbers to ride on. They had also built shooting-huts by the waterside, which I used, and had left other tokens of their passage. At T'ounobis they obtained a guide, whom I saw, and from whom I received much information, and under his escort they reached the lake.

A perfectly marvellous quantity of game congregated here; deep pools of water that were supplied by a fountain were drunk dry every night, and I therefore more readily believed in the constant assertions of the Bushmen, that there was then no water whatever for a distance twice as great as that over which we had travelled ahead.

Having now described briefly the geography of those parts that I visited, I will next state what I learnt from various natives respecting the countries ahead of me.

At T'ounobis I received the fullest description from the Bushmen natives of a lake called by them Il' Annee, which they often visited, and which I now feel assured to be Lake 'Ngami. It had been reached thence from T'ounobis by the party of Kubabees Hottentots, that I mentioned above, in 7 days in August 1850, and the direction of its nearest point was uniformly stated to lie thence N. 50 E. true (N. 75 E. compass). The chiefs of the black tribes at that point were Maharaquè and Tworrathabè, names which are identified by Mr. Oswell as being those of the chiefs of the Maclumma on the S.W. of the lake. The distance represented by 7 days' journey in these parts would be pretty nearly 120 geographical miles measured straight, certainly between the wide limits of 100 and 140 miles.

I also heard much of a large river whose "lay" was N.N.W., and which joined the lake. This evidently is the Tso, but it was described to me as being called the Beribè (in Sichuana), and as the T'guain Tl' Obo (in Hottentot). As regards the course of this river I was assured that it ran out of, and not into, the lake, but my information was not such as to withstand the more immediate testimony of Mr. Oswell, corroborated strongly as it is by the general features of the country.

This river, the Beribè (or Tso), was stated to pass entirely through the country of the adjacent tribes, and far in a N.N.W. direction to the other side of them. A much smaller stream S. of the Beribè, and having the same general course, was described as joining it just where it met the lake. This last streamlet, the Malopo (in Hottentot the T'kains), on which there are no boats, is separated during its course from the Beribè by a range of hilly country, called by the Hottentots the T'dèba. The Omoramba, at a distance of about 90 miles easterly from Omanbondé, meets another dry river-bed, and the two together ultimately reach some large water, but which I do not think to be the Malopo. It appears to be stagnant or nearly so, but I received very contradictory information about it; the large river (the Beribè) is beyond it. The Omoramba at about 60 miles from Omanbondé passes through a very hilly country, which, as far as I could make out, was continuous with the T'dèba. I have mentioned that hippopotami have constantly made their appearance at Omanbondé when there was water there; this is a sure proof that the Omoramba cannot be entirely lost in the plain, but must join a large water, some such as I have mentioned. About the lower part of the Omoramba a peculiar race of negroes (the Soun Damup) live, and extend very far to the northward. I shall refer to them again later; they were described as living N. 15 E. true from T'ounobis.

My Ovampo information refers to a large river that runs from E. to W., and which is 4 quick or 5 easier days' march (say 100 miles due N.) from Nangoro's werft. It is a broad, swift-flowing stream, to the border of which Portuguese traders come and traffic. The ferry, which is chiefly used by the Ovampo, lies N. 19 E. by compass, or N. 7 W. true, from Nangoro's werft in Ondonga, and is near the junction of the two streams which principally form this river. One of these, the larger, comes from the very far E., the other from the S.E. and from the Mationa country; Mationa being the name given both by the Ovampo and Damaras to the tribes living on the Beribè, including those belonging to the chiefs whose names I have already mentioned. The Cunene was said to run into this river, but of its point of confluence I am not satisfied. Mr. Oswell informs me that he had always conceived an idea, from what the natives told him, that the Tso was in some way connected in the far N. with a large river running to the W. The Mationa river, mentioned above, may be this link. Where the embouchure of the Ovampo river may be I have no idea, but I have many reasons for thinking it not to be the Nourse. The captains of coasting-traders in those parts assured me that the Nourse is a periodical water-course, while I learnt from the same and other authorities that a constant river of considerable size, though small at its actual mouth, flows into Little Fish Bay (Mosammedes). There is now a thriving settlement there, where a Frenchman has long resided, who is said to make distant trading journeys into the interior. It would be very desirable for any officers of the slave squadron, or others who might land at that port, to make inquiries about the lower part of this stream, which must be perfectly well known there. The Ovampo told me that it seldom ran quite into the sea, but ended in a large deep pool close by the coast, beyond which the sand was dangerous to walk over, as it was a quicksand.

There is also a Portuguese trading station on the river opposite the country of the Onganjèra; this cannot be far from the coast, for the caravan from Damara-land to that nation leaves Omaruru and travels northwards for a long way over some very high land frequently in view of the sea. From the mouth of the river a kind of sea-shell, much prized, and called by the natives Ombou, is frequently brought. As regards the size of this river it is said to be such, that when a man calls across it his voice can be heard, but not his words. Opposite to the Ovampo it is extremely swift (boats cannot paddle up it) and very deep. It appears to be a most interesting river, and well worth exploring.

I can say nothing as regards its salubrity, except that Ovampo-land appeared a remarkably healthy country, and Damara-land I know is such. Corn land extends the whole way S. of it from Ovampo-land to very near the sea. Between the two confluents of the river the Ovabuntja live. Their country is described as very marshy, and many of their houses are built on poles: of course fever is to be dreaded there.

Ethnology.—I will now pass on to the distribution of tribes in this part of South Africa. Their history is not a little involved; but they may be enumerated thus:—1. The Ovampo are corn-growing tribes to the north, who, considered as blacks, are a highly civilized people, and one with strong local attachments, well ordered, honest, laborious, and neat, yet still with much of the negro in them. 2. The Damaras are a vagabond, lazy, thieving, pastoral race. 3. The Hottentots to the south are too well known to require further comment. 4. The Mationa Caffres to the east; and lastly, 5, the Bushman Hottentots and others, who lead a Bushman's life in the barren tracts, that separate these larger nations.

The Namaqua Hottentot is an invader of the last few years, but the Bushmen have not even a tradition of another home. Living with them are outcast Damaras, and also a very peculiar race of negroes speaking the Hottentot tongue, and that only. These have no traditions indicating their descent, and are found as far south as Bethamy. They live peculiarly on the hills, and have puzzled ethnologists ever since they were first described. They call themselves Ghou Damup, and in Sir James Alexander's work and in missionary publications, are described as the Damaras of the hills. With the Damaras, however, they have nothing in common. Their features, shape, customs, and aptitudes indicate an entirely different origin, and it will be seen that an enquiry into their earlier history throws great light upon the former state of this country. The Mationa are Bechuanas, among whom, partly as slaves and partly independent, live the Soun Damup, a tribe kindred to the Ghou Damup in every respect, language, appearance, and superstitions.

To make the matter clearer I will state the results of frequent enquiries from many independent sources, the agreement in which is very striking.

About 70 years ago (certainly between 65 and 75 years), and when, from uniform testimony, water was much more abundant than it is now, the Damaras lived in the Kaoko alone. The Ovampo were within their present frontier, but the Mationa extended to Ovampantieru-land, certainly far to the westward of Otchombindé, and all between these and down to the Orange River, lived Hottentots of various tribes. The Nareneen lived by the sea, and the Ounip (called by the Dutch Toppners) about the parts of which we are now speaking, and south of these were the Keikouka, now represented by the red people, by Swartboy, the Kubabees, and Blondel Swartz. Near to the Orange River the tribes were more numerous and more civilized, from their neighbourhood to the Dutch. They had a few guns, sometimes waggons and so forth, and these were the ancestors of Jonker, Amirals, Jan Boys, and other smaller tribes, as Buchess' and Fransman's. There was also a certain admixture of bastard blood in these last, who came to be designated Oerlams (a term of half reproach) by the Dutch, and to be disavowed by the Keikouka as partly aliens. Hence a jealousy arose, and still exists, between the two great divisions of the more southern Hottentots, the Keikouka and the Oerlams, who together are usually called in the aggregate " Namaquas," in contradistinction to the northerly tribes of Bushmen.

Interspersed among the Hottentots from the north to the south were the Ghou Damup, who were invariably considered as slaves and a good deal ill-used ; they lived, when in communities, in the hills, or table-mountains, of which there are many, such as Omuvereoom, Konati, Ketjo, Erongo, and many others, of which I have often heard, more to the south and west. Two movements now began to take place ; first the Damaras, pressed for room or for some other cause, made an irruption to the eastwards, and spread over the country as far as Otchombindé, almost exterminating the Hottentots in their way and driving back the Mationa, while the Ghou Damup were pretty safe in their mountain-fortresses and received but little harm. The Toppners, however, not being at that time accustomed to the mountain-passes with which the Ghou Damup were familiar, were, as I said, greatly cut off. And it is curious, that within very late times (about eight years ago), exactly the same thing occurred to the Nareneen living west of the Kaoko.

The more northerly Toppners were thus quite cut off from all communication with those about Walfisch Bay, and remain so to the present time. There exists, however, the greatest fondness for traditional stories among these people, and I found the liveliest interest expressed on my return from the north relative to the well-being of those Hottentots whom I met among the Ovampo, and of whom scanty information only had been received from time to time. In Sir James Alexander's work mention will be found of the Navees, or Nabees, as he spells it, on information received among the Hottentots. These are the Ovampo ; Navees being the Hottentot name for them.

We have seen thus how the Damaras drove the Toppners to the same places as the Ghou Damup. Community of misfortune is gradually destroying the feeling of difference of race between them, so that intermarriage, which would have been quite unheard of in former years, is now becoming common. The Hottentots told me that 10 years ago it was quite unknown; and I have never seen any but children of the mixed race.

The Mationa made at various times reprisals on the Damaras; the last being about 20 years ago, when the Mationa came up the Epukiro River, while on a previous occasion they had passed up the Omoramba.

From the Damara invasion we now come to that of the Namaquas, which dates at a much later period, and in which Jonker Africaner played the principal part. Of all the particulars of this I have the fullest information; but I cannot expect that an interest which depends chiefly on persons and parties in South Africa, will be felt here; suffice it, therefore, to say, that by gradual encroachment the tribes, whose names you see here mentioned, strengthened and formed themselves, and plundered all before them. Sometimes they went on a professed national feeling to aid the Toppners, sometimes on none at all. In every case, however, the Toppners were thoroughly victimised; and it is only of late, when the Nareneen had obtained so many guns and so much ammunition from whalers and guano ships, that they acquired sufficient strength to be recognised as others than simply as Bushmen by the Namaquas.

The moment that I saw the Ovampo I was most strongly impressed with the national identity of the Ghou Damup; it is true that the latter are most squalid and thievish, very strikingly opposite characteristics to those of the Ovampo, but on the other hand we cannot forget that they must have been an outcast race for ages, to have so completely lost, not only their own language, but all traditions of it. They dig and plant, which neither the Hottentots nor the Damaras do; and on the other hand I was assured that the Soun Damup, who lived to the north, were the field labourers of the Mationa (the Hottentots call bread "soun" from them), and were exactly the same as the Ovampo, except in some trivial difference of dress, and that there, some spoke Ovampo, some Mationa, some Hottentot, and some all of these tongues.

I conclude, then, that the Ghou Damup were the real aborigines of the country S. of the Ovampo, that very long since the Hottentots invaded and entirely conquered them, and that they both together settled down into the condition in which I described them to be at the beginning of this account.

I may add that exactly the same process is now going on between the Namaquas and the Damaras, and probably one-half of the whole Damara population has already been enslaved or murdered by the Namaquas. Those that are made slaves are used as cattle-watchers; their children, as they grow up, learn Hottentot, and readily identify themselves with the habits of their masters, so that few generations will probably have passed before the Damara language will be obsolete among them, and they will have become a race affording an exact parallel to that of the Ghou Damup. The Namaquas are still pressing on with the peculiar restlessness and obstinacy of the race, a belief in their destiny, a scorn of blacks, and a fondness for plunder, which has already led them from the Orange river, and which now seems to be more marked than ever. As unarmed savages can never resist their guns, which number between 3000 and 4000, my belief is that not many years will have elapsed before they will have utterly destroyed the Damaras, and will come into direct conflict both with the Ovampo and the Mationa.

On the habits of the Damaras I have little to say. Physically speaking they are a striking race, with an appearance of strength, lightness, and daring that is highly imposing. They are tall, upright, and often remarkably handsome men, models for sculptors. They have a fair facial angle of about 70°; fine, manly, open countenances, and often beautifully chiselled features; but morally they are the most worthless, thieving, and murderous of vagabonds, and at the least irritation their usually placid countenance changes into one of the most diabolical expression. Much struck as I was with them at first, I came ultimately to the conclusion that, except their general good humour, there was not a single good point in their character. Their very personal strength is wonderfully small considering their immense muscular development. Often as I have had trials in lifting weights and so forth among them, I never found one who was anything like a match for the average of my own men. Idea of a Supreme Being they have none; but ceremonies and superstitions innumerable; none of which have anything poetical in their character. They are chiefly shown in smearing with fat and with cow-dung and in abstaining from eating cattle with certain marks, different according to the family they descend from: of the fetisch superstition there is no trace. A tree is supposed to be the universal progenitor, two of which divide the honour, one at Omaruru, the other on the road to the Ovampo. All the men are circumcised.

They have no government; any man with 20 cows calls himself an independent captain. They are devoid of all national or social ties to a perfectly marvellous degree. If one werft is plundered, the adjacent ones rarely rise to defend it, and thus the Namaquas have destroyed or enslaved piecemeal about one-half of the whole Damara population. As to the language, a very complete grammar and dictionary has been compiled by the

Rhenish Missionaries, and sent last year to the Professor of Philology at Bonn, who will, I believe, shortly publish it.

Very different from these in every respect are the Ovampo, who are orderly, centralised, hard-working, neat, and scrupulously honest. Ondonga is plotted out into small, well-farmed holdings of corn and pasturage, each occupied by a family, generally the grandfather, son, and children. Every one here has the appearance of plenty, and none of the squalid, wretched, uncared-for, old people, so painfully common among the Damaras, are to be found amongst them. The King, Nangoro, is despotic, and seems to rule with a patriarchal sway. Laws against theft are peculiarly severe. The tribute to the King is small, and paid by a per centage on the tobacco grown, and *not on the corn.* The marriage tie is extremely lax. The Ovampo possess the entire carrying trade between the Damaras and the Portuguese.

My map is for all practical purposes, and so far as it professes to go, very fairly accurate. I am not aware that any isolated hill is left out, though I do not profess to give the peaks in each group. I should have been involved in endless confusion, had I attempted so much. The limits however of all hills and all groups of hills are taken. I triangulated chiefly with an azimuth compass, from Walfisch Bay onwards as far as there were mountains to triangulate by, that is to Otchikoto on the N., and to Elephant Fountain on the E. I have so great a number of bearings, that I have had no difficulty in making many independent series of triangles, and checking one by the other. I thus pretty easily found out such errors, either of reading off observations, of mistaking hills, or of writing them wrongly down, which I saw in spite of all my care would occasionally occur. I then selected the series of triangles that I thought would give the most trustworthy result, guided by the size of the angles, and more particularly by the definiteness of the mountain peaks that I observed, and then protracted them. Having done this, and registered the longitudes which this triangulation gave for my three main stations, Barmen, Okamabuti, and Elephant Fountain (assuming the longitude of Pelican Point, Walfisch Bay, at 14° 27′ 5″, and determining the scale of the map by differences of latitude), I compared these longitudes with those deduced astronomically, and I am glad to say that the agreement is very satisfactory. There is an abstract of all this at the end of the paper. Some grave error had affected my instrument, so that although the observations in each group agree extremely well together, yet there is a wide difference between the longitudes derived from these several groups. I had, however, done my best when taking lunars, say E., to take others W. under as nearly as possible the same circumstances, both of altitudes and of distances, as I could, and of the same bodies also. With sun observations I used one coloured glass, and always the same one, toning the instrument of course as required. I also examined the adjustments of my sextant with all the care I could, previously to beginning to observe; and it is solely from having taken these precautions with great pains that I can account for the excellent agreement of the mean longitudes (deduced as they are from such wide extremes) with that obtained by triangulation. As regards T'ounobis and Ondonga the lunars were taken with a good, though small, and not clearly divided circle, which had to be read off by firelight; still the results of the former are very fair, and those of the latter, being checked by the position of Otchikoto, will answer sufficiently well. I am thus particular upon these matters, as it is of course a satisfactory thing to have well determined the geography of a new country, even though only in outline, for it may save much trouble and doubts to future travellers. I have altogether determined astronomically the longitudes of 6, and the latitudes of 53 stations, and I had no object in taking more.

TABLE of LATITUDES OBSERVED.

Places of Observations.	The Number of Bodies observed. N.	The Number of Bodies observed. S.	Latitudes. °	′	″
Sand Fountain	1	1	22	57	57
Scheppmansdorf					
Hycomkap	2	..	22	41	45
Oosop	3	2	22	45	38
Davieep	1	..	22	48	0
Annaas	1	..	22	43	0
Mouth of Tsobis River	1	..	22	26	3
Tsobis	1	..	22	30	55
Kurrikoop (on Swakop)	2	1	22	23	2
Otjimbinguè	2	..	22	21	50
Barmen	2	1	22	7	7
Schmelen's Hope	1	1	21	59	34
Okandu	1	..	21	56	30
Kutjiamakompè	2	..			
Okanjoè	1	..	21	27	9
On leaving the Omoramba	1	..	21	13	29
Otjikururumè	1	1	21	5	54
Ontekeremba	1	..	21	1	49
Ozukaro	1	..	20	49	57
Near Ja Kabaca	3	..	20	38	50
Otjironjuba	1	..	20	32	25
July 16th, Omoramba	1	1	20	29	45
April 1st, Werft	2	..	20	22	15
July 14th, Omoramba	1	..	20	18	43
Okavarè	2	1	20	6	37
Omanbondè	1	1	20	2	30
On Flat, April 12th	..	1	20	1	5
Okatjokeama S. Vley	1	2	19	57	15
,, N. Vley	1	2	19	55	35
Okapukua	..	1	19	47	4
Okamabuti	many		19	30	48
Omutirakanè	1	1	19	25	50
At foot of hills, April 30th	1	..	19	20	13
Otchikoto	..	2	19	10	0
Otjando	..	1	18	58	15
Small well	..	1	18	54	37
Omutchamatunda	..	2	18	47	32
South border of Flat	..	1	18	31	51
Two miles within Ondonga	1	..	18	6	41
Nangoro's Werft	..	1	17	58	40
			°	″	′
Barmen Cattle-post	1	..	22	15	55
Due E. of High Peak	2	1	22	14	36
Katjimasha's Kraal	1	..	22	26	0
Eikhams	3	..	22	34	40
On plain, Sept. 3rd	1	..	22	27	4
On Noosop R., Sept. 5th	1	..	22	16	11
Elephant Fountain	2	..	22	27	15
T'was	2	..	22	36	18
Kurrikoop (on Noosop)	1	..	22	25	21
Occultation-place	..	1	22	27	35
Near Okomavaka	1	1	22	15	20
On road to T'ounobis	1	1	22	7	35
T'ounobis	2	1	21	54	40

Lunars taken with Sextant.—Calculations by Mr. Burdwood, Hydr. Off.

—	Distinguishing Letter in Calculations.	Body observed.	East or West of Moon.	Proximate Altitude of Sun or Star.	Proximate Altitude of Moon.	Proximate Distance	Longitudes deduced.	Distinguishing Letter of Groups.	Means.	Longitude by means of East and West.	Longitude by Triangulation alone, using 14° 27′ 5″, as longitude of Pelican Point.	Difference.	Longitude used in Map.	Sets omitted as being evidently bad ones.
						°	° ′		° ′					
Elephant Fountain.	*a*			55	42	19	19 13·2	A						
	*a***			57	46		19 21·7							*a**
	b	Saturn	W.	54	52		19 17·2		19 16·2					
	c			51	53	20	19 16·7	B						*b**
	*c**			51	52		19 15·5							
	*c**			50	52		19 12·7			° ′	° ′	′	° ′	
	d_1			25	28		18 24·5			*18 53.7	18 – 59	+5·2	18 59	
	d_2			26	28	106	18 30·2			but if calculated according to method mentioned in the note we have				
	e			29	26		18 30 2	C						
	f			32	23		18 36·5							
	g	Sun .	E.	27	34		18 31·2		18 31·5	18 59.5	18 59	–′·5	18 59	
	h			30	32	93	18 30·2	D						
	i			46	19		18 32							
	j			48	17		18 36·5							
Schmelen's Hope.	k_1			37	20		16 28							
	k_2	Sun .	E.	38	19	122	16 27·7	E	16 25					l_1*
	l_1			40	17		16 17·5							
	l_2			40	16		16 26·2							l_2*
	m_1			34	20		17 23·5			16 55·6	16 56·5	+1′	16 56·5	
	m_2			33	21	119	17 17·7							
	m_1*	Sun .		32	22		17 19·7	F						
	m_2*		W	31	23		17 16 2		17 26·2					
	n			44	41		17 37 2							
	o			39	46	94	17 33	G						
	*o**	Jupiter		30	55		17 29							
	*o**			28	57		17 35 5							
Okamabuti	*p*			25	40		17 58·7							*r**
	q	Sun .	E	31	33	109	18 3·5	H	18					
	r			34	28		18 1·7							
	s			38	33		17 55·2			18 25	18 20	–5	18 20	
	t_1			26	39		18 44·5							
	2	Sun .	W.	25	40		18 53·7	I	18 50					
	t_1			24	41	98	18 52·2							
	u_2			23	42		18 49							
Occultation, 57 m. W. of Elephant Fountain.	..	..	..	..	..	..	..	..	..	18 7·7	18 2	–5·7	18 2	

* It is evident that the mean of these E. and W. observations cannot be expected to give the true longitude of Elephant Fountain; because the circumstances under which they were severally taken, both as regards the altitudes of the bodies and their distances, in no way match together. If we choose we can reject them entirely, and trust only to the accordance of the calculated result of the occultation with that obtained by triangulating, to corroborate the correctness of the latter method as regards the positions of places in *this* part of the journey. Or else, as the observations C and D were taken under very similar circumstances to the whole of those at Schmelen's Hope and Okamabuti, we can find the error which, whatever its causes may be, was found in practice to affect the results of those observations; and then, by applying this error to the longitude as obtained from C and D, we ought to obtain a much more trustworthy approximation than before to the true longitude of Elephant Fountain. Thus—

	Okamabuti.	Schmelen's Hope.
East Observations	° ′ 18 0	° ′ 16 25
West ,, ...	18 50	17 26
Difference	50	1 1
Error	25	30.5

or, 28′ = the mean error. Now, the mean of C and D is 18° 31′·5; and this + 28′= 18° 59′·5, which differs only 0′·5 from 18° 59′, which was the longitude obtained by triangulation from Walfisch Bay.

LUNARS taken with a small Circle.—Calculations by Mr. BURDWOOD, Hydr. Off.

—	Distinguishing Letter in Calculations.	Body observed.	E. or W. of Moon.	Proximate Altitude of Sun or Star.	Proximate Altitude of Moon.	Proximate Distance.	Longitudes deduced.	Means.	Longitude by Means of E. and W. Distances.	Sets not used.
				°	°	°	° ′ ″	° ′ ″	° ′	
T'ounobis . . .	*a*	Sun .	..	14	58	107	20 51·2			*b*
	d	Antares	W.	18	68	51	21 7			*c*
	e						21 7·5	21 5·3		*h*
	f	Sun .		46	16	117	21 9			*j*
	g						21 12		21 1	
	k						21 2·2			*l*
	m						20 55·7			
	n	Saturn	E.	24	68	45	20 55·5	20 57		
	o						21			
	p						20 52·5			
Ondonga, Nangoro's Werft.	*t*	Regulus	W.	38	72	46	16 17·5	..	16 14	*q* *r*
	u	Antares	E.	44	72	54	16 10·5			*s*

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f. 16

Duplicate

RECENT EXPEDITION

INTO THE

INTERIOR OF SOUTH-WESTERN AFRICA.

By FRANCIS GALTON, Esq., F.R.G.S.

[*Read before the Royal Geographical Society of London on February 23 and April 26, 1852.*]

LONDON: PRINTED BY W. CLOWES AND SONS, STAMFORD STREET.

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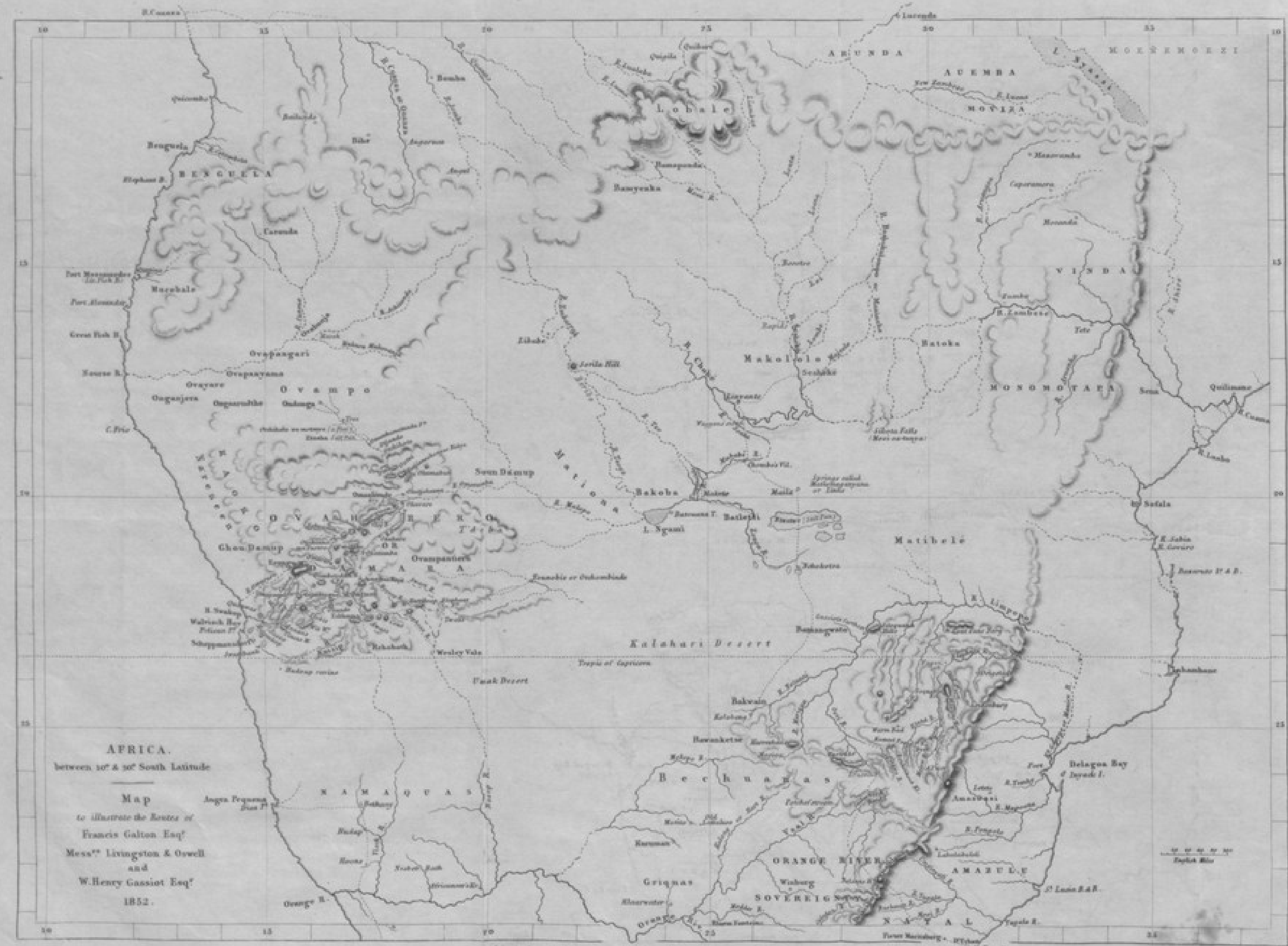

AFRICA.
between 10° & 30° South Latitude
Map
to illustrate the Routes of
Francis Galton Esq.
Mess.rs Livingston & Oswell
and
W. Henry Gassiot Esq.
1852.
BENGUELA
Benguela
Lobale
ARUNDA
AUEMBA
MOVIZA
L. Nyassa
VINDA
Makololo
Batoka
MONOMOTAPA
Ovampo
Mationa
Bakoba
L. Ngami
Matibele
Kalahari Desert
Tropic of Capricorn
Bechuanas
NAMAQUAS
ORANGE RIVER
SOVEREIGNTY
NATAL
AMAZULU
Griquas
Delagoa Bay
Port Natal
Sofala
Quilimane
Angra Pequena
Walvisch Bay
Zambese
Limpopo

RECENT EXPEDITION

INTO THE

INTERIOR OF SOUTH-WESTERN AFRICA.

Mr. President,

A little more than two years ago, urged by an excessive fondness for a wild life, I determined to travel for a second time in Africa. I then became a Fellow of your Society, and through the active kindness of Dr. Shaw your Secretary, of Mr. Arrowsmith, and of others, I was thoroughly advised as to those geographical points which more immediately awaited inquiry, and, guided by their views, chose South Africa as the field of my travels.

I left England in April 1850, accompanied by Mr. Andersson, a Swede, to whose most active and cheerful co-operation throughout a tedious and harassing journey, I am in the greatest degree indebted. He still remains in Africa, principally with a view of investigating the natural history of the lake district, and of thence bringing home a complete collection of specimens.

At the Cape, upon the strong recommendation of Sir Harry Smith, I freighted a vessel for Walfisch Bay, instead of travelling the usual route from Port Elizabeth. The emigrant Boers had at that time assumed a menacing attitude, and it was currently believed that they intended taking immediate possession of the lake country, and of refusing passage to all travellers from the Cape. Two parties had already been turned back; and as on the one hand there was every reason to believe that the same course might be adopted towards me, and cause a fruitless result to my journey, so on the other, the country to the north of Walfisch Bay was an entirely open field for exploring, and I proceeded thence.

At Cape Town I could obtain but little information about even those parts, in which missionaries had already formed stations, and what I there learnt was also much exaggerated, as the country was believed to be extremely fertile and very populous.

The Damaras, into the heart of whose country no white man had ever penetrated, were described as a most powerful, numerous, and interesting nation; and the fact that some traders had settled at Walfisch Bay, whence large droves of Damara cattle were dispatched south, shipped to St. Helena, or sold to the, at one time, numerous guano and whaling vessels, seemed to warrant the opinion of the fertility of the country. This view was again confirmed by the great jealousy shown to the attempted expedition of our late member, Mr. Ruxton, afterwards so well known by his travels in America, who, when he landed, experienced such determined opposition and obstructions, that he was compelled to set sail without having penetrated more than 20 miles into the country.

Warned by his failure I took mules with me, besides my waggons and a cart, in order that I should be, to a certain degree, independent of assistance, and be able, at least, to carry my things across the barren desert, which intervenes between the coast and the more habitable parts.

I was also requested by Sir Harry Smith to establish, if possible, friendly relations on the part of the Colonial government with such tribes as were liable to be exposed to the attack of the emigrant Boers, and to disavow strongly all sympathy on its part with them. Indeed a mere expression of good will, without holding out the least prospect of direct aid, is a custom much valued and well understood by South African tribes generally.

I landed in Walfisch Bay, the estuary of the Kuisip, in August, and was very hospitably received by Mr. Bam, the Rhenish missionary. Some time and great trouble were required to drag all my heavy things with my few mules across the sandy desert, already mentioned, to his station, where they could remain in security whilst I went up the country to buy and to break in oxen for my onward journey. It would be out of place here to allude particularly to the extreme difficulty I experienced before all this could be done and I could make my final start in February from Barmen. The country was in the utmost confusion; the Namaquas were robbing and murdering the Damaras in every direction, and had indeed, just on my landing, attacked and destroyed the Schmelen's Hope Station, that of Mr. Kolbe. I was fortunate in having good interpreters, and a black servant whom I had taken from the Cape, and who was born in some central part of Africa, found the language so much like his own, that by the time all was ready for a start he had become quite fluent in it; besides whom, I had a man, Damara born, but bred among the Hottentots, and to communicate with him, an excellent Dutch and Hottentot interpreter; so that I got on from the first extremely well.

The knowledge that Europeans possess of these parts, dates originally from Sir James Alexander, who, chiefly in company with Swartboys (a Hottentot chief), travelled from the south to Bethany, Rehoboth, and thence to Walfisch Bay; on his return he wrote to two missionary societies, recommending this country as a favourable field for missionary enterprise, in consequence of which some German Protestants and English Wesleyans were sent there. Their head-quarters were at and near Eikhams, the present home of Jonker Africaner, who was at first a warm supporter, but latterly a bitter opponent of them. Subsequently the Wesleyans extended their missions towards the interior, and the Germans along the coast. Their present stations are marked on my map. When I arrived no European foot had ever penetrated 20 miles to the northward of the 22nd parallel of latitude, or 20 miles to the eastward of Elephant Fountain.*

My course from Barmen, as marked on my map, led me through the middle and most populous part of Damara land, at the further frontier of which one of my waggons broke down. I thence rode on across a bushman tract to the Ovampo, and returning, joined my then mended waggon, and came back to Barmen, which I reached in August 1851. I thence sent a messenger overland to the Cape to make arrangements for forwarding a vessel to meet me at Walfisch Bay in December or January, and lightening one waggon as far as I could, went with all my available oxen on a quick journey eastwards. At Elephant Fountain I joined Amiral, a Hottentot chief, and leaving my waggon there, rode on with him and explored to Otchombindé (called by the Hottentots T'ounobis), and thence returning, arrived with utterly exhausted oxen on the coast in December 1851. From Walfisch Bay I sailed to St. Helena, where I waited some weeks in vain for an opportunity of making a short excursion to Little Fish Bay and of obtaining some information from thence. I consequently set sail, and arrived in England at the end of last month, after exactly two years' absence.

To avoid misconception I must give some explanation concerning the names which I have placed on my map, in the selection of which I had some difficulty. In all the border country, and where the missionary stations now exist, most places are known by two, three, or even four names. The Damaras have one, the Hottentots another—this latter, translated into Dutch, forms a third, which is used very generally—and the missionaries add a

* In Sir J. Alexander's, and in the missionary maps, the positions of the more distant parts explored by them are laid down very erroneously. In one map Elephant Fountain is placed one hundred miles too far towards the interior.

fourth; thus the place marked Scheppmansdorf, which is called Aban·hous in Hottentot, is always known as Roëbank by the traders and as Scheppmansdorf by the missionaries. It would have created great confusion to have attached all these different names to each place on the map, and I have therefore adopted the missionary names for their own stations, Damara names for all places that have them, and used Hottentot words as little as possible, for no orthography can possibly express their sound, except in rare instances, such as T'was and T'ounobis, which are capable of being pronounced. With perhaps less reason I have adhered to the Dutch word "Damara" to express the Ovaherero and Ovampantieru tribes, as it is a convenient name and one that has been long established, and which has as much right to pass current as the word "Caffre." The Hottentot name for that people is Damup in the plural, or Daman in the singular, and this is the root of the name "Damara," which it is needless to state is utterly unknown to the people themselves.

The country over which I travelled, proved to be the broadly developed end of that chain of hills and high land which runs parallel and near to the western coast from the Cape colony upwards, and separates the Fish River from the sea. Though this country is dotted over with hills and even groups of hills, and is very deeply scored on its western face with watercourses, yet in its general aspect it consists simply of a plain sloping steadily away on all sides from a small district of the greatest elevation, which is situated about the sites of the mountains Omatako, Diambotodthu, and thence to Awass, and which (from boiling-water observation) lies some 6000 feet above the sea-level. From this district, the watershed *eastwards* falls with a very gentle inclination to the cup-shaped basin of Central South Africa—to its lake, its flooded lands, and interlacing rivers; *northwards*, with still less incline, to a large river, of which the Cunene is a tributary, and which appears partly to drain that basin; *southwards* from Awass, Fish River begins its long and peculiar course towards the colony; and the comparatively steep *western* slope is ploughed up by the Kuisip, the Swakop, and five other more northerly river courses, which run into the Atlantic.

The sea-face of this broad belt is, except along the watercourses, uninhabitable, as during half the year there is no water and scarcely any pasturage. A strip of desert sand, 40 miles wide, follows the coast line, beyond which lies, north of Walfisch Bay, the barren Kaoko, and to the south of it the arid Namaqua land. The summit of the belt is a dense impracticable thorn coppice, though affording grass and a few scanty springs; but as we descend westward, and at about 220 miles from the coast, the thorns almost cease, and the land assumes the appearance of those broad plains, covered with grass and timber-trees, that have so often been described as lying between the Orange River and the Limpopo. Again, in the far north, at the latitude of Ondonga, the country becomes one of most striking and peculiar fertility.

Over all these parts the rains are periodical, and, from the nine years' experience that the Rev. Mr. Hahn has had at Eikhams and at Barmen, very variable. From the middle of May to November rain is scarcely ever known to fall, thence to January occasional and sometimes very heavy showers occur, but the true rainy season may be considered to be between the first of January and the last of April; the showers are extremely violent, but partial, and are always accompanied by thunder. The ground is seldom saturated till February, and then pools of rain-water (Vleys) are to be found everywhere; but, by June, all but the largest of these are dried up again. As a general rule, the rains fall most heavily on the summit, and on the northern and eastern slopes of the country, and, at Ondonga, they were described as being much heavier than in Damara land. The rivers are all *periodical*, and run to very different extents in different years. The Kuisip had been seven years without reaching the sea, and then almost, if not quite, reached it three times in six years. Of late, the Swakop has flowed three or four times every rainy season; yet, when it was first seen by Europeans, about ten years ago, the whole of the lower part of its course was choked with sandhills, bushes, and trees; these the first inundation swept entirely away, since which most violent torrents have passed down it. On the other hand, it was a constant complaint of the Damaras, that less rain falls now in their country than some twenty or thirty years back; and even their extensive migration from the Kaoko, to which I shall have occasion hereafter to refer, has been ascribed by the Damaras to the water failing them for their cattle.

It may, perhaps, give a more accurate notion of the country I visited if I describe in some detail a route through it. Leaving the excellent, but perfectly desert, harbour of Walfisch Bay—a journey of 16 hours across sand, soft and sinking at first and covered with shifting dunes, but afterwards hard and pebbly and crackling like frozen snow under the feet, takes us to Oosop. Ten miles before reaching the river, the plain shelves steadily and rapidly downwards to its bed, to which we descend at last through an imposing gorge about 300 feet wide and 4 miles long. The river-bed here is 100 yards broad, and consists of heavy sand, overgrown with patches of grass, and fringed on either side with a dense row of high reeds, beyond which, where the rocks leave sufficient space, are some fine groups of Unna trees. From the middle of the bed, a small streamlet springs out in all but the very driest seasons of the year, and after running some distance loses itself again in the sand. Notwithstanding a general appearance of drought, marks of violent torrents are everywhere visible,—trees lie uprooted, heaps of dead sticks and reeds and mud are washed high on the ledges of the rocks where they confine the river-bed, and also on the lower branches of the trees that still remain standing. The cliffs on each side are precipitous and magnificent. From Oosop to Davieep there is, perhaps, no one single place where an expert mountain-climber could get out of the bed on the north or right bank of the river, and only two places where cattle can be driven up from the left bank. The rocks are so bold and so broken, especially on the right bank, that a traveller can hardly realise the idea that they are not independent mountains, but only the face of a deep cutting which the river has made for itself, and that the general level of the country is from 800 to 1000 feet above his head. I ascertained this elevation as well as I could, by climbing up a hill on the left bank, the height of which I measured carefully to be a little more than 600 feet; then from the top of it I levelled across to the opposite cliffs, from the top of which the plain began, and, with my sextant, guessed at the remaining height. The plain north of the Swakop appears at this place to be quite level and barren, but not sandy; and it is almost, if not quite, uninhabited. I had good views of it from many different heights. The Canna river cuts its way through it in exactly the same manner as the Swakop, though its cliffs are described as being even higher. I could trace its course for a distance, from a hill near Hycomkap; and an appearance along the plain, as if the ground were broken up, indicated, for 20 or 30 miles, the gorge through which it ran. On leaving Davieep, the cutting through which the river flows loses the character of a gorge, and the mountainous sides open out more, continuing still to bank up the plains. Those of Onanis, 20 miles east from the left bank, give excellent pasturage, and the desert sand ceases about Tincas. Passing up the Tsobis river to avoid the deep sand of the Swakop, after 7 hours' up-hill travelling, through gorges nearly as striking as those of Oosop, we emerge on the plain, which we now find everywhere covered with thin grass, and studded over with stunted thorn trees. The "Hakis," or Fish-hook thorn, as the Dutch call it, begins to grow at Tsobis, but the land more to the westward is too barren to give sustenance even to that, and from this point to the borders of Ovampo-land the traveller has to tear his way through its cruel and tangled branches. Not a tree grows that does not bear thorns, and very few in which the thorns are not hooked; and their sharpness and strength are such as to throw a most serious difficulty in the way of exploring, especially as when travelling with a waggon, the oxen will not face them, and in difficult parts it is often quite impossible to get through the bushes, round to the struggling and fighting oxen. Cruel as the Hakis thorn is, there is yet another and much more severe opponent in a smaller, but sharper and stronger thorn. I have frequently tried the strength of all these with a spring weighing machine, by tying a loop of string to one end of it, which I hooked round the thorn, and then steadily pulled at the other end till the thorn gave way, marking the number of pounds resistance that the scale indicated at the moment the thorn broke; the Hakis thorn stood a pull of 4 or 5 lbs., and the other one of about 7, but often much more, and on one occasion I registered a strain of 24 lbs. Now, as several of these thorns generally lay hold of the traveller's clothes or person at once, it may easily be conceived what cruel laceration they cause. Continuing our route we descend to the Swakop again, near Otjimbingue, having, when at Tsobis, just caught sight of Erongo, a mountain 3000 feet above its base, but rising from the deep hollow of the Canna. From Otjimbingue to Barmen the river passes again through a broken, confused series of gorges, and among mountains; and it is not until we are far past Schmelen's Hope that we arrive at the source of the Swakop, and entirely clear of its valley. At the time of my visit to these countries, Schmelen's Hope, and a very few miles north of it, was the furthest point known to the missionaries, and other Europeans. As I travelled northwards, ascending the plateau, I saw the tops of the hills by the river, that had appeared so prominent when among them, slowly sink down below my level, and disappear among the trees. Diambotodthu no longer bounded the prospect in front, but on a sudden the two magnificent, almost faultless cones of Omatako burst full into sight, each appearing like a Teneriffe, beyond which was the broken ground of Otjihinna ma Parero, and the long wall of Koniati, that bound the arid Kaoko. I had but just left a tributary of the Swakop, still a broad river bed, when, to my surprise, I came upon another water-course of considerable size, running N.E., which I followed some distance, and which I found went towards the Omoramba. It seemed incredible that a water-course 30 or 40 yards broad, with steep banks, could have an origin in the open plain within a mile, but I found afterwards that this sudden commencement of broad river beds was the rule, and not the exception, in Damaraland. I had also constantly noticed that the breadth of the river beds was often out of all proportion to the quantity of water that they could ever carry: thus the Erora, which has not a course of more than 20 miles and is by no means an important drain to the country, is about 500 yards across, but I found that the same cause influenced both the length and the breadth of the river bed. It must be recollected that the ground is entirely sand, but well fixed on its surface by the long running roots of the grass that covers it. The wet in the rainy season drains through the sand into the river bed, and, of course, constantly washes away some with it; but the subsoil yields before the surface, and thus the banks get gradually undermined, and are always falling in, so that the river has a constant tendency to grow broader, and to push its apparent source higher up towards the water-shed. It is very curious to see the head of one of these river courses, where the ground seems to have fallen in suddenly, leaving a place like a gravel pit, whence the bed begins at once some 12 or 20 yards wide, and perhaps 10 feet deep.

In the case of the Omoramba K'omatako, whose course lies alternately over districts of sand and over hard ground, it is very curious to observe how, what in the first case is a fine magnificent river bed with high banks, suddenly, as the ground becomes hard, loses itself in the open plain, where there is not a vestige of its course; and a few miles further on, the ground becoming sandy, the river bed re-appears again, just as unexpectedly as it had been lost, and altogether as large as before.

I had made a considerable détour to avoid a very hostile tribe of Damaras, who were then encamped on the Omoramba, and through whose neighbourhood my men refused to attempt a passage. I, therefore, guided only by such vague information as I could then occasionally procure from the savages, went under the escarped sides of Omuvereoom, at the termination of which the reported lake Omanbondé was said to lie. Through the whole of this road I had to trust to chance in finding water, and in also finding a practicable road for waggons. At this time my men were undisciplined, and in no way to be depended upon. My oxen were only half broken. There was fighting going on between two powerful tribes immediately behind us, and a dense jungle of thorns surrounded us on all sides. Of game there was none, so that it was impossible to depend on anything else but my oxen for food. The waters were drying up on all sides, and the prospects of the expedition became gloomy enough. I chanced, however, to fall upon a curious watercourse, that we named the River Vley. It was a narrow strip of green, not much depressed below the country on either side, which contained frequent shallow pools. It was simply a succession of Vleys or pools, and varied from thirty yards to much more in width, and here

f. 22r

and there stretched out into broad plains; the thickest of thorn jungle, and one perfectly impassable to a waggon, pressed close upon it, so closely that the waggons were frequently taken through the water where there was not room for them between the Vleys and the thorns.

It is wonderful how little inhabitable country there is in this part of Africa. Either the thorns occupy the ground to the exclusion of everything else, or drought makes it unfit for cattle. In fact, Damara-land is made by the watercourses and a very limited number of springs; take away these, and no pastoral people could inhabit the country; whilst agriculture, except to the most limited extent, is in all cases out of the question. The watercourses, though utterly arid to all appearance, are really to a great extent reservoirs of water, which is checked in its evaporation by the great depth of sand that overlies it. There are places known to the natives in most of these river beds, and which probably correspond to the lowest parts of the longer reaches, where water can be got by digging; but it is useless, as I am well aware by long experience, to dig deeper than the sand, for no water exists in the hard ground below it. It must not, therefore, be in any way supposed that, because I have dotted out in the map a large space and called it Damara-land, the whole of that area is occupied by these people. The case is far different. The number of pasturages is extremely small; and I am sure that I myself have seen and know quite half of them. It will be easily understood also that the boundaries of a people like the Damaras are exceedingly arbitrary. It rains perhaps heavily one season, and there is abundant Vley water at a distant place, where the pasturage is also good; the neighbouring tribes, of course, flock there, and spare the grass nearer their usual haunts and more certain waters until the dry season. The boundaries are not definite and natural, excepting so far as the long range of Omuvereoom is concerned; but they are decided to a certain degree by custom, and I have endeavoured, under this view only, to represent them on the map. One point bearing on this subject must not be forgotten—that two African tribes never live close up to a common frontier. They are always fighting and robbing, and therefore a broad border-land is essential; and in these border-lands, so far as I have seen, the Bushmen and other outcasts live. As regards the water that these get, it will easily be understood that many places are found which yield enough for a small family, but which would scarcely support two or three oxen. The water oozing slowly into a well from the damp sand surrounding its bottom, at the rate say of a gallon in a day, is a case often observed, and then the tracks of Bushmen are pretty sure to be seen about it also. But to proceed with my itinerary. Just where the Vley River began to be lost the bushes became more open, and Omuvereoom sloped down into the plain, and I here met with guides who took me straight to Omanbondé. This had been constantly described to me as a large lake. I thought it might have been the Demboa of early maps, with whose position it fairly coincides, and to whose name Omanbondé bears a certain resemblance.

The occasional existence of hippopotami in it was also thoroughly substantiated, and yet, when I saw it, it was *perfectly dry*. The place is a remarkable one enough, for it is the long reach of a watercourse, closed up at both ends by a dam, which, together with its sides, slope upwards till they attain an elevation of about 100 feet above the bed. The breadth of Omanbondé is trifling, but its length is some 8 miles. Going downwards, and passing over a broad dam, we come into another reach also dammed up, and then again into another. N. of Omanbondé there are also two other places just like it. The water evidently filters through these dams in the rainy season; and I was assured that, even in the driest times of usual years, Omanbondé was a reservoir of water: I, however, to my great misfortune in many ways, chanced to travel during a year of great and almost unprecedented drought. The course of the Omoramba downwards was so clogged with thorns as to be quite impracticable for a waggon, and it was indeed with great difficulty that we even crossed it.

On leaving Omanbondé, and getting out of the valley of the Omoramba as well as I could, we came to a far more open plain than any I had hitherto met with; and suddenly, at lat. 19° 50′, found ourselves among palms—the harbingers of a better land. A little further on they flourished to the exclusion of almost every other tree; but before we came to Okamabuti had ceased almost as suddenly as they began, though during the whole road to Ovampo-land one or two were every day to be seen. We had now arrived at a much more luxuriant country, and water was plentiful: a long limestone ridge at Kutjianashongué yielded springs in numerous places, around which large herds of cattle and numbers of Damaras were collected. Timber-trees began to appear, growing in clumps, with long open grassy savannahs between them; and to me it was a constant wonder to observe the straight and perfectly defined borders that these belts of wood presented. Here and elsewhere I have seen them look, not as one would conceive that Nature could have planted them, but presenting exactly the appearance of the work of an ornamental gardener. I am in no way able to account for this striking peculiarity, as there is no perceptible difference in the soil on which the trees grow, and in that where they are absent. I cannot explain the fact, but simply state it. Okamabuti may be considered as the northern boundary of Damara-land, though in the rainy season the natives sometimes go further. The country is said to be quite impassable to the N.E. It appears to be entirely uninhabited, and is thickly wooded. I made an excursion to a hill in that direction, about 8 hours off; but, so far as I could see from the top of it, one level forest extended far away.

The masses of hills that lie to the N.W. of Okamabuti are all limestone. I saw a good deal of them from the guide having lost his way more than once when he first took us there, which ended in compulsory and anxious wanderings for more than a week about them. A great many Bushmen live among these hills. I saw there a most curious freak of nature, which I afterwards witnessed on a far more magnificent scale at Otchikoto. Wherever a piece of bare rock is to be seen (which is nearly everywhere between Ootui and Otchikoto), it is pierced with holes perfectly circular, and of all sizes, and like round smoothed tubes. Thousands of them would just admit the thumb, and are quite shallow; numbers are about the diameter of a bucket, and from 3 to 5 feet deep, forming most dangerous pitfalls: in many of them we find trees growing, some not quite filling the hole, others just fitting it, and, again, others so constricted that the trunk swells over and entirely hides the sides of the hole. I saw a few holes about 8 feet across, but I do not recollect observing any intermediate size between that and Orujo, a perfectly circular hollow in the midst of chalk about 30 feet deep and 90 feet across. The sides of this were certainly not smooth, but they formed an exact circle, like a gigantic pan, the floor of which was level, with a small well in the middle. Otchikoto was still more astonishing. Equally circular, and its sides equally steep, it measured 400 feet across, and was almost filled with the clearest of water, the level of which stood at 33 feet below the banks, with the extraordinary depth of from 170 to 180 feet, which I plumbed in five places. I heard there was another, if not two more of these places, somewhere among the Soun Damup. The water-level of Otchikoto was, as I was told, and I could myself gather from appearances, not increased in the rainy season.

I was fortunately not encumbered here with my waggons, for I do not think it would have been possible to have taken them on through the thick forest. Here there is not a single landmark to catch the eye, and nothing but the most skilful tracking could find the road when the rain had obliterated the spoors of the preceding year. We got water at Otchando, and came to the first Ovampo cattle-post at Omutchamatunda. Travelling on, we arrived suddenly at the large salt-pan of Etosha, which is about

9 miles across from N. to S., and extends a long way to the W. The mirage was too strong to admit of my measuring the distance of the high banks that there bound it, and which I could just make out both as I went and as I returned.

This lake is impassable in the rainy season, but was perfectly dry when I saw it, and its surface was covered over in many parts with very good salt. A little further on we come to the remarkable Otchihako-wa Motenya, a perfectly flat, grassy, but treeless extent of country, stretching like an estuary between high and thickly wooded banks. It is said to extend a very considerable distance W.; indeed, I cannot help thinking even down to the sea-coast. I passed it near its head, where it was only 12 miles across; but where the Ondonga and Omaruru route crosses it, it is a long day's journey from side to side, and all the Damaras who had been that route assured me that it extended as far as they knew to the W. Again, the Omaruru and Onganjera route crosses a flat of three days' extent, but in which there is some water, and which is asserted, and indeed appears, to be identical with it. It is looked upon with great horror by the Damaras from the bitter coldness of a night passed upon it, as there is of course no fuel and no shelter.

It is difficult for me to express the delight that we all felt when in the evening of the next day we suddenly emerged out of the dense and thorny coppice in which we had so long been journeying, and the charming corn country of Ondonga lay stretched like a sea before us. The agricultural wealth of the land, so far exceeding our most sanguine expectations,—the beautifully grouped groves of palms,—the dense, magnificent, park-like trees,—the broad, level fields of corn interspersed with pasturage, and the orderly villages on every side, gave an appearance of diffused opulence and content, with which I know no other country that I could refer to for a parallel.

I arrived ultimately at Nangoro, the king's werft, where I spent three weeks most pleasantly. But my oxen had fallen lame and sadly out of condition, and I felt some misgivings as to whether they could even take me back, and there was no grass for them at Nangoro's to eat. All his cattle were sent far away to the cattle posts. Half my party were left scarcely in a fit state to protect themselves among the Damaras, and I had often anxious thoughts for their safety. My provisions were getting very low, and unless more cattle could be bought in Damara-land we had not sufficient to take us back even to Barmen, where we had left the missionaries in too great want to be able to help so large a party as ourselves. The country too was fast drying up, and the road southward might become impassable; still the *great river* was only four long, or five comparatively easy, days ahead; but this and the return journey, together with the rest necessary for my oxen, I was aware would be at least a three weeks affair, and I hardly knew what course to take in case Nangoro would give me permission to proceed. It was certainly with regret, yet still with a feeling of relief, as putting an end to my indecision, that a message was at length received from Nangoro, prohibiting me from proceeding farther. If I had been in a condition to temporize, I have no doubt that I could have persuaded him to let me proceed, but that was now out of the question, and I therefore took leave and returned. Fortune now favoured me in many ways. I found my waggon mended, a sufficiency of cattle bought, and obtaining a guide, returned by a good road up the Omoramba without much difficulty, except in having nearly every day to dig or to clear out wells, which fully employed my whole party, now consisting of thirty-four people.

Returning to our starting point, Barmen, I will next describe the route which I followed to the eastwards, and which is very interesting, as it presents a peculiarly easy and open highway to the interior, and one practicable at almost all times for waggons, though indeed I—travelling at the driest time of an unusually dry year, one in which many of the Damara cattle perished of thirst, even at their own cattle posts—failed in reaching Lake 'Ngami. Proceeding up a small, but frequently running, river-course, a tributary of the Swakop, to Eikhams, and thence by a well-made Hottentot waggon road, over a very broken and arid country, we ascend out of the valley, keeping the high ridge of Awass to the right hand. We are now upon an elevated open plain, presenting no difficulties whatever to waggons if we follow the course of the Kuyip, but the ground that borders the upper part of the Noosop, by which I went, is very rugged and thorny. There is far more water to be got all about here than in Damara-land, but this being at present the border country between the Hottentots and Damaras, the wells were not generally opened. From Kurrikoop eastwards no anxiety need be felt for food, as there is plenty of game, though the animals are exceedingly shy. The ground is sandy and undulating. Proceeding on, we get to Elephant Fountain, beyond which there are no peaked hills, nor landmarks, in fact, that could be laid down in the map, and thence recognised by a future traveller. Elephant Fountain had been a Wesleyan missionary station, but was abandoned for the double reason of being subject to fever from April to June, as well as from its vicinity to some warlike Damara tribes. There is nothing in the appearance of Elephant Fountain that would suggest an idea of unhealthiness; it possesses, indeed, no peculiar feature, but it stands well on a barren and thorny hill; the—here contracted—bed of the Swart River is below, and there is a small, clear spring, which supplies water. Most fearful attacks of fever have year after year been experienced at the place, but not, so far as I could learn, anywhere else in the immediate neighbourhood. At Elephant Fountain I left my waggon, and rode on with a Hottentot chief, Amiral, and about forty of his men, to the eastwards. They had lately explored a long limestone ridge of hills that extends some 50 miles from T'was, and which is greatly intersected by water-courses, headed by springs, and along which we went. It appears to be of about 20 miles breadth, and attains a height of at least 1000 feet above the general level of the country. I consider it quite as a natural boundary between the thorny country of Damara-land and the broad, sandy, and wooded tracts of Central Africa. I contrived to get Bushmen guides to take us and about half of Amiral's party to T'ounobis, which we reached after a journey most trying to the oxen. The road passed by many large but dried up vleys; the ground was sufficiently hard, and would at ordinary seasons afford an excellent road for waggons, which after leaving T'was should pass not on the top of the ridge, as I did, but skirt it in the plain. T'ounobis is a fountain in a river-course, sufficient to supply any quantity of cattle, where I remained a week recruiting my oxen, of which I had barely sufficient to carry me back. I found a large village of Bushmen there, from whom I received much information concerning the lake and the country ahead. The land in front, up to its very borders, was described as being exactly the same as that we had now traversed. Hard sand, with plenty of trees, but not so thickly overgrown as to form any obstacle to a waggon, and growing but very few thorns,—indeed, I had great difficulty in getting thorn-bushes, of which to make my sheep kraals. So far then as T'ounobis I can guarantee the road from Walfisch Bay towards the interior to be perfectly open at any season of the year, and, except in the driest of times, from T'ounobis onwards. T'ounobis was passed by the Kubabees Hottentots in 1850. They had come upwards along the Umak Desert on a plundering and shooting excursion, with horses and oxen in great numbers to ride on. They had also built shooting-huts by the waterside, which I used, and had left other tokens of their passage. At T'ounobis they obtained a guide, whom I saw, and from whom I received much information, and under his escort they reached the lake.

A perfectly marvellous quantity of game congregated here; deep pools of water that were supplied by a fountain were drunk dry every night, and I therefore more readily believed in the constant assertions of the Bushmen, that there was then no water whatever for a distance twice as great as that over which we had travelled ahead.

Having now described briefly the geography of those parts that I visited, I will next state what I learnt from various natives respecting the countries ahead of me.

At T'ounobis I received the fullest description from the Bushmen natives of a lake called by them Il' Annee, which they often visited, and which I now feel assured to be Lake 'Ngami. It had been reached thence from T'ounobis by the party of Kubabees Hottentots, that I mentioned above, in 7 days in August 1850, and the direction of its nearest point was uniformly stated to lie thence N. 50 E. true (N. 75 E. compass). The chiefs of the black tribes at that point were Maharaquè and Tworrathabè, names which are identified by Mr. Oswell as being those of the chiefs of the Maclumma on the S.W. of the lake. The distance represented by 7 days' journey in these parts would be pretty nearly 120 geographical miles measured straight, certainly between the wide limits of 100 and 140 miles.

I also heard much of a large river whose "lay" was N.N.W., and which joined the lake. This evidently is the Tso, but it was described to me as being called the Beribè (in Sichuana), and as the T'guain Tl' Obo (in Hottentot). As regards the course of this river I was assured that it ran out of, and not into, the lake, but my information was not such as to withstand the more immediate testimony of Mr. Oswell, corroborated strongly as it is by the general features of the country.

This river, the Beribè (or Tso), was stated to pass entirely through the country of the adjacent tribes, and far in a N.N.W. direction to the other side of them. A much smaller stream S. of the Beribè, and having the same general course, was described as joining it just where it met the lake. This last streamlet, the Malopo (in Hottentot the T'kains), on which there are no boats, is separated during its course from the Beribè by a range of hilly country, called by the Hottentots the T'dèba. The Omoramba, at a distance of about 90 miles easterly from Omanbondé, meets another dry river-bed, and the two together ultimately reach some large water, but which I do not think to be the Malopo. It appears to be stagnant or nearly so, but I received very contradictory information about it; the large river (the Beribè) is beyond it. The Omoramba at about 60 miles from Omanbondé passes through a very hilly country, which, as far as I could make out, was continuous with the T'dèba. I have mentioned that hippopotami have constantly made their appearance at Omanbondé when there was water there; this is a sure proof that the Omoramba cannot be entirely lost in the plain, but must join a large water, some such as I have mentioned. About the lower part of the Omoramba a peculiar race of negroes (the Soun Damup) live, and extend very far to the northward. I shall refer to them again later; they were described as living N. 15 E. true from T'ounobis.

My Ovampo information refers to a large river that runs from E. to W., and which is 4 quick or 5 easier days' march (say 100 miles due N.) from Nangoro's werft. It is a broad, swift-flowing stream, to the border of which Portuguese traders come and traffic. The ferry, which is chiefly used by the Ovampo, lies N. 19 E. by compass, or N. 7 W. true, from Nangoro's werft in Ondonga, and is near the junction of the two streams which principally form this river. One of these, the larger, comes from the very far E., the other from the S.E. and from the Mationa country; Mationa being the name given both by the Ovampo and Damaras to the tribes living on the Beribè, including those belonging to the chiefs whose names I have already mentioned. The Cunene was said to run into this river, but of its point of confluence I am not satisfied. Mr. Oswell informs me that he had always conceived an idea, from what the natives told him, that the Tso was in some way connected in the far N. with a large river running to the W. The Mationa river, mentioned above, may be this link. Where the embouchure of the Ovampo river may be I have no idea, but I have many reasons for thinking it not to be the Nourse. The captains of coasting-traders in those parts assured me that the Nourse is a periodical water-course, while I learnt from the same and other authorities that a constant river of considerable size, though small at its actual mouth, flows into Little Fish Bay (Mosammedes). There is now a thriving settlement there, where a Frenchman has long resided, who is said to make distant trading journeys into the interior. It would be very desirable for any officers of the slave squadron, or others who might land at that port, to make inquiries about the lower part of this stream, which must be perfectly well known there. The Ovampo told me that it seldom ran quite into the sea, but ended in a large deep pool close by the coast, beyond which the sand was dangerous to walk over, as it was a quicksand.

There is also a Portuguese trading station on the river opposite the country of the Onganjèra; this cannot be far from the coast, for the caravan from Damara-land to that nation leaves Omaruru and travels northwards for a long way over some very high land frequently in view of the sea. From the mouth of the river a kind of sea-shell, much prized, and called by the natives Ombou, is frequently brought. As regards the size of this river it is said to be such, that when a man calls across it his voice can be heard, but not his words. Opposite to the Ovampo it is extremely swift (boats cannot paddle up it) and very deep. It appears to be a most interesting river, and well worth exploring.

I can say nothing as regards its salubrity, except that Ovampo-land appeared a remarkably healthy country, and Damara-land I know is such. Corn land extends the whole way S. of it from Ovampo-land to very near the sea. Between the two confluents of the river the Ovabuntja live. Their country is described as very marshy, and many of their houses are built on poles: of course fever is to be dreaded there.

Ethnology.—I will now pass on to the distribution of tribes in this part of South Africa. Their history is not a little involved; but they may be enumerated thus:—1. The Ovampo are corn-growing tribes to the north, who, considered as blacks, are a highly civilized people, and one with strong local attachments, well ordered, honest, laborious, and neat, yet still with much of the negro in them. 2. The Damaras are a vagabond, lazy, thieving, pastoral race. 3. The Hottentots to the south are too well known to require further comment. 4. The Mationa Caffres to the east; and lastly, 5, the Bushman Hottentots and others, who lead a Bushman's life in the barren tracts, that separate these larger nations.

The Namaqua Hottentot is an invader of the last few years, but the Bushmen have not even a tradition of another home. Living with them are outcast Damaras, and also a very peculiar race of negroes speaking the Hottentot tongue, and that only. These have no traditions indicating their descent, and are found as far south as Bethamy. They live peculiarly on the hills, and have puzzled ethnologists ever since they were first described. They call themselves Ghou Damup, and in Sir James Alexander's work and in missionary publications, are described as the Damaras of the hills. With the Damaras, however, they have nothing in common. Their features, shape, customs, and aptitudes indicate an entirely different origin, and it will be seen that an enquiry into their earlier history throws great light upon the former state of this country. The Mationa are Bechuanas, among whom, partly as slaves and partly independent, live the Soun Damup, a tribe kindred to the Ghou Damup in every respect, language, appearance, and superstitions.

To make the matter clearer I will state the results of frequent enquiries from many independent sources, the agreement in which is very striking.

About 70 years ago (certainly between 65 and 75 years), and when, from uniform testimony, water was much more abundant than it is now, the Damaras lived in the Kaoko alone. The Ovampo were within their present frontier, but the Mationa extended to Ovampantieru-land, certainly far to the westward of Otchombindé, and all between these and down to the Orange River, lived Hottentots of various tribes. The Nareneen lived by the sea, and the Ounip (called by the Dutch Toppners) about the parts of which we are now speaking, and south of these were the Keikouka, now represented by the red people, by Swartboy, the Kubabees, and Blondel Swartz. Near to the Orange River the tribes were more numerous and more civilized, from their neighbourhood to the Dutch. They had a few guns, sometimes waggons and so forth, and these were the ancestors of Jonker, Amirals, Jan Boys, and other smaller tribes, as Buchess' and Fransman's. There was also a certain admixture of bastard blood in these last, who came to be designated Oerlams (a term of half reproach) by the Dutch, and to be disavowed by the Keikouka as partly aliens. Hence a jealousy arose, and still exists, between the two great divisions of the more southern Hottentots, the Keikouka and the Oerlams, who together are usually called in the aggregate "Namaquas," in contradistinction to the northerly tribes of Bushmen.

Interspersed among the Hottentots from the north to the south were the Ghou Damup, who were invariably considered as slaves and a good deal ill-used: they lived, when in communities, in the hills, or table-mountains, of which there are many, such as Omuvereoom, Konati, Ketjo, Erongo, and many others, of which I have often heard, more to the south and west. Two movements now began to take place; first the Damaras, pressed for room or for some other cause, made an irruption to the eastwards, and spread over the country as far as Otchombindé, almost exterminating the Hottentots in their way and driving back the Mationa, while the Ghou Damup were pretty safe in their mountain-fortresses and received but little harm. The Toppners, however, not being at that time accustomed to the mountain-passes with which the Ghou Damup were familiar, were, as I said, greatly cut off. And it is curious, that within very late times (about eight years ago), exactly the same thing occurred to the Nareneen living west of the Kaoko.

The more northerly Toppners were thus quite cut off from all communication with those about Walfisch Bay, and remain so to the present time. There exists, however, the greatest fondness for traditional stories among these people, and I found the liveliest interest expressed on my return from the north relative to the well-being of those Hottentots whom I met among the Ovampo, and of whom scanty information only had been received from time to time. In Sir James Alexander's work mention will be found of the Navees, or Nabees, as he spells it, on information received among the Hottentots. These are the Ovampo; Navees being the Hottentot name for them.

We have seen thus how the Damaras drove the Toppners to the same places as the Ghou Damup. Community of misfortune is gradually destroying the feeling of difference of race between them, so that intermarriage, which would have been quite unheard of in former years, is now becoming common. The Hottentots told me that 10 years ago it was quite unknown; and I have never seen any but children of the mixed race.

The Mationa made at various times reprisals on the Damaras; the last being about 20 years ago, when the Mationa came up the Epukiro River, while on a previous occasion they had passed up the Omoramba.

From the Damara invasion we now come to that of the Namaquas, which dates at a much later period, and in which Jonker Africaner played the principal part. Of all the particulars of this I have the fullest information; but I cannot expect that an interest which depends chiefly on persons and parties in South Africa, will be felt here; suffice it, therefore, to say, that by gradual encroachment the tribes, whose names you see here mentioned, strengthened and formed themselves, and plundered all before them. Sometimes they went on a professed national feeling to aid the Toppners, sometimes on none at all. In every case, however, the Toppners were thoroughly victimised; and it is only of late, when the Nareneen had obtained so many guns and so much ammunition from whalers and guano ships, that they acquired sufficient strength to be recognised as others than simply as Bushmen by the Namaquas.

The moment that I saw the Ovampo I was most strongly impressed with the national identity of the Ghou Damup; it is true that the latter are most squalid and thievish, very strikingly opposite characteristics to those of the Ovampo, but on the other hand we cannot forget that they must have been an outcast race for ages, to have so completely lost, not only their own language, but all traditions of it. They dig and plant, which neither the Hottentots nor the Damaras do; and on the other hand I was assured that the Soun Damup, who lived to the north, were the field labourers of the Mationa (the Hottentots call bread "soun" from them), and were exactly the same as the Ovampo, except in some trivial difference of dress, and that there, some spoke Ovampo, some Mationa, some Hottentot, and some all of these tongues.

I conclude, then, that the Ghou Damup were the real aborigines of the country S. of the Ovampo, that very long since the Hottentots invaded and entirely conquered them, and that they both together settled down into the condition in which I described them to be at the beginning of this account.

I may add that exactly the same process is now going on between the Namaquas and the Damaras, and probably one-half of the whole Damara population has already been enslaved or murdered by the Namaquas. Those that are made slaves are used as cattle-watchers; their children, as they grow up, learn Hottentot, and readily identify themselves with the habits of their masters, so that few generations will probably have passed before the Damara language will be obsolete among them, and they will have become a race affording an exact parallel to that of the Ghou Damup. The Namaquas are still pressing on with the peculiar restlessness and obstinacy of the race, a belief in their destiny, a scorn of blacks, and a fondness for plunder, which has already led them from the Orange river, and which now seems to be more marked than ever. As unarmed savages can never resist their guns, which number between 3000 and 4000, my belief is that not many years will have elapsed before they will have utterly destroyed the Damaras, and will come into direct conflict both with the Ovampo and the Mationa.

On the habits of the Damaras I have little to say. Physically speaking they are a striking race, with an appearance of strength, lightness, and daring that is highly imposing. They are tall, upright, and often remarkably handsome men, models for sculptors. They have a fair facial angle of about 70°; fine, manly, open countenances, and often beautifully chiselled features; but morally they are the most worthless, thieving, and murderous of vagabonds, and at the least irritation their usually placid countenance changes into one of the most diabolical expression. Much struck as I was with them at first, I came ultimately to the conclusion that, except their general good humour, there was not a single good point in their character. Their very personal strength is wonderfully small considering their immense muscular development. Often as I have had trials in lifting weights and so forth among them, I never found one who was anything like a match for the average of my own men. Idea of a Supreme Being they have none; but ceremonies and superstitions innumerable; none of which have anything poetical in their character. They are chiefly shown in smearing with fat and with cow-dung and in abstaining from eating cattle with certain marks, different according to the family they descend from: of the fetisch superstition there is no trace. A tree is supposed to be the universal progenitor, two of which divide the honour, one at Omaruru, the other on the road to the Ovampo. All the men are circumcised.

They have no government; any man with 20 cows calls himself an independent captain. They are devoid of all national or social ties to a perfectly marvellous degree. If one werft is plundered, the adjacent ones rarely rise to defend it, and thus the Namaquas have destroyed or enslaved piecemeal about one-half of the whole Damara population. As to the language, a very complete grammar and dictionary has been compiled by the

Rhenish Missionaries, and sent last year to the Professor of Philology at Bonn, who will, I believe, shortly publish it

Very different from these in every respect are the Ovampo, who are orderly, centralised, hard-working, neat, and scrupulously honest. Ondonga is plotted out into small, well-farmed holdings of corn and pasturage, each occupied by a family, generally the grandfather, son, and children. Every one here has the appearance of plenty, and none of the squalid, wretched, uncared-for, old people, so painfully common among the Damaras, are to be found amongst them. The King, Nangoro, is despotic, and seems to rule with a patriarchal sway. Laws against theft are peculiarly severe. The tribute to the King is small, and paid by a per centage on the tobacco grown, and *not on the corn.* The marriage tie is extremely lax. The Ovampo possess the entire carrying trade between the Damaras and the Portuguese.

My map is for all practical purposes, and so far as it professes to go, very fairly accurate. I am not aware that any isolated hill is left out, though I do not profess to give the peaks in each group. I should have been involved in endless confusion, had I attempted so much. The limits however of all hills and all groups of hills are taken. I triangulated chiefly with an azimuth compass, from Walfisch Bay onwards as far as there were mountains to triangulate by, that is to Otchikoto on the N., and to Elephant Fountain on the E. I have so great a number of bearings, that I have had no difficulty in making many independent series of triangles, and checking one by the other. I thus pretty easily found out such errors, either of reading off observations, of mistaking hills, or of writing them wrongly down, which I saw in spite of all my care would occasionally occur. I then selected the series of triangles that I thought would give the most trustworthy result, guided by the size of the angles, and more particularly by the definiteness of the mountain peaks that I observed, and then protracted them. Having done this, and registered the longitudes which this triangulation gave for my three main stations, Barmen, Okamabuti, and Elephant Fountain (assuming the longitude of Pelican Point, Walfisch Bay, at 14° 27′ 5″, and determining the scale of the map by differences of latitude), I compared these longitudes with those deduced astronomically, and I am glad to say that the agreement is very satisfactory. There is an abstract of all this at the end of the paper. Some grave error had affected my instrument, so that although the observations in each group agree extremely well together, yet there is a wide difference between the longitudes derived from these several groups. I had, however, done my best when taking lunars, say E., to take others W. under as nearly as possible the same circumstances, both of altitudes and of distances, as I could, and of the same bodies also. With sun observations I used one coloured glass, and always the same one, toning the instrument of course as required. I also examined the adjustments of my sextant with all the care I could, previously to beginning to observe; and it is solely from having taken these precautions with great pains that I can account for the excellent agreement of the mean longitudes (deduced as they are from such wide extremes) with that obtained by triangulation. As regards T'ounobis and Ondonga the lunars were taken with a good, though small, and not clearly divided circle, which had to be read off by firelight; still the results of the former are very fair, and those of the latter, being checked by the position of Otchikoto, will answer sufficiently well. I am thus particular upon these matters, as it is of course a satisfactory thing to have well determined the geography of a new country, even though only in outline, for it may save much trouble and doubts to future travellers. I have altogether determined astronomically the longitudes of 6, and the latitudes of 53 stations, and I had no object in taking more.

Table of Latitudes observed.

Places of Observations.	The Number of Bodies observed. N.	S.	Latitudes. °	′	″
Sand Fountain	1	1	22	57	57
Scheppmansdorf					
Hycomkap	2	..	22	41	45
Oosop	3	2	22	45	38
Davieep	1	..	22	48	0
Annaas	1	..	22	43	0
Mouth of Tsobis River	1	..	22	26	3
Tsobis	1	..	22	30	55
Kurrikoop (on Swakop)	2	1	22	23	2
Otjimbinguè	2	..	22	21	50
Barmen	2	1	22	7	7
Schmelen's Hope	1	1	21	59	34
Okandu	1	..	21	56	30
Kutjiamakompè	2	..			
Okanjoè	1	..	21	27	9
On leaving the Omoramba	1	..	21	13	29
Otjikururumè	1	1	21	5	54
Ontekeremba	1	..	21	1	49
Ozukaro	1	..	20	49	57
Near Ja Kabaca	3	..	20	38	50
Otjironjuba	1	..	20	32	25
July 16th, Omoramba	1	1	20	29	45
April 1st, Werft	2	..	20	22	15
July 14th, Omoramba	1	..	20	18	43
Okavarè	2	1	20	6	37
Omanbondè	1	1	20	2	30
On Flat, April 12th	..	1	20	1	5
Okatjokeama S. Vley	1	2	19	57	15
,, N. Vley	1	2	19	55	35
Okapukua	..	1	19	47	4
Okamabuti	many		19	30	48
Omutirakanè	1	1	19	25	50
At foot of hills, April 30th	1	..	19	20	13
Otchikoto	..	2	19	10	0
Otjando	..	1	18	58	15
Small well	..	1	18	54	37
Omutchamatunda	..	2	18	47	32
South border of Flat	..	1	18	31	51
Two miles within Ondonga	1	..	18	6	41
Nangoro's Werft	..	1	17	58	40
			°	″	′
Barmen Cattle-post	1	..	22	15	55
Due E. of High Peak	2	1	22	14	36
Katjimasha's Kraal	1	..	22	26	0
Eikhams	3	..	22	34	40
On plain, Sept. 3rd	1	..	22	27	4
On Noosop R., Sept. 5th	1	..	22	16	11
Elephant Fountain	2	..	22	27	15
T'was	2	..	22	36	18
Kurrikoop (on Noosop)	1	..	22	25	21
Occultation-place	..	1	22	27	35
Near Okomavaka	1	1	22	15	20
On road to T'ounobis	1	1	22	7	35
T'ounobis	2	1	21	54	40

Lunars taken with Sextant.—Calculations by Mr. Burdwood, Hydr. Off.

—	Distinguishing Letter in Calculations.	Body observed.	East or West of Moon.	Proximate Altitude of Sun or Star.	Proximate Altitude of Moon.	Proximate Distance.	Longitudes deduced.	Distinguishing Letter of Groups.	Means.	Longitude by means of East and West.	Longitude by Triangulation alone, using 14° 27′ 5″, as longitude of Pelican Point.	Difference.	Longitude used in Map.	Sets omitted as being evidently bad ones.
						°	° ′		° ′	° ′	° ′	′	° ′	
Elephant Fountain.	*a*	Saturn	W.	55	42	19	19 13·2	A						*a**
	*a***			57	46		19 21·7							
	b			54	52	20	19 17·2	B	19 16·2					*b**
	c			51	53		19 16·7							
	*c**			51	52		19 15·5							
	*c**			50	52		19 12·7			*18 53.7	18 – 59	+5·2	18 59	
	d_1	Sun	E.	25	28	106	18 26·5	C		but if calculated according to method mentioned in the note we have				
	d_2			26	28		18 30·2							
	e			29	26		18 30·2							
	f			32	23		18 36·5		18 31·5	18 59.5	18 59	–′·5	18 59	
	g			27	34	93	18 31·2	D						
	h			30	32		18 30·2							
	i			46	19		18 32							
	j			48	17		18 36·5							
Schmelen's Hope.	k_1	Sun	E.	37	20	122	16 28	E	16 25					
	k_2			38	19		16 27·7							
	l_1			40	17		16 17·5							l_1*
	l_2			40	16		16 26·2							l_2*
	m_1	Sun	W	34	20	119	17 23·5	F		16 53·6	16 56·5	+1′	16 56·5	
	m_2			33	21		17 17·7							
	m_1*			32	22		17 19·7							
	m_2*			31	23		17 16 2		17 26·2					
	n	Jupiter		44	41	94	17 37 2	G						
	o			39	46		17 33							
	*o**			30	55		17 29							
	*o**			28	57		17 35 5							
Okamabuti	*p*	Sun	E	25	40	109	17 58·7	H	18					*s**
	q			31	33		18 3·5							
	r			34	28		18 1·7							
	s			38	33		17 55·2			18 25	18 20	–5	18 20	
	t_1	Sun	W.	26	39	98	18 44·5	I	18 50					
	2			25	40		18 53·7							
	t_1			24	41		18 52·2							
	u_2			23	42		18 49							
Occultation, 57 m. W. of Elephant Fountain.	..	..	..	..	..	..	..	..	..	18 7·7	18 2	–5′7	18 2	

* It is evident that the mean of these E. and W. observations cannot be expected to give the true longitude of Elephant Fountain; because the circumstances under which they were severally taken, both as regards the altitudes of the bodies and their distances, in no way match together. If we choose we can reject them entirely, and trust only to the accordance of the calculated result of the occultation with that obtained by triangulating, to corroborate the correctness of the latter method as regards the positions of places in *this* part of the journey. Or else, as the observations C and D were taken under very similar circumstances to the whole of those at Schmelen's Hope and Okamabuti, we can find the error which, whatever its causes may be, was found in practice to affect the results of those observations; and then, by applying this error to the longitude as obtained from C and D, we ought to obtain a much more trustworthy approximation than before to the true longitude of Elephant Fountain. Thus—

	Okamabuti.		Schmelen's Hope.	
	°	′	°	′
East Observations	18	0	16	25
West ,, ...	18	50	17	26
Difference		50	1	1
Error		25		30.5

or, 28′ = the mean error. Now, the mean of C and D is 18° 31′·5; and this + 28′ = 18° 59′·5, which differs only 0′·5 from 18° 59′, which was the longitude obtained by triangulation from Walfisch Bay.

Lunars taken with a small Circle.—Calculations by Mr. Burdwood, Hydr. Off.

—	Distinguishing Letter in Calculations.	Body observed.	E. or W. of Moon.	Proximate Altitude of Sun or Star.	Proximate Altitude of Moon.	Proximate Distance.	Longitudes deduced.	Means.	Longitude by Means of E. and W. Distances.	Sets not used.
				°	°	°	° ′ ″	° ′ ″	° ′	
T'ounobis . . .	*a*	Sun .	..	14	58	107	20 51·2			*b*
	d } *e*	Antares	W.	18	68	51	21 7 / 21 7·5	21 5·3		*c* *h*
	f } *g*	Sun .	W.	46	16	117	21 9 / 21 12		21 1	*j*
	k *m* *n* *o* *p*	Saturn	E.	24	68	45	21 2·2 / 20 55·7 / 20 55·5 / 21 / 20 52·5	20 57		*l*
Ondonga, Nangoro's Werft.	*t*	Regulus	W.	38	72	46	16 17·5	..	16 14	*q*
	u	Antares	E.	44	72	54	16 10·5			*r* *s*

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COMPOSITE PORTRAITS.

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[*Reprinted from the Journal of the Anthropological Institute, November*, 1878.]

COMPOSITE PORTRAITS, *made by combining those of many different persons into a single resultant figure.* By FRANCIS GALTON, F.R.S.

I submit to the Anthropological Institute my first results in carrying out a process that I suggested last August in my presidential address to the Anthropological Subsection of the British Association at Plymouth, in the following words:—

"Having obtained drawings or photographs of several persons alike in most respects, but differing in minor details, what sure method is there of extracting the typical characteristics from them? I may mention a plan which had occurred both to Mr. Herbert Spencer and myself, the principle of which is to superimpose optically the various drawings, and to accept the aggregate result. Mr. Spencer suggested to me in conversation that the drawings reduced to the same scale might be traced on separate pieces of transparent paper and secured one upon another, and then held between the eye and the light. I have attempted this with some success. My own idea was to throw faint images of the several portraits, in succession, upon the same sensitised photographic plate. I may add that it is perfectly easy to superimpose optically two portraits by means of a stereoscope, and that a person who is used to handle instruments will find a common double eyeglass fitted with stereoscopic lenses to be almost as effectual and far handier than the boxes sold in shops."

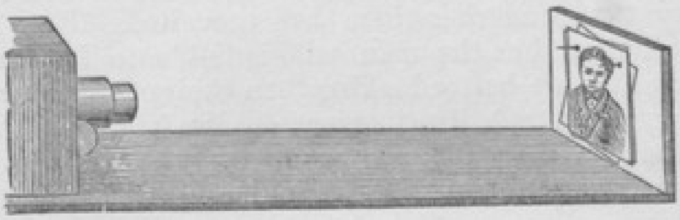
Mr. Spencer, as he informed me had actually devised an instrument, many years ago, for tracing mechanically, longitudinal, transverse, and horizontal sections of heads on transparent paper, intending to superimpose them, and to obtain an average result by transmitted light.

Since my Address was published, I have caused trials to be made, and have found, as a matter of fact, that the photographic process of which I there spoke enables us to obtain with mechanical precision a generalised picture; one that represents no man in particular, but portrays an imaginary figure possessing the average features of any given group of men. These ideal faces have a surprising air of reality. Nobody who glanced at one of them for the first time, would doubt its being the likeness of a living person, yet, as I have said, it is no such thing; it is the portrait of a type and not of an individual.

I begin by collecting photographs of the persons with whom I propose to deal. They must be similar in attitude and size, but no exactness is necessary in either of these respects. Then, by a simple contrivance, I make two pinholes in each of them, to enable me to hang them up one in front of the other, like a pack of cards, upon the same pair of pins, in such a way that the eyes of all the portraits shall be as nearly as possible superimposed; in which case the remainder of the features will also be superimposed nearly enough. These pinholes correspond to what are technically known to printers as "register marks." They are easily made: A slip of brass or card has an aperture cut out of its middle, and threads are stretched from opposite sides,

making a cross. Two small holes are drilled in the plate, one on either side of the aperture. The slip of brass is laid on the portrait with the aperture over its face. It is turned about until one of the cross threads cuts the pupils of both the eyes, and it is further adjusted until the other thread divides the interval between the pupils in two equal parts. Then it is held firmly, and a prick is made through each of the holes.

The portraits being thus arranged, a photographic camera is directed upon them. Suppose there are eight portraits in the pack, and that under existing circumstances it would require an exposure of eighty seconds to give an exact photographic copy of any one of them. The general principle of proceeding is this, subject in practice to some variation of details, depending on the different brightness of the several portraits. We throw the image of each of the eight portraits in turn upon the same part of the sensitised plate for ten seconds. Thus, portrait No. 1 is in the front of the pack; we take the cap off the object glass of the camera for ten seconds, and afterwards replace it. We then remove No. 1 from the pins, and No. 2 appears in the front; we take off the cap a second time for ten seconds, and again replace it. Next we remove No. 2 and No. 3 appears in the front,



which we treat as its predecessors, and so we go on to the last of the pack. The sensitised plate will now have had its total exposure of eighty seconds; it is then developed, and the print taken from it is the generalised picture of which I speak. It is a composite of eight component portraits. Those of its outlines are sharpest and darkest that are common to the largest number of the components; the purely individual peculiarities leave little or no visible trace. The latter being necessarily disposed equally on both sides of the average, the outline of the composite is the average of all the components. It is a band and not a fine line, because the outlines of the components are seldom exactly superimposed. The band will be darkest in its middle whenever the component portraits have the same general type of features, and its breadth, or amount of blur, will measure the tendency of the components to deviate from the common type. This is so for the very same reason that the shot-marks on a target are more thickly disposed near the bulls-eye than away from it, and in a greater degree as the marksmen are more skilful. All that has been said of the outlines is equally true as regards the shadows; the result being that the composite represents an averaged figure, whose lineaments have been softly drawn. The eyes come out with appropriate distinctness, owing to the mechanical conditions under which the components were hung.

A composite portrait represents the picture that would rise before the mind's eye of a man who had the gift of pictorial imagination in an exalted degree. But the imaginative power even of the highest artists is far from precise, and is so apt to be biassed by special cases that may have struck their fancies, that no two artists agree in any of their typical forms. The merit of the photographic composite is its mechanical precision, being subject to no errors beyond those incidental to all photographic productions.

I submit several composites made for me by Mr. H. Reynolds. The first set of portraits are those of criminals convicted of murder, manslaughter, or robbery accompanied with violence. It will be observed that the features of the composites are much better looking than those of the components. The special villainous irregularities in the latter have disappeared, and the common humanity that underlies them has prevailed. They represent, not the criminal, but the man who is liable to fall into crime. All composites are better looking than their components, because the averaged portrait of many persons is free from the irregularities that variously blemish the looks of each of them.

I selected these for my first trials because I happened to possess a large collection of photographs of criminals, through the kindness of Sir Edmund Du Cane, the Director-General of Prisons, for the purpose of investigating criminal types. They were peculiarly adapted to my present purpose, being all made of about the same size, and taken in much the same attitudes. It was while endeavouring to elicit the principal criminal types by methods of optical superimposition of the portraits, such as I had frequently employed with maps and meteorological traces,* that the idea of composite figures first occurred to me.

The other set of composites are made from pairs of components. They are selected to show the extraordinary facility of combining almost any two faces whose proportions are in any way similar.

It will, I am sure, surprise most persons to see how well defined these composites are. When we deal with faces of the same type, the points of similarity far outnumber those of dissimilarity, and there is a much greater resemblance between faces generally, than we who turn our attention to individual differences are apt to appreciate. A traveller on his first arrival among people of a race very different to his own thinks them closely alike, and a Hindu has much difficulty in distinguishing one Englishman from another.

The fairness with which photographic composites represent their components, is shown by six of the specimens. I wished to learn whether the order in which the components were photographed made any material difference in the result, so I had three of the portraits arranged successively in each of their six possible combinations. It will be observed that four at least of the six composites are closely alike. I should say that in each of this set the last of the three components was always allowed a longer exposure than the second, and the second than the first, but it is found better to allow an equal time to all of them.

The stereoscope, as I stated last August in my address at

* "Conference at the Loan Exhibition of Scientific Instruments," 1878. Chapman and Hall. Physical Geography Section, p. 312, "On Means of Combining Various Data in Maps and Diagrams," by Francis Galton, F.R.S.

Plymouth, affords a very easy method of optically superimposing two portraits, and I have much pleasure in quoting the

The accompanying woodcut is as fair a representation of one of the composites as is practicable in ordinary printing. It was photographically transferred to the wood, and the engraver has used his best endeavour to translate the shades into line engraving. This composite is made out of only three components, and its three-fold origin is to be traced in the ears, and in the buttons to the vest. To the best of my judgment the original photograph is a very exact average of its components: not one feature in it appears identical with that of any one of them, but it contains a resemblance to all, and is not more like to one of them than to another. However the judgment of the wood engraver is different. His rendering of the composite has made it exactly like one of its components, which it must be borne in mind he had never seen. It is just as though an artist drawing a child had produced a portrait closely resembling its deceased father, having overlooked an equally strong likeness to its deceased mother, which was apparent to its relatives. This is to me a most striking proof that the composite is a true combination.

following letter, pointing out this fact as well as some other conclusions to which I also had arrived. The letter was kindly forwarded to me by Mr. Darwin; it is dated last November, and was written to him by Mr. A. L. Austin, from New Zealand, thus affording another of the many curious instances of two persons being independently engaged in the same novel inquiry at nearly the same time, and coming to similar results.

"Invercargill, New Zealand,
"*November 6th*, 1877.

"To CHARLES DARWIN, Esq.

"SIR,—Although a perfect stranger to you, and living on the reverse side of the globe, I have taken the liberty of writing to you on a small discovery I have made in binocular vision in the stereoscope. I find by taking two ordinary carte-de-visite photos of two different persons' faces, the portraits being about the same sizes, and looking about the same direction, and placing them in a stereoscope, the faces blend into one in a most remarkable manner, producing in the case of some ladies' portraits, in every instance, a *decided improvement* in beauty. The pictures were not taken in a binocular camera, and therefore do not stand out well, but by moving one or both until the eyes coincide in the stereoscope the pictures blend perfectly. If taken in a binocular camera for the purpose, each person being taken on one half of the negative, I am sure the results would be still more striking. Perhaps something might be made of this in regard to the expression of emotions in man and the lower animals, &c. I have not time or opportunities to make experiments, but it seems to me something might be made of this by photographing the faces of different animals, different races of mankind, &c. I think a stereoscopic view of one of the ape tribe and some low caste human face would make a very curious mixture; also in the matter of crossing of animals and the resulting offspring. It seems to me something also might result in photos of husband and wife and children, &c. In any case, the results are curious, if it leads to nothing else. Should this come to anything you will no doubt acknowledge myself as suggesting the experiment, and perhaps send me some of the results. If not likely to come to anything, a reply would much oblige me."

"Yours very truly,

"A. L. AUSTIN, C.E., F.R.A.S."

Dr. Carpenter informs me that the late Mr. Appold, the mechanician, used to combine two portraits of himself under the stereoscope. The one had been taken with an assumed stern expression, the other with a smile, and this combination produced a curious and effective blending of the two.

Convenient as the stereoscope is, owing to its accessibility, for determining whether any two portraits are suitable in size and attitude to form a good composite, it is nevertheless a makeshift and imperfect way of attaining the required result. It cannot of itself combine two images; it can only place them so that the office of attempting to combine them may be undertaken by the brain. Now the two separate impressions received by the brain through the stereoscope do not seem to me to be relatively constant in their vividness, but sometimes the image seen by the left eye prevails over that seen by the right, and *vice versâ.* All the other instruments I am about to describe accomplish that which the stereoscope fails to do: they create true optical combinations. As regards other points in Mr. Austin's letter, I cannot think that the use of a binocular camera for taking the two portraits intended to be combined into one by the stereoscope would be of importance. All that is wanted is that the portraits should be nearly of the same size. In every other respect I cordially agree with Mr. Austin.

The best instrument I have as yet contrived and used for optical superimposition is a "double-image prism" of Iceland spar. The latest that I have had were procured for me by Mr. Tisley, optician, 172, Brompton Road. They have a clear aperture of a square, half an inch in the side, and when held at right angles to the line of sight will separate the ordinary and extraordinary images to the amount of two inches, when the object viewed is held at seventeen inches from the eye. This is quite sufficient for working with cartes-de-visite portraits. One image is quite achromatic, the other shows a little colour. The divergence may be varied and adjusted by inclining the prism to the line of sight. By its means the ordinary image of one component is thrown upon the extraordinary image of the other,

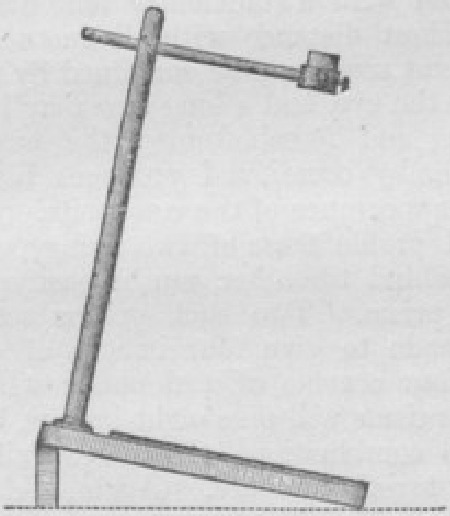


FIG. 1.

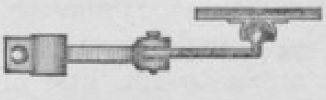


FIG. 2.

FIG. 3.

Fig. 1 shows the simple apparatus which carries the prism and on which the photograph is mounted. The former is set in a round box which can be rotated in the ring at the end of the arm and can be clamped when adjusted. The arm can be rotated and can also be pulled out or in if desired, and clamped. The floor of the instrument is overlaid with cork covered with black cloth, on which the components can easily be fixed by drawing-pins. When using it, one portrait is pinned down and the other is moved near to it, overlapping its margin if necessary, until the eye looking through the prism sees the required combination; then the second portrait is pinned down also. It may now receive its register-marks from needles fixed in a hinged arm, and this is a more generally applicable method than the plan with cross threads, already described, as any desired feature—the nose, the ear, or the hand, may thus be selected for composite purposes. Let A, B, C, . . . Y, Z, be the components. A is pinned down, and B, C, . . Y, Z; are successfully combined with A, and registered. Then before removing Z, take away A and substitute any other of the already registered portraits, say B, by combining it with Z; lastly, remove Z and substitute A by combining it with B, and register it. Fig. 2 shows one of three similarly jointed arms, which clamp on to the vertical rod. Two of these carry a light frame covered with cork and cloth, and the other carries Fig. 3, which is a frame having lenses of different powers set into it, and on which, or on the third frame, a small mirror inclined at 45° may be laid. When a portrait requires foreshortening it can be pinned on one of these frames and be inclined to the line of sight; when it is smaller than its fellow it can be brought nearer to the eye and an appropriate lens interposed; when a right-sided profile has to be combined with a left-handed one, it must be pinned on one of the frames and viewed by reflection from the mirror in the other. The apparatus I have drawn is roughly made, and being chiefly of wood is rather clumsy, but it acts well.

and the composite may be viewed by the naked eye, or through a lens of long focus, or through an opera-glass (a telescope is not so good) fitted with a sufficiently long draw-tube to see an object at that short distance with distinctness. Portraits of somewhat different sizes may be combined by placing the larger one further from the eye, and a long face may be fitted to a short one by inclining and foreshortening the former. The slight fault of focus thereby occasioned produces little or no sensible ill-effect on the appearance of the composite.

The front and profile faces of two living persons sitting side by side or one behind the other, can be easily superimposed by a double-image prism. Two such prisms set one behind the other can be made to give four images of equal brightness, occupying the four corners of a rhombus whose acute angles are 45°. Three prisms will give eight images, but this is practically not a good combination; the images fail in distinctness, and are too near together for use. Again, each lens of a stereoscope of long focus can have one or a pair of these prisms attached to it, and four or eight images may be thus combined.

Another instrument I have made consists of a piece of glass inclined at a very acute angle to the line of sight, and of a mirror beyond it, also inclined, but in the opposite direction to the line of sight. Two rays of light will therefore reach the eye from each point of the glass; the one has been reflected from its surface, and the other has been first reflected from the mirror, and then transmitted through the glass. The glass used should be extremely thin, to avoid the blur due to double reflections; it may be a selected piece from those made to cover microscopic specimens. The principle of the instrument may be yet further developed by interposing additional pieces of glass, successively less inclined to the line of sight, and each reflecting a different portrait.

I have tried many other plans; indeed the possible methods of optically superimposing two or more images are very numerous. Thus I have used a sextant (with its telescope attached); also strips of mirrors placed at different angles, their several reflections being simultaneously viewed through a telescope. I have also used a divided lens, like two stereoscopic lenses brought close together, in front of the object class of a telescope.

I have not yet had an opportunity of superimposing images by placing glass negatives in separate magic lanterns, all converging upon the same screen; but this or even a simple dioramic apparatus would be very suitable for exhibiting composite effects to an audience, and, if the electric light were used for illumination, the effect on the screen could be photographed at once. It would also be possible to construct a camera with a long focus, and many slightly divergent object glasses, each throwing an image of a separate glass negative upon the same sensitised plate.

The uses of composite portraits are many. They give us typical pictures of different races of men, if derived from a large number of individuals of those races taken at random. An assurance of the truth of any of our pictorial deductions is to be looked for in their substantial agreement when different batches of components have been dealt with, this being a perfect test of truth in all statistical conclusions. Again, we may select prevalent or strongly-marked types from among the men of the same race, just as I have done with two of the types of criminals by which this memoir is illustrated.

Another use of this process is to obtain by photography a really good likeness of a living person. The inferiority of photographs to the best works of artists, so far as resemblance is concerned, lies in their catching no more than a single expression. If many photographs of a person were taken at different times, perhaps even years apart, their composite would possess that in which a single photograph is deficient. I have already pointed out the experience of Mr. Appold to this effect. The analytical tendency of the mind is so strong that out of any tangle of superimposed outlines it persists in dwelling preferably on some one of them, singling it out and taking little heed of the rest. On one occasion it will select one outline, on another a different one. Looking at the patterns of the papered walls of our room, we see, whenever our fancy is active, all kinds of forms and features. We often catch some strange combination which we are unable to recall on a subsequent occasion, while later still it may suddenly flash full upon us. A composite portrait would have much of this varied suggestiveness.

A further use of the process would be to produce from many independent portraits of an historical personage the most probable likeness of him. Contemporaneous statues, medals, and gems would be very suitable for the purpose; photographs being taken of the same size, and a composite made from them. It will be borne in mind that it is perfectly easy to apportion different "weights" to the different components. Thus, if one statue be judged to be so much more worthy of reliance than another that it ought to receive double consideration in the composite, all that is necessary is to double either the time of its exposure or its illumination.

The last use of the process that I shall mention is of great interest as regards inquiries into the hereditary transmission of features, as it enables us to compare the average features of the produce with those of the parentage. A composite of all the brothers and sisters in a large family would be an approximation to what the average of the produce would probably be if the family were indefinitely increased in number, but the approximation would be closer if we also took into consideration those of the cousins who inherited the family likeness. As regards the parentage, it is by no means sufficient to take a composite of the two parents; the four grandparents and the uncles and aunts on both sides should be also included. Some statistical inquiries I published on the distribution of ability in families* give provisional data for determining the weight to be assigned in the composite to the several degrees of relationship. I should, however, not follow those figures in the present case, but would rather suggest, for the earlier trials, first to give equal "weights" to the male and female sides; thus the father and a brother of the male parent would count equally with the father and a brother of the female parent. Secondly, I should "weight" each parent as four, and each grandparent and each uncle and aunt as one; again, I should weight each brother and sister as four, and each of those cousins as one who inherited any part of the likeness of the family in question. The other cousins I should disregard. The weights as previously mentioned would be bestowed by giving proportionate periods of exposure.†

* "Hereditary Genius," p. 317, column D. Macmillan. 1869.

† Example:—There are 5 brothers or sisters and 5 cousins, whose portraits are available: the total period of desired exposure is 100 seconds, $5 \times 4 + 5 = 25$; $\frac{100}{25} = 4$; which gives $4 \times 4 = 16$ seconds for each brother or sister, and 4 seconds each cousin ($5 \times 16 + 5 \times 4 = 100$).

Composites on this principle would no doubt aid the breeders of animals to judge of the results of any proposed union better than they are able to do at present, and in forecasting the results of marriages between men and women they would be of singular interest and instruction. Much might be learnt merely by the frequent use of the double-image prism as described above, which enables us to combine the features of living individuals when sitting side by side into a single image.

I have as yet had few opportunities of developing the uses of the composite photographic process, it being difficult, without much explanation, to obtain the requisite components. Indeed, the main motive of my publishing these early results is to afford that explanation, and to enable me to procure a considerable variety of materials to work upon. I especially want sets of family photographs all as nearly as possible of the same size and taken in the same attitudes. The size I would suggest for family composites is that which gives four-tenths* of an inch (or say 10 millimetres) interval between the pupil of the eye and the line that separates the two lips. The attitudes, about which there can be no mistake, are full face, an exact profile (say, always showing the *right* side of the face), and an exact three-quarters, always showing the left; in this the outer edge of the right eyelid will be only just in sight. In each case the sitter should look straight before him. Such portraits as these go well into cartes de visite, and I trust that not a few amateur photographers may be inclined to make sets of all the members of their family, young and old, and of both sexes, and to try composites of them on the principles I have described. The photographs used for that purpose need not be in the least injured, for the register marks may be made in the case into which they are slipped, and not in the photographs themselves.

DISCUSSION.

Sir EDMUND DUCANE said: I had no intention of making observations on the lecture given Mr. Galton, but as I have been called on, I will explain my connection with the observations on making which, as Mr. Galton has explained, his experiments originated. In considering how best to deal with and repress crime, it occurred to me that we ought to try and track it out to its source and see if we cannot check it there instead of waiting till it has developed and then striking at it. To track crime to its source we must follow up the history of those who practise it, and specially in such lines as are likely (as has been alleged) to contain the true clue to their criminal

* I said *half-an-inch* in the original paper, but have since, for various reasons, adopted four-tenths of an inch instead, as my standard size.—August, 1878.

career. Among these subjects for observation that of the hereditary disposition is one of the most important, and to disentangle the effect of this from the effect of the bringing up. Mr. Galton very kindly undertook to try and ascertain if anything could be established on these points, and I therefore furnished him with the particulars of the personal characteristics and career of a great number of criminals and with their photographs. It seems to me to be a correct inference that if criminals are found to have certain special types of features, that certain personal peculiarities distinguish those who commit certain classes of crime; the tendency to crime is in those persons born or bred in them, and either they are incurable or the tendency can only be checked by taking them in hand at the earliest periods of life. Mr. Galton's process would help to establish this point, because if there is any such distinguishing feature it would come out in his mixed photographs in a clear line, whereas in those features which do not correspond the lines would be more or less blurred. I should anticipate that a great number of those who commit certain classes of crimes would be found to show an entirely inferior mental and bodily organisation; but on the other hand a very large number of criminals are rather superior in intelligence; so much so that I was quite recently informed by Colonel Pasley, the Director of Admiralty Works, that his observation was that convicts picked up a knowledge of a new trade with much greater rapidity than free workmen. In fact, it is often misplaced and unbalanced cleverness that leads to the attempt to commit crime, and this characteristic might very probably be found in the features of criminals of this class.

Mr. Cornelius Walford, after expressing his interest in the subject under discussion, drew attention to the fact that changes of location and of climate, possibly also of food, tended very materially to alter family and even national types of facial expression. As an instance, children of Irish parents born in the United States present usually quite a classical form of face, notwithsanding that the parents, in many cases, bore the strongest marks of nationality. Sir Charles Dilke, in his "Greater Britain," says that the same thing takes place in the Australian Colonies. It seems clear from this that even criminal types will not hold good under all circumstances. He did not quite know how this might affect Mr. Galton's theory. He also thought that experimenting upon a number of persons tended rather to generalise than to particularise the expression. These remarks were to be regarded as suggestions only.

Mr. Robert des Ruffières said: Mr. Galton's paper on "Composite Portraits" is both curious and suggestive, and may perhaps lead to important results in time to come. As it is, the author considers his discovery may be turned to good account in several ways, and notably as a means of comparing the average features of a family with those of its near ancestry. If I recollect rightly, Mr. Galton laid great stress on the eyes as one of the most important features, and especially in connection with his views, and no doubt with good reason; but it should not be forgotten that the mouth also is a very characteristic feature, and it is not many years ago that a celebrated French painter undertook to show that it was possible to group the several personages of a historical picture, in such a way as to bring visibly before the mind of the spectator the passing scene, and that without the eyes of any of the *dramatis personæ* being visible. Mr. Galton's discovery has been spoken of elsewhere as a toy, but the same was said at the time of the Kaleidoscope, which has done such good service in the Arts, and very recently of the Radiometer, which it has been shown can be sucessfully applied in Climatology for testing gas-light, and other purposes.

Mr. Hyde Clarke said it was necessary to accept Mr. Galton's results under the reservations and conditions he had imposed. Otherwise there was a danger of adopting wrong conclusions, as a mean or average did not represent a natural fact, but was an artificial term. Thus in the examples before them the criminal characteristics were eliminated, and they had a natural type of man instead. Thus, instead of a typical figure or a distinctive type, only an average was obtained. With regard to the question which had been raised as to change of character in America, he had termed the phenomena Creolism. Some men and animals underwent change and removal from one district to another, and it was recorded that in India some horses died by simple removal. It was remarkable that the phenomena known to us as "Yankeesim" were common to the United States and Australia. In the case of an emigrant bringing children of English type, then one child subsequently born might be of American type and another of English type. This appeared to affect English and Celts, but he had not traced it to Spaniards. It was to be observed that all Americans had not the Yankee type, but that many had a thorough English type. This showed that Creolism is not purely an influence of soil. Further, the Yankee type was produced in England, but rarely. There were various influences of removal, as, for instance, the effect on the skin and eyes of our African travellers.

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Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, February 9, 1877.

SIR W. FREDERICK POLLOCK, Bart. M.A. Vice-President, in the Chair.

FRANCIS GALTON, Esq. F.R.S. F.G.S. *M.R.I.*

Typical Laws of Heredity.

WE are far too apt to regard common events as matters of course, and to accept many things as obvious truths which are not obvious truths at all, but present problems of much interest. The problem to which I am about to direct attention is one of these.

Why is it, when we compare two groups of persons selected at random from the same race, but belonging to different generations of it, we find them to be closely alike? Such statistical differences as there may be, are always to be ascribed to differences in the general conditions of their lives; with these I am not concerned at present; but so far as regards the processes of heredity alone, the resemblance of consecutive generations is a fact common to all forms of life.

In each generation there will be tall and short individuals, heavy and light, strong and weak, dark and pale; yet the proportions of the innumerable grades in which these several characteristics occur tend to be constant. The records of geological history afford striking evidences of this statistical similarity. Fossil remains of plants and animals may be dug out of strata at such different levels, that thousands of generations must have intervened between the periods in which they lived; yet in large samples of such fossils we seek in vain for peculiarities that will distinguish one generation taken as a whole from another, the different sizes, marks, and variations of every kind, occurring with equal frequency in both. The processes of heredity are found to be so wonderfully balanced, and their equilibrium to be so stable, that they concur in maintaining a perfect statistical resemblance so long as the external conditions remain unaltered.

If there be any who are inclined to say there is no wonder in the matter, because each individual tends to leave his like behind him, and therefore each generation must resemble the one preceding, I can assure them that they utterly misunderstand the case. Individuals do *not* equally tend to leave their like behind them, as will be seen best from an extreme illustration.

Let us then consider the family history of widely different groups, say of 100 men, the most gigantic of their race and time, and the same number of medium men. Giants marry much more rarely than medium men, and when they do marry they have but few children. It is a matter of history that the more remarkable giants have left no issue at all. Consequently the offspring of the 100 giants would be much fewer in number than those of the medium men. Again, these few would, on the average, be of lower stature than their fathers, for two reasons. First, their breed is almost sure to be diluted by marriage. Secondly, the progeny of all exceptional individuals tends to "revert" towards mediocrity. Consequently the children of the giant group would not only be very few, but they would also be comparatively short. Even of these the taller ones would be the least likely to live. It is by no means the tallest men who best survive hardships; their circulation is apt to be languid and their constitution consumptive.

It is obvious from this that the 100 giants will not leave behind them their quota in the next generation. The 100 medium men, on the other hand, being more fertile, breeding more truly to their like, being better fitted to survive hardships, &c., will leave more than their proportionate share of progeny. This being so, it might be expected that there would be fewer giants and more medium-sized men in the second generation than in the first. Yet, as a matter of fact, the giants and medium-sized men will, in the second generation, be found in the same proportions as before. The question, then, is this: How is it, that although each individual does *not* as a rule leave his like behind him, yet successive generations resemble each other with great exactitude in all their general features?

It has, I believe, become more generally known than formerly, that although the characteristics of height, weight, strength, and fleetness are very different in themselves, and though different species of plants and animals exhibit every kind of diversity, yet the differences in height, weight, and every other characteristic, among members of the same species, are universally distributed in fair conformity with a single law.

The phenomena with which that law deals are like those perspectives spoken of by Shakespeare, which, when viewed awry, show nothing but confusion.

Our ordinary way of looking at individual differences is awry: thus we naturally, but wrongly, judge of differences in stature by differences in heights measured from the ground, whereas on changing our point of view to that whence the law of deviation regards them, by taking the average height of the race, and not the ground, as the point of reference, all confusion disappears, and uniformity prevails.

It was to Quetelet that we were first indebted for a knowledge of the fact, that the amount and frequency of deviation from the average among members of the same race, in respect to each and every characteristic, tends to conform to the mathematical law of deviation.

The diagram contains extracts from some of the tables by which he corroborates his assertion. Three of the series in them refer to the heights of Americans, French, and Belgians respectively, and the fourth to the strength of Belgians. In each series there are two parallel columns, one entitled "observed," and the other "calculated," and the close conformity between each of the pairs is very striking.

Scale of Heights.	American Soldiers (25,878 Observations).		France (Hargenvilliers).		Belgium, Quetelet. 20 years' Observations.	
	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.
mètres.						
1·90	1	3				
1·90	7	5	..	..		
·87	14	13	..	1	1	1
·84	25	28		3	2	3
·81	45	52	25	7	7	7
·79	99	84		16	14	14
·76	112	117	32	32	34	28
·73	138	142	55	55	48	53
·70	148	150	88	87	102	107
·68	137	137	114	118	138	136
·65	93	109	144	140	129	150
·62	109	75	140	145	162	150
·60	49	45	116	132	106	136
·57	14	24	..	105	110	107
·54	8	11		73		53
·51	1	4		44		28
·48	..	1		24		14
·45	..	..	286	11	147	7
·42	..	..		4		3
·39	..	..		2		1
·36	..	..		1		
	1000	1000	1000	1000	1000	1000

Degrees of Dynamometer.	Lifting Power of Belgian Men.	
	Observed.	Calculated.
200	1	1
190	9	10
180		
170	23	23
160		
150	32	32
140		
130	22	23
120		
110	12	10
100		
90	1	1
	100	100

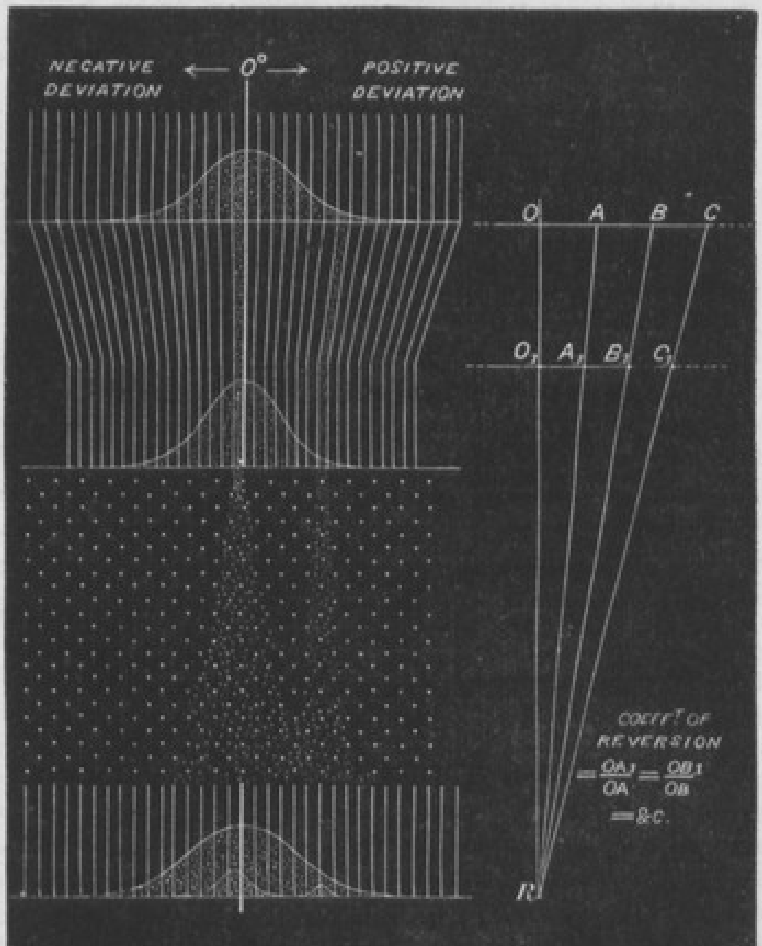
f. 2v

These tables serve another purpose; they enable those who have not had experience of such statistics to appreciate the beautiful balance of the processes of heredity in ensuring the repetition of such finely graduated proportions as those that the tables record.

The outline of my problem of this evening is, that since the characteristics of all plants and animals tend to conform to the law of deviation, let us suppose a typical case, in which the conformity shall be exact, and which shall admit of discussion as a mathematical problem, and find what the laws of heredity must then be to enable successive generations to maintain statistical identity.

I shall have to speak so much about the law of deviation, that it is

Fig. 1.



absolutely necessary to tax your attention for a few minutes to explain the principle upon which it is based, what it is that it professes to show, and what the two numbers are, which enable long series to be calculated like those in the tables just referred to. The simplest way of explaining the law is to begin by showing it in action. For this purpose I will use an apparatus that I employed three years ago in this very theatre, to illustrate other points connected with the law of deviation. An extension of its performance will prove of great service to us to-night; but I will begin by working the instrument as I did on the previous occasion. The portion of it that then existed, and to which I desire now to confine your attention, is shown in the lower part of Fig. 1, where I wish to direct your notice to the stream issuing from either of the divisions just above the dots, to its dispersion among them, and to the little heap that it forms on the bottom line. This part of the apparatus is like a harrow with its spikes facing us; below these are vertical compartments; the whole is faced with a glass plate. I will pour pellets from either of these divisions or from any other point above the spikes; they will fall against the spikes, tumble about among them, and after pursuing devious paths, each will finally sink to rest in the compartment that lies beneath the place whence it emerges from its troubles.

The courses of the pellets are extremely irregular; it rarely happens that any two starting from the same point will pursue the same path from beginning to end; yet, notwithstanding this, you will observe the regularity of the outline of the heap formed by the accumulation of pellets.

This outline is the geometrical representation of the curve of deviation. If the rows of spikes had been few, the deviation would have been slight, almost all the pellets would have lodged in the compartment immediately below the point whence they were dropped, and would then have resembled a column; if they had been very numerous, they would have been scattered so widely that the part of the curve for a long distance to the right and left of the point whence they were dropped would have been of uniform width, like an horizontal bar. With intermediate numbers of rows of teeth, the curved contour of the heap would assume different shapes, all having a strong family resemblance. I have cut some of these out of cardboard; they are represented in the diagrams 2, 3, 4 and 5, below. Theoretically speaking, every possible curve of deviation may be formed by an apparatus of this sort, using extremely numerous and delicate spikes and minute pellets, and by varying the length of the harrow and the number of pellets poured in. Or if I draw a curve on an elastic sheet of indiarubber, by stretching it laterally I produce the effects of increased dispersion; by stretching it vertically I produce that of increased numbers. The latter variation is shown by the three curves in each of the four diagrams; but it does not concern us to-night, as we are dealing with internal proportions, which are not affected by the absolute number of the sample employed. To specify the variety of curve so far as dispersion is concerned, we must measure the amount of lateral stretch of the indiarubber sheet. The curve has no

f. 3v

FIG. 2.

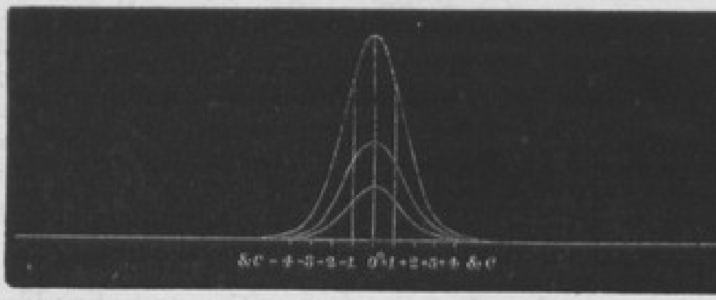


FIG. 3.

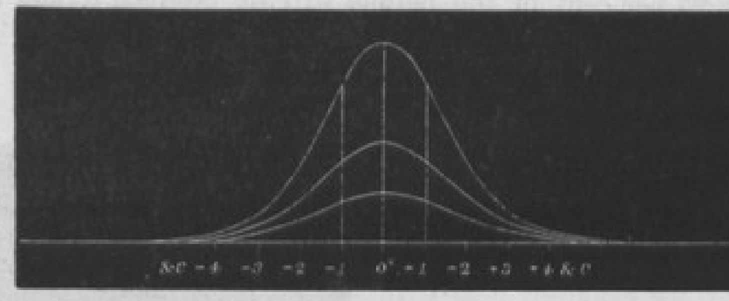


FIG. 4.

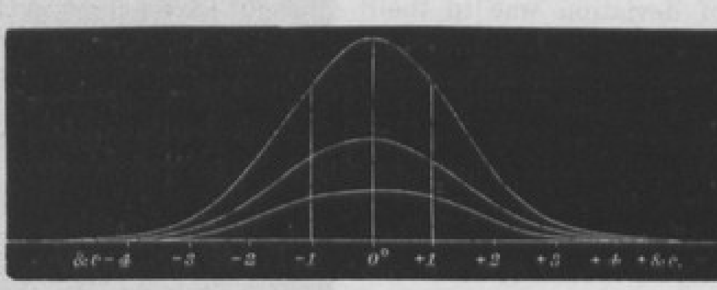
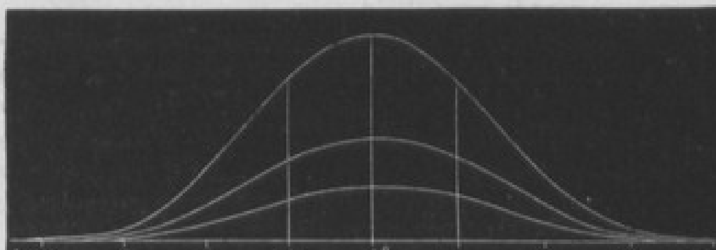
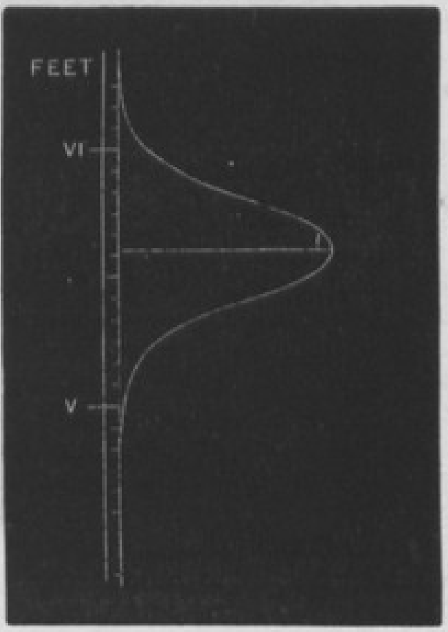


FIG 5.



definite ends, so we have to select and define two points in its base, between which the stretch may be measured. One of these points is always taken directly below the place whence the pellets were poured in. This is the point of no deviation, and represents the mean position of all the pellets, or the average of a race. It is marked as 0°. The other point is conveniently taken at the foot of the vertical line that divides either half of the symmetrical figure into two equal areas. I take a half curve in cardboard that I have again divided into two portions along this line; the weight of the two portions is equal. This distance is the value of 1° of deviation, appropriate to each curve. We extend the scale on either side of 0° to as many degrees as we like, and we reckon deviation as positive, or to be added to the average, on one side of the centre, say to the right, and negative on the other, as shown on the diagrams. Owing to the construction, one-quarter or 25 per cent. of the pellets will lie between 0° and 1°, and the law shows that 16 per cent. will lie between + 1° and + 2°, 6 per cent. between + 2° and + 3° and so on. It is unnecessary to go more minutely into the figures, for it will be easily understood that a formula is capable of giving results to any minuteness and to any fraction of a degree.

Fig. 6.



Let us, for example, deal with the case of the American soldiers. I find, on referring to Gould's Book, that 1° of deviation was in their case 1·676 inches. The curve I hold in my hand, Fig. 6, has been drawn to that scale. I also find that their average height was 67·24 inches. I have here a standard marked with feet and inches. I apply the curve to the standard, and immediately we have a geometrical representation of the statistics of height of all those soldiers. The lengths of the ordinates show the proportion of men at and about their heights, and the area between any pairs of ordinates gives the proportionate number of men between those limits. It is indeed a strange fact, that any one of us sitting quietly at his table could, on being told the two numbers just mentioned, draw out a curve on ruled paper, from which thousands of vertical lines might be chalked side by side on a wall, at the distance apart that is taken up by each man in a rank of American soldiers, and know that if the same number of these American soldiers, taken indiscriminately, had been sorted according to their stature and marched up to the wall, each man of them would find the chalked line which he saw opposite to him to be of exactly his own height. So far as I can judge from the run of the figures in the table, the error would never exceed a quarter of an inch, except at either extremity of the series.

The principle of the law of deviation is very simple. The important influences that acted upon each pellet were the same; namely, the position of the point whence it was dropped, and the force of gravity. So far as these are concerned, every pellet would have pursued an identical path. But in addition to these, there were a host of petty disturbing influences, represented by the spikes among which the pellets tumbled in all sorts of ways. The theory of combination shows that the commonest case is that where a pellet falls equally often to the right of a spike as to the left of it, and therefore drops into the compartment vertically below the point where it entered the harrow. It also shows that the cases are very rare of runs of luck carrying the pellet much oftener to one side than the other of the successive spikes. The law of deviation is purely numerical; it does not regard the fact whether the objects treated of are pellets in an apparatus like this, or shots at a target, or games of chance, or any other of the numerous groups of occurrences to which it is or may be applied.*

I have now done with my description of the law. I know it has been tedious, but it is an extremely difficult topic to handle on an occasion like this. I trust the application of it will prove of more interest.

First, let me point out a fact which Quetelet and all writers who have followed in his path have unaccountably overlooked, and which has an intimate bearing on our work to-night. It is that, although characteristics of plants and animals conform to the law, the reason of their doing so is as yet totally unexplained. The essence of the law is that differences should be wholly due to the collective actions of a host of independent *petty* influences in various combinations, which were represented by the teeth of the harrow, among which the pellets tumbled in various ways. Now the processes of heredity that limit the number of the children of one class, such as giants, that diminish their resemblance to their fathers, and kill many of them, are not petty influences, but very important ones. Any selective tendency is ruin to the law of deviation, yet among the processes of heredity there is the large influence of natural selection. The conclusion is of the greatest importance to our problem. It is, that the processes of heredity must work harmoniously with the law of deviation, and be themselves in some sense conformable to it. Each of the processes must show this conformity separately, quite irrespectively of the rest. It is not an

* Quetelet, apparently from habit rather than theory, always adopted the binomial law of error, basing his tables on a binomial of high power. It is absolutely necessary to the theory of the present paper to get rid of binomial limitations and to consider the law of deviation or error in its exponential form.

f. 5r

admissible hypothesis that any two or more of them, such as reversion and natural selection, should follow laws so exactly inverse to one another that the one should reform what the other had deformed; because characteristics, in which the relative importance of the various processes is very different, are none the less capable of conforming closely to the typical condition.

When the idea first occurred to me, it became evident that the problem might be solved by the aid of a very moderate amount of experiment. The properties of the law of deviation are not numerous, and they are very peculiar. All, therefore, that was needed from experiment was suggestion. I did not want proof, because the theoretical exigencies of the problem would afford that. What I wanted was to be started in the right direction.

I will now allude to my experiments. I cast about for some time to find a population possessed of some measurable characteristic that conformed fairly well to the law, and that was suitable for investigation. I determined to take seeds and their weights, and after many preparatory inquiries, fixed upon those of sweet-peas. They were particularly well suited to my purposes; they do not cross-fertilise, which is a very exceptional condition; they are hardy, prolific, of a convenient size to handle, and their weight does not alter when the air is damp or dry. The little pea at the end of the pod, so characteristic of ordinary peas, is absent in sweet-peas. I weighed seeds individually, by thousands, and treated them as a census officer would treat a large population. Then I selected with great pains several sets for planting. Each set contained seven little packets, and in each packet were ten seeds, precisely of the same weight. Number one of the packets contained giant seeds, all as nearly as might be of $+ 3^\circ$ of deviation. Number seven contained very small seeds, all of $- 3^\circ$ of deviation. The intermediate packets corresponded severally to the intermediate degrees $\pm 2^\circ \pm 1^\circ$ and 0°. As the seeds are too small to exhibit, I have cut out discs of paper in strict proportion to their sizes, and strips in strict proportion to their weights, and have hung below them the foliage produced by one complete set. Many friends and acquaintances each undertook the planting and culture of a complete set, so that I had simultaneous experiments going on in various parts of the United Kingdom. Two proved failures, but the final result was this: that I obtained the more or less complete produce of seven sets, that is, of $7 \times 7 \times 10$, or 490 carefully weighed seeds.

It would be wholly out of place if I were to enter into the details of the experiments themselves, the numerous little difficulties and imperfections in them, or how I balanced doubtful cases, how I divided returns into groups, to see if they confirmed one another, or how I conducted any other of the well-known statistical operations. Suffice it to say that I took immense pains, which, if I had understood the general conditions of the problem as clearly as I do now, I should not perhaps have cared to bestow. The results were most satisfactory. They gave me two data, which were all that I required in order to

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understand the simplest form of descent, and so I got at the heart of the problem at once.

Simple descent means this. The parentage must be single, as in the case of the sweet-peas which are not cross-fertilised, and the rate of production and the incidence of natural selection must both be independent of the characteristic. The only processes concerned in simple descent that can affect the characteristics of a sample of a population are those of Family Variability and Reversion. It is well to define these words clearly. By family variability is meant the departure of the children of the same or similarly descended families, from the ideal mean type of all of them. Reversion is the tendency of that ideal mean filial type to depart from the parent type, "reverting" towards what may be roughly and perhaps fairly described as the average ancestral type. If family variability had been the only process in simple descent that affected the characteristics of a sample, the dispersion of the race from its mean ideal type would indefinitely increase with the number of the generations; but reversion checks this increase, and brings it to a standstill, under conditions which will now be explained.

On weighing and sorting large samples of the produce of each of the seven different classes of the peas, I found in every case the law of deviation to prevail, and in every case the value of 1° of deviation to be the same. I was certainly astonished to find the family variability of the produce of the little seeds to be equal to that of the big ones; but so it was, and I thankfully accept the fact; for if it had been otherwise, I cannot imagine, from theoretical considerations, how the typical problem could be solved.

The next great fact was that reversion followed the simplest possible law; the proportion being constant between the deviation of the mean weight of the produce generally and the deviation of the parent seed, reckoning in every case from one standard point. In a typical case, that standard must be the mean of the race, otherwise the deviation would become unsymmetrical, and cease to conform to the law.

I have adjusted an apparatus (Fig. 1) to exhibit the action of these two processes. We may consider them to act not simultaneously, but in succession, and it is purely a matter of convenience which of the two we suppose to act the first. I suppose first Reversion, then Family Variability. That is to say, I suppose the parent first to revert, and then to *tend* to breed his like. So there are three stages: (1) the population of parents, (2) that of reverted parents, (3) that of their offspring; or, what comes to the same thing, (1) the population of parents, (2) that of the *mean* produce of each parent, (3) that of their actual produce. In arranging the apparatus I have supposed the population to continue uniform in numbers. This is a matter of no theoretical concern, as the whole of this memoir relates to the distinguishing peculiarities of samples irrespectively of the absolute number of individuals in those samples. The apparatus consists of a row of vertical compartments, with trap-doors below them, to hold pellets which serve as representatives of a population of seeds. I will begin with showing how it expresses Reversion. In the upper stage of the apparatus the number of pellets in each compartment represents the relative number in a population of seeds, whose weight deviates from the average, within the limits expressed by the distances of the sides of that compartment from the middle point. The correct shape of the heap has been ensured by a slit of the proper curvature in the board that forms the back of the apparatus. As the apparatus is glazed in front, I have only to pour pellets from above until they reach the level of the slit. Such overplus as may have been poured in will run through the slit, to waste, at the back. The pellets to the right of the heap represent the heaviest seeds, those to the left the lightest. I shall shortly open the trap-door on which the few representatives of the giant seeds rest. They will run downwards through an inclined shoot, and fall into another compartment nearer the centre than before. I shall repeat the process on a second compartment in the upper stage, and successively on all the others. Every shoot converges towards one standard point in the middle vertical line; therefore the present shape of the heap of pellets is more contracted in width than it was before, and is of course more humped up in the middle. We need not regard the humping up; what we have to observe is, that each degree of deviation is simultaneously lessened. The effect is as though the curve of the first heap had been copied on a stretched sheet of indiarubber that was subsequently released. It is obvious from this that the process of reversion co-operates with the general law of deviation. The diagram that I annexed to Fig. 1, shows the principle of the process of reversion in a way that will be readily understood by many of those who are present.

I have now to exhibit the effects of variability among members of the same family. It will be recollected that the produce of peas of the same class deviated normally on either side of their own mean weight; consequently, I must cause the pellets which were in each of the upper compartments to deviate on either side of the compartment in which they now lie, which corresponds to that of the medium weight of their produce. I open the trap-door below one of the compartments in the second stage, the pellets run downwards through the harrow, dispersing as they run, and form a little heap in the lowest compartments, the centre of which heap lies vertically below the trap-door through which they fell. This is the contribution to the succeeding generation of all the individuals belonging to the compartment in the upper stage from which they came. They first reverted and then dispersed. I open another trap-door, and a similar process is gone through; a few extreme pellets in this case add themselves to the first formed heap. Again I continue the process; heap adds itself to heap, and when all the pellets have fallen through, we see that the aggregate contributions bear an exact resemblance to the heap from which we originally started. A formula (see Appendix) expresses the conditions of equilibrium. I attended to these conditions, when I cut out the slit in the backboard of the upper compartment, by which the shape of the original heap was regulated. As an example of the results that follow from the formula, I may mention that if deviation after reversion is to deviation before reversion as 4 to 5, and if 1° of family variability is six units, then the value of 1° in the population must be ten units.

It is easy to prove that the bottom heap is strictly a curve of deviation, and that its scale tends invariably to become the same as that of the upper one. It will be recollected that I showed that every variety of curve of deviation was producible by variations in the length of the harrow, and that if the pellets were intercepted at successive stages of their descent they would form a succession of curves of increasing scales of deviation. The curve in the second stage may therefore be looked upon as one of these intercepts; all that it receives in sinking to the third stage being an additional dose of dispersion.

As regards the precise scale of deviation that characterises each population, let us trace, in imagination, the history of the descendants of a single medium-sized seed. In the first generation the differences are merely those due to family variability; in the second generation the tendency to wider dispersion is somewhat restrained by the effect of reversion; in the third, the dispersion again increases, but is more largely restrained, and the same process continues in successive generations, until the step-by-step progress of dispersion has been overtaken and exactly checked by the growing antagonism of reversion. Reversion acts precisely after the law of an elastic spring, as was well shown by the illustration of the indiarubber sheet. Its tendency to recoil increases the more it is stretched, hence equilibrium must at length ensue between reversion and family variability, and therefore the scale of deviation of the lower heap must after many generations always become identical with that of the upper one.

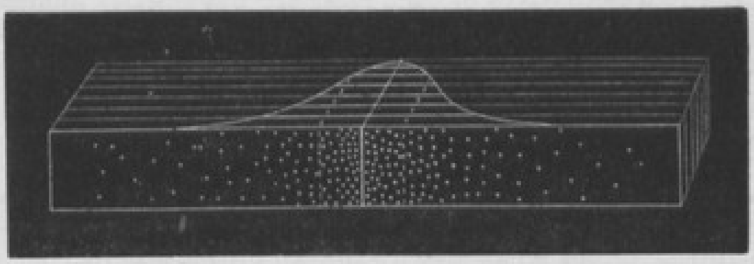
We have now surmounted the greatest difficulty of our problem; what remains will be shortly disposed of. This refers to sexual selection, productiveness, and natural selection. Let us henceforth suppose the heights and every other characteristic of all members of a population to be reduced to a uniform adult male standard so that we may treat it as a single group. Suppose, for example, a female whose height was equal to the average female height + 3° of female deviation, the equivalent in terms of male stature is the average male height + 3° of male deviation. Hence the female in question must be registered not in the feet and inches of her actual height, but in those of the equivalent male stature.

On this supposition we may take the numerical mean of the stature of each couple as the equivalent of a single hermaphrodite parent, so that a male parent plant having 1° deviation, and of a female parent plant having 2° of deviation, would together rank as a single self-fertilised plant of $+ 1\frac{1}{2}°$.

In order that the law of sexual selection should co-operate with the conditions of a typical population, it is necessary that selection should be *nil;* that is, that there should not be the least tendency for tall men to marry tall women rather than short ones. Each strictly typical quality taken by itself must go for nothing in sexual selection. Under these circumstances, one of the best known properties of the law of deviation (technically called that of "two fallible measures") shows that the population of sums of couples would conform truly to the law, and the value of 1° would be that of the original population multiplied by $\sqrt{2}$. Consequently the population of *means of couples* would equally conform to the law; but in this case, as the deviations of means of couples are half those of sums of couples, the 1° of original deviation would have to be divided by $\sqrt{2}$.

The two remaining processes are Productiveness and Survival. Physiologically they are alike, and it is reasonable to expect the same general law to govern both. Natural selection is measured by the percentage of survival among individuals born with like characteristics. Productiveness is measured by the average number of children from all parents who have like characteristics, but it may physiologically be looked upon as the percentage of survival of a vast and unknown number of possible embryos, producible by such parents. The number being unknown creates no difficulty, if there may be considered to be, on an average, the same in every class. Experiment could tell me little about either natural selection or productiveness. What I have to say is based on plain theory. I can explain this best by the process of natural selection. In each species, the height, &c., the most favoured by natural selection, is the one in which the demerits of excess or deficiency are the most frequently balanced. It is therefore not unreasonable to look at nature as a marksman, her aim being subject to the same law of deviation as that which causes the shot on a target to be dispersed on either side of the point aimed at. It would not be difficult, but it would be tedious, to justify the analogy; however, it is unnecessary to do so, as I propose to base the analogy on the exigencies of the typical formula, no other supposition being capable of fulfilling its requirements. Suppose for a

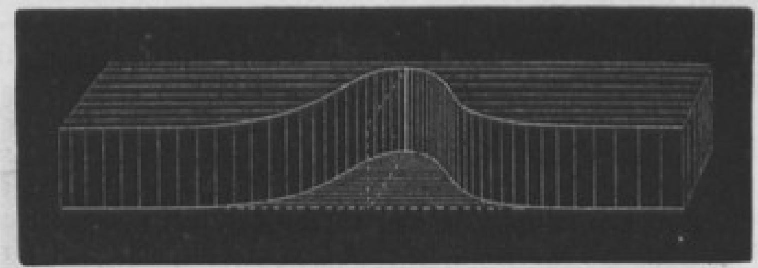
FIG. 7.



moment that nature aims, as a marksman, at the medium class, on purpose to destroy and not to save it. Let a block of stone, as in Fig. 7, represent a rampart, and let a gun be directed at a vertical line on its side on purpose to breach it, the shots would fall with the greatest frequency in the neighbourhood of the vertical line, and their marks would diminish in frequency as the distance increased, in conformity with the law of deviation. Each shot would batter away a bit of stone, and the shape of the breach would be such that its horizontal outline will be the well-known curve. This would be the action of nature were she to aim at the destruction of medium sizes. Her action as preserver of them is the exact converse, and would be represented by a cast that filled the gap and exactly replaced the material that had been battered away. The percentage of thickness of wall that had been destroyed at each degree of deviation is represented by the ordinate of the curve, therefore the percentage of survival is also an ordinate of the same curve of deviation. Its scale has a special value in each instance, subject to the general condition in every typical case, that its 0° shall correspond to the 0° of deviation of height, or whatever the characteristic may be.

In Fig. 8, the thickness of wall that has been destroyed at each

Fig. 8.



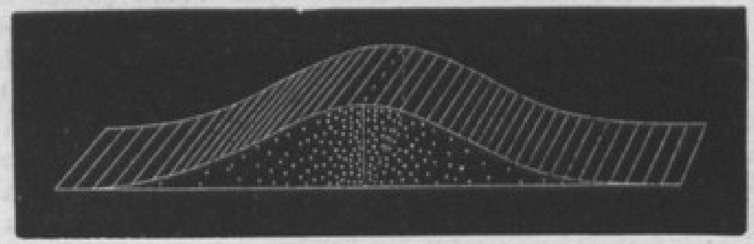
degree of deviation is represented by the corresponding ordinate of the horizontal outline of the portion which remains. Similarly, in the case of an imaginary population, in which each class was *equally* numerous, the amount of survivors at each degree of deviation will be represented by the corresponding ordinate of this or a similar curve.

But in the original population at which we are supposing nature to aim, the representatives of each class are not equally numerous, but are arranged according to the law of deviation; the middle class being most numerous, while the extreme classes are but scantily represented. The ordinate of the above-mentioned outline will in this case represent, not the *absolute number*, but the *percentage* of survivors at each degree of deviation.

If a graphic representation is desired, that shall give the absolute number of survivors at each degree, we must shape the rampart which forms nature's target so as to be highest in the middle and to slope away at each side according to the law of deviation. Thus Fig. 9 represents the curved rampart before the battered part has been removed; Fig. 10, afterwards.

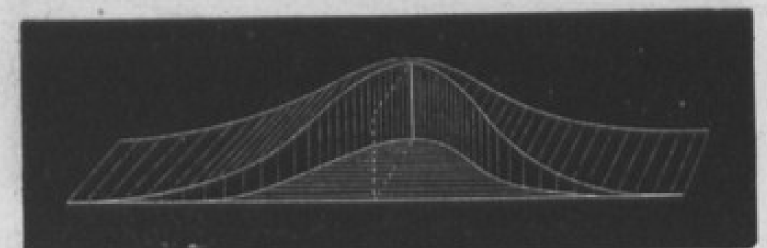
I have taken a block of wood similar to Fig. 7, to represent the rampart; it is of equal height throughout. A cut has been made at right angles to its base with a fret-saw, to divide it into two portions —that which would remain after it had been breached, Fig. 8, and the

FIG. 9.



cast of the breach. Then a second cut with the fret-saw has been made at right angles to its face, to cut out of the rampart an equivalent to the heap of pellets that represents the original population. The gap that would be made in the heap and the cast that would fill the gap are curved on two faces, as in the model. This is sufficiently represented in Fig. 10.

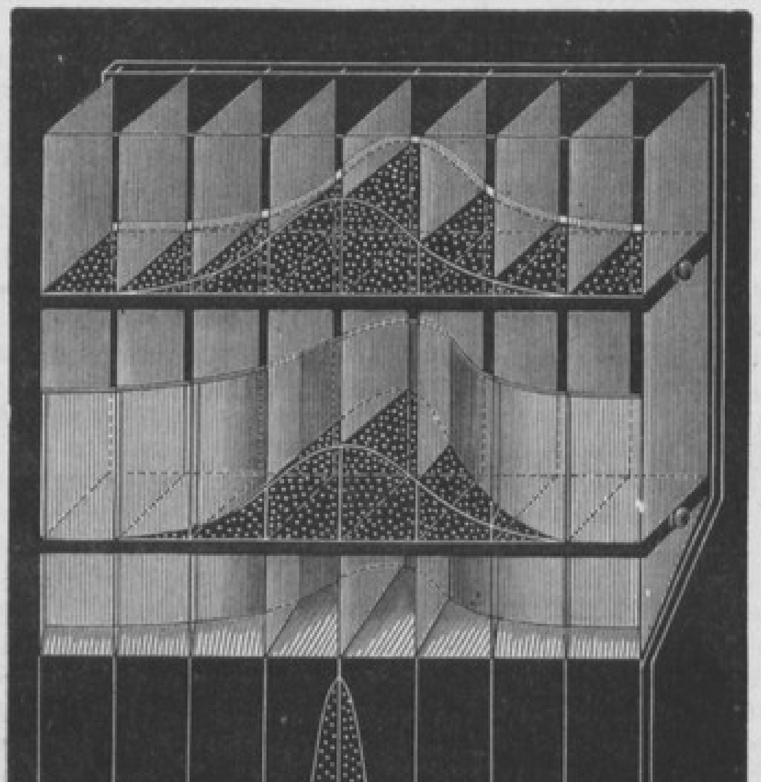
FIG. 10.



The operation of natural selection on a population already arranged according to the law of deviation is represented more completely in an apparatus, Fig. 11, which I will set to work immediately.

It is faced with a sheet of glass. The heap, as shown in the upper compartment of the apparatus, is 3 inches in thickness, and the pellets rest on slides. Directly below the slides, and running from side to side of the apparatus, is a curved partition, which will separate the pellets as they fall upon it, into two portions, one that runs to waste at the back, and another that falls to the front, and forms a new heap. The curve of the partition is a curve of deviation. The shape of this heap is identical with the cast of the gap in Fig. 10. It is highest and thickest in the middle, and it fines away towards either extremity. When the slide upon which it rests is removed, the pellets run down an inclined plane that directs them into a frame of uniform and shallow depth. The pellets from the deep central compartments (it has been impossible to represent in the diagram as many of these as there were in the apparatus) will stand very high from the bottom of the shallow frame, while those that came from the distant compartments will stand even lower than they did before. It follows that the selected pellets form, in the lower compartment, a heap of which the

Fig. 11.



scale of deviation is much more contracted than that of the heap from which it was derived. It is perfectly normal in shape, owing to an interesting theoretical property of the law of deviation (see formula at end of this memoir).

Productiveness follows the same general law as survival, being a percentage of possible production, though it is usual to look on it as a simple multiple, without first multiplying and then dividing by the 100. Looking upon it as a simple multiple, the front face of each compartment in the upper heap represents the number of the parents of the same class, and the depth of the partition below compartment represents the average number that each individual of that class produces.

To sum up. We now see clearly the way in which the resemblance of a population is maintained. In the purely typical case, all the processes of heredity and selection are subject to well-defined and simple laws, which I have formulated in the appendix. Family variability, productiveness, and survival are all subject to the law of deviation, and reversion is expressed by a simple fractional coefficient. It follows that when we know in respect to any characteristic, the values of 1° in the several curves of family variability, productiveness and survival, and when we know the coefficient of reversion, we know absolutely all about the ways in which the characteristic in question will be distributed among the population at large.

I have confined myself in this explanation to purely typical cases, but it is easy to understand how the actions of the processes would be modified in those that were not typical. Reversion might not be directed towards the mean of the race; neither productiveness nor survival might be greatest in the medium classes, and none of their laws may be strictly of the typical character. However, in all cases the general principles would be the same, and the same actions that restrain variability are capable of restraining the departure of average values beyond certain limits in cases where any of the above-mentioned processes are unsymmetrical in their actions. The typical laws are those which most nearly express what takes place in nature generally; they may never be exactly correct in any one case, but at the same time they will always be approximately true and always serviceable for explanation. We estimate through their means the effects of the laws of sexual selection, of productiveness, and of survival, in aiding that of reversion in bridling the dispersive effect of family variability. They show us that natural selection does not act by carving out each new generation according to a definite pattern on a Procrustean bed, irrespective of waste. They also explain how small a contribution is made to future generations by those who deviate widely from the mean, either in excess or deficiency, and they enable us to discover the precise sources whence the deficiencies in the produce of exceptional types are supplied, and their relative contributions. We see by them that the ordinary genealogical course of a race consists in a constant outgrowth from its centre, a constant dying away at its margins, and a tendency of the scanty remnants of all exceptional stock to revert to that mediocrity, whence the majority of their ancestors originally sprang.

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APPENDIX.

I will now proceed to formulate the typical laws. In what has been said, 1° of deviation has been taken equal to the "probable error" = C × 0·4769 in the well-known formula

$$y = \frac{1}{c\sqrt{\pi}} . e^{-\frac{x^2}{c^2}}.$$

According to this, if x = amount of deviation in feet, inches, or any other external unit of measurement, then the number of individuals in any sample who deviate between x and $x + \delta x$ will vary as $e^{-\frac{x^2}{c^2}} \delta x$ (it will be borne in mind that we are for the most part not concerned with the coefficient in the above formula).

Let the modulus of deviation (c) in the original population, after the process has been gone through, of converting the measurements of all its members (in respect to the characteristic in question) to the adult male standard, be written c_0.

1. Sexual selection has been taken as *nil*, therefore the population of "parentages" is a population of which each unit consists of the mean of a couple taken indiscriminately. This, as well known, will conform to the law of deviation, and its modulus, which we will write c_1, has already been shown to be equal to $\frac{1}{\sqrt{2}} . c_0$.

2. Reversion is expressed by a simple fractional coefficient of the deviation, which we will write r. In the "reverted" parentages (a phrase whose meaning and purport have already been explained),

$$y = \frac{1}{rc\sqrt{\pi}} . e^{-\frac{x^2}{r^2c^2}}.$$

In short, the population of which each unit is a reverted parentage follows the law of deviation, and has its modulus, which we will write c_2, equal to $r\,c_1$.

3. Productiveness. We saw that it followed the law of deviation; let its modulus be written f. Then the number of children to each parentage that differs by the amount of x from the mean of the parentages generally (i.e. from the mean of the race) will vary as $e^{-\frac{x^2}{f^2}}$; but the number of such parentages varies as $e^{-\frac{x^2}{c_2^2}}$, therefore if each child absolutely resembled his parent, the number of children who deviated x would vary as $e^{-\frac{x^2}{f^2}} \times e^{-\frac{x^2}{c_2^2}}$, or as $e^{-x^2\left\{\frac{1}{f^2}+\frac{1}{c_2^2}\right\}}$. Hence the deviations of such children in their amount and frequency would conform to the law, and the modulus of the population of

children in the supposed case of absolute resemblance to their parents, which we will write c_3, is such that

$$\frac{1}{c_3} = \sqrt{\left(\frac{1}{f^2} + \frac{1}{c^2}\right)}.$$

We may, however, consider the parents to be multiplied, and the productivity of each of them to be uniform; it is more convenient than the converse supposition, and it comes to the same thing. So we will suppose the reverted parentages to be more numerous but equally prolific, in which case their modulus will be c_3, as above.

4. Family variability was shown by experiment to follow the law of deviation, its modulus, which we will write v, being the same for all classes. Therefore the amount of deviation of any one of the offspring from the mean of his race is due to the combination of two influences—the deviation of his "reverted" parentage and his own family variability; both of which follow the law of deviation. This is obviously an instance of the well-known law of the "sum of two fallible measures."* Therefore the modulus of the population in the present stage, which we will write c_4, is equal to $\sqrt{(v^2 + c_3^2)}$.

5. Natural selection follows, as has been explained, the same general law as productiveness. Let its modulus be written s, then the percentage of survivals among children, who deviate x from the mean, varies as $e^{-\frac{x^2}{s^2}}$; and for the same reasons as those already given, its effect will be to leave the population still in conformity with the law of deviation, but with an altered modulus, which we will write c_5, and

$$\frac{1}{c^5} = \sqrt{\left(\frac{1}{s^2} + \frac{1}{c_4^2}\right)}.$$

Putting these together, we have, starting with the original population having a modulus $= c_0$,

$$1. \quad c_1 = \frac{1}{\sqrt{2}} c_0.$$

$$2. \quad c_2 = r c_1.$$

$$3. \quad c_3 = \sqrt{\left\{\frac{f^2 c^2_2}{f^2 + c_2^2}\right\}}.$$

$$4. \quad c_4 = \sqrt{\{v^2 + c_3^2\}}.$$

$$5. \quad c_5 = \sqrt{\left\{\frac{s^2 c_4^2}{s^2 + c_4^2}\right\}}.$$

And lastly, as the condition of maintenance of statistical resemblance in consecutive generations,

$$6. \quad c_5 = c_0.$$

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* Airy, 'Theory of Errors,' § 43.

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Hence, given the coefficient r and the moduli v, f, s, the value of c_0 (or c_5) can be easily calculated.

In the case of simple descent, which was the one first considered, we have nothing to do with c_0, but begin from c_1. Again, as both fertility and natural selection are in this case uniform, the values of f and s are infinite. Consequently our equations are reduced to

$$c_2 = r\,c_1;\quad c_4 = \sqrt{\{v^2 + c_2^2\}};\quad c_4 = c_1,$$

whence

$$c_1^2 = \frac{v^2}{1 - r^2}.$$

Suppose, for example, that $r = \frac{4}{5}$ and $v = 6$, then

$$c_1^2 = \frac{36}{1 - \frac{16}{25}} = \frac{36 \times 25}{9} = 100,$$

or

$$c_1 = 10,$$

as was mentioned in the course of the foregoing remarks.

[F. G.]

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PSYCHOMETRIC EXPERIMENTS.

BY FRANCIS GALTON, F.R.S.

PSYCHOMETRY, it is hardly necessary to say, means the art of imposing measurement and number upon operations of the mind, as in the practice of determining the reaction-time of different persons. I propose in this memoir to give a new instance of psychometry, and a few of its results. They may not be of any very great novelty or importance, but they are at least definite, and admit of verification; therefore I trust it requires no apology for offering them to the readers of this Journal, who will be prepared to agree in the view, that until the phenomena of any branch of knowledge have been subjected to measurement and number, it cannot assume the status and dignity of a science.

The processes of thought fall into two main categories: in the first of these, ideas present themselves by association either with some object newly perceived by the senses or with previous ideas; in the second process, such of the associated ideas are fixed and vivified by the attention, as happen to be germane to the topic on which the mind is set. In this memoir I do not deal with the second process at all, so I need not speak more in detail concerning it, but I address myself wholly to the first. It is an automatic one; the ideas arise of their own accord, and we cannot, except in indirect and imperfect ways, compel them to come.

My object is to show how the whole of these associated ideas, though they are for the most part exceedingly fleeting and obscure, and barely cross the threshold of our consciousness, may be seized, dragged into daylight, and recorded. I

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shall then treat the records of some experiments statistically, and will make out what I can from them.

I should be glad if the reader would refer to an article written by me in the 'Nineteenth Century' of last March, which was based on the observations I am about to describe. It travels somewhat further afield than the present memoir, but does not enter so much into details.

When we attempt to trace the first steps in each operation of our minds, we are usually baulked by the difficulty of keeping watch, without embarrassing the freedom of its action. The difficulty is much more than the common and well-known one of attending to two things at once. It is especially due to the fact that the elementary operations of the mind are exceedingly faint and evanescent, and that it requires the utmost painstaking to watch them properly. It would seem impossible to give the required attention to the processes of thought and yet to think as freely as if the mind had been in no way preoccupied. The peculiarity of the experiments I am about to describe is that I have succeeded in evading this difficulty. My method consists in allowing the mind to play freely for a very brief period, until a couple or so of ideas have passed through it, and then, while the traces or echoes of those ideas are still lingering in the brain, to turn the attention upon them with a sudden and complete awakening; to arrest, to scrutinise them, and to record their exact appearance. Afterwards I collate the records at leisure, and discuss them and draw conclusions. It must be understood that the second of the two ideas was never derived from the first, but always directly from the original object. This was ensured by absolutely withstanding all temptation to reverie. I do not mean that the first idea was of necessity a simple elementary thought: sometimes it was a glance down a familiar line of associations, sometimes it was a well-remembered mental attitude or mode of feeling, but I mean that it was never so far indulged in as to displace the object that had suggested it, from being the primary topic of attention.

I must add, that I found the experiments to be extremely trying and irksome, and that it required much resolution to go through with them, using the scrupulous care they demanded.

Nevertheless, the results well repaid the trouble. They gave me an interesting and unexpected view of the number of the operations of the mind, and of the obscure depths in which they took place, of which I had been little conscious before. The general impression they have left upon me is like that which many of us have experienced when the basement of our house happens to be under thorough sanitary repairs, and we realise for the first time the complex system of drains and gas- and water-pipes, flues, bell-wires, and so forth, upon which our comfort depends, but which are usually hidden out of sight, and of whose existence, so long as they acted well, we had never troubled ourselves.

The first experiments I made were imperfect, but sufficient to inspire me with keen interest in the matter, and suggested the form of procedure that I have already partly described. My first experiments were these. On several occasions, but notably on one when I felt myself unusually capable of the kind of effort required, I walked leisurely along Pall Mall, a distance of 450 yards, during which time I scrutinised with attention every successive object that caught my eyes, and I allowed my attention to rest on it until one or two thoughts had arisen through direct association with that object; then I took very brief mental note of them, and passed on to the next object. I never allowed my mind to ramble. The number of objects viewed was, I think, about 300, for I have subsequently repeated the same walk under similar conditions and endeavouring to estimate their number, with that result. It was impossible for me to recal in other than the vaguest way the numerous ideas that had passed through my mind; but of this, at least, I was sure, that samples of my whole life had passed before me, that many bygone incidents, which I never suspected to have formed part of my stock of thoughts, had been glanced at as objects too familiar to awaken the attention. I saw at once that the brain was vastly more active than I had previously believed it to be, and I was perfectly amazed at the unexpected width of the field of its everyday operations. After an interval of some days, during which I kept my mind from dwelling on my first experiences, in order that it might retain as much freshness as possible for a second experiment, I repeated the walk, and was struck just as much as before by the variety of the ideas that presented themselves, and the number of events to which they referred, about which I had never consciously occupied myself of late years. But my admiration at the activity of the mind was seriously diminished by another observation which I then made, namely that there had been a very great deal of repetition of thought. The actors in my mental stage were indeed very numerous, but by no means so numerous as I had imagined. They now seemed to be something like the actors in theatres where large processions are represented, who march off one side of the stage, and, going round by the back, come on again at the other. I accordingly cast about for means of laying hold of these fleeting thoughts, and, submitting them to statistical analysis, to find out more about their tendency to repetition and other matters, and the method I finally adopted was the one already mentioned. I selected a list of suitable words and wrote them on different small sheets of paper. Taking care to dismiss them from my thoughts when not engaged upon them, and allowing some days to elapse before I began to use them, I laid one of these sheets with all due precautions under a book, but not wholly covered by it, so that when I leant forward I could see one of the words, being previously quite ignorant of what the word would be. Also I held a small chronograph, which I started by pressing a spring the moment the word caught my eye, and which stopped of itself the instant I released the spring; and this I did so soon as about a couple of ideas in direct association with the word had arisen in my mind. I found that I could not manage to recollect more than two ideas with the needed precision, at least not in a general way; but sometimes several ideas occurred so nearly together that I was able to record three or even four of them, while sometimes I only managed one. The second ideas were, as I have already said, never derived from the first, but always direct from the word itself, for I kept my attention firmly fixed on the word, and the associated ideas were seen only by a half glance. When the two ideas had occurred, I stopped the chronograph and wrote them down, and the time they occupied. I soon got into the way of doing all this in a very methodical and automatic manner, keeping the mind perfectly calm and neutral, but intent and, as it were, at full cock and on hair trigger, before displaying the word. There was no disturbance occasioned by thinking of the imminent revulsion of the mind when the chronograph was stopped. My feeling before stopping it was simply that I had delayed long enough, and this in no way interfered with the free action of the mind. I found no trouble in ensuring the complete fairness of the experiment, by using a number of little precautions, hardly necessary to describe, that practice quickly suggested, but it was a most repugnant and laborious work, and it was only by strong self-control that I went through my schedule according to programme. The list of words that I finally secured was 75 in number, though I began with more. I went through them on four separate occasions, under very different circumstances, in England and abroad, and at intervals of about a month. In no case were the associations governed to any degree worth recording, by remembering what had occurred to me on previous occasions, for I found that the process itself had great influence in discharging the memory of what it had just been engaged in, and I of course took care between the experiments never to let my thoughts revert to the words. The results seem to me to be as trustworthy as any other statistical series that has been collected with equal care.

On throwing these results into a common statistical hotch-pot, I first examined into the rate at which these associated ideas were formed. It took a total time of 660 seconds to form the 505 ideas; that is at about the rate of 50 in a minute or 3000 in an hour. This would be miserably slow work in reverie, or wherever the thought follows the lead of each association that successively presents itself. In the present case, much time was lost in mentally taking the word in, owing to the quiet unobtrusive way in which I found it necessary to bring it into view, so as not to distract the thoughts. Moreover, a substantive standing by itself is usually the equivalent of too abstract an idea for us to conceive it properly without delay. Thus it is very difficult to get a quick conception of the word "carriage," because there are so many different kinds—two-wheeled, four-wheeled, open and closed, and all of them in so many different possible positions, that the mind possibly hesitates amid an obscure sense of many alternatives that cannot blend together. But limit the idea to, say, a landau, and the mental association declares itself more quickly. Say a landau coming down the street to opposite the door, and an image of many blended landaus that have done so, forms itself without the least hesitation.

Next, I found that my list of 75 words gone over 4 times, had given rise to 505 ideas and 13 cases of puzzle, in which nothing sufficiently definite to note occurred within the brief maximum period of about 4 seconds, that I allowed myself to any single trial. Of these 505, only 289 were different. The precise proportions in which the 505 were distributed in quadruplets, triplets, doublets or singles, is shown in the uppermost lines of Table I. The same facts are given under another form in the lower lines of the table, which show how the 289 different ideas were distributed in cases of fourfold, treble, double, or single occurrences.

TABLE I.

RECURRENT ASSOCIATIONS.

Total number of Associations.	Occurring in quadruplets.	triplets.	doublets.	singles.
505	116	108	114	167
per cent. . 100	23	21	23	33
Total number of different Associations.	**Occurring four times.**	**three times.**	**twice.**	**once.**
289	29	36	57	167
per cent. . 100	10	12	20	58

I was fully prepared to find much iteration in my ideas, but had little expected that out of every hundred words twenty-three would give rise to exactly the same association in every one of the four trials; twenty-one, to the same association in three out of the four, and so on, the experiments having been purposely conducted under very different conditions of time and local circumstances. This shows much less variety in the mental stock of ideas than I had expected, and makes us feel that the roadways of our minds are worn into very deep ruts. I conclude from the proved number of faint and barely conscious thoughts, and from the proved iteration of them, that the mind is perpetually travelling over familiar ways without our memory retaining any impression of its excursions. Its footsteps are so light and fleeting, that it is only by such experiments as I have described that we can learn anything about them. It is apparently always engaged in mumbling over its old stores, and if any one of these is wholly neglected for a while, it is apt to be forgotten, perhaps irrecoverably. It is by no means keen interest and attention when first observing an object, that fixes it in the recollection. We pore over the pages of a 'Bradshaw,' and study the trains for some particular journey with the greatest interest; but the event passes by, and the hours and other facts which we once so eagerly considered become absolutely forgotten. So in games of whist, and in a large number of similar instances. As I understand it, the subject must have a continued living interest in order to retain an abiding-place in the memory. The mind must refer to it frequently, but whether it does so consciously or unconsciously, is not perhaps a matter of much importance. Otherwise, as a general rule, the recollection sinks, and appears to be utterly drowned in the waters of Lethe.

The instances, according to my personal experience, are very rare, and even those are not very satisfactory, in which some event recalls a memory that had lain *absolutely* dormant for many years. In this very series of experiments, a recollection which I thought had entirely lapsed appeared under no less than three different aspects on different occasions. It was this: when I was a boy, my father, who was anxious that I should learn something of physical science, which was then never taught at school, arranged with the owner of a large chemist's shop to let me dabble at chemistry for a few days in his laboratory. I had not thought of this fact, so far as I was aware, for many years; but in scrutinising the fleeting associations called up by the various words, I traced two mental visual images (an alembic and a particular arrangement of tables and light), and one mental sense of smell (chlorine gas) to that very laboratory. I recognised that these images appeared familiar to me, but I had not thought of their origin. No doubt if some strange conjunction of circumstances had suddenly recalled those three associations at the same time, with perhaps two or three other collateral matters which may still be living in my memory, but which I do not as yet identify, a mental perception of startling vividness would be the result, and I should have falsely imagined that it had supernaturally, as it were, started into life from an entire oblivion extending over many years. Probably many persons would have registered such a case as evidence that things once perceived can never wholly vanish from the recollection, but that in the hour of death, or under some excitement, every event of a past life may reappear. To this view I entirely dissent. Forgetfulness appears absolute in the vast majority of cases, and our supposed recollections of a past life are, I believe, no more than that of a large number of episodes in it, to be reckoned in hundreds or thousands, certainly not in tens of hundreds of thousands, which have escaped oblivion. Every one of the fleeting, half-conscious thoughts which were the subject of my experiments admitted of being vivified by keen attention, or by some appropriate association; but I strongly suspect that ideas which have long since ceased to fleet through the brain, owing to the absence of current associations to call them up, disappear wholly. A comparison of old memories with a newly-met friend of one's boyhood, about the events we then witnessed together, shows how much we had each of us forgotten. Our recollections do not tally. Actors and incidents that seem to have been of primary importance in those events to the one, have been utterly forgotten by the other. The recollection of our earlier years are, in truth, very scanty, as any one will find who tries to enumerate them.

My associated ideas were for the most part due to my own unshared experiences, and the list of them would necessarily differ widely from that which another person would draw up who might repeat my experiments. Therefore one sees clearly, and I may say, one can see *measurably*, how impossible it is in a general way for two grown-up persons to lay their minds side by side together in perfect accord. The same sentence cannot produce precisely the same effect on both, and the first quick impressions that any given word in it may convey, will differ widely in the two minds.

I took pains to determine as far as feasible the dates of my life at which each of the associated ideas was first attached to the word. There were 124 cases in which identification was satisfactory, and they were distributed as in Table II.

TABLE II.

Relative number of Associations formed at different Periods of Life.

Total number of different Associations.		Occurring four times.		three times.		twice.		once.		Whose first formation was in
	per cent.		per cent.		per cent.		per cent.		per cent.	
48	39	12	10	11	9	9	7	16	13	boyhood and youth,
57	46	10	8	8	7	6	5	33	26	subsequent manhood,
19	15	—	—	4	3	1	1	14	11	quite recent events.
124	100	22	18	23	19	16	13	63	50	Totals.

It will be seen from the table that out of the 48 earliest associations no less than 12, or one quarter of them occurred in each of the four trials; of the 57 associations first formed in manhood, 10, or about one-sixth of them had a similar recurrence, but as to the 19 other associations first formed in quite recent times, not one of them occurred in the whole of the four trials. Hence we may see the greater fixity of the earlier associations, and might measurably determine the decrease of fixity as the date of their first formation becomes less remote.

The largeness of the number 33 in the fourth column, which disconcerts the run of the series, is wholly due to a visual memory of places seen in manhood. I will not speak about this now, as I shall have to refer to it further on. Neglecting, for the moment, this unique class of occurrences, it will be seen that one-half of the associations date from the period of life before leaving college; and it may easily be imagined that many of these refer to common events in an English education. Nay further, on looking through the list of all the associations it was easy to see how they are pervaded by purely English ideas, and especially such as are prevalent in that stratum of English society in which I was born and bred, and have subsequently lived. In illustration of this, I may mention an anecdote of a matter which greatly impressed me at the time. I was staying in a country house with a very pleasant party of young and old, including persons whose education and versatility were certainly not below the social average. One evening we played at a round game, which consisted in each of us drawing as absurd a scrawl as he or she could, representing some historical event; the pictures were then shuffled and passed successively from hand to hand, every one writing down independently their interpretation of the picture, as to what the historical event was that the artist intended to depict by the scrawl. I was astonished at the sameness of our ideas. Cases like Canute and the waves, the Babes in the Tower, and the like, were drawn by two and even three persons at the same time, quite independently of one another, showing how narrowly we are bound by the fetters of our early education. If the figures in the above table may be accepted as fairly correct for the world generally, it shows, still in a measurable degree, the large effect of early education in fixing our associations. It will of course be understood that I make no absurd profession of being able by these very few experiments to lay down statistical constants of universal application, but that my principal object is to show that a large class of mental phenomena, that have hitherto been too vague to lay hold of, admit of being caught by the firm grip of genuine statistical inquiry.

The results that I have thus far given are hotch-potch results. It is necessary to sort the materials somewhat, before saying more about them.

After several trials, I found that the associated ideas admitted of being divided into three main groups. First there is the imagined sound of words, as in verbal quotations or names of persons. This was frequently a mere parrot-like memory which acted instantaneously and in a meaningless way, just as a machine might act. In the next group there was every other kind of sense-imagery; the chime of imagined bells, the shiver of remembered cold, the scent of some particular locality, and, much more frequently than all the rest put together, visual imagery. The last of the three groups contains what I will venture, for want of a better name, to call "histrionic" representations. It includes those cases where I either act a part in imagination, or see in imagination a part acted, or, most commonly by far, where I am both spectator and all the actors at once, in an imaginary mental theatre. Thus I feel a nascent sense of some muscular action while I simultaneously witness a puppet of my brain—a part of myself—perform that action, and I assume a mental attitude appropriate to the occasion. This, in my case, is a very frequent way of generalising, indeed I rarely feel that I have secure hold of a general idea until I have translated it somehow into this form. Thus the word "abasement" presented itself to me, in one of my experiments, by my mentally placing myself in a pantomimic attitude of humiliation with half-closed eyes, bowed back, and uplifted palms, while at the same time I was aware of myself as of a mental puppet, in that position. This same word will serve to illustrate the other groups also. It so happened in connection with "abasement" that the word "David" or "King David" occurred to me on one occasion in each of three out of the four trials; also that an accidental misreading, or perhaps the merely punning association of the words "a basement," brought up on all four occasions the image of the foundations of a house that the builders had begun upon.

So much for the character of the association; next as to that of the words. I found, after the experiments were over, that the words were divisible into three distinct groups. The first contained "abbey," "aborigines," "abyss," and others that admitted of being presented under some mental image. The second group contained "abasement," "abhorrence," "ablution," &c., which admitted excellently of histrionic representation. The third group contained the more abstract words, such as "afternoon," "ability," "abnormal," which were variously and imperfectly dealt with by my mind. I give the results in the upper part of Table III., and, in order to save trouble, I have reduced them to percentages in the lower lines of the table.

TABLE III.

COMPARISON BETWEEN THE QUALITY OF THE WORDS AND THAT OF THE IDEAS IN IMMEDIATE ASSOCIATION WITH THEM.

Number of words in each series.		Sense imagery.	Histrionic.	Purely names of persons.	Verbal phrases and quotations.	Total.
26	"Abbey" series	46	12	32	17	107
20	"Abasement" "	25	26	11	17	79
29	"Afternoon" "	23	27	16	38	104
75						290
	"Abbey" series	43	11	30	16	100
	"Abasement" "	32	33	13	22	100
	"Afternoon" "	22	25	16	37	100

We see from this that the associations of the "abbey" series are nearly half of them in sense imagery, and these were almost always visual. The names of persons also more frequently occurred in this series than in any other. It will be recollected that in Table II. I drew attention to the exceptionally large number, 33, in the last column. It was perhaps 20 in excess of what would have been expected from the general run of the other figures. This was wholly due to visual imagery of scenes with which I was first acquainted after reaching manhood, and shows, I think, that the scenes of childhood and youth, though vividly impressed on the memory, are by no means numerous, and may be quite thrown into the background by the abundance of after experiences; but this, as we have seen, is not the case with the other forms of association. Verbal memories of old date, such as Biblical scraps, family expressions, bits of poetry, and the like, are very numerous, and rise to the thoughts so quickly, whenever anything suggests them, that they commonly outstrip all competitors. Associations connected with the "abasement" series are strongly characterised by histrionic ideas, and by sense-imagery, which to a great degree merges into a histrionic character. Thus the word "abhorrence" suggested to me, on three out of the four trials, an image of the attitude of Martha in the famous picture of the raising of Lazarus by Sebastian del Piombo in the National Gallery. She stands with averted head, doubly sheltering her face by her hands from even a sidelong view of the opened grave. Now I could not be sure how far I saw the picture as such, in my mental view, or how far I had thrown my own personality into the picture and was acting it as actors might act a mystery play, by the puppets of my own brain, that were parts of myself. As a matter of fact, I entered it under the heading of sense-imagery, but it might very properly have gone to swell the number of the histrionic entries.

The "afternoon" series suggested a great preponderance of mere catch-words, showing how slowly I was able to realise the meaning of abstractions; the phrases intruded themselves before the thoughts became defined. It occasionally occurred that I puzzled wholly over a word, and made no entry at all; in thirteen cases either this happened, or else after one idea had occurred the second was too confused and obscure to admit of record, and mention of it had to be omitted in the foregoing table. These entries have forcibly shown to me the great imperfection in my generalising powers; and I am sure that most persons would find the same if they made similar trials. Nothing is a surer sign of high intellectual capacity than the power of quickly seizing and easily manipulating ideas of a very abstract nature. Commonly we grasp them very imperfectly, and hold on to their skirts with great difficulty.

In comparing the order in which the ideas presented themselves, I find that a decided precedence is assumed by the Histrionic ideas, wherever they occur; that Verbal associations occur first and with great quickness on many occasions, but on the whole that they are only a little more likely to occur first than second; and that Imagery is decidedly more likely to be the second, than the first, of the associations called up by a word. In short, gesture-language appeals the most quickly to our feelings.

It would be very instructive to print the actual records at length, made by many experimenters, if the records could be clubbed together and thrown into a statistical form; but it would be too absurd to print one's own singly. They lay bare the foundations of a man's thoughts with curious distinctness, and exhibit his mental anatomy with more vividness and truth than he would probably care to publish to the world.

It remains to summarise what has been said in the foregoing memoir. I have desired to show how whole strata of mental operations that have lapsed out of ordinary consciousness, admit of being dragged into light, recorded and treated statistically, and how the obscurity that attends the initial steps of our thoughts can thus be pierced and dissipated. I then showed measurably the rate at which associations sprung up, their character, the date of their first formation, their tendency to recurrence, and their relative precedence. Also I gave an instance showing how the phenomenon of a long-forgotten scene, suddenly starting into consciousness, admitted in many cases of being explained. Perhaps the strongest of the impressions left by these experiments regards the multifariousness of the work done by the mind in a state of half-unconsciousness, and the valid reason they afford for believing in the existence of still deeper strata of mental operations, sunk wholly below the level of consciousness, which may account for such mental phenomena as cannot otherwise be explained. We gain an insight by these experiments into the marvellous number and nimbleness of our mental associations, and we also learn that they are very far indeed from being infinite in their variety. We find that our working stock of ideas is narrowly limited, but that the mind continually recurs to them in conducting its operations, therefore its tracks necessarily become more defined and its flexibility diminished as age advances.

f. 8

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F. 1

Reprint with additions from p. 61 of "Chemistry, Biology, &c" volume of the South Kensington Conferences in 1876 (in connection with Loan Collection Scient. Instr.)
See also Catalogue of the Loan Collection No 689, page 178.

GALTON'S WHISTLES

For determining the upper limits of audible sound in different persons,

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f. 2r

Whistles for determing the upper limits of audible sound in different individuals,

BY

FRANCIS GALTON, F.R.S.

The base of the inner tube of the whistle is the foremost end of a plug, that admits of being advanced or withdrawn by screwing it out or in; thus the depth of the inner tube of the whistle can be varied at pleasure. The more nearly the plug is screwed home, the less is the depth of the whistle and the more shrill does its note become, until a point is reached at which, although the air that proceeds from it vibrates as violently as before, its note ceases to be audible.

The number of vibrations per second in the note of a whistle or other " closed pipe " depends on its depth. The theory of acoustics shews that the length of each complete vibration is four times that of the depth of the closed pipe, and since experience proves that all sound, whatever may be its pitch, is propagated at the same rate, which under ordinary conditions of temperature and barometric pressure may be taken at 1120 feet, or 13440 inches, per second,—it follows that the number of vibrations in the note of a whistle may be found by dividing 13440 by four times the depth, (measured in inches) of the inner tube of the whistle. This rule however supposes the vibrations of the air in the tube to be strictly longitudinal, and ceases to apply when the depth of the tube is less than about one-and-a-half times its

f. 2v

diameter. When the tube is reduced to a shallow pan, a note may still be produced by it, but that note has reference rather to the diameter of the whistle than to its depth, being sometimes apparently unaltered by a further decrease of depth. The necessity of preserving a fair proportion between the diameter and the depth of a whistle is the reason why these instruments having necessarily little depth, are made with such small bores.

The depth of the inner tube of the whistle at any moment, is shewn by the graduations on the outside of the instrument, The lower portion of the instrument is a cap that surrounds the body of the whistle, and is itself fixed to the screw that forms the plug. One turn of the cap increases or diminishes the depth of the whistle, by an amount equal to the interval between two threads of the screw. For mechanical convenience, a screw is used whose pitch is 25 to the inch, therefore one complete turn of the cap moves the plug one twenty-fifth of an inch, or ten two hundred-and-fiftieths. The edge of the cap is divided into ten parts, each of which corresponds to the tenth of a complete turn; and, therefore, to one two-hundred-and-fiftieth of an inch. Hence in reading off the graduations the tens are shewn on the body of the whistle, and the units are shewn on the edge of the cap.

The scale of the instrument having for its unit the two-hundred-and-fiftieth part of an inch, it follows that the number of vibrations in the note of the whistle is to be found by dividing $\frac{13440 \times 250}{4}$ or 84000, by the graduations read off on its scale.

A short table is annexed, giving the number of vibrations calculated by this formula, for different depths, bearing in mind that the upper lines cannot be relied upon unless the whistle has a very minute bore, and consequently a very feeble note. The largest whistles suitable for experiments on the human ear, have an inner tube of about 0.16 inches in diameter which is equal to 40 units of the scale. Consequently in these instruments the theory of closed pipes ceases to be reliable when the depth of the whiste is less than about 60 units. In short, we cannot be sure of sounding with them a higher note than one of 14000 vibrations to the second.

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Scale Readings (One division = $\frac{1}{250}$ of an inch.)	Corresponding number of Vibrations per Second.
10	84000
15	56000
20	42000
25	33600
30	28000
35	24000
40	21000
45	18666
50	16800
55	15273
60	14000
65	12923
70	12000
75	11200
80	10500
85	9882
90	9333
95	8842
100	8400
105	8000
110	7591
115	7305
120	7000
125	6720
130	6461

The following popular account of these instruments was given at one of the Conferences held in connection with the Loan Collection of Scientific Instruments at South Kensington in 1876. It is reprinted with a few verbal corrections, from the published Reports of those Conferences. (Physical Science Section, p. 61).

Mr. Galton: I thought it would be of convenience to experimenters, that I should exhibit some little instruments I have contrived for ascertaining what the upper limits of audible sound may be in different persons of the same race, and in individuals of different races, and in different kinds of animals. It is, of course, a matter of great interest to know whether insects and such small creatures can hear sounds, and can in any sense of the word, converse in language which to our ears is utterly inaudible. When I first desired to make experiments, I was checked by the great difficulty of finding instruments that vibrated with sufficient rapidity for the purpose in question. Dr. Wollaston (to whom we are indebted for the first experiments ever made on this subject, and for the knowledge of the fact that vibrations exist which the ear is incompetent to seize and render into sound) found very great difficulty in making his small pipes. I tried several plans for obtaining acute notes, and the one I finally adopted was this: I made a very small whistle, whose internal diameter was much less than one-tenth of an inch—I have many such here, made for me by Messrs. Tisley and Spiller, Opticians, 172, Brompton-road,—with a plug at the bottom, which plug is screwed up by a graduated screw. The graduations are marked on the side so that when you use the instrument you know the depth of the tube, and knowing what that is, it is a matter of calculation to learn the rate of vibration. There is, however, a good deal of uncertainty in the matter, because there must be some fair proportion between the length and width of the tube in order that the calculations should give a correct result. A short whistle with a diameter exceeding two-thirds of its length, will certainly not give a note whose shrillness is governed wholly by its shortness. Therefore in some of my experiments I was driven to use very fine tubes indeed, not wider than those little glass tubes that hold the smallest leads for Mordan's pencils. It occurred to me, in order to produce a note that should be both shrill and powerful, and so correspond to a battery of small whistles, that a simple plan would be to take a piece of brass tube and flatten it, and pass another sheet of brass up it, and thus form a whistle the whole width of the sheet, but of very small diameter from front to back. I have such a whistle here, it makes a powerful note, but not a very pure one. I also made an annular whistle by means of three cylinders, one sliding within the other two, and graduated as before. I find that when the limits of audibility are approached, the sound becomes much fainter, and when that limit is reached, the sound usually gives place to a peculiar sensation, which is not sound but more like dizziness, and which some persons experience to a high degree. I am afraid it is of little use attempt-

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Duplicate

GALTON'S WHISTLES

For determining the upper limits of audible sound in different persons,

SOLD BY

S. C. TISLEY & Co.,

172, BROMPTON ROAD, LONDON, S.W.

Printed by H. & W. Brown, at 261, Brompton Road, S.W

f. 6r

Whistles for determing the upper limits of audible sound in different individuals,

BY

FRANCIS GALTON, F.R.S.

The base of the inner tube of the whistle is the foremost end of a plug, that admits of being advanced or withdrawn by screwing it out or in; thus the depth of the inner tube of the whistle can be varied at pleasure. The more nearly the plug is screwed home, the less is the depth of the whistle and the more shrill does its note become, until a point is reached at which, although the air that proceeds from it vibrates as violently as before, its note ceases to be audible.

The number of vibrations per second in the note of a whistle or other " closed pipe " depends on its depth. The theory of acoustics shews that the length of each complete vibration is four times that of the depth of the closed pipe, and since experience proves that all sound, whatever may be its pitch, is propagated at the same rate, which under ordinary conditions of temperature and barometric pressure may be taken at 1120 feet, or 13440 inches, per second,—it follows that the number of vibrations in the note of a whistle may be found by dividing 13440 by four times the depth, (measured in inches) of the inner tube of the whistle. This rule however supposes the vibrations of the air in the tube to be strictly longitudinal, and ceases to apply when the depth of the tube is less than about one-and-a-half times its

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diameter. When the tube is reduced to a shallow pan, a note may still be produced by it, but that note has reference rather to the diameter of the whistle than to its depth, being sometimes apparently unaltered by a further decrease of depth. The necessity of preserving a fair proportion between the diameter and the depth of a whistle is the reason why these instruments having necessarily little depth, are made with such small bores.

The depth of the inner tube of the whistle at any moment, is shewn by the graduations on the outside of the instrument, The lower portion of the instrument is a cap that surrounds the body of the whistle, and is itself fixed to the screw that forms the plug. One turn of the cap increases or diminishes the depth of the whistle, by an amount equal to the interval between two threads of the screw. For mechanical convenience, a screw is used whose pitch is 25 to the inch, therefore one complete turn of the cap moves the plug one twenty-fifth of an inch, or ten two hundred-and-fiftieths. The edge of the cap is divided into ten parts, each of which corresponds to the tenth of a complete turn; and, therefore, to one two-hundred-and-fiftieth of an inch. Hence in reading off the graduations the tens are shewn on the body of the whistle, and the units are shewn on the edge of the cap.

The scale of the instrument having for its unit the two-hundred-and-fiftieth part of an inch, it follows that the number of vibrations in the note of the whistle is to be found by dividing $\frac{13440 \times 250}{4}$ or 84000, by the graduations read off on its scale.

A short table is annexed, giving the number of vibrations calculated by this formula, for different depths, bearing in mind that the upper lines cannot be relied upon unless the whistle has a very minute bore, and consequently a very feeble note. The largest whistles suitable for experiments on the human ear, have an inner tube of about 0.16 inches in diameter which is equal to 40 units of the scale. Consequently in these instruments the theory of closed pipes ceases to be reliable when the depth of the whiste is less than about 60 units. In short, we cannot be sure of sounding with them a higher note than one of 14000 vibrations to the second.

Scale Readings (One division $= \frac{1}{250}$ of an inch.)	Corresponding number of Vibrations per Second.
10	84000
15	56000
20	42000
25	33600
30	28000
35	24000
40	21000
45	18666
50	16800
55	15273
60	14000
65	12923
70	12000
75	11200
80	10500
85	9882
90	9333
95	8842
100	8400
105	8000
110	7591
115	7305
120	7000
125	6720
130	6461

The following popular account of these instruments was given at one of the Conferences held in connection with the Loan Collection of Scientific Instruments at South Kensington in 1876. It is reprinted with a few verbal corrections, from the published Reports of those Conferences. (Physical Science Section, p. 61).

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1877

ADDRESS

TO THE

ANTHROPOLOGICAL DEPARTMENT

OF THE

BRITISH ASSOCIATION,

PLYMOUTH, 1877.

BY FRANCIS GALTON, F.R.S.

LONDON:
PRINTED BY WILLIAM CLOWES AND SONS, STAMFORD STREET
AND CHARING CROSS.
1877.

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ADDRESS.

PERMIT me to say a few words of personal explanation to account for the form of the Address I am about to offer. It has been the custom of my predecessors to give an account of recent proceedings in Anthropology, and to touch on many branches of that wide subject. But I am at this moment unprepared to follow their example with the completeness I should desire and you have a right to expect, owing to the suddenness with which I have been called upon to occupy this chair. I had indeed the honour of being nominated to the post last spring, but circumstances arising which made it highly probable that I should be prevented from attending this meeting, I was compelled to ask to be superseded. New arrangements were then made by the Council, and I thought no more about the matter. However, at the last moment, the accomplished ethnologist who otherwise would have presided over you was himself debarred by illness from attending, and the original plan had to be reverted to.

Under these circumstances I thought it best to depart somewhat from the usual form of Addresses, and to confine myself to certain topics with which I happen to have been recently engaged, even at the risk of incurring the charge of submitting to you a memoir, rather than an address.

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I propose to speak of the study of those groups of men who are sufficiently similar in their mental characters or in their physiognomy, or in both, to admit of classification; and I especially desire to shew that many methods exist of pursuing the inquiry in a strictly scientific manner, although it has hitherto been too often conducted with extreme laxity.

The types of character of which I speak, are such as those described by Theophrastus, La Bruyére and others, or such as may be read of in ordinary literature and are universally recognised as being exceedingly true to nature. There are no worthier professors of this branch of Anthropology than the writers of the higher works of fiction, who are ever on the watch to discriminate varieties of character, and who have the art of describing them. It would, I think, be a valuable service to Anthropology, if some person well versed in literature were to compile a volume of extracts from novels and plays that should illustrate the prevalent types of human character and temperament. What, however, I especially wish to point out, is that it has of late years become possible to pursue an inquiry into certain fundamental qualities of the mind by the aid of exact measurements. Most of you are aware of the recent progress of what has been termed Psycho-physics, or the science of subjecting mental processes to physical measurements and to physical laws. I do not now propose to speak of the laws that have been deduced, such as that which is known by the name of Fechner, and its numerous offshoots, including the law of fatigue, but I will briefly allude to a few instances of measurement of mental processes, merely to recall them to your memory. They will shew what I desire to lay stress upon, that the very foundations of the differences between the mental qualities of man and man admit of being gauged by a scale of inches and a clock.

Take, for example, the rate at which a sensation or a volition travels along the nerves, which has been the subject of numerous beautiful experiments. We now know that it is far from instantaneous, having indeed no higher velocity than that of a railway express train. This slowness of pace, speaking relatively to the requirements that the nerves have to fulfil, is quite sufficient to account for the fact that very small animals are quicker than very large ones in evading rapid blows, and for the other fact that the eye and the ear are situated in almost all animals in the head, in order that as little time as possible should be lost on the road, in transmitting their impressions to the brain. Now the velocity of the complete process of to and fro nerve transmission in persons of different temperaments has not been yet ascertained with the desired precision. Such difference as there may be is obviously a fundamental characteristic and one that well deserves careful examination. I may take this opportunity of suggesting a simple inquiry that would throw much light on the degree in which the velocity varies in different persons, and how far it is corrêlated with temperament and external physical characteristics. Before I describe the inquiry I suggest, and towards which I have already collected a few data, it is necessary that I should explain the meaning of a term in common use among astronomers, namely "personal equation." It is a well known fact that different observers make different estimates of the exact moment of the occurrence of any event. There is a common astronomical observation, in which the moment has to be recorded at which a star that is travelling athwart the field of view of a fixed telescope, crosses the fine vertical wire by which that field of view is intersected. In making this observation it is found that some observers are over sanguine and anticipate the event, while others are sluggish and allow the event to pass by before they succeed in noting it. This is by no means the effect of inexperience or maladroitness, but it is a persistent characteristic of each individual, however practised in the art of making observations or however attentive he may be. The difference between the time of a man's noting the event and that of its actual occurrence is called his personal equation. It remains curiously constant in every case for successive years, it is carefully ascertained for every assistant in every observatory, it is published along with his observations, and is applied to them just as a correction would be applied to measurements made by a foot rule, that was known to be too long or too short by some definite amount. Therefore the magnitude of a man's personal equation indicates a very fundamental peculiarity of his constitution; and the inquiry I would suggest, is to make a comparison of the age, height, weight, colour of hair and eyes, and temperament (so far as it may admit of definition) in each observer in the various observatories at home and abroad, with the amount of his personal equation. We should thus learn how far the more obvious physical characteristics may be correlated with certain mental ones, and we should perhaps obtain a more precise scale of temperaments than we have at present.

Another subject of exact measurement is the time occupied in forming an elementary judgment. If a simple signal be suddenly shewn, and if the observer presses a stop as quickly as he can when he sees it, some little time will certainly be lost, owing to delay in nerve transmission and to the sluggishness of the mechanical apparatus. In making experiments on the rate of judgment, the amount of this interval is first ascertained. Then the observer prepares himself for the exhibition of a signal that may be either black or white, but he is left ignorant which of the two it will be. He is to press a stop with his right hand in the first event, and another stop with his left hand in the second one. The trial is then made, and a much longer interval is found to have elapsed between the exhibition of the alternative signal, and the record of it, than had elapsed when a simple signal was used. There has been hesitation and delay: in short, the simplest act of judgment is found to consume a definite time. It is obvious that here, again, we have means of ascertaining differences in the rapidity of forming elementary judgments and of classifying individuals accordingly.

It would be easy to pursue the subject of the measurement of mental qualities to considerable length, by describing other kinds of experiment, for they are numerous and varied. Among these is the plan of Professor Jevons, of suddenly exhibiting an unknown number of beans in a box, and requiring an estimate of their number to be immediately called out. A comparison of the estimate with the fact, in a large number of trials, brought out a very interesting scale of the accuracy of such estimates, which would of course vary in different individuals, and might be used as a means of classification. I can imagine few greater services to Anthropology than the collection of the various experiments that have been imagined to reduce the faculties of the mind to exact measurement. They have engaged the attention of the highest philosophers, but have never, so far as I am aware, been brought compendiously together, and have certainly not been introduced, as they deserve, to general notice.

Wherever we are able to perceive differences by intercomparison, we may reasonably hope that we may at some future time succeed in submitting those differences to measurement. The history of science is the history of such triumphs. I will ask your attention to a very notable instance of this, namely, that of the establishment of the scale of the thermometer. You are aware that the possibility of making a standard thermometric scale wholly depends upon that of determining two fixed points of temperature, the interval between them being graduated into a scale of equal parts. These points are, I need hardly say, the temperatures of freezing and of boiling water respectively. On this basis we are able to record temperature with minute accuracy, and the power of doing so has been one of the most important aids to

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Physics and Chemistry as well as to other branches of investigation. We have been so accustomed, from our childhood, to hear of degrees of temperature, and our scientific knowledge is so largely based upon exact thermometric measurement, that we cannot easily realise the state of science when the thermometer, as we now use it, was unknown. Yet such was the condition of affairs so recently as two hundred years ago, or thereabouts. The invention of the thermometer, in its present complete form, was largely due to Boyle, and I find in his Memoirs (London, 1772, vol. vi. p. 403) a letter that cannot fail to interest us, since it well expresses the need of exact measurement that was then felt in a particular case, where it was soon eminently well supplied, and therefore encourages hope that our present needs as Anthropologists may hereafter, in some way or other, be equally well satisfied. The letter is from Dr. John Beale, a great friend and correspondent of Boyle, and is dated February, 1663. He says in it:—

"I see by several of my own thermometers, that the glass-men are by you so well instructed to make the stems in equal proportions, that if we could name some degrees, . . . we might by the proportions of the glass make our discourses intelligible in mentioning what degrees of cold our greatest frosts do produce. . . . If we can discourse of heat and cold in their several degrees, so as we may signify the same intelligibly, . . . it is more than our forefathers have taught us to do hitherto."

The principal experiments by which the mental faculties may be measured require, unfortunately for us, rather costly and delicate apparatus, and until physiological laboratories are more numerous than at present, we can hardly expect that they will be pursued by many persons.

Let us now suppose that, by one or more of the methods I have described or alluded to, we have succeeded in obtaining a group of persons resembling one another in some mental quality, and that we desire to determine the external physical characteristics and features most commonly associated with it. I have nothing new to say as regards the usual anthropometric measurements, but I wish to speak of the great convenience of photographs in conveying those subtle but clearly visible peculiarities of outline which almost elude measurement. It is strange that no use is made of photography to obtain careful studies of the head and features. No single view can possibly exhibit the whole of a solid, but we require for that purpose views to be taken from three points at right angles to one another. Just as the architect requires to know the elevation, side view and plan of a house, so the Anthropologist ought to have the full face, profile, and view of the head from above, of the individual whose features he is studying.

It might be a great convenience, when numerous portraits have to be rapidly and inexpensively taken for the purpose of anthropological studies, to arrange a solid framework supporting three mirrors, that shall afford the views of which I have been speaking, by reflexion, at the same moment that the direct picture of the sitter is taken. He would present a three-quarter face to the camera for the direct picture, one adjacent mirror would reflect his profile towards it, another on the opposite side would reflect his full face, and a third sloping over him would reflect the head as seen from above. All the reflected images would lie at the same optical distance from the camera, and would, therefore, be on the same scale, but they would be on a somewhat smaller scale than the picture taken directly. The result would be an ordinary photographic picture of the sitter surrounded by three different views of his head. Scales of inches attached to the framework would appear in the picture and give the means of exact measurement.

Having obtained drawings or photographs of several persons alike in most respects but differing in minor details, what sure method is there of extracting the typical characteristics from them? I may mention a plan which had occurred both to Mr. Herbert Spencer and myself, the principle of which is to superimpose optically the various drawings and to accept the aggregate result. Mr. Spencer suggested to me in conversation that the drawings reduced to the same scale might be traced on separate pieces of transparent paper and secured one upon another, and then held between the eye and the light. I have attempted this with some success. My own idea was to throw faint images of the several portraits, in succession, upon the same sensitised photographic plate. I may add that it is perfectly easy to superimpose optically two portraits by means of a stereoscope, and that a person who is used to handle instruments will find a common double eyeglass fitted with stereoscopic lenses to be almost as effectual and far handier than the boxes sold in shops.

In illustration of what I have said about photographic portraits, I will allude to some recent experiences of my own in a subject that I have still under consideration. In previous publications I have treated of men who have been the glory of mankind, I would now call your attention to those who are its disgrace. The particular group of men I have in view are the criminals of England, who have been condemned to long terms of penal servitude for various heinous offences.

It is needless to enlarge on the obvious fact that many persons have become convicts who, if they had been afforded the average chances of doing well, would have lived up to a fair standard of virtue. Neither need I enlarge on the other equally obvious fact, that a very large number of men escape criminal punishment, who in reality deserve it quite as much as an average convict. Making every allowance for these two elements of uncertainty, no reasonable man can entertain a doubt that the convict class includes a large proportion of consummate scoundrels, and that we are entitled to expect to find in any large body of convicts a prevalence of the truly criminal characteristics, whatever these may be.

Criminality, though not very various in its development, is extremely complex in its origin; nevertheless, certain general conclusions are arrived at by the best writers on the subject, among whom I would certainly rank Prosper Despine. The ideal criminal has three peculiarities of character: his conscience is almost deficient, his instincts are vicious, and his power of self-control is very weak. As a consequence of all this, he usually detests continuous labour. This statement applies to the criminal classes generally; the special conditions that determine the description of crime being the character of the instincts; and the fact of the absence of self-control being due to ungovernable temper, or to passion, or to mere imbecility.

The deficiency of conscience in criminals, as shewn by the absence of genuine remorse for their guilt, appears to astonish all who first become familiar with the details of prison life. Scenes of heartrending despair are hardly ever witnessed among prisoners; their sleep is broken by no uneasy dreams—on the contrary, it is easy and sound; they have also excellent appetites. But hypocrisy is a very common vice; and all my information agrees in one particular, as to the utter untruthfulness of criminals, however plausible their statements may appear to be.

The subject of vicious instincts is a very large one: we must guard ourselves against looking upon them as perversions, inasmuch as they may be strictly in accordance with the healthy nature of the man, and, being transmissible by inheritance, may become the normal characteristics of a healthy race, just as the sheep-dog, the retriever, the pointer, and the bull-dog have their several instincts. There can be no greater popular error than the supposition that natural instinct is a perfectly trustworthy guide, for there are striking contradictions to such an opinion in individuals of every description of animal. All that we are entitled to say is, that the prevalent instincts of each race are trustworthy, not those of every individual. A man who is counted as an atrocious criminal by society, and is punished as such by the law, may nevertheless have acted in strict accordance with his instincts. The ideal criminal is deficient in qualities that oppose his vicious instincts; he has neither the natural regard for others which lies at the base of conscience, nor has he sufficient self-control to enable him to consider his own selfish interests in the long run. He cannot be preserved from criminal misadventure, either by altruistic or by intelligently egoistic sentiments.

It becomes an interesting question to know how far these peculiarities may be correlated with physical characteristics and features. Through the cordial and ready assistance of Sir Edmund Du Cane, the Surveyor-General of Prisons, who has himself contributed a valuable memoir to the Social Science Congress on the subject, I was enabled to examine the many thousand photographs of criminals that are preserved for purposes of identification at the Home Office, to visit prisons and confer with the authorities, and lastly to procure for my own private statistical inquiries a large number of copies of photographs of heinous criminals. I may as well say, that I begged that the photographs should be furnished me without any names attached to them, but simply classified in three groups according to the nature of the crime. The first group included murder, manslaughter, and burglary; the second group included felony and forgery; and the third group referred to sexual crimes. The photographs were of criminals who had been sentenced to long terms of penal servitude.

By familiarizing myself with the collection, and continually sorting the photographs in tentative ways, certain natural classes began to appear, some of which are exceedingly well marked. It was also very evident that the three groups of criminals contributed in very different proportions to the different physiognomic classes.

This is not the place to go further into details: indeed my inquiry is far from complete. I merely quote my experiences in order to shew the way in which questions of character, physiognomy and temperament admit of being scientifically approached, and to give an instance of the helpfulness of photography. If I had had the profiles and the shape of the head as seen from above, my results would have been much more instructive. Thus, to take a single instance, I have seen many pencil studies in outline of selected criminal faces drawn by Dr. Clarke, the accomplished and zealous medical officer of Pentonville Prison; and in these profile sketches, a certain very characteristic profile seemed to me conspicuously prevalent. I should have been very glad of photographs to corroborate this. So, again, if I had had photographic views of the head taken from above, I could have tested, among other matters, the truth of Professor Benedict's assertion about the abnormally small size of the back of the head in criminals.

I have thus far spoken of the characters and physiognomy of well-marked varieties of men: the Anthropologist has next to consider the life history of those varieties, and especially their tendency to perpetuate themselves, whether to displace other varieties and to spread, or else to die out. In illustration of this, I will proceed with what appears to be the history of the criminal class. Its perpetuation by heredity is a question that deserves more careful investigation than it has received; but it is on many accounts more difficult to grapple with than it may at first sight appear to be. The vagrant habits of the criminal classes, their illegitimate unions and extreme untruthfulness, are among the difficulties. It is, however, easy to shew that the criminal nature tends to be inherited; while, on the other hand, it is impossible that women who spend a large portion of the best years of their life in prison can contribute many children to the population. The true state of the case appears to be that the criminal population receives steady accessions from classes who, without having strongly marked criminal natures, do nevertheless belong to a type of humanity that is exceedingly ill suited to play a respectable part in our modern civilization, though well suited to flourish under half-savage conditions, being naturally both healthy and prolific. These persons are apt to go to the bad; their daughters consort with criminals, and become the parents of criminals. An extraordinary example of this is given by the history of the infamous Jukes family in America, whose pedigree has been made out, with extraordinary care, during no less than seven generations, and is the subject of an elaborate memoir printed in the thirty-first annual report of the Prison Association of New York, 1876. It includes no less than 540 individuals of Jukes blood; among whom the number of persons who degraded into criminality, pauperism, or disease, is frightful to contemplate.

It is difficult to summarise the results in a few plain figures, but I will state those respecting the 5th generation, through the eldest of the five prolific daughters of the man who is the common ancestor of the race. The total number of these was 103, of whom 38 came through an illegitimate granddaughter, and 85 through legitimate grandchildren. Out of the 38, 16 have been in jail, 6 of them for heinous offences, one of these having been committed no less than nine times; 11 others were paupers or led openly disreputable lives; 4 were notoriously intemperate; the history of 3 had not been traced, and only 4 were known to have done well. The great majority of the women consorted with criminals. As to the 85 legitimate descendants, they were less flagrantly bad, for only 5 of them had been in jail and only 13 others had been paupers. Now the ancestor of all this mischief, who was born about the year 1730, is described as having been a hunter and a fisher, a jolly companionable man, averse to steady labour, working hard and idling by turns, and who had numerous illegitimate children, whose issue has not been traced. He was, in fact, a somewhat good specimen of a half-savage, without any seriously

f. 8

criminal instincts. The girls were apparently attractive, marrying early and sometimes not badly; but the gipsy-like character of the race was unsuited to success in a civilised country. So the descendants went to the bad, and the hereditary moral weaknesses they may have had, rose to the surface and worked their mischief without a check. Cohabiting with criminals, and being extremely prolific, the result was the production of a stock exceeding 500 in number, of a prevalent criminal type. Through disease and intemperance the breed is now rapidly diminishing; the infant mortality has of late been horrible among them, but fortunately the women of the present generation bear usually but few children and many of them are altogether childless.

This is not the place to go further into details. I have alluded to the Jukes family in order to shew what extremely important topics lie open to inquiry in a single branch of anthropological research and to stimulate others to follow it out. There can be no more interesting subject to us than the quality of the stock of our countrymen and of the human race generally, and there can be no more worthy inquiry than that which leads to an explanation of the conditions under which it deteriorates or improves.

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ADDRESS

TO THE

ANTHROPOLOGICAL DEPARTMENT

OF THE

BRITISH ASSOCIATION,

PLYMOUTH, 1877.

BY FRANCIS GALTON, F.R.S.

LONDON:
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ADDRESS.

Permit me to say a few words of personal explanation to account for the form of the Address I am about to offer. It has been the custom of my predecessors to give an account of recent proceedings in Anthropology, and to touch on many branches of that wide subject. But I am at this moment unprepared to follow their example with the completeness I should desire and you have a right to expect, owing to the suddenness with which I have been called upon to occupy this chair. I had indeed the honour of being nominated to the post last spring, but circumstances arising which made it highly probable that I should be prevented from attending this meeting, I was compelled to ask to be superseded. New arrangements were then made by the Council, and I thought no more about the matter. However, at the last moment, the accomplished ethnologist who otherwise would have presided over you was himself debarred by illness from attending, and the original plan had to be reverted to.

Under these circumstances I thought it best to depart somewhat from the usual form of Addresses, and to confine myself to certain topics with which I happen to have been recently engaged, even at the risk of incurring the charge of submitting to you a memoir, rather than an address.

I propose to speak of the study of those groups of men who are sufficiently similar in their mental characters or in their physiognomy, or in both, to admit of classification; and I especially desire to shew that many methods exist of pursuing the inquiry in a strictly scientific manner, although it has hitherto been too often conducted with extreme laxity.

The types of character of which I speak, are such as those described by Theophrastus, La Bruyére and others, or such as may be read of in ordinary literature and are universally recognised as being exceedingly true to nature. There are no worthier professors of this branch of Anthropology than the writers of the higher works of fiction, who are ever on the watch to discriminate varieties of character, and who have the art of describing them. It would, I think, be a valuable service to Anthropology, if some person well versed in literature were to compile a volume of extracts from novels and plays that should illustrate the prevalent types of human character and temperament. What, however, I especially wish to point out, is that it has of late years become possible to pursue an inquiry into certain fundamental qualities of the mind by the aid of exact measurements. Most of you are aware of the recent progress of what has been termed Psycho-physics, or the science of subjecting mental processes to physical measurements and to physical laws. I do not now propose to speak of the laws that have been deduced, such as that which is known by the name of Fechner, and its numerous offshoots, including the law of fatigue, but I will briefly allude to a few instances of measurement of mental processes, merely to recall them to your memory. They will shew what I desire to lay stress upon, that the very foundations of the differences between the mental qualities of man and man admit of being gauged by a scale of inches and a clock.

Take, for example, the rate at which a sensation or a volition travels along the nerves, which has been the subject of numerous beautiful experiments. We now know that it is far from instantaneous, having indeed no higher velocity than that of a railway express train. This slowness of pace, speaking relatively to the requirements that the nerves have to fulfil, is quite sufficient to account for the fact that very small animals are quicker than very large ones in evading rapid blows, and for the other fact that the eye and the ear are situated in almost all animals in the head, in order that as little time as possible should be lost on the road, in transmitting their impressions to the brain. Now the velocity of the complete process of to and fro nerve transmission in persons of different temperaments has not been yet ascertained with the desired precision. Such difference as there may be is obviously a fundamental characteristic and one that well deserves careful examination. I may take this opportunity of suggesting a simple inquiry that would throw much light on the degree in which the velocity varies in different persons, and how far it is correlated with temperament and external physical characteristics. Before I describe the inquiry I suggest, and towards which I have already collected a few data, it is necessary that I should explain the meaning of a term in common use among astronomers, namely "personal equation." It is a well known fact that different observers make different estimates of the exact moment of the occurrence of any event. There is a common astronomical observation, in which the moment has to be recorded at which a star that is travelling athwart the field of view of a fixed telescope, crosses the fine vertical wire by which that field of view is intersected. In making this observation it is found that some observers are over sanguine and anticipate the event, while others are sluggish and allow the event to pass by before they succeed in noting it. This is by no means the effect of inexperience or maladroitness, but it is a persistent characteristic of each individual, however practised in the art of making observations or however attentive he may be. The difference between the time of a man's noting the event and that of its actual occurrence is called

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his personal equation. It remains curiously constant in every case for successive years, it is carefully ascertained for every assistant in every observatory, it is published along with his observations, and is applied to them just as a correction would be applied to measurements made by a foot rule, that was known to be too long or too short by some definite amount. Therefore the magnitude of a man's personal equation indicates a very fundamental peculiarity of his constitution; and the inquiry I would suggest, is to make a comparison of the age, height, weight, colour of hair and eyes, and temperament (so far as it may admit of definition) in each observer in the various observatories at home and abroad, with the amount of his personal equation. We should thus learn how far the more obvious physical characteristics may be correlated with certain mental ones, and we should perhaps obtain a more precise scale of temperaments than we have at present.

Another subject of exact measurement is the time occupied in forming an elementary judgment. If a simple signal be suddenly shewn, and if the observer presses a stop as quickly as he can when he sees it, some little time will certainly be lost, owing to delay in nerve transmission and to the sluggishness of the mechanical apparatus. In making experiments on the rate of judgment, the amount of this interval is first ascertained. Then the observer prepares himself for the exhibition of a signal that may be either black or white, but he is left ignorant which of the two it will be. He is to press a stop with his right hand in the first event, and another stop with his left hand in the second one. The trial is then made, and a much longer interval is found to have elapsed between the exhibition of the alternative signal, and the record of it, than had elapsed when a simple signal was used. There has been hesitation and delay: in short, the simplest act of judgment is found to consume a definite time. It is obvious that here, again, we have means of ascertaining differences in the rapidity of forming elementary judgments and of classifying individuals accordingly.

It would be easy to pursue the subject of the measurement of mental qualities to considerable length, by describing other kinds of experiment, for they are numerous and varied. Among these is the plan of Professor Jevons, of suddenly exhibiting an unknown number of beans in a box, and requiring an estimate of their number to be immediately called out. A comparison of the estimate with the fact, in a large number of trials, brought out a very interesting scale of the accuracy of such estimates, which would of course vary in different individuals, and might be used as a means of classification. I can imagine few greater services to Anthropology than the collection of the various experiments that have been imagined to reduce the faculties of the mind to exact measurement. They have engaged the attention of the highest philosophers, but have never, so far as I am aware, been brought compendiously together, and have certainly not been introduced, as they deserve, to general notice.

Wherever we are able to perceive differences by intercomparison, we may reasonably hope that we may at some future time succeed in submitting those differences to measurement. The history of science is the history of such triumphs. I will ask your attention to a very notable instance of this, namely, that of the establishment of the scale of the thermometer. You are aware that the possibility of making a standard thermometric scale wholly depends upon that of determining two fixed points of temperature, the interval between them being graduated into a scale of equal parts. These points are, I need hardly say, the temperatures of freezing and of boiling water respectively. On this basis we are able to record temperature with minute accuracy, and the power of doing so has been one of the most important aids to

Physics and Chemistry as well as to other branches of investigation. We have been so accustomed, from our childhood, to hear of degrees of temperature, and our scientific knowledge is so largely based upon exact thermometric measurement, that we cannot easily realise the state of science when the thermometer, as we now use it, was unknown. Yet such was the condition of affairs so recently as two hundred years ago, or thereabouts. The invention of the thermometer, in its present complete form, was largely due to Boyle, and I find in his Memoirs (London, 1772, vol. vi. p. 403) a letter that cannot fail to interest us, since it well expresses the need of exact measurement that was then felt in a particular case, where it was soon eminently well supplied, and therefore encourages hope that our present needs as Anthropologists may hereafter, in some way or other, be equally well satisfied. The letter is from Dr. John Beale, a great friend and correspondent of Boyle, and is dated February, 1663. He says in it:—

"I see by several of my own thermometers, that the glass-men are by you so well instructed to make the stems in equal proportions, that if we could name some degrees, . . . we might by the proportions of the glass make our discourses intelligible in mentioning what degrees of cold our greatest frosts do produce. . . . If we can discourse of heat and cold in their several degrees, so as we may signify the same intelligibly, . . . it is more than our forefathers have taught us to do hitherto."

The principal experiments by which the mental faculties may be measured require, unfortunately for us, rather costly and delicate apparatus, and until physiological laboratories are more numerous than at present, we can hardly expect that they will be pursued by many persons.

Let us now suppose that, by one or more of the methods I have described or alluded to, we have succeeded in obtaining a group of persons resembling one another in some mental quality, and that we desire to determine the external physical characteristics and features most commonly associated with it. I have nothing new to say as regards the usual anthropometric measurements, but I wish to speak of the great convenience of photographs in conveying those subtle but clearly visible peculiarities of outline which almost elude measurement. It is strange that no use is made of photography to obtain careful studies of the head and features. No single view can possibly exhibit the whole of a solid, but we require for that purpose views to be taken from three points at right angles to one another. Just as the architect requires to know the elevation, side view and plan of a house, so the Anthropologist ought to have the full face, profile, and view of the head from above, of the individual whose features he is studying.

It might be a great convenience, when numerous portraits have to be rapidly and inexpensively taken for the purpose of anthropological studies, to arrange a solid framework supporting three mirrors, that shall afford the views of which I have been speaking, by reflexion, at the same moment that the direct picture of the sitter is taken. He would present a three-quarter face to the camera for the direct picture, one adjacent mirror would reflect his profile towards it, another on the opposite side would reflect his full face, and a third sloping over him would reflect the head as seen from above. All the reflected images would lie at the same optical distance from the camera, and would, therefore, be on the same scale, but they would be on a somewhat smaller scale than the picture taken directly. The result would be an ordinary photographic picture of the sitter surrounded by three different views of his head. Scales of inches attached to the framework would appear in the picture and give the means of exact measurement.

Having obtained drawings or photographs of several persons alike in most respects but differing in minor details, what sure method is there of extracting the typical characteristics from them? I may mention a plan which had occurred both to Mr. Herbert Spencer and myself, the principle of which is to superimpose optically the various drawings and to accept the aggregate result. Mr. Spencer suggested to me in conversation that the drawings reduced to the same scale might be traced on separate pieces of transparent paper and secured one upon another, and then held between the eye and the light. I have attempted this with some success. My own idea was to throw faint images of the several portraits, in succession, upon the same sensitised photographic plate. I may add that it is perfectly easy to superimpose optically two portraits by means of a stereoscope, and that a person who is used to handle instruments will find a common double eyeglass fitted with stereoscopic lenses to be almost as effectual and far handier than the boxes sold in shops.

In illustration of what I have said about photographic portraits, I will allude to some recent experiences of my own in a subject that I have still under consideration. In previous publications I have treated of men who have been the glory of mankind, I would now call your attention to those who are its disgrace. The particular group of men I have in view are the criminals of England, who have been condemned to long terms of penal servitude for various heinous offences.

It is needless to enlarge on the obvious fact that many persons have become convicts who, if they had been afforded the average chances of doing well, would have lived up to a fair standard of virtue. Neither need I enlarge on the other equally obvious fact, that a very large number of men escape criminal punishment, who in reality deserve it quite as much as an average convict. Making every allowance for these two elements of uncertainty, no reasonable man can entertain a doubt that the convict class includes a large proportion of consummate scoundrels, and that we are entitled to expect to find in any large body of convicts a prevalence of the truly criminal characteristics, whatever these may be.

Criminality, though not very various in its development, is extremely complex in its origin; nevertheless, certain general conclusions are arrived at by the best writers on the subject, among whom I would certainly rank Prosper Despine. The ideal criminal has three peculiarities of character: his conscience is almost deficient, his instincts are vicious, and his power of self-control is very weak. As a consequence of all this, he usually detests continuous labour. This statement applies to the criminal classes generally; the special conditions that determine the description of crime being the character of the instincts; and the fact of the absence of self-control being due to ungovernable temper, or to passion, or to mere imbecility.

The deficiency of conscience in criminals, as shewn by the absence of genuine remorse for their guilt, appears to astonish all who first become familiar with the details of prison life. Scenes of heartrending despair are hardly ever witnessed among prisoners; their sleep is broken by no uneasy dreams—on the contrary, it is easy and sound; they have also excellent appetites. But hypocrisy is a very common vice; and all my information agrees in one particular, as to the utter untruthfulness of criminals, however plausible their statements may appear to be.

The subject of vicious instincts is a very large one: we must guard ourselves against looking upon them as perversions, inasmuch as they may be strictly in accordance with the healthy nature of the man, and, being transmissible by inheritance, may become the normal characteristics of a healthy race, just as the sheep-dog, the retriever, the pointer, and the bull-dog have their several instincts. There can be no greater popular error than the supposition that natural instinct is a perfectly trustworthy guide, for there are striking contradictions to such an opinion in individuals of every description of animal. All that we are entitled to say is, that the prevalent instincts of each race are trustworthy, not those of every individual. A man who is counted as an atrocious criminal by society, and is punished as such by the law, may nevertheless have acted in strict accordance with his instincts. The ideal criminal is deficient in qualities that oppose his vicious instincts; he has neither the natural regard for others which lies at the base of conscience, nor has he sufficient self-control to enable him to consider his own selfish interests in the long run. He cannot be preserved from criminal misadventure, either by altruistic or by intelligently egoistic sentiments.

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[*From the* Proceedings of the Royal Society, No 136, 1872.]

ON BLOOD-RELATIONSHIP.

BY

FRANCIS GALTON, F.R.S.

I PROPOSE in this memoir to deduce, by fair reasoning from acknowledged facts, a more definite notion than now exists of the meaning of the word "kinship." It is my aim to analyze and describe the complicated connexion that binds an individual, hereditarily, to his parents and to his brothers and sisters, and, therefore, by an extension of similar links, to his more distant kinsfolk. I hope by these means to set forth the doctrines of heredity in a more orderly and explicit manner than is otherwise practicable.

From the well-known circumstance that an individual may transmit to his descendants ancestral qualities which he does not himself possess, we are assured that they could not have been altogether destroyed in him, but must have maintained their existence in a latent form. Therefore each individual may properly be conceived as consisting of two parts, one of which is latent and only known to us by its effects on his posterity, while the other is patent, and constitutes the person manifest to our senses.

The adjacent and, in a broad sense, separate lines of growth in which the patent and latent elements are situated, diverge from a common group and converge to a common contribution, because they were both evolved out of elements contained in a structureless ovum, and they, jointly, contribute the elements which form the structureless ova of their offspring.

The annexed diagram illustrates my meaning, and serves to show clearly that the span of each of the links in the general chain of heredity extends from one structureless stage to another, and not from person to person :—

Structureless elements in Father	.........Adult Father ....Latent in Father.........	Structureless elements in offspring.

I will now proceed to consider the quality of the several relationships by which the above terms are connected together.

The observed facts of Reversion enable us to prove that the latent elements must be greatly more varied than those that are personal or patent. The arguments are as follows:—(1) there must be *room* for very great variety, because a single strain of impure blood will reassert itself after more than eight generations; (2) an individual has 256 progenitors in the eighth degree, if there have been no ancestral intermarriages, while under the ordinary conditions of social and neighbourly life he will certainly have had a considerable, though a smaller number of them; (3) the gradual waning of the tendency to reversion as the generations increase conforms to what would occur if each fresh marriage contributed a competing element for the same place, thus diluting the impure strain until its relative importance was reduced to an insignificant amount. It follows from these arguments that for each place among the personal elements there may exist, and probably often does exist, a great variety of latent elements that formerly competed to fill it.

I have spoken of the primary elements as they exist in the newly impregnated ovum, where they are structureless but contain the materials out of which structure is evolved; the embryonic elements are segregated from among them. On what principle are they segregated? Since for each place there have been many unsuccessful but qualified competitors, it must have been on some principle whose effects may be described as those of "*Class Representation,*" using that phrase in a perfectly general sense as indicating a mere fact, and avoiding any hypothesis or affirmation on points of detail, about most, if not all, of which we are profoundly ignorant. I give as broad a meaning to the expression as a politician would give to the kindred one, a "representative assembly." By this he means to say that the assembly consists of representatives from various constituencies, which is a distinct piece of information so far as it goes, and is a useful one, although it deals with no matter of detail; it says nothing about the number of electors, their qualifications, or the motives by which they are influenced; it gives no information as to the number of seats; it does not tell us how many candidates there are usually for each seat, nor whether the same person is eligible for, or may represent at the same time, more than one place, nor whether the result of the elections at one place may or may not influence those at another (on the principle of correlation). After these explanations there can, I trust, be no difficulty in accepting my definition of the general character of the relation between the embryonic and the structureless elements, that the former are the result of election from the latter on some method of Class Representation.

The embryonic elements are *developed* into the adult person. "Development" is a word whose meaning is quite as distinct in respect to form, and as vague in respect to detail, as the phrase we have just been considering; it embraces the combined effects of growth and multiplication, as well as those of modification in quality and proportion, under both internal and external influences. If we were able to obtain an approximate knowledge of the original elements, statistical experiences would no doubt enable us to predict the average value of the form into which they would become developed, just as a knowledge of the seeds that were sown would enable us to predict in a general way the appearance of the garden when the plants had grown up; but the individual variation of each case would of course be great, owing to the large number of variable influences concerned in the process of development.

The latent elements in the embryonic stage must be developed by a parallel, I do not say by an identical process, into those of the adult stage. Therefore, to avoid all chance of being misapprehended when I collate them, I will call, in the diagram I am about to give (see fig. 1, p. 398), the one process "Development *a*" and the other "Development *b*."

It is not intended to affirm, in making these subdivisions, that the embryonic and adult stages are distinctly separated; they are continuous, and it is impossible but that they should overlap, some elements remaining embryonic while others are completely formed. Nevertheless the two, speaking broadly, may fairly be looked upon as consecutive.

Again, the two processes are not wholly distinct; on the contrary, the embryo, and even the adult in some degree, must receive supplementary contributions derived from their contemporary latent elements, because ancestral qualities indicated in early life frequently disappear and yield place to others. The reverse process is doubtful; it may exist in the embryonic stage, but it certainly does not exist in a sensible degree in the adult stage, else the later children of a union would resemble their parents more nearly than the earlier ones.

Lastly, I must guard myself against the objection that though structure is largely correlated, I have treated it too much as consisting of separate elements. To this I answer, first, that in describing how the embryonic are derived from the structureless elements, I expressly left room for a small degree of correlation; secondly, that in the development of the adult elements from the embryonic there is a perfectly open field for natural selection, which is the agency by which correlation is mainly established; and, thirdly, that correlation affects groups of elements rather than the complete person, as is proved by the frequent occurrence of small groups of persistent peculiarities, which do not affect the rest of the organism, so far as we know, in any way whatever.

The ground we have already gained may be described as follows:—

Out of the structureless ovum the embryonic elements are taken by *Class Representation*, and these are *developed* (*a*) into the visible adult individual; on the other hand, returning to our starting-point at the structureless ovum, we find, after the embryonic elements have been segregated, the large *Residue* is *developed* (*b*) into the latent elements contained in the adult individual. All this is summarily expressed in the first two columns of the diagram (fig. 1). I might have inserted vertical arrows to show the minor connexions between the corresponding stages in the two parallel processes, but it would have complicated the figure.

In what way do the patent and latent adult elements respectively contribute representatives towards the structureless stage of the next generation? We know that every quality they possess *may* be transmitted to it, but it does not follow that they *are* invariably transmitted. The contributions from the patent elements cannot be by "Class," because their own original elements have been themselves specialized, and therefore can contain no more than one or a few members of each class (which, it is true, must have been somewhat developed, both in numbers and variety, into what we may call "families"). Their contributions may therefore be justly described as being effected on some principle that has resulted in a "*Family representation,*" though whether in the representation of every family I do not profess to say.

As regards the large variety of adult latent elements, they cannot all be transmitted, for the following obvious reason—the corresponding qualities of no two parents can be considered exactly alike; therefore the accumulation of subvarieties, if they were all preserved as the generations rolled onwards, would exceed in multitude the wildest flights of rational theory. The heritage of peculiarities through the contributions of 1000 consecutive generations, even supposing a great deal of ancestral intermarriage, must far exceed what could be packed into a single ovum. The contributions from the latent adult elements are therefore no more than *Representative*; but they have to furnish all the various members of each Class whence its representatives have afterwards to be drawn. Therefore, bearing in mind what has been just argued, that it is impossible for the elements of every individual quality to be contributed, we are driven to suppose, as in the previous case, a "*Family representation,*" the similar elements contributed by the two parents ranking, of course, as of the same family. It is most important to bear in mind that this phrase states a fact and not an hypothesis; it does not mean that each and every Family has just one representative, for it is absolutely reticent on all such matters of detail as those I enumerated when speaking of Class Representation. To show the importance which I attach to this disclaimer, I may be permitted to mention what appears to me the most probable *modus operandi*, namely, that it is in reality a large selection made out of larger and not out of smaller constituencies than those I have called "classes," similar to that which would be obtained by an indiscriminate conscription: thus, if a large army be drawn from the provinces of a country by a general conscription, its constitution, according to the laws of chance, will reflect with surprising precision the qualities of the population whence it was taken; each village will be found

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to furnish a contingent, and the composition of the army will be sensibly the same as if it had been due to a system of immediate representation from the several villages.

Fig. 1.

Structureless elements of parent ..	through "Class Representation" afford	Embryonic elements	which by "Development *a*" become	Adult elements	which by "Family Representation" contribute to ...	Structureless elements of offspring.
	"Residue" after the above have been abstracted, become	Latent elements in embryo ..	which by "Development *b*" become	Latent elements in adult	which by "Family Representation" contribute to ...	

The diagram (fig. 1) expresses the whole of the foregoing results; it begins with the structureless elements whence the parent individual was formed, and ends with his contributions to the structureless elements whence his offspring is formed.

I will now inquire what are, roughly speaking, the relative proportions of the contributions to the elements of the offspring made respectively by the patent and latent elements of the adult parent. It is better not to complicate the inquiry by speaking, at first, of these elements in their entirety, but rather of some special characteristic: thus, to fix the ideas, suppose we are speaking about a peculiar skin-mark in an animal; the peculiarity in question may be conceived (1) as purely personal, without the concurrence of any latent equivalents, (2) as personal but conjoined with latent equivalents, and (3) as existent wholly in a latent form. It can be shown that, in the first case, the power of hereditary transmission is exceedingly feeble; for, notwithstanding some exceptions (as in the lost power of flight in domestic birds), the effects of the use and disuse of limbs, and those of habit, are transmitted to posterity in only a very slight degree. Again, it can be fairly argued that many instances which seem at first sight to fall under case (1), that is, to be purely personal, and to prove a larger hereditary influence than what I assign to it, do really belong to case (2): thus, when individuals born with a peculiar mark are reputed to be the first of their race in whom it had ever appeared, it would be hazardous in the extreme to argue that the latent elements of that mark were wholly deficient in them. It is very remarkable (I was indebted for a knowledge of this fact to Mr. Tegetmeier) how nearly every bar or spot found in any species of an animal in its wild state may be bred into existence in the domesticated variety of that species, showing that the elements of all these bars and spots are universally present in all varieties of the species, though their manifestation may be overborne and suppressed. We therefore see that the hereditary influences of an animal with respect to any particular spot are, I will not say in every case, but certainly on the *average of many cases,* much more numerous than if that spot had been purely a personal characteristic, without the concurrence of any latent elements. Bearing this argument in mind, we shall more justly estimate the import of the statistical evidence to be obtained from breeders of animals. I should judge, from the impression left by many scattered statistics, that it is perfectly safe to affirm that breeders, when they mate two animals, each having the same unusual characteristic, not through known hereditary transmission, but by supposed variation, would consider themselves fortunate if one quarter of the progeny inherited that quality. Now these successful cases are, as I have shown, on the average, the produce of parents having the peculiarity not only in a personal but also, to some degree, in a latent form. We may therefore reasonably conclude that, had the latter portion been non-existent, the ratio of successful cases would have been materially diminished.

Fig. 2.

- Structureless elements of father .... { * *Embryonic* * *Adult FATHER* * / * Latent in embryo * Latent in adult father * }
- Structureless elements of mother .... { * *Embryonic* * *Adult MOTHER* * / * Latent in embryo * Latent in adult mother * }
- (both combined:) Structureless elements of offspring .... } * Embryonic * Adult OFFSPRING.

I should demur, on precisely the same grounds, to objections based on the fact of the transmission of qualities to grandchildren being more frequent through children who possess those qualities than through children who do not; for I maintain that the personal manifestation is, on the average, though it need not be so in every case, a certain proof of the existence of some latent elements.

Having proved how small is the power of hereditary transmission of the personal elements, we can easily show how large is the transmission of the purely latent elements, in the case (3), by appealing to the well-known facts of Reversion; but into these it is hardly necessary for me to enter at length. The general and safe conclusion is, that the contribution from the patent elements is very much less than from the latent ones.

If we now combine our results into a diagram (fig. 2), showing the fainter streams of heredity by *italic lines,* and indicating those processes by asterisks (*) which were described at length in the previous figure, we shall easily recognize the complexity of hereditary problems. We see that parents are very indirectly and only partially related to their own children, and that there are two lines of connexion between them, the one of large and the other of small relative importance. The former is a collateral kinship and very distant, the parent being descended through two stages (two asterisks) from a structureless source, and he child (so far as that parent is concerned) through five totally distinct stages from the same source; the other, but unimportant line of connexion, is direct and connects the child with the parent through two stages. We shall therefore wonder that, notwithstanding the fact of an average resemblance between parent and child, the amount of individual variation should not be much greater than it is, until we have realized how complete must be the harmony between every variety and its environments in order that the variety should be permanent.

We also infer from the diagram how much nearer, and yet how subject to variation, is the kinship between the children of the same parents; for only two stages are required to trace back their descent to a common origin, which, however, proceeds from four separate streams of heredity, namely the adult patent and latent elements of each of the two parents.

An approximate notion of the nearest conceivable relationship between a parent and his child may be gained by supposing an urn containing a great number of balls, marked in various ways, and a handful to be drawn out of them at random as a sample: this sample would represent the person of a parent. Let us next suppose the sample to be examined, and a few handfuls of new balls to be marked according to the patterns of those found in the sample, and to be thrown along with them back into the urn. Now let the contents of another urn, representing the influences of the other parent, be mixed with those of the first. Lastly, suppose a second sample to be drawn out of the combined contents of the two urns, to represent the offspring. There can be no nearer connexion justly conceived to subsist between the parent and child than between the two samples; on the contrary, my diagram shows the relationship to be in reality much more remote, and consisting of many consecutive stages, and therefore hardly to be expressed by such simple chances. Whenever the balls in the urns are much of the same pattern, the samples will be alike, but not otherwise. The offspring of a mongrel stock necessarily deviate in appearance from each other and from their parents.

We cannot now fail to be impressed with the fallacy of reckoning inheritance in the usual way, from parents to offspring, using those words in their popular sense of visible personalities. The span of the true hereditary link connects, as I have already insisted upon, not the parent with the offspring, but the primary elements of the two, such as they existed in the newly impregnated ova, whence they were respectively developed. No valid excuse can be offered for not attending to this fact, on the ground of our ignorance of the variety and proportionate values of the primary elements: we do not mend matters in the least, but we gratuitously add confusion to our ignorance, by dealing with hereditary facts on the plan of ordinary pedigrees—namely, from the *persons* of the parents to those of their offspring.

It will be observed that, owing to the clearer idea we have now obtained of the meaning of kinship and of the consecutive phases of the chain of life, the various causes of individual variation can be easily and surely sorted into their proper places. I will mention a few of them, merely as examples.

Previous to the segregation of the embryonic elements, if the structureless ones be diverse without any strongly preponderating element, it is impossible to foresee the character of the embryo, just as it is impossible to foresee the character of a handful chosen from an urn containing a mixed assemblage of variously coloured balls; but if they be not diverse, then the embryonic elements will be a true sample of the structureless ones, the conditions of purity of blood are fulfilled, and the offspring will resemble its parents.

We also see, in the process by which the embryonic elements are obtained, how the curious phenomenon may occur of inheritance occasionally skipping alternate generations. The more that has been removed from the structureless group for the supply of the embryonic (which, as we have seen, is a nearly sterile destination) the less remains for the "residue," too little, it may be, to assert itself by that, the only prolific, line of transmission. In the supposed case it would recuperate itself during the succeeding generation, where the elements in question will have remained wholly latent, owing to their insignificance in the structureless stage of that generation, which would be sufficient to secure any portion of it from selection for the embryonic form.

Again, it is in the process of selection of elements, both latent and patent, from the adult parents for the structureless stage of the next generation, where I suppose the curious and unknown conditions usually to occur through which a change in the habits of life, after the adult age has been reached, is apt to produce sterility. I may be permitted to remark, hypothetically, that this view appears to be corroborated by the fact that many grains of pollen or many spermatozoa are required to fertilize each ovum, because, as it would seem, each separate one does not contain a sufficiently complete representation of the primary elements to supply the needs of an individual life, and that it is only by the accumulation of several separate consignments (so to speak) of the representative elements that the necessary variety is ensured. I argue from this that there is a tendency to a large individual variation in the constituents of each grain of pollen, or spermatozoon, and, by analogy, that there is a similar though smaller tendency in each ovum; also that changes in the habits of life may increase this variation to a degree that involves sterility.

Lastly, it is often remarked (1) that the immediate offspring of different races or even varieties resemble their parents equally, but (2) that great diversities appear in the next and in succeeding generations. In which stage does the variability occur? It cannot be in the first (class representation) nor in the second (development), else (1) could not have been true; therefore it must be in the third stage. A white parent necessarily contributes white elements to the structureless stage of his offspring, and a black, black; but it does not in the least follow that the contributions from a true mulatto must be truly mulatto.

One result of this investigation is to show very clearly that large variation in individuals from their parents is not incompatible with the strict doctrine of heredity, but is a consequence of it wherever the breed is impure. I am desirous of applying these considerations to the intellectual and moral gifts of the human race, which is more mongrelized than that of any other domesticated animal. It has been thought by some that the fact of children frequently showing marked individual variation in ability from that of their parents is a proof that intellectual and moral gifts are not strictly transmitted by inheritance. My arguments lead to exactly the opposite result. I show that their great individual variation is a necessity under present conditions; and I maintain that results derived from large averages are all that can be required, and all we could expect to obtain, to prove that intellectual and moral gifts are as strictly matters of inheritance as any purely physical qualities.

ADDRESS

TO

THE GEOGRAPHICAL SECTION

OF THE

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE,

AT BRIGHTON, AUG. 15, 1872.

By FRANCIS GALTON, F.R.S.

LONDON:
PRINTED BY WILLIAM CLOWES AND SONS, STAMFORD STREET,
AND CHARING CROSS.
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ADDRESS

TO THE GEOGRAPHICAL SECTION

OF THE

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE,

At Brighton, August 15, 1872.

By Francis Galton, F.R.S.

The functions of the several Sections of the British Association differ from those of other Institutions which pursue corresponding branches of science. We, who compose this Section, are not simply a Geographical Society, meeting in a hospitable and important provincial town, but we have a distinct individuality of our own. We have purposes to fulfil, which are not easily to be fulfilled elsewhere; and, on the other hand, there are many functions performed by Geographical Societies which we could not attempt without certain failure. Our peculiarities lie in the brief duration of our existence, combined with extraordinary opportunities for ventilating new ideas and plans, and of promoting the success of those that deserve to succeed. We are constituents of a great scientific organization, which enables us to secure e th attention of representatives of all branches of science to any projects in which we are engaged; and if those projects have enough merit to earn their deliberate approval, they are sure of the hearty and powerful support of the whole British Association.

These considerations indicate the class of subjects to which our brief existence may be devoted with most profit. They are such as may lead to a definite proposal being made by the Committee of our Section for the aid of the Association generally; and there are others, of high popular interest, which cannot be thoroughly discussed except by a mixed assemblage, which includes persons who are keen critics though not pure geographers, and who have some wholesome irreverence for what Lord Bacon would have called "the idols of the geographical den."

We may congratulate ourselves that many excellent memoirs will be submitted to us, which fulfil one or other of these conditions. They will come before us in due order, and it is needless that I should occupy your attention by imperfect anticipations of them. But I must say, that their variety testifies to the abundance of the objects of geographical pursuit, other than exploration. There is no reason to fear that the most interesting occupation of geographers will be gone, when the main features of all the world are known. On the contrary, it is to be desired, in the interests of the living pursuit of our science, that the primary facts should be well ascertained, in order that geographers may have adequate materials, and more leisure to devote themselves to principles and relations. I look forward with eagerness to the growth of Geography as a science, in the usually accepted sense of that word; for its problems are as numerous, as interesting, and as intricate as those of any other. The configuration of every land, its soil, its vegetable covering, its rivers, its climate, its animal and human inhabitants, act and re-act upon one another. It is the highest problem of Geography to analyse their correlations, and to sift the casual from the essential. The more accurately the crude facts are known, the more surely will induction proceed, the further will it go, and, as the analogy of other sciences assures us, the interest of its results will in no way diminish.

As a comparatively simple instance of this, I would mention the mutual effects of climate and vegetation, on which we are at present very imperfectly informed, though I hope we shall learn much that is new and valuable during this Meeting. Certain general facts are familiar to us: namely, that rain falling upon a barren country drains away immediately. It ravages the hill-slopes, rushes in torrents over the plains, and rapidly finds its way to the sea, either by rivers or by subterranean water-courses, leaving the land unrefreshed and unproductive. On the other hand, if a mantle of forest be nursed into existence, the effects of each rainfall are far less sudden and transient. The water has to soak through much vegetation and humus before it is free to run over the surface; and, when it does so, the rapidity of its course is checked by the stems of the vegetation. Consequently, the rain-supplies are held back and stored by the action of the forest, and the climate among the trees becomes more equable and humid. We also are familiar with the large differences between the heat-radiating power of the forest and of the desert, also between the amount of their evaporation; but we have no accurate knowledge of any of these data. Still less do we know about the influences of forest and desert on the rate of passage, or upon the horizontality, of the water-laden winds from the sea over the surface of the land: indeed I am not aware that this subject has ever been considered, although it is an essential element in our problem. If we were thoroughly well informed on the matters about which I have been speaking, we might attempt to calculate the precise difference of climate under such and such conditions of desert and of forest, and the class of experiencies whence our data were derived, would themselves furnish tests of the correctness of our computations. This will serve as an example of what I consider to be the geographical problems of the future; it is also an instance of the power of man over the phenomena of nature. He is not always a mere looker-on, and a passive recipient of her favours and slights; but he has power, in some degree, to control her processes, even when they are working on the largest scale. The effects of human agency on the aspect of the earth would be noticeable to an observer far removed from it. Even were he as distant as the moon is, he could see them; for the colour of the surface of the land would have greatly varied during historic times, and in some places the quantity and the drift of cloud would have perceptibly changed. It is no trifling fact in the physical geography of the globe, that vast regions to the east of the Mediterranean, and broad tracts to the south of it, should have been changed from a state of verdure to one of aridity, and that immense European forests should have been felled.

We are beginning to look on our heritage of the earth much as a youth might look upon a large ancestral possession, long allowed to run waste, visited recently by him for the first time, whose boundaries he was learning, and whose capabilities he was beginning to appreciate. There are tracts in Africa, Australia, and at the Poles, not yet accessible to geographers, and wonders may be contained in them; but the region of the absolutely unknown is narrowing, and the career of the explorer, though still brilliant, is inevitably coming to an end. The geographical work of the future is to obtain a truer knowledge of the world. I do not mean by accumulating masses of petty details, which subserve no common end, but by just and clear generalizations. We want to know all that constitutes the individuality, so to speak, of every geographical district, and to define and illustrate it in a way easily to be understood; and we have to use that knowledge to show how the efforts of our human race may best conform to the geographical conditions of the stage on which we live and labour.

I trust it will not be thought unprofitable, on an occasion like this, to have paused for a while, looking earnestly towards the future of our science, in order to refresh our eyes with a sight of the distant land to which we are bound, and to satisfy ourselves that our present efforts lead in a right direction.

The work immediately before us is full of details, and now claims your attention. There is much to be done and discussed in this room, and I am chary of wasting time by an address on general topics. It will be more profitable that I should lay before you two projects of my own about certain maps, which it is desirable that others than pure geographers should consider, and on which I shall hope to hear the opinions of my colleagues in the Committee-room of this Section.

They both refer to the Ordnance Maps of this country, and the first of them to the complete series well known to geographers, that are published on the scale of one inch to a mile. It is on these alone that I am about to speak; for, though many of my remarks will be applicable more or less to the other Government map publications, I think it better not to allude to them in direct terms, to avoid distracting attention by qualifications and exceptions.

English geographers are justly proud of these Ordnance Maps of their country, whose accuracy and hill-shading are unsurpassed elsewhere, though the maps do not fulfil, in all particulars, our legitimate desires. I shall not speak here of the absence from the coast-maps of the sea *data*, such as the depth and character of the bed of the sea, its currents and its tides (although these are determined and published by another Department of the Government, namely, the Admirality), neither shall I speak of the want of a more frequent revision of the sheets, but shall confine myself to what appear to be serious, though easily remediable, defects in the form and manner of their publication. It is much to be regretted that these beautiful and cheap maps are not more accessible. They are rarely to be found even in the principal booksellers' shops of important country towns, and I have never observed one on the bookstall of a railway station. Many educated persons seldom, if ever, see them, they are almost unknown to the middle and lower classes; and thus an important work, made at the expense of the public, is practically unavailable to a large majority of those interested in it, who, when they want a local map, are driven to use a common and inferior one out of those which have the command of the market. I am bound to add, that this evil is not peculiar to our country, but is felt almost as strongly abroad, especially in respect to the Government maps of France. I account for it by two principal reasons. The first is, that the maps are always printed on stiff paper, which makes them cumbrous and unfit for immediate use: it requires large portfolios or drawers, to keep them smooth, clean and in separate sets, and an unusually large table to lay them out side by side, to examine them comfortably, and to select what is wanted. These conditions do not exist on the crowded counter of an ordinary bookseller's shop, where it is impossible to handle them without risk of injury, and without the certainty of incommoding other customers. Moreover, their stiffness and size, even when published in quarter-sheets, make them most inconvenient to the purchaser. Either he has to send them to be mounted in a substantial and therefore costly manner, or he must carry a roll home with him, and cut off the broad ornamental borders, and divide the sheet into compartments suitable for the pocket, which, to say the least, is a troublesome operation to perform with neatness. The other of the two reasons why the maps are rarely offered for sale, is that the agents for their publication are themselves map-makers, and therefore competitors, and it is not to be expected of human nature that they should push the sale of maps adversely, in however small a degree, to their own interests.

The remedy I shall propose for the consideration of the Committee of this Section is, to memorialize Government to cause an issue of the maps to be made in quarter-sheets on thin paper, and to be sold, folded into a pocket-size, like the county maps seen at every railway station, each having a portion of an index-map impressed on its outside, to show its contents and those of the neighbouring sheets, as well as their distinguishing numbers. Also, I would ask that they should be sold at every "Head Post-office" in the United Kingdom. There are about seven hundred of these offices, and each might keep nine adjacent quarter-sheets in stock, the one in which it was situated being the centre of the nine. An index-map of the whole survey might be procurable at each of these post-offices, and, by prepayment, any map not kept in stock might be ordered at any one of them, and received in the ordinary course of the post. This is no large undertaking that I have proposed. The price of a quarter-sheet in its present form, which is more costly than what I ask for, is only sixpence, therefore the single complete set of nine sheets for each office has a value of not more than four shillings and sixpence, and for all the seven hundred Head Post-offices, of not more than 160*l*.

I believe that these simple reforms would be an immense public boon, by enabling any one to buy a beautiful and accurate pocket-map of the district in which he resides for only sixpence, and without any trouble. They would certainly increase the sale of Government maps to a great extent, they would cause the sympathies of the people and of their representatives in Parliament to be enlisted on the side of the Survey, and they would probably be imitated by Continental nations.

It has often been objected to any attempt to increase the sale of Government maps, that the State ought not to interfere with private enterprise. I confess myself unable to see the applicability of that saying. It would be an argument against making Ordnance Maps at all: but the nation has deliberately chosen to undertake that work, on the ground that no private enterprise could accomplish it satisfactorily; and, having done so, I cannot understand why it should restrict the sale of its own work in order to give a fictitious protection to certain individuals, against the interests of the public. It seems to me to be a backward step in political economy, and one that has resulted in our getting, not the beautiful maps for which we, as taxpayers, have paid, but copies, or reductions of them, not cheaper than the original, and of very inferior workmanship and accuracy.

So much for the first of the two projects which I propose to bring before the consideration of the Committee of this Section. It is convenient that I should preface my second one with a few remarks on colour-printing, its bearing on the so-called "bird's-eye views," and its recent application to cartography. Colour-printing is an art which has made great advances in recent years, as may be seen by the specimens struck off in the presence of visitors to the present International Exhibition. One of these receives no less than twenty-four consecutive impressions, each of a different colour from a different stone. This facility of multiplying coloured drawing will probably lead to a closer union than heretofore between geography and art. There is no reason now why "bird's-eye views" of large tracts of country should not be delicately drawn, accurately coloured, and cheaply produced. We all know what a geographical revelation is contained in a clear view from a mountain top, and we also know that there was an immense demand for the curiously coarse bird's-eye views which were published during recent wars, because even such as they are capable of furnishing a more pictorial idea of the geography of a country than any map. It is therefore to be hoped, that the art of designing the so-called "bird's-eye views" may become studied, and that real artists should engage in it. Such views of the environs of London would form very interesting, and it might be, very artistic pictures.

The advance of colour-printing has already influenced cartography in foreign countries, and it is right that it should do so, for a black and white map is but a symbol—it can never be a representation of the many-coloured aspects of Nature. The Governments of Belgium, Russia, Austria, and many other countries, have already issued coloured maps; but none have made further advance than the Dutch, whose maps of Java are printed with apparently more than ten different colours, and succeed in giving a vivid idea of the state of cultivation in that country.

I now beg to direct your attention to the following point. It is found that the practice of printing maps in more than one colour has an incidental advantage of a most welcome kind, namely, that it admits of easy revision, even in the most beautifully executed maps, for the following reason. The hill-work, in which the delicacy of execution lies, is drawn on a separate plate, having perhaps been photographically reduced; this has never to be touched, because the hills are permanent. But it is in the plate which contains nothing else but the road-work where the corrections have to be made, and that is a very simple matter. I understand that the Ordnance Survey Office has favourably considered the propriety of printing at some future time an edition of the one-inch maps on this principle, and at least in two colours—the one for the hills and the other for the roads.

This being stated, I will now proceed to mention my second proposal.

Recollecting what I have urged about the feasibility of largely increasing the accessibility and the sale of Government maps, by publishing them in a pocket form and selling them at the Head Post-offices, it seems to me a reasonable question for the Committee of this Section to consider whether Government might not be memorialised to consider the propriety of undertaking a *reduced* Ordnance Map of the country, to serve as an accurate route-map and to fulfil the demand to which the coarse county maps, which are so largely sold, are a sufficient testimony. The scale of the reduced Govern-

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ment Map of France corresponds to what I have in view; it is one of 5 miles to an inch, within a trifle ($\frac{1}{320000}$ of Nature), which is just large enough to show every lane and footpath. Of course it would be a somewhat costly undertaking to make such a map, but much less so than it might, at first sight, appear. Its area would be only one twenty-fifth that of the ordinary Ordnance Map, and the hill-work of the latter might perhaps be photographically reduced and rendered available at once. The desirability of maps such as these, accurately executed and periodically revised, is undoubted, while it seems impossible that private enterprise should supply them except at a prohibitive cost, because private publishers are necessarily saddled with the cost of re-obtaining much of what the Ordnance Survey Office has already in hand for existing purposes. A Government Department has unrivalled facilities for obtaining a knowledge of every alteration in roads, paths, and boundaries of commons, and Government also possesses an organized system in the post-offices, fitted to undertake their sale. The production of an accurate route-map seems a natural corollary to that of the larger Ordnance Maps, and has been considered to be so by many Continental Governments.

I therefore intend to propose to the Committee of this Section to consider the propriety of memorialising Government to cause enquiries to be made as to the cost of construction, and the probability of a remunerative sale, of maps such as those I have described; and, if the results are satisfactory, to undertake the construction of a reduced Ordnance Map, on the same scale as that of France, to be printed in colours, and frequently revised.

These, then, are the two projects to which I alluded—the one to secure the sale of one-inch Ordnance Maps, on paper folded into a pocket form, to be sold at the Head Post-offices of the United Kingdom, 700 or thereabouts in number, each office keeping in stock the maps of the district in which it is situated; and the other to obtain a reduced Ordnance Map of the kingdom, on the scale of about 5 miles to an inch, to fulfil all the purposes of a road map, and to be sold throughout the country at the post-offices, in the way I have just described.

I will now conclude my Address, having sufficiently taxed your patience, and beg you to join with me in welcoming, with your best attention, the eminent Geographers, whose communications are about to be submitted to your notice.

f. 6

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8X

Statistics by Intercomparison

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From the PHILOSOPHICAL MAGAZINE for January 1875.

STATISTICS BY INTERCOMPARISON,

WITH

REMARKS ON THE LAW

OF

FREQUENCY OF ERROR.

BY

FRANCIS GALTON, F.R.S.

MY object is to describe a method for obtaining simple statistical results which has the merit of being applicable to a multitude of objects lying outside the present limits of statistical inquiry, and which, I believe, may prove of service in various branches of anthropological research. It has already been proposed (Lecture, Royal Institution, Friday evening, February 27, 1874), and in some degree acted upon ('Hereditary Genius,' p. 26), by myself. What I have now to offer is a more complete explanation and a considerable development of previous views.

The process of obtaining mean values &c. now consists in measuring each individual with a standard that bears a scale of equal divisions, and afterwards in performing certain arithmetical operations upon the mass of figures derived from these numerous measurements. I wish to point out that, in order to procure a specimen having, in one sense, the mean value of the quality we are investigating, we do not require any one of the appliances just mentioned: that is, we do not require (1) independent measurements, nor (2) arithmetical operations; we are (3) able to dispense with standards of reference, in the common acceptation of the phrase, being able to create and afterwards indirectly to define them; and (4) it will be explained how a rough division of our standard into a scale of degrees may not unfrequently be effected. Therefore it is theoretically possible, in a great degree, to replace the ordinary process of obtaining statistics by another, much simpler in conception, more convenient in certain cases, and of incomparably wider applicability.

Nothing more is required for the due performance of this process than to be able to say which of two objects, placed side by side, or known by description, has the larger share of the quality we are dealing with. Whenever we possess this power of discrimination, it is clear that we can marshal a group of objects in the order in which they severally possess that quality. For example, if we are inquiring into the statistics of height, we can marshal a number of men in the order of their several heights. This I suppose to be effected wholly by *intercomparison,* without the aid of any external standard. The object then found to occupy the middle position of the series must possess the quality in such a degree that the number of objects in the series that have more of it is equal to that of those that have less of it. In other words, it represents the *mean* value of the series in at least one of the many senses in which that term may be used. Recurring to the previous illustration, in order to learn the mean height of the men, we have only to select the middlemost one and measure him; or if no standard of feet and inches is obtainable, we must describe his height with reference to numerous familiar objects, so as to preserve for ourselves and to convey to strangers as just an idea of it as we can. Similarly the mean speed of a number of horses would be that of the horse which was middlemost in the running.

If we proceed a step further and desire to compare the mean height of two populations, we have simply to compare the representative man contributed by each of them. Similarly, if we wish to compare the performances of boys in corresponding classes of different schools, we need only compare together the middle boys in each of those classes.

The next great point to be determined is the divergency of the series—that is, the tendency of individual objects in it to diverge from the mean value of all of them. The most convenient measure of divergency is to take the object that has the mean value, on the one hand, and those objects, on the other, whose divergence in either direction is such that one half of the objects in the series on the same side of the mean diverge more than it does, and the other half less. The difference between the mean and either of these objects is the measure in question, technically and rather absurdly called the "probable error." Statisticians find this by an arithmetical treatment of their numerous measurements; I propose simply to take the objects that occupy respectively the first and third quarter points of the series. I prefer, on principle, to reckon the divergencies in excess separately from those in deficiency. They cannot be the same unless the series is symmetrical, which experience shows me to be very rarely the case. It will be observed that my process fails in giving the difference (probable error) in numerical terms; what it does is to select specimens whose differences are precisely those we seek, and which we must appreciate as we best can.

We have seen how the mean heights &c. of two populations may be compared; in exactly the same way may we compare the divergencies in two populations whose mean height is the same, by collating representative men taken respectively from the first and third quarter points of the series in each case.

We may be confident that if any group be selected with the ordinary precautions well known to statisticians, it will be so far what may be called "generic" that the individual differences of members of that group will be due to various combinations of pretty much the same set of variable influences. Consequently, by the well-known laws of combinations, medium values will occur very much more frequently than extreme ones, the rarity of the latter rapidly increasing as the deviation slowly increases. Therefore, when the objects are marshalled in the order of their magnitude along a level base at equal distances apart, a line drawn freely through the tops of the ordinates which represent their several magnitudes will form a curve of ~~double curvature~~ contrary flexure. It will be nearly horizontal over a long space in the middle, if the objects are very numerous; it will bend down at one end until it is nearly vertical, and it will rise up at the other end until there also it is nearly vertical. Such a curve is called, in the phraseology of architects, an "ogive," and is represented by O G in the diagram (fig. 1), in which the process of statistics by intercomparison is clearly shown. If $n =$ the length of the base of the ogive, whose ordinate y represents the magni-

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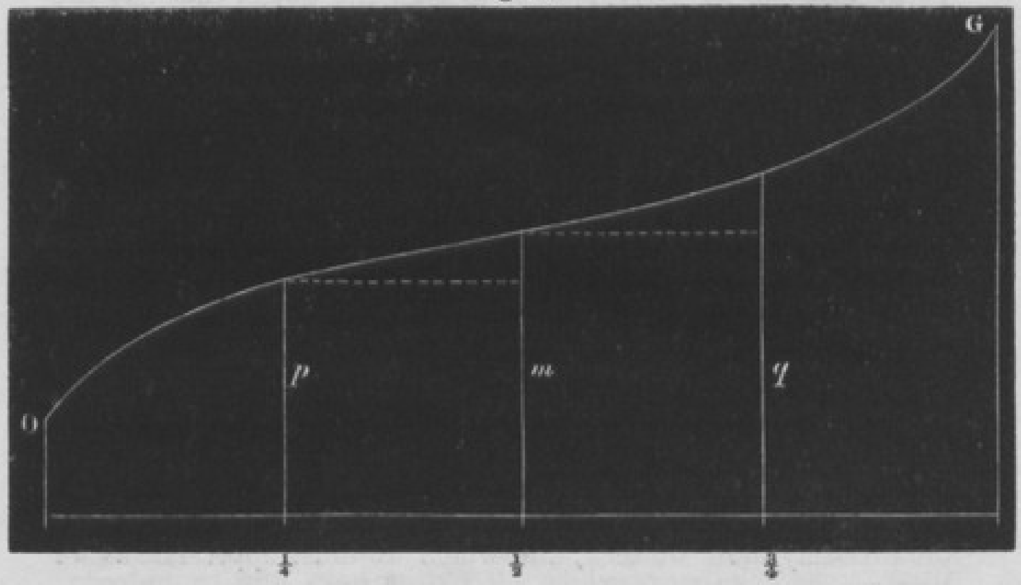
ude of the object that stands at a distance x from that end of the base where the ordinates are smallest, then the number of

Fig. 1.



objects less than y: the number of objects greater than y : : $x : n-x$. The ordinate m at $\frac{1}{2}$ represents the mean value of the series, and p, q at $\frac{1}{4}$ and $\frac{3}{4}$, taken in connexion with m, give data for estimating the divergence; thus $q-m$ is the divergence (probable error) of at least that portion of the series that is in excess of the mean, and $m-p$ is that of at least the other portion. When the series is symmetrical, $q-m=p-q$, and either, or the mean of both, may be taken as the divergence of the series generally. No doubt we are liable to deal with cases in which there may be some interruption in the steady sweep of the ogive; but the experience of qualities which we *can* measure, assures us that we need fear no large irregularity of that kind when dealing with those which, as yet, we have no certain means of measuring.

When we marshal a series, we may arrange them roughly, except in the neighbourhood of the critical points; and thus much labour will be saved. But the most practical way of setting to work would probably depend not on the mere discrimination of greater and less, but also on a rough sense of what is much greater or much less. We have called the objects at the $\frac{1}{4}$, $\frac{1}{2}$, and $\frac{3}{4}$ distances p, m, and q respectively; let us sort the objects into two equal portions P and Q, of small and great, taking no more pains about the sorting than will ensure that P contains p and all smaller than p, and that Q contains q and all larger than q. Next, beginning, say, with group P, sort away alternately to right and left the larger and the smaller objects, roughly at first, but proceeding with more care as the residuum diminishes and the differences become less obvious. The last remaining object will be p. Similarly we find q. Then m will be found in the same way from the group compounded of those that were sorted to the right from P and to the left from Q.

There are not a few cases where both the ordinary method and that by intercomparison are equally applicable, but in which the latter would prove the more rapid and convenient. I would mention one of some importance to those anthropologists who may hereafter collect data in uncivilized countries. A barbarian chief might often be induced to marshal his men in the order of their heights, or in that of the popular estimate of their skill in any capacity; but it would require some apparatus and a great deal of time to measure each man separately, even supposing it possible to overcome the usually strong repugnance of uncivilized people to any such proceeding.

The practice of sorting objects into classes may be said to be coextensive with commerce, the industries, and the arts. It is adopted in the numerous examinations, whether pass or competitive, some or other of which all youths have now to undergo. It is adopted with every thing that has a money-value; and all acts of morality and of intellectual effort have to submit to a verdict of "good," "indifferent," or "bad."

The specimen values obtained by the process I have described are capable of being reproduced so long as the statistical conditions remain unchanged. They are also capable of being described in various ways, and therefore of forming permanent standards of reference. Their importance then becomes of the same kind as that which the melting-points of well-defined alloys or those of iron and of other metals had to chemists when no reliable thermometer existed for high temperatures. These were excellent for reference, though their relations *inter se* were subject to doubt. But we need never remain wholly in the dark as to the relative value of our specimens, methods appropriate to each case being sure to exist by which we may gain enlightenment. The measurement of work done by any faculty when trained and exerted to its uttermost, would be frequently available as a test of its absolute efficacy.

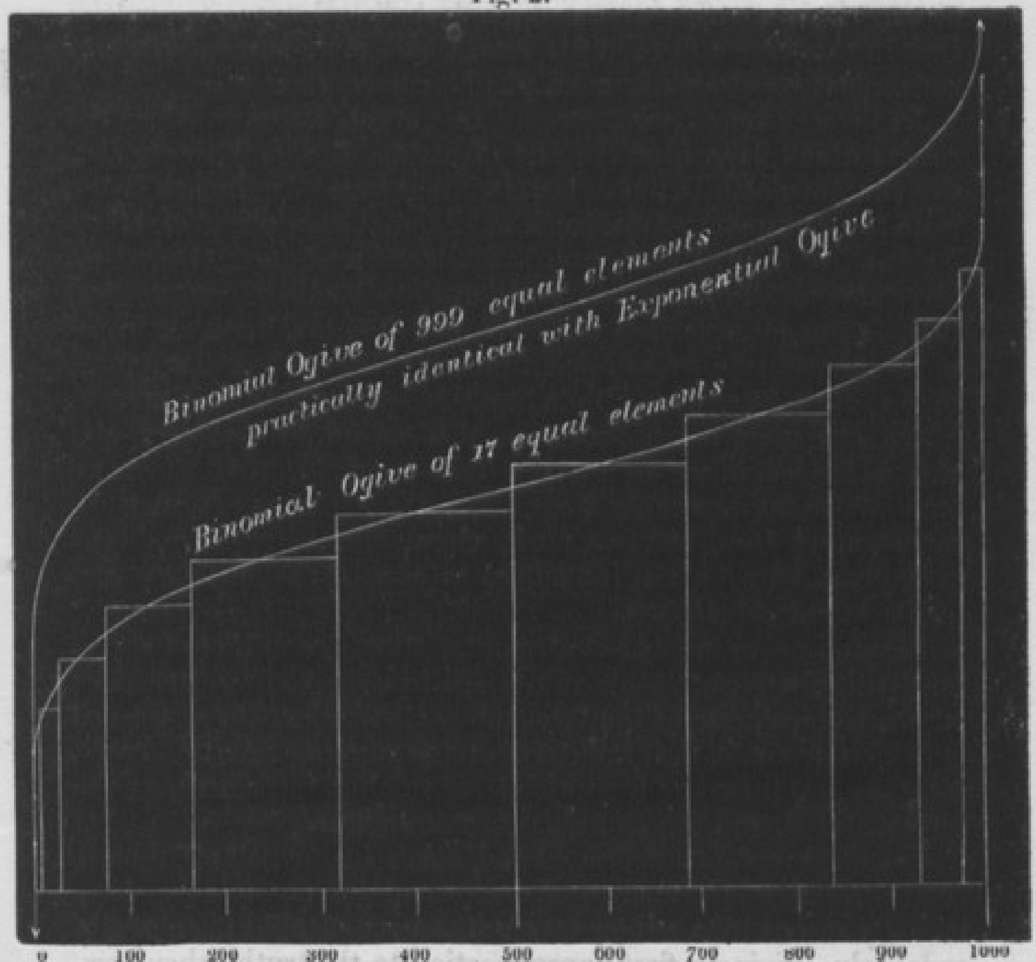
There is another method, which I have already advocated and adopted, for gaining an insight into the absolute efficacies of qualities, on which there remains more to say. Whenever we have grounds for believing the law of frequency of error to apply, we may *work backwards*, and, from the relative frequency of occurrence of various magnitudes, derive a knowledge of the true relative values of those magnitudes, expressed in units of probable error. The law of frequency of error says that "magnitudes differing from the mean value by such and such multiples of the probable error, will occur with such and such degrees of frequency." My proposal is to reverse the process, and to say, "since such and such magnitudes occur with such and such degrees of frequency, therefore the differences between them and the mean value are so and so, as expressed in units of probable error." According to this process, the positions of the first divisions of the scale of divergence, which are those of the mean value *plus* or *minus* one unit of probable error, are of course p and q, lying at the $\frac{1}{4}$ and $\frac{3}{4}$ points of the ogive, or, if the base consist of 1000 units, at the 250th point from the appropriate end. The second divisions being those of mean value *plus* or *minus* two units of probable error, will, according to the usual Tables, be found at the 82nd point from the appropriate end, the third divisions will be at the 17th, and the fourth at the 3rd. If we wished to pursue the scale further, we should require a base long enough to include very many more than 1000 units.

Remarks on the Law of Frequency of Error.

Considering the importance of the results which admit of being derived whenever the law of frequency of error can be shown to apply, I will give some reasons why its applicability is more general than might have been expected from the highly artificial hypotheses upon which the law is based. It will be remembered that these are to the effect that individual errors of observation, or individual differences in objects belonging to the same generic group, are entirely due to the aggregate action of variable influences in different combinations, and that these influences must be (1) all independent in their effects, (2) all equal, (3) all admitting of being treated as simple alternatives "above average" or "below average;" and (4) the usual Tables are calculated on the further supposition that the variable influences are infinitely numerous.

As I shall lay much stress on matters connected with the last condition, it will save reiteration if I be permitted the use of a phrase to distinguish between calculations based on the supposition of a moderate number (r) of elements (in which case the frequency of error or the divergence is expressed by the coefficients of the expansion of the binomial $(a+b)^r$) and one based on the supposition of the number being infinite (which is expressed by the exponential $e^{-\frac{x^2}{c^2}}$), by calling the one the binomial and the other the exponential process, the latter being the process to be understood whenever the "law of frequency of error" is spoken of without further qualification. When the results of these two processes have to be protracted, as in figure 2, the unit of vertical measurement in the case of a series of bino-

Fig. 2.



mial grades will be a single grade, or, what comes to the same thing, the difference of the effect produced by the plus and minus phase of any one of the alternative elements, upon the value of the whole. The unit of the exponential curve will be $q-m$ of fig. 1, or the probable error. This latter unit is equally applicable to what we may call the binomial ogive, which is the curve drawn with a free hand through the grades. The justification for such a conception as a binomial ogive will be fully established further on. Suffice it for the present to remark that, by the adoption of a unit of this kind, the middle portion of a binomial ogive of 999 elements is compared in the figure with one of 17.

The first three of the above-mentioned conditions may occur in games of chance, but they assuredly do not occur in vital and social phenomena; nevertheless it has been found in numerous

	In binomial ogive of 17 elements.	In exponential ogive, or in binomial ogive of 999 elements.
The mean	500	500
The mean ±1 unit probable error ...	250	250
„ ±2 units	71	82
„ ±3 units	16	17

The closeness of the resemblance is striking. It rapidly increases and extends in its range as the number of elements in the binomial increases; there need therefore be no hesitation in recognizing the fact that a binomial of, say, 30 elements or upwards is just as conformable to ordinary statistical observation as is the exponential. If one agrees, the other does, because they agree with one another.

The fewest number of elements that suffice to form a binomial having the above-mentioned conformity is a criterion of the meaning of the word "small," which was lately employed, because each of those elements would be just entitled to rank as small.

I obtain the value of any one of them in an ogive by protracting the series and noticing how many grades are included in the interval $q-m$. It will be found that in a binomial of 17 elements $q-m$ is equal to eight fifths of one grade. Thence I conclude that in any generic series an influence the range of whose mean effects in the two alternatives of above and below average is not greater than, say, one half of the probable error of the series, is entitled to be considered "small."

I now proceed to show how a medley of small and minute causes may, as a first approximation to the truth, be looked upon as an aggregate of a moderate number of "small" and *equal* influences. In doing this, we may accept without hesitation, the usual assumption that all small, and *à fortiori* all minute influences, may be dealt with as simple alternatives of excess or deficiency—the values of this excess and deficiency being the mean of all the values in each of these two phases. The way in which I propose to build up the fictitious groups may be exactly illustrated by a game of odd and even, in which it might be agreed that the *predominance* of "heads" in a throw of three fourpenny pieces, shall count the same as the simple "head" of a shilling. The three fourpenny pieces may fall all heads, 2 heads and 1 tail, 1 head and 2 tails, or all tails—the relative frequency of these events being, as is well known, 1, 3, 3, 1. But by our hypothesis we need not concern ourselves about these minute peculiarities; the question for us is simply the alternative one, are the "heads" in a majority or not? We may therefore treat a ternary system of the third order of smallness exactly as a simple alternative of the first order of smallness. Or, again, suppose a crown were our "small" unit, and we had a medley of 10 crowns, 33 shillings, and 100 fourpenny pieces, with which to make successive throws, throwing the whole number of them at once: we might theoretically sort them into fictitious groups each equivalent to a crown. There would be 29 such groups, viz.:—10 groups, each consisting of 1 crown; 6 groups, each of 5 shillings; 1 group of three shillings and 6 fourpenny pieces; 6 groups each of 15 fourpenny pieces; and a residue of 4 fourpenny pieces, which may be disregarded. Hence, on the already expressed understanding that we do not care to trouble ourselves about smaller sums than a crown, the results of the successive throws of the medley of coins would be approximately the same as those of throwing at a time 29 crowns, and would be expressed by the coefficients of a binomial of the 29th power. Hence I conclude that all miscellaneous influences of a few small and many minute kinds, may be treated for a first approximation exactly as if they consisted of a moderate number of small and equal alternatives.

The second approximation has already been alluded to; it consists in taking some account of the minute influences which we had previously agreed to ignore entirely, the effect of which is to turn the binomial grades into a binomial ogive. I effect it by drawing a curve with a free hand through the grades, which affords a better approximation to the truth than any other that can *à priori* be suggested.

I will now show from quite another point of view (1) that the exponential ogive is, on the face of it, fallacious in a vast number of cases, and (2) that we may learn what is the greatest possible number of elements in the binomial whose ogive most nearly represents the generic series we may be considering. The value of $\frac{m}{q-m}$ is directly dependent on the number of elements; hence, by knowing its value, we ought to be able to determine the number of its elements. I have calculated it for binomials of various powers, protracting and interpolating, and obtain the following very rough but sufficient results for their ogives (not *grades*):—

Number of (equal) elements.	Value of $\frac{m}{q-m}$.
17	5
32	10
65	15
107	20
145	25
186	30
999	48

f. 7v

Now, if we apply these results to observed facts, we shall rarely find that the series has been due to any large number of equal elements. Thus, in the stature of man ~~the probable error~~, $\frac{m}{q-m}$, is about 30, which makes it impossible that it can be looked upon as due to the effect of more than 200 equally small elements. On consideration, however, it will appear that in certain cases the number may be *less*, even considerably less, than the tabular value, though it can never exceed it. As an illustration of the principle upon which this conclusion depends, we may consider what the value of $\frac{m}{q-m}$ would be in the case of a wall built of 17 courses of stone, each stone being 3 inches thick, and subject to a mean error in excess or deficiency of one fifth of an inch. Obviously the mean height m of the wall would be 3×17 inches; and its probable error $q-m$ would be very small, being derived from a binomial ogive of 17 elements, each of the value of only one fifth of an inch. Now we saw from our previous calculation that this would be eight fifths, or 1·6 inch, which would give the value to $\frac{m}{q-m}$ of $\frac{51}{1 \cdot 6}$, or about 321; consequently we should be greatly misled if, after finding by observation the value of that fraction, and turning to the Table and seeing there that it corresponded to more than 200 equal elements, we should conclude that that was the number of courses of stones. The Table can only be trusted to say that the number of courses certainly does not exceed that number; but it may be less than that.

The difficulty we have next to consider is that which I first mentioned, but have intentionally postponed. It is due to the presence of influences of extraordinary magnitude, as Aspect in the size of fruit. These influences must be divided into more than two phases, each differing by the same constant amount from the next one, and that difference must not be greater than exists between the opposite phases of the "small" alternatives. If we had to divide an influence into three phases, we should call them "large," "moderate," and "small;" if into four, they would be "very large," "moderately large," "moderately small," and "very small," and so on. Any objects (say, fruit) which are liable to an influence so large as to make it necessary to divide it into three phases, really consist of three series generically different which are entangled together, and ought theoretically to be separated. If there had been two influences of three phases, there would be nine such series, and so on. In short, the fruit, of which we may be considering some hundred or a few thousand specimens, ought to be looked upon as a multitude of different sorts mixed together. The proportions *inter se* of the different sorts may be accepted as constant; there is no difficulty arising from that cause. The question is, why a mixture of series radically different, should in numerous cases give results apparently identical with those of a simple series.

For simplicity's sake, let us begin with considering only one large influence, such as aspect on the size of fruit. Its extreme effect on their growth is shown by the difference in what is grown on the north and south sides of a garden-wall, which in such kinds of fruit as are produced by orchard-trees, is hardly deserving of being divided into more than three phases, "large," "moderate," and "small." Now if it so happens that the "moderate" phase occurs approximately *twice as often* as either of the extreme phases (which is an exceedingly reasonable supposition, taking into account the combined effects of azimuth, altitude, and the minor influences relating to shade from leaves &c.), then the effect of aspect will work in with the rest, just like a binomial of two elements. Generally the coefficients of $(a+b)^n$ are the same as those of $(a+b)^{n-r} \times (a+b)^r$. Now the latter factor may be replaced by any variable function the frequency and number of whose successive phases, into which it is necessary to divide it, happen to correspond with the value of the coefficients of that factor.

It will be understood from what went before, that we are in a position to bring these phases to a common measure with the rest, by the process of fictitious grouping with appropriate doses of minute influences, as already described.

On considering the influences on which such vital phenomena depend as are liable to be treated together statistically, we shall find that their mean values very commonly occur with greater frequency than their extreme ones; and it is to this cause that I ascribe the fact of large influences frequently working in together with a number of small ones without betraying their presence by any sensible disturbance of the series.

The last difficulty I shall consider, arises from the fact that the individuals which compose a statistical group are rarely affected by exactly the same number of variable influences. For this cause they ought to have been sorted into separate series. But when, as is usually the case, the various intruding series are weak in numbers, and when the number of variable influences on which they depend does not differ much from that of the main series, their effect is almost insensible. I have tried how the figures would run in many supposititious cases; here is one taken at haphazard, in which I compare an ordinary series due to 10 alternatives, giving $2^{10}=1024$ events, with a compound series.

f 1

Mr. GALTON, F.R.S., submitted a proposal, which had received the sanction of the Council, for obtaining anthropological statistics from schools, etc., as follows:

PROPOSAL *to* APPLY *for* ANTHROPOLOGICAL STATISTICS *from* SCHOOLS. By FRANCIS GALTON, F.R.S.

NOTWITHSTANDING the many efforts made by statisticians, materials do not yet exist from which the physical qualities of the British people may be deduced with such precision as is needed for various theoretical inquiries. We do not know whether the general physique of the nation remains year after year at the same level, or whether it is distinctly deteriorating or advancing in any respects. Still less are we able to ascertain how we stand at this moment in comparison with other nations, because the necessary statistical facts are, speaking generally, as deficient with them as with ourselves.

Yet an important part of this information seems easy of acquirement, if it be sought for in the right direction and not on too large a scale. My object in these short remarks is to point out a method by which I believe the Anthropological Institute might successfully promote the collection of very important materials, and be enabled to publish general results of high value.

The Anthropological Institute could never undertake to deal with individual cases in the way that the census does; but it might deal with the authorities of a moderate number of homogeneous societies, each representing a well-defined class, if such could be found, who would undertake the collection and classification of their own statistics. Then, by referring to the census, we should learn the proportion which these several classes bear to the entire nation, and be enabled to combine the returns in that proportion, so as to obtain figures true for the kingdom at large.

Homogeneous groups of boys, girls, and youths already exist in several large schools, under conditions which offer extraordinary facilities for obtaining the required statistics. The masters are trustworthy and intelligent in no common degree; they are in habitual face to face communication with every pupil; and the general organisation of schools is in every way favourable to collecting full and accurate statistics. As different grades of schools represent different orders of the community, their statistics, combined on the principle already explained, ought to give an excellent picture of the younger portion of the British nation. In these short remarks I shall dwell exclusively upon schools, because I believe their authorities might be induced, in not a few instances, to co-operate heartily and with great intelligence; and if they did so, the object of the inquiry and the value of the results would become very generally appreciated. The boys when they grow up into men would retain favourable recollections of the whole procedure, and application might then be made to Universities, Factories, and other large bodies of adults, with greater probability than at present of obtaining the required information.

I suppose the authorities at each school not only to make the necessary measurements, but also to classify them, according to a form previously agreed upon, and common to all, in order that the results from the different schools may be combined together by a no more difficult process than that of simple addition.

I will now submit for consideration, suggestion, and discussion, a plan as to the specific inquiries to be made and the form in which the returns should be sent to us.

Height.—The returns I propose we should ask for, relating to height, would be of the following character.

Returns from School.

TABLE, shewing the number of boys in School of the various degrees of height, classified according to their ages.

Height (without Shoes).		Years of Age on the last Birthday.							
Above.	Under.	Etc.	9	10	11	12	13	14	Etc.
Etc.	Etc.								
5 ft. 1 in	5 ft. 2 in.								
5 ft. 2 in.	5 ft. 3 in.								
5 ft. 3 in.	5 ft. 4 in.								
Etc.	Etc.								

When we had received returns from a sufficient number of schools, we should sort them into separate groups, and publish the total results of each group. I should deprecate printing the returns of the schools separately, with their names attached, because it would give the appearance of a comparison of the schools, as to which turned out the best developed boys. It is not within our province to do this, and any suspicion that it might be done would foster a tendency to insert doubtful cases in the higher class. What I propose is, to treat the returns, so far as we are concerned, as confidential; to group the schools in natural classes; and to publish aggregate returns in percentages. Thus—schools A, B, C, etc., would be shown to give such and such general results. Of course, the schools could make any other use they pleased of their own statistics.

It will be observed that the figures in the above schedule would not only give us the information we primarily sought, but they would also give us the law of growth in different classes, both in town and in country. This is known to vary exceedingly under different conditions, but exact numerical determinations have yet to be established.

Weight is the second requirement. Its importance in estimating the physique of a nation is even greater than that of height. Taken in conjunction with the latter, it shows in what degree the different classes vary in bulk of frame and general robustness. The returns would refer to the weight of the boy in the dress ordinarily worn in-doors, and they would be classified in exactly the same way as those of height; that is to say, in classes differing each from its predecessor by equal degrees As the weights furnished with the various forms of large scales commonly in use in England, are adapted to stones of 14 lbs., I should suggest that the above-mentioned degrees be in half stones. Thus—above 6 stones and under $6\frac{1}{2}$ stones; above $6\frac{1}{2}$ stones and under 7 stones, etc.

Thus, there are three subjects of statistical record which I propose on the ground of their being of primary importance.—1. Age, which runs through both the other groups; 2. Height; 3. Weight. It does not seem unreasonable to hope that returns of these might be obtained through the agency of the Anthropological Institute from many large schools of every well-defined grade, condition of residence, and class of society.

It seems to me better not to speak at present of the attractive and numerous problems that might be solved by a wider range of inquiry; because, if we confine the attention of those we ask to few and simple questions, we are far more likely to have them well and thoroughly answered, than if we had issued a more ambitious programme. We shall soon learn the amount and value of the co-operation we may rely upon, and can arrange our future proceedings accordingly.

Discussion.

Mr. Serjeant Cox said he believed that Quetelet, in his recent work, had made an extensive and valuable collection of statistics of the kind suggested by Mr. Galton, which would form a basis for comparison with those proposed to be collected by the institute. Some hints might be taken from his book as to the facts most desirable to be obtained.

Sir Duncan Gibb remarked, that, unless the inquiries suggested in Mr. Galton's paper, were extended to the schools throughout the entire country, they would have no reliable value as statistics. To select a few schools only, in particular places, as he proposed, would be useless for the purposes of comparison as relating to the entire youthful scholastic population. There could be no doubt whatever, of the value of the information to be derived in its bearing upon the physical development of the English people generally, and it should not be limited nor restricted in the mode adopted to obtain it, and he, the speaker, did not see that there would be any material difficulty in obtaining it, when the intelligence and co-operation of the masters of the schools was frankly appealed to.

Mr. Galton explained, that it was his desire to obtain statistics from schools of all description, as public schools, middle class schools, and others, down to those of pauper children.

Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, February 27, 1874.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Francis Galton, Esq. F.R.S. M.R.I.

On Men of Science, their Nature and their Nurture.

The purport of this discourse is to specify the chief qualities by which the English men of science of the present day are characterized, to show the possibility of defining and roughly measuring the amount of any of those qualities, and to conclude by summarizing the opinions of the scientific men on the merits and demerits of their own education, giving an interpretation of what, according to their own showing, they would have preferred. My data are obtained from a large collection of autobiographical notes, most obligingly communicated to me by a large part of the leading members of the scientific world. Applications were addressed to 180 Fellows of the Royal Society, who, in addition to their "F.R.S.," had gained medals or filled posts of recognized scientific position; 115 answers have already been received, of which 80 or 90 are full and minute replies to my long and varied series of questions. But I can deal with only a few deductions from this valuable material, and must refer to a forthcoming work for the rest.

It is of interest to know the ratio which the members of the leading scientific men bear to the population of England generally. I obtain it in this way. Although 180 persons only were on my list, I reckon that it would have been possible to have included 300 of the same ages, without descending in the scale of scientific position; also it appears that the ages of half of the number on my list lie between 50 and 65, and that about three-quarters of these may be considered English. I combine these numbers, and compare them with that of the male population of England and Wales, between the same limits of age, and find the required ratio to be about one in 10,000. What then are the conditions of nature, and the various circumstances and conditions of life,—which I include under the general name of nurture,—which have selected that one and left the remainder? Some may feel surprise that so many as 300 persons are to be found in the United Kingdom who deserve the title of scientific men; probably they have been accustomed to concentrate their attention upon a few notabilities, and to ignore their colleagues. It must, however, be recollected that all biographies, even of the greatest men, reveal numerous associates and competitors whose merit and influence were far greater than had been suspected by the outside world. Great discoveries have often been made simultaneously by workers ignorant of each other's labours. This shows that they had derived their inspiration from a common but hidden source, as no mere chance would account for simultaneous discovery. It would appear that few discoveries are wholly due to a single man, but rather that vague and imperfect ideas, which float in conversation and literature, must grow, gather, and develop, until some more perspicuous and prompt mind than the rest, clearly sees them. The first discoverers beat their contemporaries in point of time, and it is therefore due to them, not that science progresses, but that her progress is as rapid as it is. We must neither underrate nor overrate their achievements. I would compare the small band of men who have achieved a conspicuous scientific position, to islands, which are not the detached phenomena they appear, but only the uppermost portions of hills, whose bulk is unseen. To pursue this metaphor; the range of my inquiry dips a few fathoms below the level at which popular reputation begins.

I proceed to speak of the qualities which the returns specify as most conspicuous in scientific men, and I shall endeavour to make them tell their own tale by quoting anonymous extracts from their communications.

The first in order of importance is energy, both of body and of mind. It appears to be possessed in an unusual degree by three-fourths of the men in my list. I should mention that the list contains a very few names of travellers of extraordinary endurance, such as the late Dr. Livingstone; but that I do not speak of these in the following extracts:—1. "Have rowed myself in a skiff 105 miles in 21 hours whilst undergraduate at Cambridge." (This is, I believe, a feat that not one undergraduate in 500 could do.) "Rowed in every race during my stay at the University; rowed two years in the University crews." 2. "Walked many a time 50 miles a day without fatigue, and kept up five miles an hour for three or four hours." 3. "Excelled at school and college in athletic sports, especially in jumping (18 feet). Almost incapable of mental fatigue up to the age of 38. Usually engaged in literary work until long after midnight." 4. "As a boy of 17, I worked for three months all day and all night with not more than four or five hours' sleep. When full of a subject and interested in it, I have written for seven or eight hours without interruption."

Severe scientific work is often done during the night by men engaged all day in anxious business; thus:—"In early life as a boy, I was engaged in business from twelve to fourteen hours a day, yet always found time to study and make my own instruments. Later on, my studies and scientific work were always accomplished after business hours, and it was generally my habit to commence after dinner, and to work at science until 2, 3, or 4 A.M., and to begin business again at 9 A.M. I never thought of rest if I had anything in hand of interest."

I may mention that energy appears to be correlated with smallness of head, a fact which comes out conspicuously here, although the average circumference of head among the scientific men is great. Energy is also, as we have seen, strongly marked among them; but it is much the more strongly marked among those who have small heads. I have ninety-nine returns, many of which I have verified myself, using the hat-maker's whalebone-hoop and measuring inside the hats. It appears that the average circumference of an English gentleman's head is $22\frac{1}{4}$ to $22\frac{1}{2}$ inches. Now, I have only thirteen cases under 22 inches and eight cases of 24 inches or upwards. The general scientific position of the small-headed and large-headed men seems equally good; but the fact is conspicuous that, out of the thirteen of the former, there are only two or three who have not remarkable energy; and out of eight of the latter there is only one who has. A combination of great energy and great intellectual capacity is the most effective of all conditions; but, like the combination of swiftness and strength in muscular powers, it is very rare.

The excellence of the health of the men in my list is remarkable, considering that the majority are of middle and many of advanced ages. One quarter of the whole have excellent or very good health, a second quarter have good or fair, a third have had good health since they attained manhood, and only one quarter make complaints or reservations. Here are two examples of excellent health:—1. "Only absent from professional duties two days in thirty years; only two headaches in my life." The next is from a correspondent who is between 70 and 80 years of age. 2. "Never ill for more than two or three days except with neuralgia; no surgical operations except inoculation, drawing of one tooth, and cutting of corns." It is positively startling to observe in these returns the strongly hereditary character of good and indifferent constitutions. I have classified the entries, each entry giving the health of the scientific man, of his father and of his mother respectively, and find as follows:—First, a long row of such terms as these: "Excellent; excellent; excellent;" or "Good; good; good;" then comes another row in which some ailment is specified by the scientific man as affecting himself, and as having also affected one or other of his parents. Examples:—1. "Excellent, but hay fever; father, excellent, but severe hay fever." 2. "Good in early life, subject to headache; father, good, subject to headache." 3. "Delicate in early life, one lung seriously affected; mother delicate and phthisical." I can find only two cases, neither very strongly marked, in which both parents were described as unhealthy, although marriages between such persons are not infrequent. These returns seem to show that the issue of such marriages are barely capable of pushing their way to the front ranks of life. All statistical data concur in proving that healthy persons

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are far more likely than others to have healthy progeny; and this truth cannot be too often illustrated, until it has taken such hold of the popular mind, that considerations of health and energy shall be of recognized importance in questions of marriage, as much so as the more immediately obvious ones of rank and fortune.

Steady perseverance is a third quality on which much stress is laid, but this might have been anticipated, and it is unnecessary to quote instances.

Some prevalence of practical business habits might also have been anticipated, but it proves much more common than I had expected. Among those who have sent me returns, I count no less than seventeen who are active heads of great commercial undertakings. There are also ten medical men in the highest rank of practice, and eighteen others who fill or have filled important official posts. A most eminent biologist wrote as follows, in reply to the inquiry whether he had any special tastes bearing on scientific success, in addition to those for his own line of investigation:—"I have no special talent except for business, as evinced by keeping accounts, being regular in correspondence, and investing money very well." It is clear that method and order are essential to the man who hopes to deal successfully with masses of details.

Next, as regards the more special qualities; those already mentioned, of energy, health, steadiness of pursuit, and business habits being of general application. The first of these is independence of character. Fifty of my correspondents show that they possess it in excess, and in only two is it below par. Here are a few examples:— 1. "Left æt. 12" [that is, ran away from] "a school where I had received injustice from the master." 2. "Opinions in almost all respects opposed to those in which I was educated." 3. "I have always taken my own independent line. My heresy prevented my advancement." 4. "Preference for whatever is not the fashion, not popular, not rich, not very able to help itself, yet with qualities unworthily overlooked or unjustly oppressed." The home atmosphere which the scientific men breathed in their youth was generally saturated with the spirit of independence. Examples:—1. "My father was extremely independent, in some respects more so than I am. He never took off his hat to anyone in his life, and never addressed anyone as Esq." 2. "My father was a Liberal when Liberalism (then styled Jacobinism) was highly obnoxious, an early denouncer of slavery and advocate of religious liberty, a free trader when the world was protectionist, and an opponent of unrighteous war when war was most popular. He was for mitigating our criminal code when hanging was regarded as the sheet-anchor, and, in a word, was politically and socially a very independent spirit." In confirmation of the assertion that the scientific men were usually brought up in families characterized by independence of disposition, I would refer to the strange variety of small and unfashionable religious sects to which they or their parents belonged. We all know that Dalton, the discoverer of the atomic theory, and Dr. Young, of the undulatory theory of light, were both Quakers, and that Faraday was a Sandemanian. So I find in these returns numerous cases of Quaker pedigree; and I know of one man, not as yet technically on my list, who was born a Sandemanian. There are also representatives of several other small sects, as Moravians and Bible Christians, and the Unitarians are numerous. It will be understood that the object of saying this is not to throw light on the religious tendencies of the scientific men (concerning which I have much material), because so off-hand a statement would mislead, but to prove that they and their parents had the habit of doing what they preferred, without considering the fashion of the day. The man of science is thoroughly independent in character.

We now come to what I look upon as the salt of the character of most scientific men, namely, strong innate taste for science or for some special branch of it. It is not universal even among those who have had the highest success, but it is very common, and it sometimes attains to the height of a passion which is not transient, but abides. Though decidedly hereditary in numerous cases, its appearance is more capricious than health or energy, and it often happens that the scientific man is the only member of his family in whom the taste has shown itself. The following are a few examples of innate taste:— 1. "Thoroughly innate; I had no regular instruction, and can think of no event which especially helped to develop it. Bones and shells were attractive to me before I could consider them with apparent profit, and I had a fair zoological collection by the time I was 15." 2. "If any tastes be innate, mine were. They date from beyond my recollection. They were not determined by events occurring after manhood, but I think the reverse; they were discouraged in every way." 3. "I should say innate. As to whether they were largely determined by events occurring after manhood, I think not. All I can say is, that neither profession, nor marriage, nor sickness, have been able to affect them." 4. "As far back as I can remember, I loved nature and desired to learn her secrets, and I have spent my whole life in searching for them. While a schoolboy I taught myself . . . under great difficulties."

Let us now put these results together. We have seen that energy, health, steady pursuit of purpose, business habits, independence of character, and a strong innate taste for science, are characteristics of scientific men. Probably one half of the men on my list possess every one of them in a considerable and some of them in a very high degree. If one or more of these qualities be deficient, success becomes impossible, unless its absence is supplemented by other and as yet unclassified conditions. The want of time prevents me from entering into these, and I must postpone further results to a future publication. However, two groups of cases may be specified in which only a few of the above-mentioned qualities are present, and which end in an abortive career. The one is the possession of energy, health, and independence of character in excess, and little else to control them.

These are dangerous gifts. Those who have them are apt to renounce guidances by which the great body of mankind move safely, and to follow out a career in which they are almost certain to blunder and fail egregiously. Probably every large emigrant ship takes out many such men, full of unjustifiable self-confidence, who, to use a current phrase, "knock about in the world," waste their health, youth, and opportunities, and end broken down. Another common group of cases are those where a strong innate taste for science is accompanied by independence of character and steadiness of pursuit, but with no other quality helpful to success, and who therefore fail. There is hardly a village where some ingenious man may not be found who has ideas and much shrewdness, but is crotchety and impracticable. He wants energy and business habits, and so he never rises. There are many who brood over subjects like perpetual motion, whose peculiarities are well illustrated in De Morgan's book of paradoxes. We also frequently meet persons of the stamp that justifies the old-fashioned caricature of scientific men, being absorbed in some petty investigation, utterly deficient in business habits, and noted for absence of mind. I may add that even idiots have often strongly quasi-scientific tastes, as love for simple mechanism, or objects of natural history, and they have a pleasure in collecting. Also, we all know that madmen have often persistency, as shown by their brooding on a single topic.

Lastly, I wish to give some idea of the very general prevalence of mechanical tastes among the scientific men generally. One would have expected to find it among mechanicians and physicists, but it is just as strong among the biologists and others. One chemist made a 12-inch reflecting telescope; two eminent surgeons have an extraordinary aptitude for and love of mechanical manipulation; two very eminent biologists had a passion for it, and both, if they had followed the bent of their own minds, would have been engineers by profession.

All tends to show that the scientific mind is directed to facts and abstract theories, and not to persons or human interests. The man of science is deficient in the purely emotional element, and in the desire to influence the beliefs of others. Thus I find that two out of every ten do not care for politics at all; they are devoid of partisanship. They school a naturally equable and independent mind to a still more complete subordination to their judgment. In many respects their character is strongly anti-feminine. It is a curious proof of this, that in the very numerous answers which have reference to parental influence, that of the father is quoted three times as often as that of the mother. It would not have been the case, judging from inquiries I elsewhere made, if I had been discussing literary men, commanders, or statesmen, or, still more, divines.

I regret much that time makes it impossible for me now to dive deeper into the rich mine of facts contained in my returns. It becomes necessary for me to leave this branch of the subject and to pass on to some interesting considerations regarding the measurement of qualities such as those we have been engaged upon. These considerations are of the most general application, and are as applicable to magnitude as they are to intellect and morals, and to every form of animal or vegetable life as they are to men. I shall therefore speak about the size of nuts, and peas, and acorns, as being easily experimented on, and deduce from these the results which I would fain apply to the moral and intellectual qualities of mankind.

The law of statistical constancy may be taken for granted. It is evidenced by the experience of insurance offices against fire, death, shipwreck, and other contingencies, always with the proviso that the facts are gathered with discretion, on well-known general principles. Hence we may say with assurance, that although two common nuts may differ, yet the contents of different packets, each containing 1000 nuts, will be scarcely distinguishable, for the same number of nuts of different sizes will be found in each. Let the contents of the several packets be each arranged in a long row, in order of size, beginning with the biggest nut and ending with the smallest, and place the rows rank behind rank; then by the law of statistical constancy the nuts in the same *files* will in all cases be closely alike (except the outside ones, where more irregularity prevails). Again, if we incorporate two rows into one of double length, still preserving the arrangement as to regular gradation in size, the centre nuts of the two original series will still be found at or near the centre of the compound series, the nuts in quarter positions will still be in quarter positions, and so on. Hence, whatever be the length of the series the *relative* position in it of the nut will be a *strict criterion* of its size. This is of course equally true of all groups of qualities or characters whatever, in which the law of statistical constancy prevails, the series, in each case, being arranged according to gradations of the quality in question. Each individual is measured against his neighbour, and it is quite unnecessary to have recourse to any external standard. As regards a scale of equal parts, I make use of a converse application of the law of "frequency of error" [this was illustrated by many experiments], which shews that in a row (say as before) of nuts, if we take those which occupy the three quarterly divisions (1st quarter, centre, 3rd quarter) as three elementary graduations of size, a continuous scale of graduations will be determined by the following series, in which the places of the nuts are supposed to be reckoned from the end of the row where the large nuts are situated, and to be given in per-thousandths of the entire length of the row. It might be called the "Common Statistical Scale" (S. S.). The place of $+4°$ would be at 4 thousandths from large end; $+3°$ at 21 thousandths; $+2°$ at 89; $+1°$ at 250; $0°$ at 500; $-1°$ at 750; $-2°$ at 911; $-3°$ at 979; and $-4°$ at 996, or 4 thousandths from the small end of the row. Thus if we say that the size of a nut is $+2°$ S. S., we absolutely define, or rather identify, what we are speaking about. Anybody can procure such a nut independently by getting a quart of nuts and arranging them. Also we know that the difference between a nut of $+4°$ S. S. and $+1°$ S. S. is $3°$, and therefore three times as great as between one of + 2° S. S. and the latter. It cannot be affirmed that this is a precise scale of equal parts for all qualities, but it is found to hold surprisingly well in a great variety of vital statistics; perhaps, too, the mere thickness of tissues may be a chief element in the physical basis of life. This scale appears, at all events, more likely to be nearly approximative to one of equal parts, for qualities generally, than any other that can be specified, and it certainly affords definite standards subject to the law of statistical constancy. The habit should therefore be encouraged in biographies, of ranking a man among his contemporaries, in respect to every quality that is discussed, and to give ample data in justification of the rank assigned to him. By the general use of a system like the above, which is universally applicable, social and political science would be greatly raised in precision.

I now pass on to the education which the scientific men had in their youth, in the hope that my results may give assistance to those who are endeavouring to frame systems of education suitable to the wants of the day. What I have to say, is very partial; it refers solely to the opinions the scientific men entertain of the merits and faults of their own several educations. Their views are remarkably unanimous, considering the very different branches of inquiry they are interested in, and the great dissimilarities in their education. I should mention, that one-third have been educated at Oxford or Cambridge, one-third at Scotch, Irish, or London Universities, and the remaining third have been at no University at all. I am totally unable to decide which of the three groups occupies the highest scientific positions, they seem to me very much alike in this respect. The merits they all ascribe to variety of education are to be gathered from the following examples:—1. "Not tied down to old courses of classics and mathematics." 2. "Sufficient groundwork in many subjects to avoid error." 3. "Early introduced to many subjects of interest." 4. "A well-balanced education, including chemistry, botany, logic, and political economy." 5. "A variety of subjects, and attention to details." 6. "Coming in contact with persons of every rank and sitting in the same form" [in a Scotch school] "with the sons of tradesmen, and ploughmen, as well as gentlemen." In contrast to this, here are some examples as to the faults of their education:—1. "No mathematics, nor modern languages, nor any habits of observation or reasoning." 2. "Enormous time devoted to Latin and Greek, with which languages I am not conversant." 3. "Omission of almost everything useful and good, except being taught to read; Latin, Latin, Latin!" 4. "In an otherwise well-balanced education, three years were spent on Latin and Greek grammar, a blank waste of time." 5. "Neglect of many subjects for the attainment of one or two; not pushing mathematics to a useful end." Evidence such as this, which could be largely added to, establishes the advantage of variety of study. One group of men speak gratefully because they had it, and another group speak regretfully because they had it not. I find none who had a reasonable variety who disapproved of it, none who had a purely old-fashioned education who were satisfied with it. The scientific men who came from the large public schools usually did nothing when there; they could not assimilate the subjects taught, and have abused the old system heartily. There are several serious complaints about superficial and bad teaching which I need not quote. Overteaching is thoroughly objected to; thus, in speaking of merits of education, I find :—1. "Freedom to follow my own inclinations, and to choose my own subjects of study, or the reverse." 2. "The great proportion of time left free to do as I liked, unwatched and uncontrolled." 3. "Unusual degree of freedom." I should add, that there are many touching evidences of the strong effect of home encouragement and teaching. As regards the subjects specially asked for, even by biologists, mathematics take a prominent place. Two of my correspondents speak strongly of the advantages derived from logic, and the weighty judgment of the late John S. Mill powerfully corroborates their opinions. Accuracy of delineation is also spoken of, and, owing to the extraordinary prevalence of mechanical aptitudes, I believe that the teaching of mechanical manipulation would be greatly prized. The interpretation that I put on the answers as a whole, is as follows: To teach a few congenial and useful things very thoroughly, to encourage curiosity concerning as wide a range of subjects as possible, and not to overteach. As regards the precise subjects for rigorous instruction, the following seem to me in strict accordance with what would have best pleased those of the scientific men who have sent me returns :—1. Mathematics, pushed as far as the capacity of the learner admits, and its processes utilized as far as possible for interesting ends and practical application. 2. Logic (on the grounds already stated, but on those only). 3. Observation, theory, and experiment, in at least one branch of science; some boys taking one branch and some another, to ensure variety of interests in the school. 4. Accurate drawing of objects connected with the branch of science pursued. 5. Mechanical manipulation, for the reasons already given, and also because mechanical skill is occasionally of great use to nearly all scientific men in their investigations. These five subjects should be *rigorously* taught. They are anything but an excessive programme, and there would remain plenty of time for that variety of work which is so highly prized, as: ready access to books; much reading of interesting literature, history and poetry; languages learnt, probably best during the vacation, in the easiest and swiftest manner, with the sole object of enabling the learners to read ordinary books in them. This seems sufficient, because my returns show that men of science are not made by much teaching, but rather by awakening their interests, encouraging their pursuits when at home, and leaving them to teach themselves continuously throughout life. Much teaching fills a youth with knowledge, but tends prematurely to satiate his appetite for more. I am surprised at the mediocre degrees which the leading scientific men, who were at the Universities, have usually taken, always excepting the mathematicians. They prefer to fix of their own accord on certain subjects, and seem averse to learn what is put before them as a task. Their independence of spirit and coldness of disposition are not conducive to success in competition, they doggedly go their own way and refuse to run races.

Science has hitherto been at a disadvantage compared with other competing pursuits, in enlisting the attention of the best intellects of the nation, for reasons that are partly inherent and partly artificial. To these I will briefly refer in conclusion, with especial reference to the very important question, as to how far the progress of events tends to counterbalance or remove them.

If we class energy, intellect, and the like, under the general name of ability, it follows that, other circumstances being the same, those able men who have vigour to spare for extra professional pursuits, will be mainly governed in the choice of them by the instinctive tastes of their manhood. The majority will address themselves to topics nearly connected with human interests, a few only will turn to science. This tendency to abandon the colder attractions of science for those of political and social life, must always be powerfully reinforced by the very general inclination of women to exert their influence in the latter direction. Again, those who select some branch of science as a profession, must do so in spite of the fact that it is more unremunerative than any other pursuit. A great and salutary change has undoubtedly come over the feeling of the nation since the time when the present leading men of science were boys, for the state of education was then such as an enemy might have invented on purpose to exterminate science. It crushed the inquiring spirit, the love of observation, the pursuit of inductive studies, the habit of independent thought, and it protected classics and mathematics by giving them the monopoly of all prizes for intellectual work, such as scholarships, fellowships, church livings, canonries, bishoprics, and the rest. This gigantic monopoly is yielding, but obstinately and slowly, and it is unlikely that the friends of science will be able, for many years to come, to relax their efforts in educational reform. As regards the future provision for successful followers of science, it is to be hoped that, in addition to the many new openings in industrial pursuits, the gradual but sure development of sanitary administration and statistical inquiry may in time afford the needed profession. These may, as I sincerely hope they will even in our days, give rise to the establishment of a sort of scientific priesthood throughout the kingdom, whose high duties would have reference to the health and well-being of the nation in its broadest sense, and whose emoluments and social position would be made commensurate with the importance and variety of their functions.

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[*From the* Proceedings of the Royal Society, No. 144, 1873.]

ON THE EMPLOYMENT OF METEOROLOGICAL STATISTICS IN DETERMINING THE BEST COURSE FOR A SHIP WHOSE SAILING QUALITIES ARE KNOWN.

BY

FRANCIS GALTON, F.R.S.

If we desire to estimate which of two alternative passages between the same ports would be performed most quickly on the average of many voyages, no knowledge can be more immediately useful than that of the distance which the ship could accomplish at various points of the routes

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in a unit of time. The intention of the present memoir is to show how this desideratum may be most readily obtained, and the precise method by which, when it has been obtained, it should be turned to account. It should be added that in the earlier part of it I am obliged to recapitulate views which I have already published in the Transactions of the British Association, 1866 (Transactions of Sections), p. 17.

Suppose the meteorological statistics of some ocean district to show that, on the average, out of every 100 ships that visited it the weather recorded in the following Table was experienced :—

TABLE I. Statistics of weather.

30	ships	find	the wind	N.,	with an	average	force of	3
25	,,	,,	,,	E.,	,,	,,	,,	2
15	,,	,,	,,	S.,	,,	,,	,,	1
10	,,	,,	,,	W.,	,,	,,	,,	2
20	,,	,,	,,	calm,	,,	,,	,,	0
100								

At first I will suppose no current to exist. I have grouped the winds under the 4 cardinal points for the sake of simplicity in explanation; but it must be recollected that in practice they would be grouped under at least 8 points, and probably 16.

Let the sailing qualities of the ship be those specified in Table II., in which the figures have been extracted from an elaborate but, I fear, only approximate schedule of the performances of the standard ship of meteorologists, commonly described as the "Beaufort Ship." I have

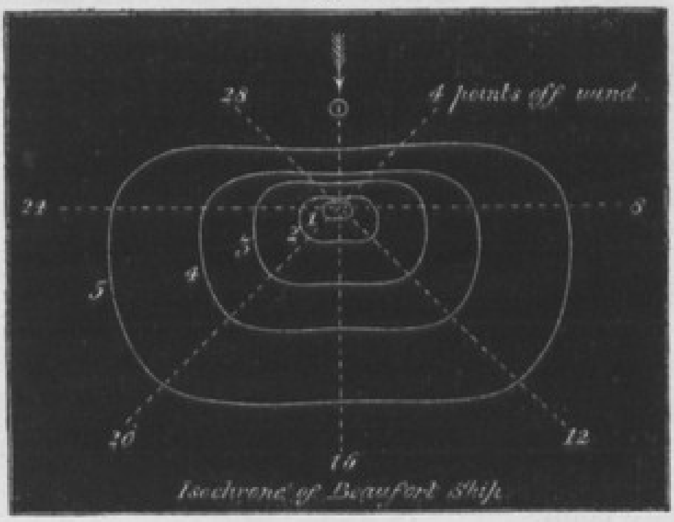
TABLE II. Sailing qualities of Ship.

Force of wind.	Number of miles made good in one day's sail. Number of points at which the course of the ship lies off the wind, reckoning to the right.			
	0 points, or wind right ahead.	8 points, or wind abeam (same value as 24 points).	16 points, or wind astern.	24 points, or wind abeam (same value as 8 points).
1	5	12	12	12
2	10	38	34	38
3	24	89	79	89
4	38	139	125	139
5	62	230	202	230

protracted the data contained in the original schedule, and formed from them the curves represented in the annexed diagram (fig. 1).

From the materials contained in Tables I. and II. we have to calcu-

Fig. 1.

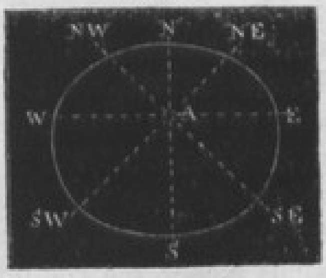


late the average distance towards each of the 4 cardinal points that the ship is capable of accomplishing in a day.

The ship that on each of the 100 occasions makes the best of her way to the N. experiences the following weather. The wind is right ahead of her (that is, is N.) on 30 occasions, with an average force of 3; therefore, under those circumstances, as we learn from Table II., she will sail an aggregate distance of $30 \times 24 = 720$ miles. On 25 occasions the wind is abeam (E.) of her, with a force of 2, and she sails under these conditions $25 \times 38 = 950$ miles; on 15 occasions it is astern (S.), with force 1, by which she makes $15 \times 12 = 180$ miles; and, lastly, it is abeam (W.) on 10 occasions, with force 2, contributing 380 miles. Therefore the total distance she would sail in the 100 voyages, each of one day's duration, and in every case to the N., is $720 + 950 + 180 + 380 = 2230$ miles; and consequently her average daily performance to that point of the compass is 22·30 miles.

Similar computations give 34·40 for an E. course, 37·75 for a S., and 38·00 for one to the W. The form of calculation is appended in Table III.

Fig. 2.



If we take a point A, fig. 2, and mark from it the distances we have just obtained, and draw a contour, with a free hand, enclosing the dots, we shall have a figure such as is represented, which determines the performance of the ship from A to every point of the compass. It should be borne in mind that, although there must be much guess work in drawing the curve under the guidance of only 4 dots, there will be considerably less chance of error when we have 8, and none of appreciable amount when we have 16. In calculating to only 4 points, we have, as is shown in Table III., no more than 4 lines and 4 columns, or $4^2=16$ entries. If we dealt with 8 points, the number of entries would amount to $8^2=64$, and if with 16, to $16^2=256$ entries; but the amount of labour involved in such tedious computations need excite no apprehension, because I shall show how calculation may be entirely dispensed with, and the results obtained by the aid of a machine with remarkable facility and quickness.

TABLE III.

Course of ship.	Relative frequency and force of wind.				Total miles on each course.
	N. 30 per cent. Force 3.	E. 25 per cent. Force 2.	S. 15 per cent. Force 1.	W. 10 per cent. Force 2.	
N.	Wind ahead, 0 point. 720 miles.	Abeam, 8 950	Astern, 16 180	Abeam, 24 380	2230
E.	Abeam, 24 2670	Ahead, 0 250	Abeam, 8 180	Astern, 16 340	3440
S.	Astern, 16 2370	Abeam, 24 950	Ahead, 0 75	Abeam, 8 380	3775
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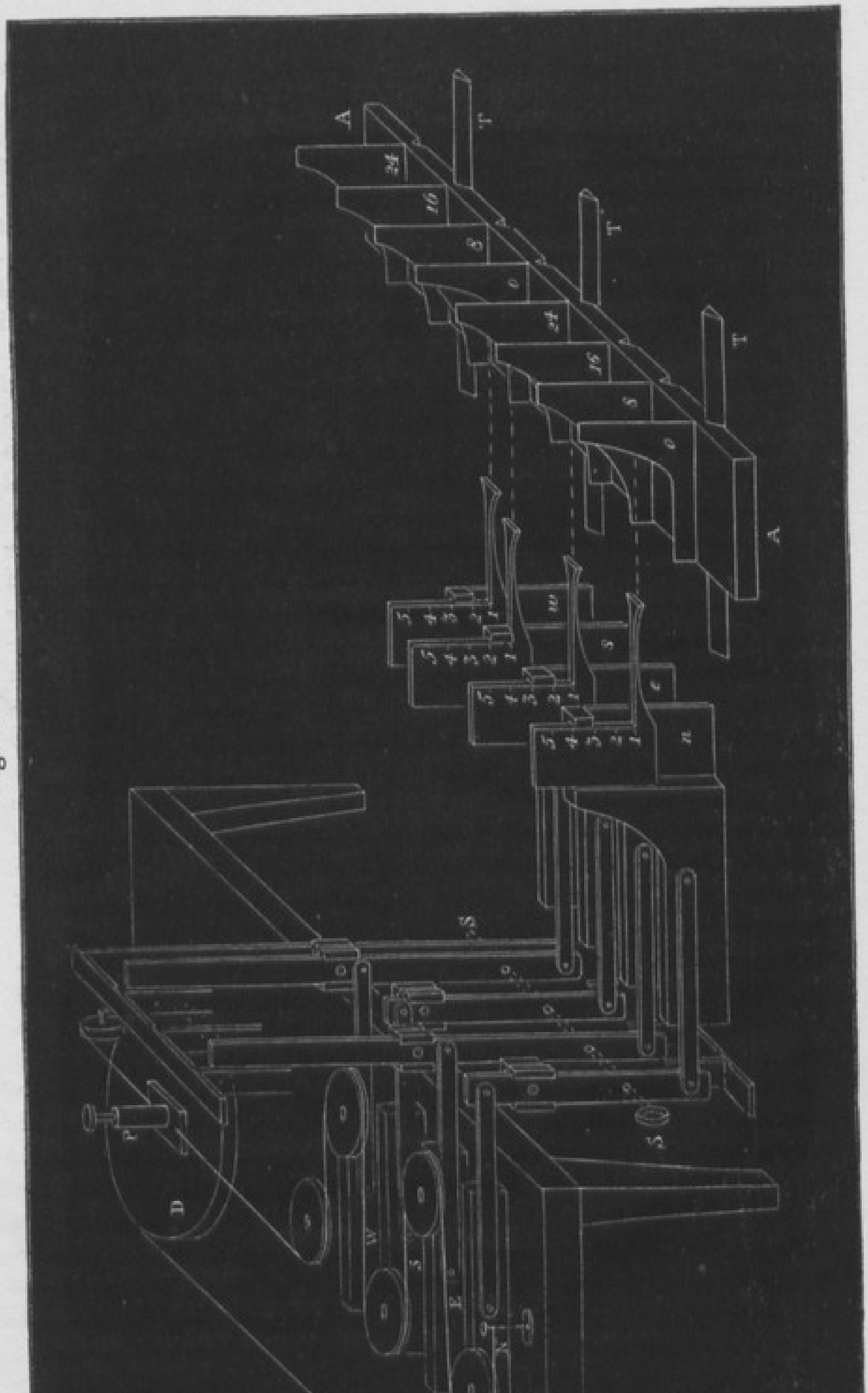

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The slides *n*, *e*, *s*, and *w* move to and fro parallel to the tramways, and are each furnished with bars that can be pushed vertically up and down, and a rod projects horizontally from each of the bars towards the zinc plate opposite to it. Graduations referring to the force of the wind are placed on the bars, which are thereby adjusted to come in contact at the proper levels with the zinc plates, and to be pushed back through a distance equal to the length of the abscissæ at those levels, when A A is run forwards on its tramway.

Thus far we have obtained the result that *n*, *e*, *s*, and *w* shall be severally pushed back to the distances which would be sailed over in one day if the wind blew on 100 occasions with specified forces from *each* of those quarters, and if the ship were sailing to that quarter whose initial is opposite to the zinc plate marked 0. As A A is adjusted in the figure, that quarter would be N.; if it were moved one step to the left it would be W., if 2 steps it would be S., and if 3 steps it would be E.

We have now to diminish the movements impressed upon *n*, *e*, *s*, and *w* in the ratio of the percentage of occurrence of the several winds. This is effected by linking them to another series of slides, N, E, S, and W, as shown in the figure, and by attaching adjustable centres to the arms, there shown in a vertical position. The standards to which those centres would be clamped are almost wholly removed in the figure, but the top and bottom of them can be seen. The reduction will be correct within such limits as we need, when the links are somewhat longer than in the figure and the arm does not swing through more than 40°. I therefore do not care to propose in this case the somewhat more complicated, but perfectly accurate arrangement which I contrived for the parallel slides of the pantagraphs now in use at the Meteorological Office, of which a description is given in the Report of the Meteorological Committee for 1870, p. 30.

I place the adjustable centres between the links in the cases of N and S, and above them in those of E and W; consequently, when *n*, *e*, *s*, and *w* are all pushed back, N and S will advance, and E and W will retreat: the reason for doing this will be seen presently. In order to graduate the arms for the adjustable centres, we must take the $\frac{1}{100}$ part of the distance between the pivots of the links as the unit of measurement, and measure in both cases from the pivot of the upper link as the zero-point. Then if p=the percentage to which the movements are to be reduced, the graduations for p between the links are determined by the formula $\frac{100p}{100+p}$, and when above the links by $\frac{100p}{100-p}$.

The results we have now attained are that, when A A is pushed forward, N, E, S, W shall move alternately forwards and backwards, each through a distance corresponding to that which a ship would sail towards the quarter whose initial is opposite to the zinc plate marked 0, under the several influences of the N., E., S., and W. winds as they are found to occur. What remains is to sum up these movements.

I put a pulley, running easily on its axis, upon each of the upper slides, as in the figure, and pass a band, one of whose ends is secured to a fixed peg, round these pulleys, alternately over and under them. The free end of the band is kept stretched by a light weight, and a framework, carrying a vertical pricker, is urged to and fro by the movements of the band. A disk (D), upon which the drawing-paper is secured, has its centre exactly below the pricker when the machine is at zero; and whenever A is pushed home, the pricker travels in a radial distance to an amount equal to twice the sum of the movements of the several slides, and therefore through a distance proportionate to a day's sail of the ship towards the quarter whose initial letter is opposite to the zinc plate marked 0. If desired, the numerical value of the movement of the string could of course be read off.

The manipulation of the instrument would be as follows:—

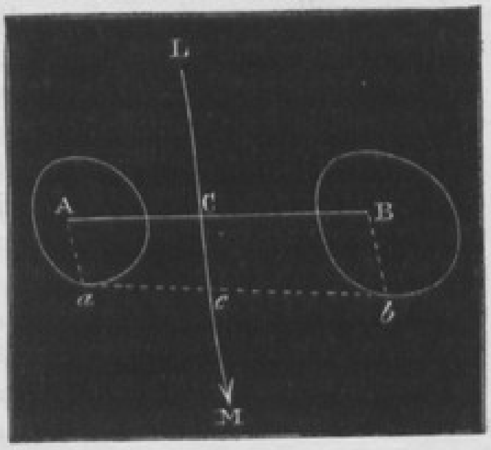
1. Remove A A. 2. Push or pull the slides *n*, *e*, *s*, *w* into their mean position. 3. Thrust a skewer, S S, through the holes in the arms and framework to hold every thing fast. 4. Adjust the centres, and clamp them if necessary. 5. Adjust the bars for force of wind. 6. Remove the skewer. 7. Push the slides *n*, *e*, *s*, *w* as far back as they will go. 8. Replace A A, and pull the slides forward to it. The machine is now in working order. *a*. Push A A home. *b*. Press the pricker, not forgetting to mark the N point. *c*. Turn the disk through a quadrant. *d*. Pull back A A, and set it one step in advance on the tramways. *e*. Pull the slides up to it.

The series 1 to 8 has to be gone through once for all for each isochrone; that from *a* to *e* for each of the 4 points of the compass. The whole of these actions are simple and rapid, and the adjustments are of the easiest kind. An isochrone based upon 4 points ought to be leisurely plotted out in 2½ minutes, and one based upon 8 points in 4 minutes.

This step-by-step arrangement is far easier of construction than one, which may suggest itself to many persons, in which the movement should be continuous, using a curved surface instead of a set of curved edges, and by which the entire curve should be drawn instead of a few points pricked out; also it is far more convenient and compact not to arrange the machine in a circular form, which is that which would most naturally first be thought of.

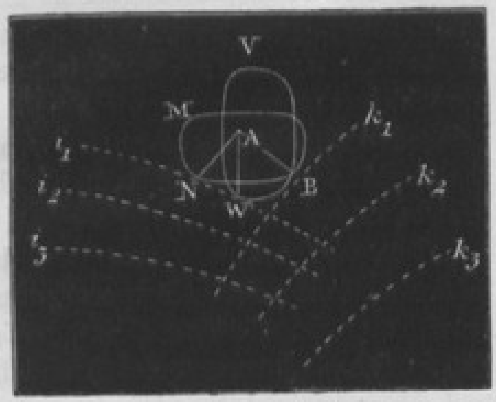
When the isochrones have been drawn to scale on a chart, isochronic lines at various points along any proposed route could readily be found by graphical interpolation. Thus, in fig. 4, let the route be from L to M, and let the isochrones round A and B be known. Draw A *a*, B *b* parallel to L M, and join A B, *a b*, cutting L M in C and *c*; then C *c* is one day's sail from C. We can do more than this; for we may find the distance of a day's sail from *any* point of L M by the following device. Erect C c' perpendicular to L M, and equal to C c; similarly, erect other perpendiculars (L l', M m') from points L, M, &c., of which the day's sail along the route has been laid down in the same way that C c was, and draw with a free hand a line through l', c', m'; then the perpendicular distance X x' from any point X on the line L M to its intersection at x' with the line l', c', m' will be the length of a day's sail from X, and it can be transferred with a pair of compasses to find x on the line L, X, M.

Fig. 4.



Another application of the principle of isochrones is to draw them at distances of 1, 2, 3, &c. days for ships bound *towards*, not from, a given port. The first day's isochrone requires no explanation. For that of the second day, a few neighbouring points must be selected on the contour already drawn, and from these such small portions as are needed of other isochrones must be constructed; the line sketched with a free hand to bound these figures will be the isochrone of two days' journey towards the port, and similarly for the third and subsequent days. The lines of shortest passage from curve to curve join the points whence the subsidiary isochrones were drawn with the points where the latter are in contact with their bounding line. The appearance of a chart so constructed would be that of a series of roughly concentric curves round the port and of radial lines of shortest passage. I give no illustration of such a chart, because it seemed a waste of labour to calculate one upon the scanty data available at the present moment, when far more ample and trustworthy materials have been collected and are in the course of gradual publication by the Meteorological Office; and calculations would have been necessary, because the machine just described exists only in the form of a rude model.

Fig. 5.

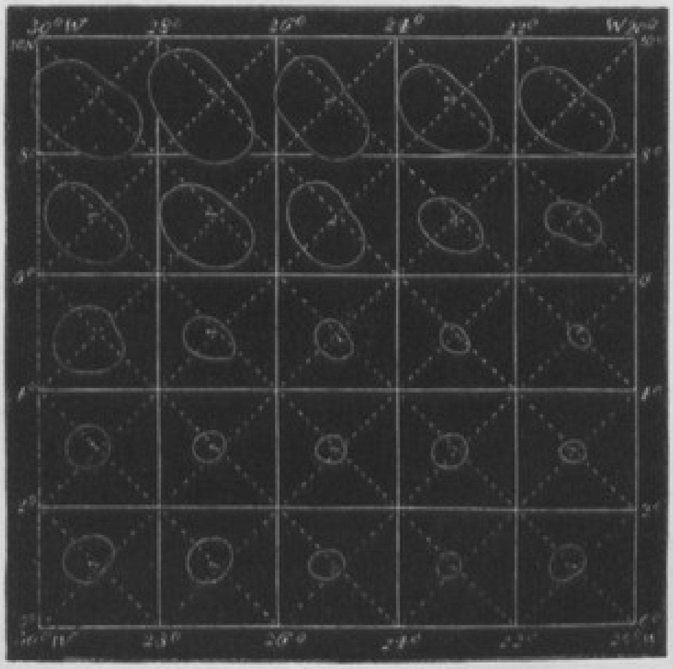


We have, lastly, to explain how the navigator would use one of these charts on specific occasions. Suppose (fig. 5) he finds himself at A with such and such weather, present and probable, how should he steer? He must use an isochrone appropriate to the occasion, most likely out of a stock which he would keep ready to hand; and applying it to his map, he would note the direction in which it will cut the largest number of isochrones towards the port. If we take cases where the isochrones for the occasion are (1) N M B and (2) W V B, the first being with a N. and the second with a W. wind, then, if the isochrones towards the port be as in the series k_1, k_2, &c., the course to be steered in both instances is A B; but if they are as in the series i_1, i_2, &c., the course in case (1) will be A N, and in case (2) A W, quite independently of the direction in which the port may happen to lie.

There is much to say about the proper method of discussing the crude statistics derived from ocean districts artificially bounded by lines of latitude and longitude in order to obtain the most probable meteorological values, but I will only allude to them here. First, homogeneous districts and periods of time have to be made out; secondly, the crude observations in each subdivision of those districts have to be discussed in connexion with those made at adjacent subdivisions in the same district; and, thirdly, they have to be discussed in reference to those made in preceding and succeeding periods of time. There is no doubt but that labour spent in these discussions would after a time become more remunerative than the same amount of labour in accumulating fresh observations.

I submit (fig. 6) a series of 8-hour isochrones computed to 8 points from the

Fig. 6.



crude observations taken in each "2-degree square" of the ocean between

10° north latitude and the equator, and between 20° and 30° west longitude, in the month of January. The uniformity of their sequence is very striking; and it would no doubt have been still more so if the data had previously been discussed in the above-mentioned manner. The short line with an arrow-head shows the direction and amount of current; but the centre of each square is the point of departure, for which the contour shows the joint effects of winds and current.

To recapitulate. I have shown in this memoir :—1, what isochrones are, and their great importance; 2, how to calculate them; 3, how to construct a machine to supersede their calculation and to make it possible to have them drawn for special cases at a trifling cost; 4, how to make an isochronal chart; and, 5, how to use it on individual occasions.

I should be glad if one result of this memoir were to bring into greater prominence than at present the high value of the ocean statistics collected and now being published by the Meteorological Office, and the fact that no degree of precision of meteorological knowledge need be thrown away in the practice of navigation. Such knowledge will be good for all time, and will always afford the requisite data whence isochrones conformable to the varying performances of new varieties of ships and to new lines of commerce may be calculated.

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[*From the* Proceedings of the Royal Society, No. 144, 1873.]

ON

THE EMPLOYMENT

OF

METEOROLOGICAL STATISTICS

IN

DETERMINING THE BEST COURSE FOR A SHIP WHOSE SAILING QUALITIES ARE KNOWN.

BY

FRANCIS GALTON, F.R.S.

If we desire to estimate which of two alternative passages between the same ports would be performed most quickly on the average of many voyages, no knowledge can be more immediately useful than that of the distance which the ship could accomplish at various points of the routes in a unit of time. The intention of the present memoir is to show how this desideratum may be most readily obtained, and the precise method by which, when it has been obtained, it should be turned to account. It should be added that in the earlier part of it I am obliged to recapitulate views which I have already published in the Transactions of the British Association, 1866 (Transactions of Sections), p. 17.

Suppose the meteorological statistics of some ocean district to show that, on the average, out of every 100 ships that visited it the weather recorded in the following Table was experienced :—

TABLE I. Statistics of weather.

30	ships	find	the wind	N.,	with	an average	force of	3
25	,,	,,	,,	E.,	,,	,,	,,	2
15	,,	,,	,,	S.,	,,	,,	,,	1
10	,,	,,	,,	W.,	,,	,,	,,	2
20	,,	,,	,,	calm,	,,	,,	,,	0
100								

At first I will suppose no current to exist. I have grouped the winds under the 4 cardinal points for the sake of simplicity in explanation; but it must be recollected that in practice they would be grouped under at least 8 points, and probably 16.

Let the sailing qualities of the ship be those specified in Table II., in which the figures have been extracted from an elaborate but, I fear, only approximate schedule of the performances of the standard ship of meteorologists, commonly described as the "Beaufort Ship." I have

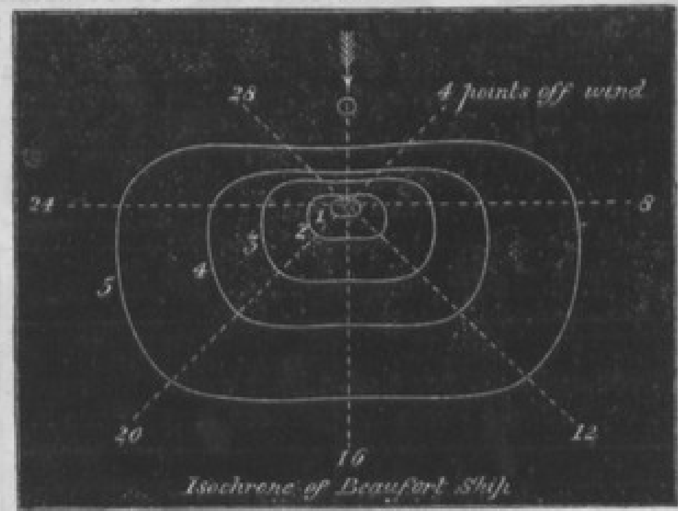
TABLE II. Sailing qualities of Ship.

Force of wind.	Number of miles made good in one day's sail. Number of points at which the course of the ship lies off the wind, reckoning to the right.			
	0 points, or wind right ahead.	8 points, or wind abeam (same value as 24 points).	16 points, or wind astern.	24 points, or wind abeam (same value as 8 points).
1	5	12	12	12
2	10	38	34	38
3	24	89	79	89
4	38	139	125	139
5	62	230	202	230

protracted the data contained in the original schedule, and formed from them the curves represented in the annexed diagram (fig. 1).

From the materials contained in Tables I. and II. we have to calcu-

Fig. 1.

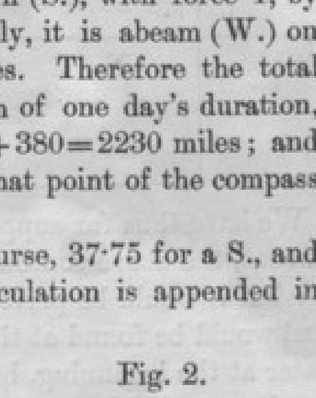


late the average distance towards each of the 4 cardinal points that the ship is capable of accomplishing in a day.

The ship that on each of the 100 occasions makes the best of her way to the N. experiences the following weather. The wind is right ahead of her (that is, is N.) on 30 occasions, with an average force of 3; therefore, under those circumstances, as we learn from Table II., she will sail an aggregate distance of $30 \times 24 = 720$ miles. On 25 occasions the wind is abeam (E.) of her, with a force of 2, and she sails under these conditions $25 \times 38 = 950$ miles; on 15 occasions it is astern (S.), with force 1, by which she makes $15 \times 12 = 180$ miles; and, lastly, it is abeam (W.) on 10 occasions, with force 2, contributing 380 miles. Therefore the total distance she would sail in the 100 voyages, each of one day's duration, and in every case to the N., is $720 + 950 + 180 + 380 = 2230$ miles; and consequently her average daily performance to that point of the compass is 22·30 miles.

Similar computations give 34·40 for an E. course, 37·75 for a S., and 38·00 for one to the W. The form of calculation is appended in Table III.

Fig. 2.



If we take a point A, fig. 2, and mark from it the distances we have just obtained, and draw a contour, with a free hand, enclosing the dots, we shall have a figure such as is represented, which determines the performance of the ship from A to every point of the compass. It should be borne in mind that, although there must be much guess work in drawing the curve under the guidance of only 4 dots, there will be considerably less chance of error when we have 8, and none of appreciable amount when we have 16. In calculating to only 4 points, we have, as is shown in Table III., no more than 4 lines and 4 columns, or $4^2=16$ entries. If we dealt with 8 points, the number of entries would amount to $8^2=64$, and if with 16, to $16^2=256$ entries; but the amount of labour involved in such tedious computations need excite no apprehension, because I shall show how calculation may be entirely dispensed with, and the results obtained by the aid of a machine with remarkable facility and quickness.

TABLE III.

Course of ship.	Relative frequency and force of wind. N. 30 per cent. Force 3.	E. 25 per cent. Force 2.	S. 15 per cent. Force 1.	W. 10 per cent. Force 2.	Total miles on each course.
N.	Wind ahead, 0 point. 720 miles.	Abeam, 8 950	Astern, 16 180	Abeam, 24 380	2230
E.	Abeam, 24 2670	Ahead, 0 250	Abeam, 8 180	Astern, 16 340	3440
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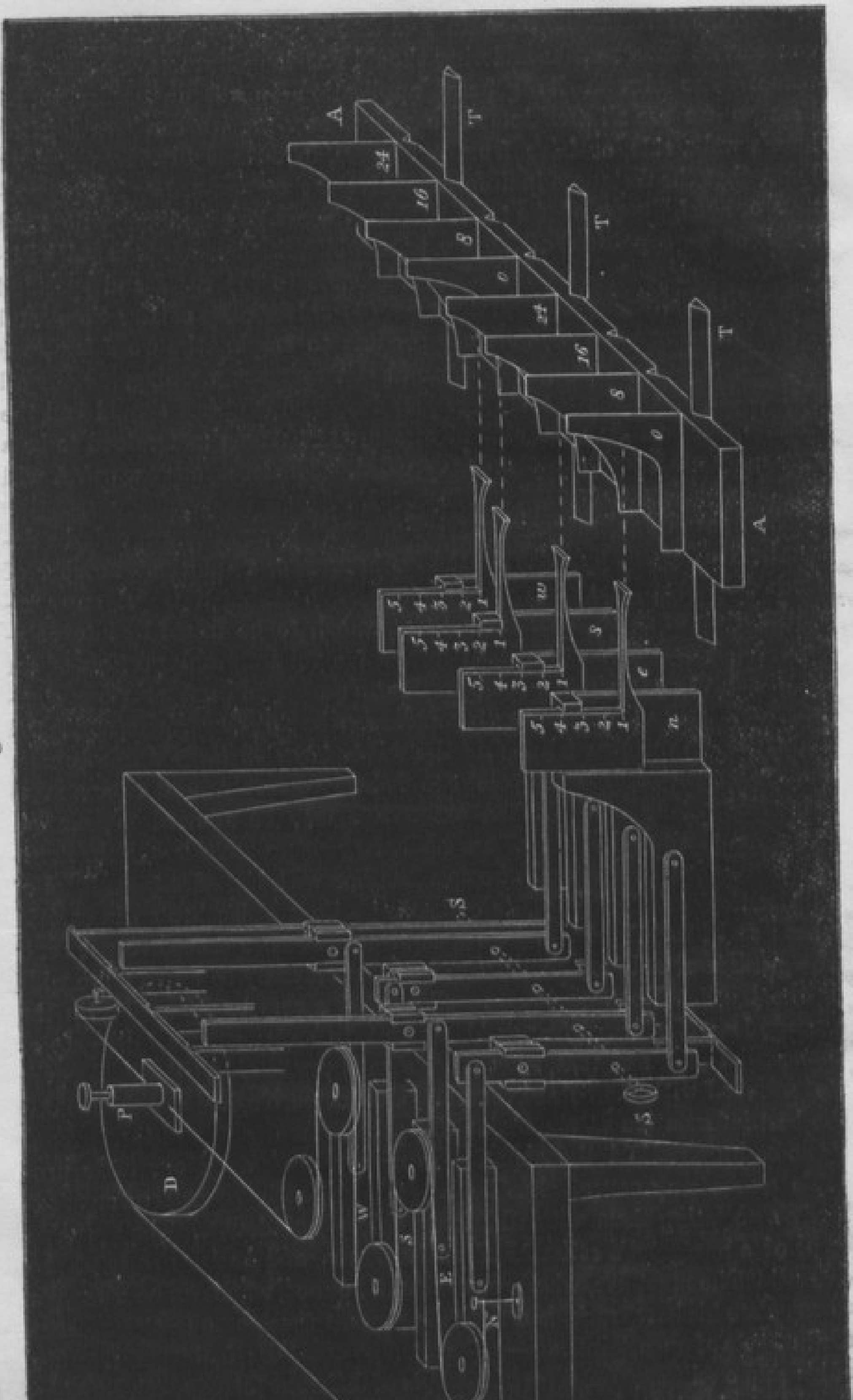

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The drawing I give (fig. 3) is only a diagram to explain the principle of the machine; the framework is left out, and the proportions are somewhat varied for the convenience of illustration. Also, for simplicity of explanation, I have supposed the machine constructed to apply to no more than 4 points of the compass, and it is represented as adjusted to work out the example already given in Table III.

A long tray, open at the top and almost wholly open along the front, has 8 grooves, into which pieces of zinc, thin wood, or even stout cardboard may be dropped, much like the glass plates in a photographer's box. In fig. 3 we only see the base of this tray, A A, and the pieces of zinc standing upon it in the position in which they would be held by the grooves. The zinc plates have curved edges in front, which refer to the sailing qualities of the particular ship under consideration; these are cut out from the data in Table II., each plate corresponding to the column whose heading it bears. The ordinate of the curve is proportionate to the force of the wind, and the abscissa to the distance sailed in one day on the specified course (0, 8, 16, or 24) with that force of wind. There are grooves cut in A A, one under each zinc plate, and there are tramways, T, upon which A A may be set, in gear with those grooves. In the figure it is so set that 0 is opposite to *n*, 8 to *e*, 16 to *s*, and 24 to *w*; but if it were lifted up and laid one groove more to the left, 0 (on the right hand) would be opposite to *w*, 8 to *n*, 16 to *e*, and 24 to *s*. Similarly, by setting it two or three grooves to the left, the other possible variations would be gone through. The slides *n*, *e*, *s*, and *w* refer to the course of the ship shown in the first column of Table III., and the four different settings correspond to the four lines in the body of that Table.

The slides *n*, *e*, *s*, and *w* move to and fro parallel to the tramways, and are each furnished with bars that can be pushed vertically up and down, and a rod projects horizontally from each of the bars towards the zinc plate opposite to it. Graduations referring to the force of the wind are placed on the bars, which are thereby adjusted to come in contact at the proper levels with the zinc plates, and to be pushed back through a distance equal to the length of the abscissæ at those levels, when A A is run forwards on its tramway.

Thus far we have obtained the result that *n*, *e*, *s*, and *w* shall be severally pushed back to the distances which would be sailed over in one day if the wind blew on 100 occasions with specified forces from *each* of those quarters, and if the ship were sailing to that quarter whose initial is opposite to the zinc plate marked 0. As A A is adjusted in the figure, that quarter would be N.; if it were moved one step to the left it would be W., if 2 steps it would be S., and if 3 steps it would be E.

We have now to diminish the movements impressed upon *n*, *e*, *s*, and *w* in the ratio of the percentage of occurrence of the several winds. This is effected by linking them to another series of slides, N, E, S, and W, as shown in the figure, and by attaching adjustable centres to the arms, there shown in a vertical position. The standards to which those centres would be clamped are almost wholly removed in the figure, but the top and bottom of them can be seen. The reduction will be correct within such limits as we need, when the links are somewhat longer than in the figure and the arm does not swing through more than 40°. I therefore do not care to propose in this case the somewhat more complicated, but perfectly accurate arrangement which I contrived for the parallel slides of the pantagraphs now in use at the Meteorological Office, of which a description is given in the Report of the Meteorological Committee for 1870, p. 30.

I place the adjustable centres between the links in the cases of N and S, and above them in those of E and W; consequently, when *n*, *e*, *s*, and *w* are all pushed back, N and S will advance, and E and W will retreat: the reason for doing this will be seen presently. In order to graduate the arms for the adjustable centres, we must take the $\frac{1}{100}$ part of the distance between the pivots of the links as the unit of measurement, and measure in both cases from the pivot of the upper link as the zero-point. Then if p=the percentage to which the movements are to be reduced, the graduations for p between the links are determined by the formula $\frac{100p}{100+p}$, and when above the links by $\frac{100p}{100-p}$.

The results we have now attained are that, when A A is pushed forward, N, E, S, W shall move alternately forwards and backwards, each through a distance corresponding to that which a ship would sail towards the quarter whose initial is opposite to the zinc plate marked 0, under the several influences of the N., E., S., and W. winds as they are found to occur. What remains is to sum up these movements.

I put a pulley, running easily on its axis, upon each of the upper slides, as in the figure, and pass a band, one of whose ends is secured to a fixed peg, round these pulleys, alternately over and under them. The free end of the band is kept stretched by a light weight, and a framework, carrying a vertical pricker, is urged to and fro by the movements of the band. A disk (D), upon which the drawing-paper is secured, has its centre exactly below the pricker when the machine is at zero; and whenever A is pushed home, the pricker travels in a radial distance to an amount equal to twice the sum of the movements of the several slides, and therefore through a distance proportionate to a day's sail of the ship towards the quarter whose initial letter is opposite to the zinc plate marked 0. If desired, the numerical value of the movement of the string could of course be read off.

The manipulation of the instrument would be as follows:—

1. Remove A A. 2. Push or pull the slides *n*, *e*, *s*, *w* into their mean position. 3. Thrust a skewer, S S, through the holes in the arms and framework to hold every thing fast. 4. Adjust the centres, and clamp them if necessary. 5. Adjust the bars for force of wind. 6. Remove the skewer. 7. Push the slides *n*, *e*, *s*, *w* as far back as they will go. 8. Replace A A, and pull the slides forward to it. The machine is now in working order. *a*. Push A A home. *b*. Press the pricker, not forgetting to mark the N point. *c*. Turn the disk through a quadrant. *d*. Pull back A A, and set it one step in advance on the tramways. *e*. Pull the slides up to it.

The series 1 to 8 has to be gone through once for all for each isochrone; that from *a* to *e* for each of the 4 points of the compass. The whole of these actions are simple and rapid, and the adjustments are of the easiest kind. An isochrone based upon 4 points ought to be leisurely plotted out in 2½ minutes, and one based upon 8 points in 4 minutes.

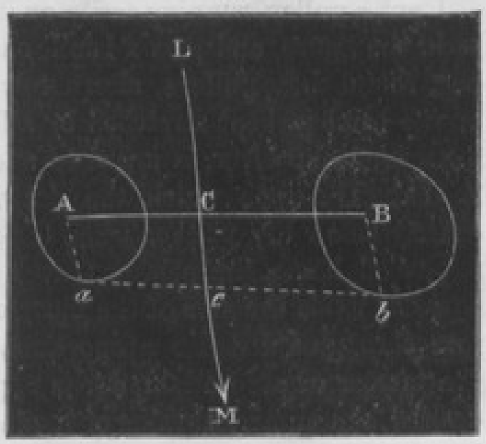
This step-by-step arrangement is far easier of construction than one, which may suggest itself to many persons, in which the movement should be continuous, using a curved surface instead of a set of curved edges, and by which the entire curve should be drawn instead of a few points pricked out; also it is far more convenient and compact not to arrange the machine in a circular form, which is that which would most naturally first be thought of.

When the isochrones have been drawn to scale on a chart, isochronic lines at various points along any proposed route could readily be found by graphical interpolation. Thus, in fig. 4, let the route be from L to M, and let the isochrones round A and B be known. Draw A *a*, B *b* parallel to L M, and join A B, *a b*, cutting L M in C and *c*; then C *c* is one day's sail from C. We can do more than this; for we may find the distance of

f. 14v

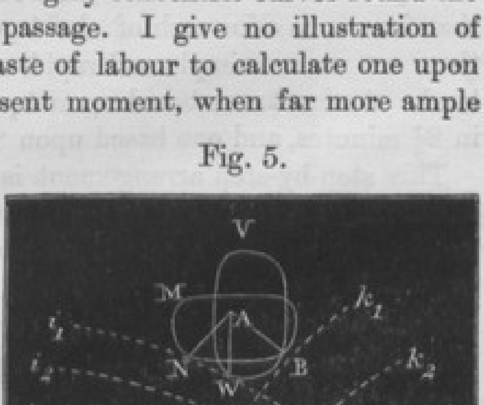
a day's sail from *any* point of L M by the following device. Erect C c' perpendicular to L M, and equal to C c; similarly, erect other perpendiculars (L l', M m') from points L, M, &c., of which the day's sail along the route has been laid down in the same way that C c was, and draw with a free hand a line through l', c', m'; then the perpendicular distance X x' from any point X on the line L M to its intersection at x' with the line l', c', m' will be the length of a day's sail from X, and it can be transferred with a pair of compasses to find x on the line L, X, M.

Fig. 4.



Another application of the principle of isochrones is to draw them at distances of 1, 2, 3, &c. days for ships bound *towards*, not from, a given port. The first day's isochrone requires no explanation. For that of the second day, a few neighbouring points must be selected on the contour already drawn, and from these such small portions as are needed of other isochrones must be constructed; the line sketched with a free hand to bound these figures will be the isochrone of two days' journey towards the port, and similarly for the third and subsequent days. The lines of shortest passage from curve to curve join the points whence the subsidiary isochrones were drawn with the points where the latter are in contact with their bounding line. The appearance of a chart so constructed would be that of a series of roughly concentric curves round the port and of radial lines of shortest passage. I give no illustration of such a chart, because it seemed a waste of labour to calculate one upon the scanty data available at the present moment, when far more ample and trustworthy materials have been collected and are in the course of gradual publication by the Meteorological Office; and calculations would have been necessary, because the machine just described exists only in the form of a rude model.

Fig. 5.

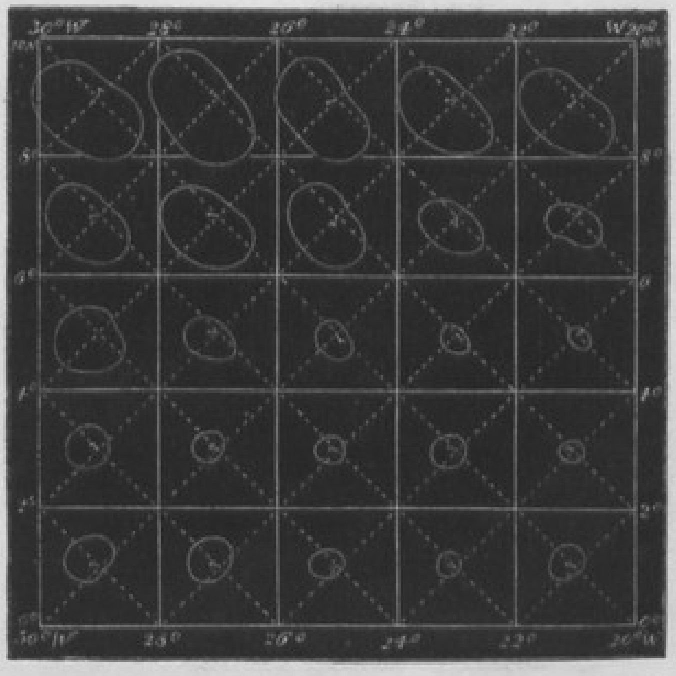


We have, lastly, to explain how the navigator would use one of these charts on specific occasions. Suppose (fig. 5) he finds himself at A with such and such weather, present and probable, how should he steer? He must use an isochrone appropriate to the occasion, most likely out of a stock which he would keep ready to hand; and applying it to his map, he would note the direction in which it will cut the largest number of isochrones towards the port. If we take cases where the isochrones for the occasion are (1) N M B and (2) W V B, the first being with a N. and the second with a W. wind, then, if the isochrones towards the port be as in the series k_1, k_2, &c., the course to be steered in both instances is A B; but if they are as in the series i_1, i_2, &c., the course in case (1) will be A N, and in case (2) A W, quite independently of the direction in which the port may happen to lie.

There is much to say about the proper method of discussing the crude statistics derived from ocean districts artificially bounded by lines of latitude and longitude in order to obtain the most probable meteorological values, but I will only allude to them here. First, homogeneous districts and periods of time have to be made out; secondly, the crude observations in each subdivision of those districts have to be discussed in connexion with those made at adjacent subdivisions in the same district; and, thirdly, they have to be discussed in reference to those made in preceding and succeeding periods of time. There is no doubt but that labour spent in these discussions would after a time become more remunerative than the same amount of labour in accumulating fresh observations.

I submit (fig. 6) a series of 8-hour isochrones computed to 8 points from the

Fig. 6.



crude observations taken in each "2-degree square" of the ocean between

10° north latitude and the equator, and between 20° and 30° west longitude, in the month of January. The uniformity of their sequence is very striking; and it would no doubt have been still more so if the data had previously been discussed in the above-mentioned manner. The short line with an arrow-head shows the direction and amount of current; but the centre of each square is the point of departure, for which the contour shows the joint effects of winds and current.

To recapitulate. I have shown in this memoir :—1, what isochrones are, and their great importance; 2, how to calculate them; 3, how to construct a machine to supersede their calculation and to make it possible to have them drawn for special cases at a trifling cost; 4, how to make an isochronal chart; and, 5, how to use it on individual occasions.

I should be glad if one result of this memoir were to bring into greater prominence than at present the high value of the ocean statistics collected and now being published by the Meteorological Office, and the fact that no degree of precision of meteorological knowledge need be thrown away in the practice of navigation. Such knowledge will be good for all time, and will always afford the requisite data whence isochrones conformable to the varying performances of new varieties of ships and to new lines of commerce may be calculated.

A paper followed "On the Conversion of Wind-charts into Passage-charts," by Mr. Francis Galton, F.R.S. The most direct line between two points of the ocean is seldom the quickest route for sailing vessels. A compromise has always to be made between directness of route on the one hand, and the best chance of propitious winds and currents on the other. Hence it is justly argued that an inquiry into the distribution of the winds over all parts of the ocean is of high natural importance to a seafaring people like ourselves. A knowledge of the distribution of the winds would clearly enable a calculation to be made which would show the most suitable passage in any given case.

But as a matter of fact, no calculations have yet been made upon this basis, much less have charts been contrived to enable a navigator to estimate by simple measurement the probable duration of a proposed passage. The wind-charts compiled by the Meteorological Department of the Board of Trade are seldom used by navigators, for they do not afford the results that seamen principally require; they only give data, from which those results might be calculated by some hitherto unexplained process, which would certainly be a tedious one. To convert wind-charts or the tables of wind-direction from which they have been compiled—into passage-charts, he must ascertain the distances that ships of different classes would attain in an hour, if they made the best of their way under the same wind towards different points of the compass. With a moderate wind, a merchantman of the class that usually navigates the Atlantic, will, by beating to windward, make two and one-third miles an hour, right in the wind's eye. At two points off the wind it will make 3 miles; at four, 4 miles; at six, 7; at eight, $8\frac{1}{2}$; at ten, 9; at twelve, $9\frac{1}{2}$; at fourteen, $8\frac{3}{4}$; and at sixteen, or with the wind right astern, it will make $7\frac{1}{2}$ miles. We next have to turn to the wind charts, or to the tables, to ascertain the proportion of the winds that blow from different points of the compass in the region we are investigating. Thus in one particular case we find out of 100 observations, that six referred to N. winds; fourteen to N.N.E.; seventeen to N.E.; six to E.N.E.; three E.; two E.S.E.; two S.E.; five S.S.E.; six S.; six S.S.W.; six S.W.; three W.S.W.; three W.; three W.N.W.; four N.W.; five N.N.W.; and nine calms. The force of the winds was not recorded in this instance, we must therefore on the present occasion assume them to be moderate. We have then to calculate the progress that ships could make towards each point of the compass under the several influences of each of these winds. In the example taken, the N. wind will be reckoned as lasting six per cent. of an hour, and therefore ships would be able to sail during its prevalence; ·014 miles to the N; ·018 to the N.N.E., and so on. The N.N.E. wind lasting 14 per cent. of an hour will enable ships to sail ·042 miles to the N., ·033 miles to N.N.E., and so on. The N.E., E.N.E., and all the other winds would have their influence similarly calculated. We thus obtain a table of 16 lines and of 16 columns in each line, whose addition gives the total progress of ships in one hour to all points of the compass under the influence of the average of the winds that blow in the ocean district under consideration. These are the data from which the Passage Chart would be constructed. In the case we have taken they would be N. 5.3 miles; N.N.E., 5.0; N.E., 5.2; E.N.E., 5.6; E., 6.0; E.S.E., 6.6; S.E., 6.8; S.S.E., 6.7; S., 6.5; S.S.W., 6.3; S.W., 6.3; W.S.W., 6.7; W., 6.9; W.N.W., 6.8; N.W., 6.5; N.N.W., 5.9. We should not be justified in usually adopting "average force" for the winds. If we did so, an alternation of squalls and calms would be improperly reckoned as moderate weather. We must group the winds; not necessarily to each degree of force; but it may be in two or perhaps three groups. The tables would, therefore, consist not of 16 lines, but of twice or thrice that number. For the rapid performance of these calculations we should tabulate the passages of ships to each of the 16 points of the compass, under the influence of the winds of say 30 different degrees of duration, and six of force, making a total of 180 lines for each class of ships. If these are printed on separate slips of paper, the labour of copying them would be wholly avoided. For each line the figures should be repeated, so as to sweep not only once but twice round the compass. The same slips could be used over again, and if of sufficient length to include the data for every class of ship, a single operation will simultaneously build up tables for all. A navigator wishing to find the probable duration of his intended voyage would refer to a chart, on which these numbers had been protracted in the form of a diagram. Taking those of the winds in the wind charts, he will set his compass to the radius of the nearest of these diagrams measuring it in a direction parallel to his passage. He will thereby obtain a scale of probable distance for one hour's sail in the simplest possible manner. The method of altering the diagram so as to include the effect of a current is too simple to require explanation.

From the JOURNAL OF THE STATISTICAL SOCIETY OF LONDON, MARCH, 1873.

The RELATIVE SUPPLIES *from* TOWN *and* COUNTRY FAMILIES, *to the* POPULATION *of* FUTURE GENERATIONS. *By* FRANCIS GALTON, F.R.S.

[Read before the Statistical Society, 21st January, 1873.]

THIS is an inquiry into the relative fertility of the labouring classes of urban and rural populations, not as regards the number of children brought into the world, but as regards that portion of them who are destined to live and become the parents of the next generation. It is well known that the population of towns decays, and has to be recruited by immigrants from the country, but I am not aware that statistical measurement has yet been attempted of the rate of its decay. This inquiry is part of a larger one, on the proportionate supply to the population from the various social classes, and which has an obvious bearing on investigations into the influences that tend to deteriorate or to improve our race. If the poorer classes, that is to say, those who contain an undue proportion of the weak, the idle, and the improvident, contribute an undue supply of population to the next generation, we are justified in expecting that our race will steadily deteriorate, so far as that influence is concerned. The particular branch of the question to which I address myself in this memoir is very important, because the more energetic of our race, and therefore those whose breed is the most valuable to our nation, are attracted from the country to our towns. If, then, residence in towns seriously interferes with the maintenance of their race, we should expect the breed of Englishmen, so far as that influence is concerned, to steadily deteriorate.

I am well aware that the only perfectly trustworthy way of conducting the inquiry, is by direct investigation. I mean that a large number of women living under urban or rural conditions of life, and the same number in either case, should be noted as they arrived at a marriageable age, say æt. 20, and that the number of children they bear, who survive to a marriageable age, should also be noted. We might do this prospectively, but it is impossible, from want of historical data, to work backwards. I therefore have had recourse to an indirect method, based on a selection from the returns made at the last census, which I submit to the criticism of others as appearing to myself calculated to give a fair approximation to truth. The principle on which I have proceeded, is this:—

I find (A) the number of children of an equal number of urban and rural mothers, within certain limitations of age, and I correct the results on the following grounds, which I will shortly explain more fully, namely, (B) the relative mortality of the two classes between childhood and maturity; (C) the relative mortality of the mothers during childbearing ages; (D) relative celibacy; and (E) the span of a generation. It will be seen that B and C are substantial corrections, but that I have not occasion to pay regard to D and E.

Returns were made in the census schedules of the names and ages of the members of each "family," by which word we are to understand those members of the family in its ordinary sense, who are alive and resident in the same house with their parents. Where the mothers are still young, the children are necessarily very young and nearly always (in at least those classes who are unable to send their children to boarding schools), live at home. If, therefore, we limit our inquiries to the census families of young mothers, the results will be identical within the same limits of age with what we should have obtained if we had direct means of ascertaining the number of their living children. The limits of age of the mothers which I adopted in my selection were, 24 and 40 years. Had I to begin the work afresh, I should prefer the period from 20 to 35, but I have reason to feel pretty well contented with my present data.

In deciding on the districts to be investigated, it was important to choose well marked specimens of urban and rural populations. In the former, a town was wanted where there were various industries, and where the population was not increasing. A town where only one industry was pursued, would not be a fair sample, because the particular industry might be suspected of having a special influence, and a town that was increasing would have attracted numerous immigrants from the country, who are undistinguishable as such in the census returns. Guided by these considerations, I selected Coventry, where silk weaving, watchmaking, and other industries are carried on, and whose population has scarcely varied during the last decade. It is an open town, in which the crowded alleys of larger places are not frequent. Its urban peculiarities are therefore minimised, and its statistical returns would give a picture somewhat too favourable of the average condition of life in towns. For specimens of rural districts, I chose small agricultural parishes in Warwickshire.

By the courteous permission of Dr. Farr, our president, I was enabled to procure extracts from the census returns concerning 1,000 "families" of factory hands at Coventry, in which the age of the mother was neither less than 24 nor more than 40 years, and concerning another 1,000 families of agricultural labourers in rural parishes of Warwickshire, under the same limitations as to the age of the mother. When these returns were classified (see Table I, p. 24), I found the figures to run in such regular sequence as to make it certain that the cases were sufficiently numerous to give trustworthy results. It appeared that:—

(A). The 1,000 families of factory hands comprised 2,681 children, and the 1,000 of agricultural labourers comprised 2,911; hence, the children in the urban "families," the mothers being between the ages of 24 and 40, are on the whole about 8 per cent. less numerous than the rural. I see no reason why these numbers should not be accepted as relatively correct for families, in the ordinary sense of that word, and for mothers of all ages. An inspection of the table does indeed show that if the selection had begun at an earlier age than 24, there would have been an increased proportion of sterile and of small families among the factory hands, but not sufficient to introduce any substantial modification of the above results. It is, however, important to recollect that the small error, whatever its amount may be, is a concession in favour of the towns.

(B). I next make an allowance for the mortality between childhood and maturity, which will diminish the above figures in different proportions, because the conditions of town life are more fatal to children than those of the country. No life tables exist for Coventry and Warwickshire; I am therefore obliged to seek elsewhere to learn the amount of the allowance that should be made. The life tables of Manchester* will afford the necessary data for towns, and those of the healthy districts† will suffice for the country. By applying these, we could learn the number of the children of ages specified in the census returns who would attain maturity. I regret extremely that when I had the copies taken, I did not give instructions to have the ages of all the children inserted; but I did not, and it is too late now to remedy the omission. I therefore proceeded as follows to make a very rough, but not unfair, estimate. The average age of the children is about 3 years; now, taking 25 years as representing the age of maturity, it will be found that 74 per cent. of children in Manchester, of the age of 3, reach that of 25, while 86 per cent. is the proportion in the healthy districts. Therefore, if my rough method of correction be accepted as approximately fair, the number of adults who will be derived from the children of the 1,000 factory families should be reckoned at $2681 \times \frac{74}{100}$, and those from the 1,000 agricultural at $2911 \times \frac{86}{100}$.

* "Seventh Annual Report of Registrar-General."

† Healthy Districts Life Table, by Dr. Farr. "Phil. Trans. Royal Society," 1859.

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(C). We ought to compare the families of the same number of urban and rural women who had reached the age of 24. Many of them will not marry at all; I postpone the consideration of these to the next paragraph. Many of the rest will die before they reach the age of 40, and more of them will die in the town than in the country. It appears from data furnished by the above-mentioned tables, that if 100 women of the age of 24 had annually been added to a population, the number of those so added, living between the ages of 24 and 40 (an interval of seventeen years) would be 1,539 under the conditions of life in Manchester, and 1,585 under those of the healthy districts. Therefore the factors to be applied respectively to the two cases, on account of this correction, are $\frac{1539}{17 \times 100}$ and $\frac{1585}{17 \times 100}$.

(D). I have no trustworthy data for the relative prevalence of celibacy in town and country. All that I have learnt from the census returns is, that when searching them for the 1,000 families, there were noted 131 bachelors between the ages of 24 and 40, among the factory hands, and 144 among the agricultural labourers. If these figures be accepted as correct guides to the amount of celibacy among the women, it would follow that I must be considered to have discussed the cases of 1,131 factory, and 1,144 agricultural women, when dealing with those of 1,000 mothers in either class. Consequently that the respective corrections to be applied, are given by the factors $\frac{1000}{1131}$ and $\frac{1000}{1141}$. These would have so small an effect on the relative number of the two classes, as not to be worth applying, for it would be less than 1 per cent., and I do not like to apply it, because it seems to me erroneous and to act in the wrong direction, inasmuch as unmarried women can obtain employment more readily in the town than in the country, and celibacy is therefore more likely to be common in the former than in the latter.

(E). The average length of a generation in town and country, must not be omitted from our consideration. We, however, know that the correction on this ground will be insignificant, because the length of a generation is found to be constant under very different circumstances of race, and therefore we should expect it to be equally constant in the same race under different conditions. I find that one-half of the mothers in my schedule are under 31·25 years of age in the town and 32·5 in the country, but this difference of $1\frac{1}{4}$ years is fully compensated by the effects of the greater mortality of the children of the former. The omission to which I referred in (B), prevents an exact calculation being made. If the ages of the children had been copied, it would have been easy to have made the necessary reductions, and to have obtained a table whence the average age of mothers of children destined to reach adult life could have been calculated for town and country.

Let us now sum up the results. The corrections are not to be applied for (D) and (E), so we have only to regard (A) × (B) × (C), that is—

$$\frac{2681 \times \frac{74}{100} \times \frac{1539}{1700}}{2911 \times \frac{86}{100} \times \frac{1585}{1700}} = \frac{1796}{2334} = \frac{77}{100}.$$

In other words, the rate of supply in towns to the next adult generation is only 77 per cent., or, say, three-quarters of that in the country. In two generations the proportion falls to 59 per cent., that is, the adult grandchildren of artisan townsfolk are little more than half as numerous as those of labouring people who live in healthy country districts.

TABLE I.—*Census Returns of 1,000 Families of Factory Hands in Coventry, and the Age of the Mother and the*

Age of Mother.	Number of Children in Family.									
	0.		1.		2.		3.		4.	
	Factory.	Agricultural.	Factory.	Agricultural.	Factory.	Agricultural.	Factory.	Agricultural.	Factory.	Agricultural.
24 to 25	28	17	40	31	24	32	12	10	2	—
26 " 27	19	18	36	24	36	28	23	26	8	8
28 " 29	18	17	32	16	20*	33	36	23	14	23
30 " 31	13	4	23	18	24	21	28*	31	18	22
32 " 33	18	11	16	14	19	13	22*	27	23	26
34 " 35	14	15	11	6	17	16	28	18	31	34
36 " 37	12	17	4	11	10	13	22	14	16	20
38 " 39	8	6	9	15	14	17	16	21	22	23
40	8	7	3	10	8	9	13	14	8	10
Total within outline	96	67	158	109	116	111	171	149	—	—
Total between outlines	42	45	16	36	56	71	29	35	142	166
Total beyond outline	—	—	—	—	—	—	—	—	—	—
Total	138	112	174	145	172	182	200	184	142	166

* These three cases are anomalous, the factory being less than the agricultural. In the neither of these can be correct; certainly not the first of them.

Note.—It will be observed that within the outline, that is, in the upper and left hand predominate, while the agricultural are the most numerous between the outlines, that is are from four to five in number. The two are equally numerous without the outlines, that

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1,000 *Families of Agricultural Labourers in Warwickshire, Grouped according to Number of Children in the Family.*

Number of Children in Family.										Age of Mother.
5.		6.		7.		8.		9.		
Factory.	Agricultural.	Factory.	Agricultural.	Factory.	Agricultural.	Factory.	Agricultural.	Factory.	Agricultural.	
1	1	—	—	—	—	—	—	—	—	24 to 25
—	—	—	—	—	—	—	—	—	—	26 „ 27
6	6	4	1	2	—	—	—	—	—	28 „ 29
12	15	2	5	—	2	—	1	—	—	30 „ 31
21	25	9	5	—	1	—	2	—	—	32 „ 33
14	18	12	9	5	3	—	1	—	—	34 „ 35
15	25	12	10	4	5	5	2	—	—	36 „ 37
14	22	10	15	6	7	—	2	1	—	38 „ 39
7	11	3	9	7	7	2	1	—	—	40
—	—	—	—	—	—	—	—	—	—	Total within outline
90	123	—	—	—	—	—	—	—	—	Total between outlines
—	—	52	54	24	25	7	9	1	—	Total beyond outline
90	123	52	54	24	25	7	9	1	—	Total

instance of 20—33, the anomaly is double, because the sequence of the figures shows that

of the table, where the mothers are young and the children few, the factory families
especially in the middle of the table, where the mothers are less young, and the families
is, to the right of the the table, where the families are large.

Table II.

	Number of Families.		Number of Children.	
	Factory.	Agricultural.	Factory.	Agricultural.
Within outline	541	436	903	778
Between outlines	375	476	1,233	1,562
Beyond „	84	88	545	571
Total	1,000	1,000	2,681	2,911

HARRISON AND SONS, PRINTERS IN ORDINARY TO HER MAJESTY, ST. MARTIN'S LANE.

VISUALISED NUMERALS.

BY

FRANCIS GALTON, F.R.S.

A MEMOIR READ BEFORE THE ANTHROPOLOGICAL INSTITUTE ON MARCH 9, 1880, WITH THE REMARKS OF VARIOUS SPEAKERS THEREON.

(*Reprinted from the Journal of the Anthropological Institute.*)

LONDON:
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1880.

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[*Reprinted from the Journal of the Anthropological Institute, August*, 1880.]

Visualised Numerals. By Francis Galton, F.R.S.

I PROPOSE to describe a peculiar habit of mind which characterises, so far as I can judge, about one man in 30, and one woman in 15; but before doing so, I must say a word of warning against a too-frequent tendency to assume that the minds of every other sane and healthy person must be like one's own. The psychologist should inquire into the minds of others as he should into those of animals of different races, and be prepared to find instances of much to which his own experience can afford little, if any clue.

This is especially the case with psychologists who are not *imaginative* in the strict but unusual sense of that ambiguous word. I do not by imagination mean an uncontrolled fancy and inaccurate recollection. I apply the word imaginative to those who while they may be exceedingly matter-of-fact and precise, are apt to think in visual images; not in fancied words, nor in a more abstract manner. The mental state of imaginative persons is amidst a series of pictures, vivid in colour, and well defined in form, and it happens in many cases that what they mentally see appears external to themselves. There is no doubt that abstract thought is best carried on without the aid of this concrete imagery, and that a natural tendency to indulge in it is liable to be repressed by vigorous brain-workers. It is consequently uncommon among those scientific men whose attention I chiefly desire to gain. Every one, however, recognises the fact that some men of the highest order of genius and artistic temperament have had the gift of vivid mental presentation in a remarkable degree; they also know that chess-players exist, who have no mean capacity in other respects, who can play 10 or more games blindfold, having all the time a perfectly vivid picture of each board in succession before them, and seeing the chessmen on each, as made of wood or ivory, as the case may be. I therefore ask you all to take for granted the existence of imaginative persons, in the sense of the word in which I have used it, although many of yourselves may never have had the tendency to think in visual forms, or if you once had it, may have long since abandoned it.

Let me also remark, that if the existence of colour-blindness which affects about one man in 30 was unsuspected, or at all events wholly undescribed and unnamed, until the time of Dalton, it need not astonish us that the psychological peculiarity which I am about to describe, and which is about equally rare (at least in adults), should hitherto have escaped notice.

Persons who are imaginative almost invariably think of *numerals* in visual imagery. If the idea of *six* occurs to them, the word "six," does not sound in their mental ear, but the figure 6 in a written or printed form rises before their mental eye. The clearness of the images of numerals, and the number of them that can be mentally viewed at the same time, differs greatly in different persons. The most common case is to see only two or three figures at once, and in a position too vague to admit of definition. There are a few persons in whom the visualising faculty is so low that they can mentally see neither numerals nor anything else; and again there are a few in whom it is so high as almost to give rise to hallucinations. The images of these persons, whether of numerals or not, are so vivid as to be undistinguishable from reality, except by the aid of accidental circumstances; thus the images may be transparent, or apt to vary in brightness from moment to moment, and to change more or less in outline. They may appear in the air without support, or any other of the innumerable conditions of objective reality may be absent, the want of which will render the visionary character of the image immediately manifest to a sane mind. Those who are able to visualise a numeral with a distinctness comparable to reality, and to behold it as if it were before their eyes, and not in some sort of dreamland, will define the direction in which it seems to lie, and the distance at which it appears to be. If they were looking at a ship on the horizon at the moment that the figure 6 happened to present itself to their minds, they could say whether the image lay to the left or right of the ship, and whether it was above or below the line of the horizon; they could always point to a definite spot in space, and say with more or less precision that that was the direction in which the image of the figure they were thinking of, first appeared.

Now the strange psychological fact to which I desire to draw attention, is that among persons who visualise figures clearly, there are many who notice that the image of the same figure invariably makes its first appearance in the same direction, and at the same distance. Such a person would always see the figure when it first appeared to him at (we may suppose) one point of the compass to the left of the ship at which he was looking, and upon the line of the horizon, and at 20 feet distance. Similarly, we may suppose that he would see the figure 7 invariably half a point to the left of the ship and at an altitude equal to the sun's diameter above the horizon, and at 30 feet distance; similarly for all the other figures. Consequently, when he thinks of the series of numerals 1, 2, 3, 4, &c., they show themselves in a definite pattern that always occupies an identical position in respect to the direction in which he is looking.

Those who do not see figures with the same objectivity, use nevertheless the same expressions with reference to their *mental* field of view. They can draw what they see in a manner fairly satisfactory to themselves, but they cannot locate it in reference to their axis of sight and to the horizontal plane that passes through it. It is with them as it is with all of us in dreams, the imagery is before and around, but our eyes during sleep are turned inwards and upwards.

The pattern or "Form" in which the numerals are seen is by no means the same in different persons, but assumes the most grotesque variety of shapes. I have placed on the table or suspended against the walls copies of nearly sixty of them, which will be seen to run in all sorts of angles, bends, curves and zigzags. They have however for the most part certain characteristics in common. They are stated in all cases to have been in existence, at least so far as the earlier numbers in the Form are concerned, as long back as the memory extends; they come into view quite independently of the will, and their shape and position, at all events in the *mental* field of view, is nearly invariable. They have other points in common to which I shall shortly draw attention, but first I will endeavour to remove all shadow of doubt as to the authenticity of these statements.

I see no "Form" myself, and first ascertained that such a thing existed through a letter from Mr. Bidder, in which he described his own case as a very curious peculiarity. I was at the time making inquiries about the strength of the visualising faculty in different persons, and among the numerous replies that reached me I soon collected ten or twelve other cases in which the writers spoke of their seeing numerals in definite forms and in much the same terms that Mr. Bidder had used. Though the information came from independent sources, the expressions used were so closely alike that they strongly corroborated one another. Of course I eagerly followed up the inquiry and when I had collected enough material to justify publication, I wrote an account which appeared in "Nature" on January 15th, with several illustrations. This has led to a wide correspondence and to a much increased store of information, which enables me to arrive at the conclusions I shall lay before you. The answers I received whenever I have pushed my questions have been straightforward and precise. I have not unfrequently procured a second sketch of the Form and found it to agree closely with

f. 3v

the first one. I have also questioned many of my own friends in general terms as to whether they visualise numbers in any particular way. The large majority are unable to do so. But every now and then I meet with persons who possess the faculty, and I have become familiar with the quick look of intelligence with which they receive my question. It is as though some chord had been struck which had not been struck before, and the verbal answers they give me are precisely of the same type as those written ones of which I have now so many. I cannot doubt of the authenticity of independent statements which closely confirm one another, nor of the general accuracy of the accompanying sketches, because I find now that my collection is large enough for classification, that they tend to form a continuous series. I am often told that the peculiarity is common to the speaker and to some near relative, and that they had found such to be the case by accident. I have the strongest evidence of its hereditary character after allowing, and over allowing, for all conceivable influences of education and family tradition.

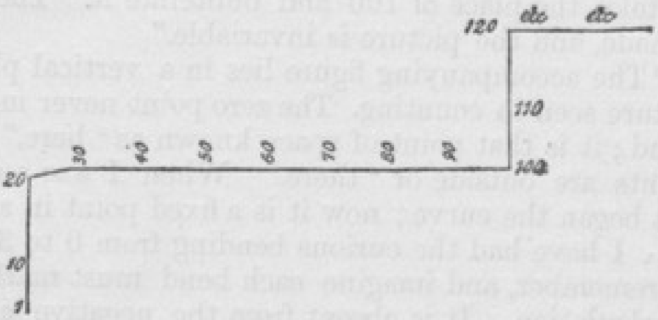
Last of all, I have taken advantage of the opportunity afforded by a meeting of this Society, to bring with me many gentlemen well known in the scientific world, who have this habit of seeing numerals in Forms, and whose diagrams are in the collection before you. Amongst them are Mr. G. Bidder, Q.C., the Rev. Mr. G. Henslow, the botanist, Mr. Schuster, F.R.S., the physicist, Mr. Roget, Mr. Woodd Smith, and Colonel Yule, C.B., the geographer. I wish that some of my foreign correspondents could also have been present, such as M. Antoine d'Abbadie the well-known French traveller and Membre de l'Institut, and Baron v. Osten Sacken, the Russian diplomatist and entomologist, for they have given and procured me much information.

I feel sure that I have now said enough to authenticate my data; it remains to treat them in the same way as any other scientific facts and to extract as much meaning from them as possible.

To repeat in part what has already been said, this peculiarity is found so far as my observations have extended, in about 1 out of every 30 adult males or 15 females. It consists in the sudden and automatic appearance of a vivid and invariable "Form" in the mental field of view, whenever a numeral is thought of, and in which each numeral has its own definite place. This Form may consist of a mere line of any shape, of a peculiarly arranged row or rows of figures, or of a shaded space.

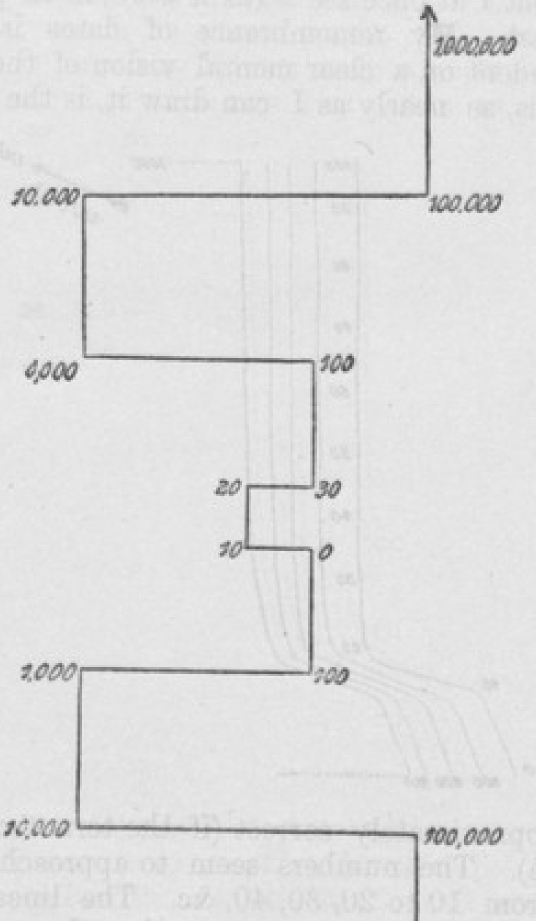
I give wood-cuts of some of these forms, and very brief descriptions of them extracted from the letters of my correspondents. (The wood-cuts have already appeared in "Nature."* Many other drawings on a smaller scale on two lithographed

J.S.



plates will be found at the end of these pages, and brief descriptions of some of them are given partly in an appendix, and partly by the sides of the figures themselves.)

I.J.C.



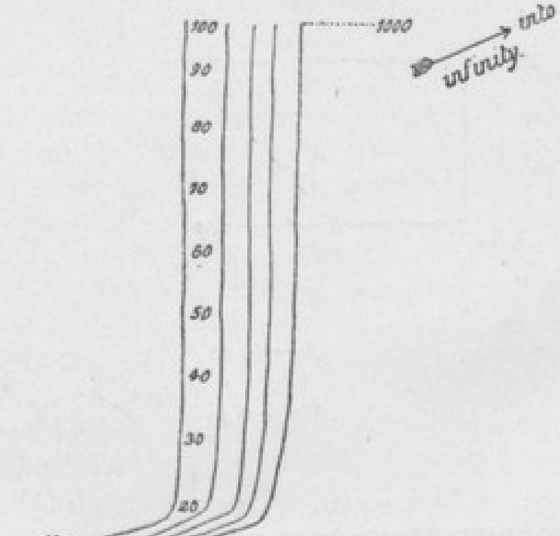
* I am indebted to the courtesy of the publishers of "Nature" for the use of these woodcuts.

I.S. "The figures are about a quarter of an inch in length, and in ordinary type. They are black on a white ground. 200 generally take the place of 100 and obliterate it. There is no light or shade, and the picture is invariable."

I.J.C. "The accompanying figure lies in a vertical plane, and is the picture seen in counting. The zero point never moves, it is *in* my mind; it is that point of space known as "here," while all other points are outside or "there." When I was a child the zero point began the curve; now it is a fixed point in an infinite circle . . . I have had the curious bending from 0 to 30 as long as I can remember, and imagine each bend must mark a stage in early calculation. It is absent from the negative side of the scale, which has been added since childhood."

T.M. "The representation I carry in my mind of the numerical series is quite distinct to me, so much so that I cannot think of any number but I at once see it (as it were) in its peculiar place in the diagram. My remembrance of dates is also nearly entirely dependent on a clear mental vision of their *loci* in the diagram. This, as nearly as I can draw it, is the following:—

T.M.



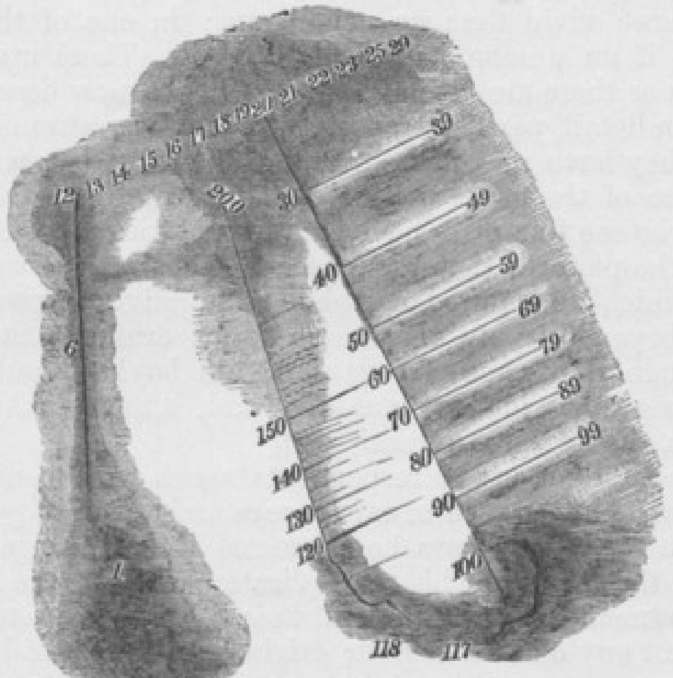
It is only approximately correct (if the term "correct" be at all applicable). The numbers seem to approach more closely as I ascend from 10 to 20, 30, 40, &c. The lines embracing a hundred numbers also seem to approach as I go on to 400, 500, to 1,000. Beyond 1,000 I have only the sense of an infinite line in the direction of the arrow, losing itself in darkness towards the millions. Any special number of thousands returns

f. 5r

in my mind to its position in the parallel lines from 1 to 1,000. The diagram was present in my mind from early childhood; I remember that I learnt the multiplication table by reference to it, at the age of seven or eight. I need hardly say that the impression is not that of perfectly straight lines, I have therefore used no ruler in drawing it."

D.A. "From the very first I have seen numerals up to nearly 200, range themselves always in a particular manner, and in thinking of a number it always takes its place in the figure. The more attention I give to the properties of numbers and their interpretations, the less I am troubled with this clumsy framework for them, but it is indelible in my mind's eye even when for a long time less consciously so. The higher numbers are to me quite abstract and unconnected with a shape. This rough and untidy production is the best I can do towards representing what I see. There was a little difficulty in the performance, because it is only

D.A.



by catching oneself at unawares, so to speak, that one is quite sure that what one sees is not affected by temporary imagination. But it does not seem much like, chiefly because the mental picture never seems *on* the flat but *in* a thick, dark grey atmosphere deepening in certain parts, especially where 1 emerges, and about 20. How I get from 100 to 120 I hardly know, though if I could require these figures a few times without thinking of them on purpose, I should soon notice. About 200 I lose all framework. I do not see the actual figures very distinctly, but what there is of them is distinguished from the dark by a thin whitish tracing. It is the place they take and the shape they make collectively which is invariable. Nothing more definitely takes its place than a person's age. The person is usually there so long as his age is in mind."

[The engraver took much pains to interpret the meaning of the rather faint but carefully made drawing, by strengthening some of the shades. The result was very very satisfactory, judging from the author's own view of it, which is as follows:—"Certainly if the engraver has been as successful with all the other representations as with that of my shape and its accompaniments, your article must be entirely correct."]

In some cases, the mental eye has to travel along the faintly-marked and blank paths of a form, to the place where the numeral that is wanted is known to reside, and then the figure starts into sight. In other cases, all the numerals as far as 100 or more, are faintly seen at once, but the figure that is wanted grows more vivid than its neighbours; in one of the cases it rises as if an unseen hand had lifted it. There are as many varieties as there are persons, but I will not now describe their shapes in detail, partly because I want to draw attention to the points they have in common, and principally because I hope that some of the forms will be explained by the persons themselves who see them. I have, however, written at the side of each of the pictures that are suspended against the walls, those details which are required to explain their individual peculiarities.

It is beyond dispute that these forms originate at an early age, though they are so far developed in boyhood and youth as to include the higher numbers, and, among mathematical students, the negative values.

Nearly all of my correspondents speak with confidence of their forms having been in existence as far back as they recollect. One states that he knows he possessed it at the age of four; another, that he learnt his multiplication table by the aid of the elaborate mental diagram he still uses. Not one in ten is able to suggest any clue as to their origin. They cannot be due to anything written or printed, because they do not simulate what is found in ordinary writings or books.

The figures run frequently to the left, and more often upwards than downwards. They do not even lie in the same plane. Sometimes a form has twists as well as bends, sometimes it is turned upside down, sometimes it plunges into an abyss of immeasurable depth, or it rises and disappears in the sky. In one case it proceeds, at first straightforward, then it makes a backward sweep high above head, and finally recurves into the pocket, of all places! It is often sloped upwards at a slight inclination from a little below the level of the eye, just as objects on a table would appear to a child whose chin was barely above it.

All this contrasts strongly with the character of the Forms under which historical dates are visualised by the same persons. These are sometimes copied from the numerical ones, but they are more commonly based both clearly and consciously on the diagrams used in the school-room.

The same may be said of the imaged letters of the alphabet; therefore the numerical Form is the oldest of all. I suppose that it first came into existence when the child was learning to count, and was used by him as a natural mnemonic diagram, to which he referred the spoken words "one," "two," "three," &c. Also, that as soon as he began to read figures, their visual symbols supplanted the verbal sounds, and permanently established themselves on the Form.

Hence the Form is of an older date than that at which the child began to learn to read, it represents his mental processes at a time of which no other record remains. It persists in vigorous activity, and offers itself freely to our examination.

The teachers of some schools have kindly questioned their pupils for me, and I find that the proportion of young people who see numerals in Forms is much greater than that of adults. But for the most part their forms are neither well defined nor complicated. I conclude that when they are too faint to be of service they are gradually neglected, and become wholly forgotten, while if they are vivid and useful they increase in vividness and definition by the effect of habitual use. Hence, in adults, the two classes of seers and non-seers are rather sharply defined, the connecting link of intermediate cases which is observable in childhood having disappeared.

These Forms are the most remarkable existing instances of what is called "topical" memory, the essence of which appears to lie in the establishment of a more exact system of division of labour in the different parts of the brain than is usually carried on. Topical aids to memory are of the greatest service to many persons, and teachers of mnemonics make large use of them, as by advising a speaker to mentally associate the corners &c. of a room with the chief divisions of the speech he is about to deliver. Those who feel the advantage of these aids most strongly are the most likely to cultivate the use of numerical forms.

The question remains, why do the lines of the Forms run in such strange and peculiar ways? the reply is, that different persons have natural fancies for different lines and curves. Their handwriting shows this, for handwriting is by no means solely dependent on the balance of the muscles of the hand, causing such and such strokes to be made with greater facility than others. Handwriting is greatly modified by the fashion of the time. It is in reality a compromise between what the writer most likes to produce, and what he can produce with the greatest ease to himself. I am sure too, that I can trace a connection between the general look of the handwritings of my various correspondents and the lines of their Forms. If a spider were to visualise numerals, we might expect he would do so in some web-shaped fashion, and a bee in hexagons. The definite domestic architecture of all animals as seen in their nests and holes, shows the universal tendency of each species to work according to definite lines. The same is seen in the groups and formations of flocks of gregarious animals, and in the wedge-shaped or other flights of gregarious birds.

The rambling character of the lines that characterise the majority of the Forms are natural to the taste of a child. They may be recognised in their drawings, in the castles they construct on the sand, and in the outlines of the borders of their flower-gardens. The appreciation of firm curves can hardly co-exist with the imperfectly developed physique of the child; it is related to the accurate hand, the steady tread, and the generally well-adjusted muscles of manhood. A natural instinct in favour of those rigidly straight lines in which printed matter is disposed in schedules, or of the circular outlines of many diagrams, can hardly as yet have become frequent in our race. No savage possesses it. Our habitual use of the straight line and circle has grown up as it were yesterday, under the requirements of manufactures based on careful measurements with a rule, and carried out by the plane and the turning lathe, which instruments make it now much more easy to work in accordance with these lines than any other. The rambling numerical Forms being based on the instinctive preferences of childhood, show the solidity of their foundation by persisting in defiance of subsequently acquired tastes.

Children learn their figures to some extent by those on the clock. I cannot, however, trace the influence of the clock on the numerical Forms in more than three cases out of all my collection, which amounts to nearly 80 pictures of one kind or another. In one of them, the clock-face actually appears, in another it has evidently had a strong influence, and in the third, its influence is indicated, but nothing more. I suppose the Roman numerals in the clock do not fit in sufficiently well with ideas based upon the Arabic ones.

The paramount influence proceeds from the names of the numerals. Our nomenclature is perfectly barbarous, and that of other civilised nations is not better than ours and frequently worse, as the French "quatre-vingt dix-huit." We speak of ten, eleven, twelve, thirteen, etc., in defiance of the beautiful system of decimal notation in which we write those numbers. What we see is one-nought, one-one, one-two, etc., and we should pronounce on that principle, with this proviso, that the word for the one having to show both the place and the value, should have a sound suggestive of "one" but not identical with it. Let us suppose it to be the letter *o* pronounced short as in "on," then instead of ten, eleven, twelve, thirteen, etc., we might say *on-one, on-two, on-three*, etc.

The conflict between the two systems creates a perplexity, to which conclusive testimony is borne by these numerical forms. In almost all of them there is a marked hitch at the 12, and this repeats itself at the 120. The run of the lines between 1 and 20 is rarely analogous to that between 20 and 100, where it usually first becomes regular. The teens frequently occupy a larger space than their due. It is not easy to define in words the variety of traces of the difficulty and annoyance caused by our unscientific nomenclature, that are portrayed vividly, and so to speak painfully in these pictures. They testify by the evidence of indelible scars to the effort and ingenuity with which a sort of compromise is struggled for and has finally been effected between the verbal and decimal systems. I am sure that this difficulty is more serious and abiding than has been suspected, not only from the persistency of these twists which would have long since been smoothed away if they did not continue to subserve some useful purpose, but from the results of experiments on my own mind. I find I can deal mentally with simple sums with much less strain if I audibly conceive the figures as one-nought, one-one, etc., and I can both dictate and write from dictation with much less trouble when that system or some similar one is adopted. I have little doubt that our nomenclature is a serious though unsuspected hindrance to the ready adoption by the public of a decimal system of weights and measures.

These Forms are no doubt of some convenience for mnemonic purposes and it is worth considering what shape is most likely to suit the majority of those who wish for the first time to make one for their use. It ought of course to be based on the decimal system and judging from the majority of the Forms it need not go higher than 100. I am sure that symmetrical divisions at each ten would be too elaborate and uniform for general convenience, and that a system of scores and half scores would be the best. In short a pentagon, with a mark in the middle of each side, seems most likely to fulfil the conditions; it certainly suits me well. In that figure the angle at the bottom would stand indifferently for 0 or 100, and the other angles for 20, 40, 60 and 80; the place of 50 being in the middle of the horizontal top line. I find that my own mind has a decided left-handed twist, so that I cannot without an effort reckon the divisions in this imaginary pentagon in the direction in which the hands of a clock would move, but I must proceed reverse ways.

This concludes what I desired to say and I trust that the gentlemen whose names I have mentioned will kindly explain their own Forms and favour us with any remarks that may help to throw light on this curious subject. The lithographed page with 8 drawings, contains copies of their Forms (made by a camera lucida) from those they were so good as to send me and the following are brief explanatory extracts from their letters. The other lithograph contains 24 forms of other persons; they will sufficiently explain themselves.

APPENDIX.

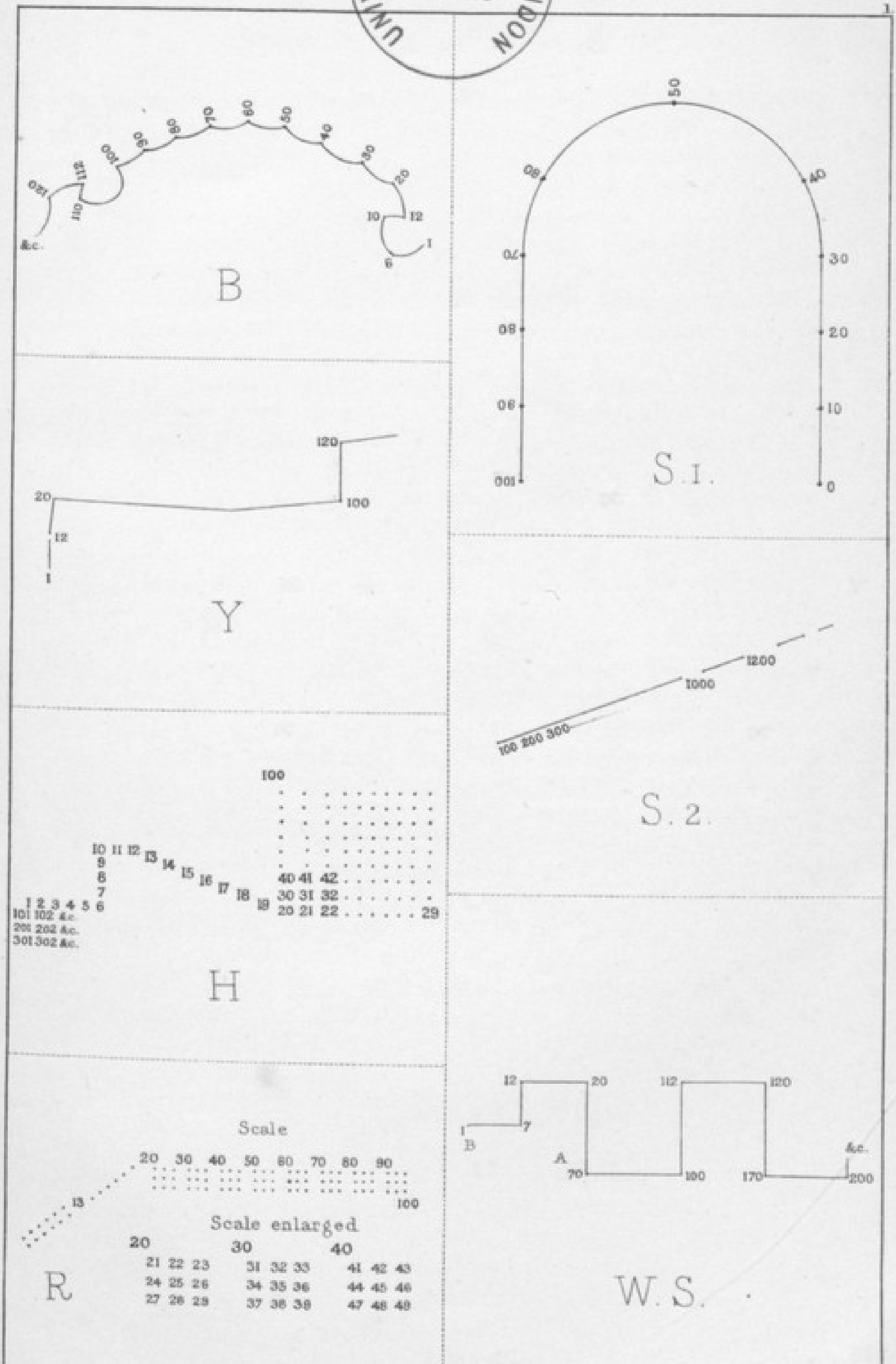
Brief Extracts from a few letters, with illustrations see Plate I *(the letter accompanying each illustration is the initial of the Correspondent.)*

GEORGE BIDDER, Q.C.—One of the most curious peculiarities in my own case, is the arrangement of the arithmetical numerals. I have sketched this to the best of my ability, every number (at least within the first thousand, and afterwards thousands take the place of units) is always thought of by me in its own definite place in the series, where it has if I may say so, a home and an individuality. I should, however, qualify this by saying that when I am multiplying together two large numbers, my mind is engrossed in the operation, and the idea of locality in the series for the moment sinks out of prominence. You will observe that the first part of the diagram roughly follows the arrangement of figures on a clock-face, and I am inclined to think that may have been in part the unconscious source of it, but I have always been utterly at a loss to account for the abrupt change at 10 and again at 12.

Colonel YULE, C.B.—I am not sure that the angle at 20 is a right angle, nor the line from 20 to 100 straight. Neither do I (or *did* I perhaps more correctly) see them in type, or black on white ground. I used to see them in gradations of colour, but I cannot fix these now with truth. I can only remember that 30 and up to 40 were of a subdued sunny colour; a division of the shade took place at 12.

The Rev. G. HENSLOW.—I have always associated my numbers from childhood upwards as in the accompanying arrangement, but am quite at a loss to know how it arose. My alphabet corresponds with it.

1.

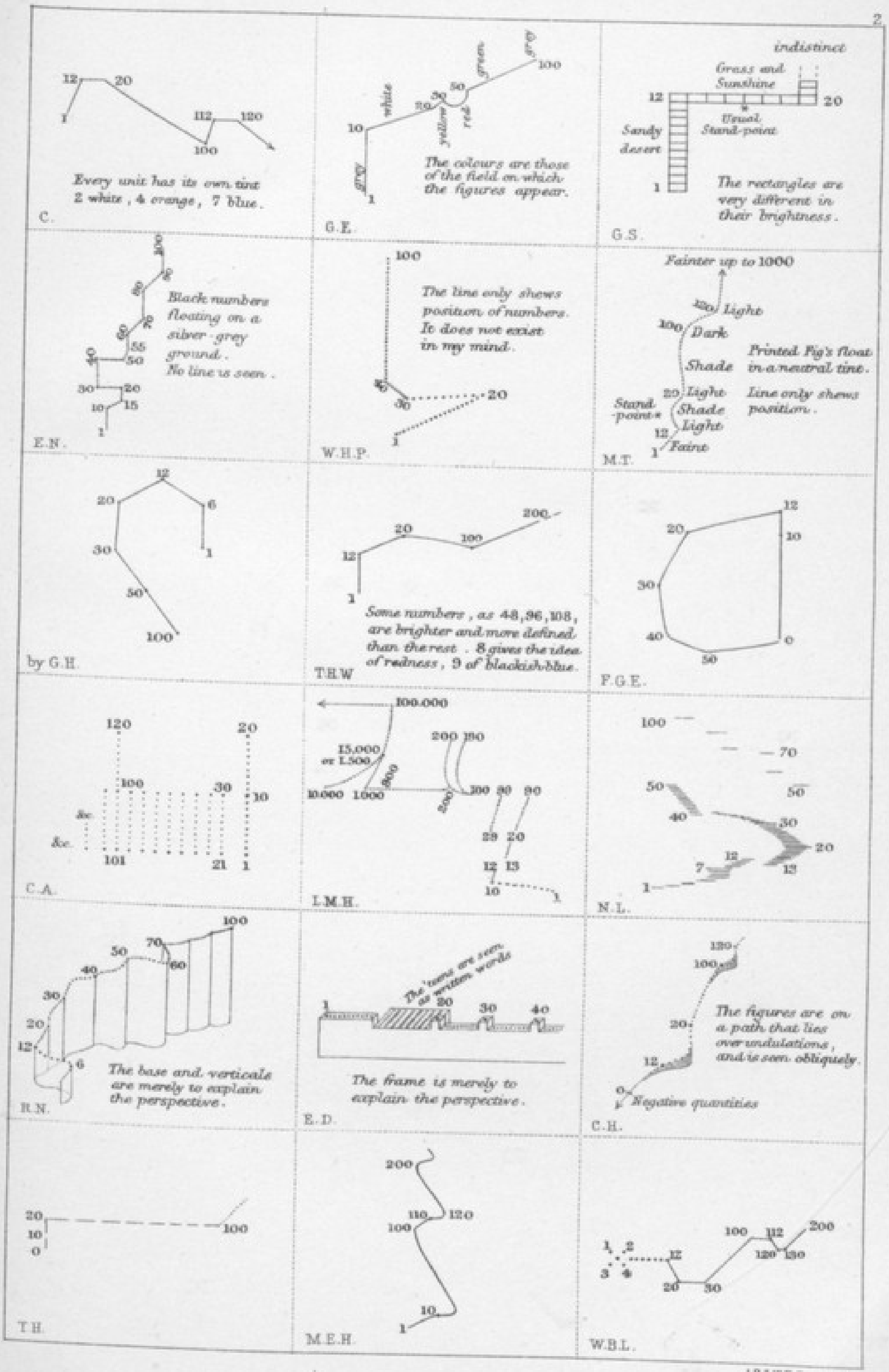


J.P. & W.R. Emslie, lith.

VISUALISED NUMERALS

BY FRANCIS GALTON, F.R.S.

f. 9



VISUALISED NUMERALS,
by Francis Galton, F.R.S.

Arthur Schuster, F.R.S.—The first figure shows the appearance the diagram 0-100 would have if looked at perpendicularly. It recedes from the eye with a slight upward slope of about 1 in 12. I make extensive use of this diagram, it seems to me to act as a shelf on which I can put any number and take it out again when required. There is however, a good deal of elasticity in this (as well as in the second figure), when I am specially occupied with one part of it, say between 70 and 80, as in thinking over what has happened in the last 10 years, that part would seem to become larger and encroach on the territory of its neighbours. On certain occasions also, the diagram would become distorted so as to join the 100 to the 0.

This is not the only figure on which I visualise numbers; the hundreds seem to me to be arranged as in the second figure, in a line sloping upwards. Between 1200 and 1500 the diagram becomes confused; above 1500 I cannot visualise numbers. I have almost daily to deal with such up to four or five figures, but they are only figures to me, I cannot represent them in a diagram.

John Roget.—The first twelve are clearly derived from the spots on dominoes. After 100 there is nothing clear but 108 (*i.e.* 9×12), and then I begin with the units and tens only as above.

B. Woodd Smith.—In my case the numerals follow the route shown in the accompanying figure. Above 200 it becomes vague and is soon lost, except that 999 is always in a corner like 99. The lines bear no reasonable proportion to the numbers they contain, my own position in regard to them is generally at A, nearly opposite my own age, 50, and has shifted as I have grown older, but it sometimes varies between A and B. When at B I always stand with 1-7 to my left, but when at A I can face either towards 7-12 or towards 12-20, or 20-70, but never (I think) with my back to 12-20.

Discussion.

George Bidder, Esq., observed that he had possessed the faculty of mental visualisation referred to in the paper, so long as he could remember. He imagined the mental pictures to be survivals of some early association of childhood, which however, in most cases, it is impossible to trace. In the mental picture or diagram that numerals appear to him to assume, the first twelve numbers are placed as if on a clock face, and probably the idea was originally derived from that source. In his diagram, there was an angle at 10, and again at 12. He could only account for this, by supposing it to be the result of a struggle between the decimal and duodecimal systems of notation. He explained also that not only numbers, but almost all subjects of thought and memory present themselves to his mind in a visualised form:—For example, the months of the year are arranged in a circle. The days of the week in a line from right to left. The dates and events of history have also a definite local arrangement. As regards the latter, he believed that he could identify part of it with the arrangement in a certain historical puzzle-map, which he once, as a child, possessed.

He pointed out in connection with the subject, the curious value of *memoria technica* in assisting the memory, which usually consists of the arbitrary association of the fact to be remembered, with some totally incongruous, and perhaps ludicrous topic, and that apparently the very incongruity is an aid to memory; he also explained that the visualised pictures were not in his case to be confounded with impressions real or false of the organs of external sense, and did not seem to rank with them at all.

Dr. Hack Tuke: With reference to a question just put by Major-General Lane Fox, as to "Whether the cause of the difference between different people in the power to visualise mental impressions depends upon the perfection of the organs of sight? I see no reason to suppose such to be the case. I have no doubt the optic nerve is as well developed, and the sight as good in those who are destitute of this power as in the 1 in 30 who possess it. Dr. Ferrier and others, believe they have made out the visual centre in the grey matter of the cerebral convolutions; and it is probably here that this remarkable power resides. It is not in the peripheral expansion of the optic nerve. If we could examine, I hope it may be long hence, the grey matter of the visual centre of Mr. Bidder and others who have given us their experience to-night, we ought to find under the microscope a greater perfection of structure than in that of ordinary people. If our knowledge were sufficiently advanced, we ought to discover cells exquisitely adapted to their purpose; cells possessing a receptive and retentive power in a superlative degree. This visualising of forms might be called a faculty of physiological hallucination, as distinguished from what I am more familiar with—pathological hallucination. I have paid some attention to this among the insane, and have observed marked differences among them on careful inquiry into their sensations, although at first sight, they seemed identical. Thus, with auditory hallucinations, I find that when a man hears an imaginary voice, he sometimes hears it as clearly as he hears my own; while in other cases it is only heard internally. It is an inward voice. Corresponding conditions, I suspect, occur with those who visualise figures. In some, there is a distinct objective form; in others, the internal representation, however vivid, does not reach the point of objectivity. It would take too long to go into the physiological causes of these differences. There is no doubt that the researches of Mr. Galton in regard to these remarkable mental representations, which are consistent with perfect health, present great interest to those who study the hallucinations which result from disease. In both instances, they are alike purely subjective in their nature.

Mr. Schuster: The diagram of numerals which I see, has roughly the shape of a horse-shoe, lying on a slightly inclined plane, with the open end towards me. It always first comes into view, in front of me, a little to the left, so that the right-hand branch of the horseshoe, at the bottom of which I place 0, is in front of my left eye. The numbers then succeed each other, going upwards and to the left; 50 is placed at the highest point. When I move my eyes without moving my head, the diagram remains fixed in space, and does not follow the movement of my eye. When I move the head, the diagram unconsciously follows the movement, but I can, by an effort, have it fixed in space as before. I can also shift it from one part of the field of view to the other, and even turn it upside down. I use the diagram as a resting-place for the memory, placing a number on it, and finding it again when wanted. A remarkable property of the diagram is a sort of elasticity which enables me to join the two open ends of the horse-shoe together when I want to connect 100 with 0. The same elasticity causes me to see that part of the diagram on which I fix my attention larger than the rest.

I also have a diagram on which I place the months of the year. The diagram is an oval curve. The months follow each other in the direction of motion of the hands of a watch. The summer months take up a much larger space than the winter months.

I see the days of the week arranged in a straight line from right to left.

Although both the numerals and the days of the week succeed each other from right to left, I am not left-handed.

Mr. A. Tylor: Mr. Bidder in his remarkable and most valuable account of the workings of his own mind, and of the hereditary power which he possesses of visualising, has stated: First, That the face of the clock itself (but with the figures XI and XII deficient) from which as a child he had learnt to tell the time, recurs to his mind when he visualises. Second, That the picture of a certain number of the kings of England following William the Conqueror, appears still in his mind in the same row that he first saw them in the child's pictorial history book from which he learnt their names, dates, and order. From the statement made by Mr. Galton on the authority of most of the visualists, the impressions of this kind made in childhood are the most permanent, brightest and clearest. The events happening since childhood are more difficult to visualise than the earlier periods of history.

This statement refers us to the importance of object lessons for children, the Kinder Garten system, and explains why children should be taught by objects. A block, with three dimensions, faced with a picture of an object used to illustrate a letter or word, seem to enable any child to visualise and make the first great abstract step in education.

I may mention my own experience on a subject not touched on by Mr. Galton: viz., the manner of learning to distinguish the right hand from the left.

I found that difficult, and when a young child invented for myself a plan, of overcoming that difficulty, I pictured, or as it will now be called (after the valuable discovery of Mr. Galton), visualised myself always in the same position in the same room riding on a rocking-horse, with a whip in my right hand; as I knew that the hand with the whip must be always between the horse and the wall, I could determine which was my right hand in whatever position I actually was, by placing myself visually in the proper position on the horse. No doubt most children do something of this kind in learning lessons, music, or ciphering.

Had I known how to interpret what had happened to myself and to Mr. Galton's other observers—when I read before the Institute, my paper on the "Object-Origin of Pre-historic Thoughts and Ideas,"* I should have strengthened my argument on Thought. Mr. Galton's researches extend the principle I thus advocated very much. I believe now, that the only thoughts that young children can attain to, have a distinct object-origin, and on this point children resemble the whole animal world. Not only has Mr. Galton's inquiry a local value, but his investigation will probably affect the theory of the working of the human mind, and have an important application on other questions of biology.

Mr. Roget on being called upon, stated that the form which the numbers from 1 to 100 instinctively assumed in his imagination, did not seem to exhibit any remarkable peculiarities as compared with those of other persons who saw such forms. It was, however, so deeply engraven in his mind, that a strong effort of the will was required to substitute for it any artificial arrangement. This he had found to be the case in the endeavour to fix dates in his memory. He had, in childhood, been trained by his father (the late Dr. Roget), to the use of a well-known system of *memoria technica* advocated by Feinaigle, in which each year has its special place on the walls of a particular room, and the rooms of a house represent successive centuries. This plan his father had made great use of, and it had always served the speaker well for the chronology of earlier ages; but, for that in which we live, particularly for events during his own life, he had, in spite of various attempts, never succeeded in fairly locating the dates in the room assigned to them. They *would* go to what seemed to be their natural homes in the arrangement above referred to, which had come to him from some other, probably prior, but unknown source. The numbers from 1 to 12, taken separately, usually appeared to him in symmetrical forms, chiefly learnt, he had little doubt, from the spots on dominoes.

Mr. Richard B. Martin: I should like to ask Mr. Galton if he has observed the singular power which is the subject of his paper to exist in any particular class of persons, or to be associated with any special pursuits, artistic, mathematical, or otherwise.

The Rev. G. Henslow described his own scheme of visualised numerals, which, like several others, had an angular bend at 10, and another at 12. The figures 1–6 being horizontal, fig. 6 was in the usual point of sight, 7 to 10 being vertically arranged. The

* "Trans. Anthropological Institute," vol. vi, p. 125.

whole range from 1 to 100 (101 recommencing at 1) was in sight at once, and any figure could be observed in its normal place; but if the head was turned, the whole scheme moved accordingly. By an effort of the will, if the eyes were *alone* turned and not the head, the scheme could be shifted also, so that the fig. 6 would still retain its position in the line of sight.

His mental alphabet was described as partially coloured; several of the letters being the initial letters of colours, partake of the same hues. Thus, B, G, R, P, are blue, green, red, purple, respectively. I is black, being the initial letter of Ink, while C and O are white, apparently due to the white space included within the circle of black; that others are coloured, such as A being yellow, and several grey. He could not account for these facts.

Mr. Henslow also described his experience of *Visual Objects.* On shutting the eyes and waiting for a minute or so, some object, real or nondescript, is sure to appear. Something in its form appears to be suggestive of some other object, into which it spontaneously turns, the latter resolving itself into a third, and so on till the series vanishes. The visual objects are thus purely automatic creations of the brain. Sometimes an object will appear which had been previously seen, but entirely forgotten, showing that unconscious or automatic memory was at work. The objects often seen are elaborately cut glass bowls, etc., highly ornamental; embossed, chased or frosted or filagreed gold and silver ornaments, flower-stands, etc., of exquisite beauty; as well as common objects, fruit, flowers, jugs, sofas, etc. Brilliant and elaborate patterns of textile fabrics are not unfrequent. Choice bits of scenery, such as a narrow gorge, covered with ferns and mosses, with cascades, etc., or again, well-remembered scenes of childhood, will spontaneously appear.

If an attempt be made to foist some object into the dioramic series, a great effort of the will is required. The first attempt may either fail entirely or some nondescript hybrid structure, part automatic and part volitional, will appear. By a continued and determined effort to see the object thought of, the will or volitional effort may overcome the automatic action of the brain, so that the object determined upon will at last appear distinct and sharply defined.

Every object is generally very distinct, though if of some length, the whole of it cannot always be seen at once, thus the stock of a gun was only visible, not the barrel. They are at focal distance, excepting scenery, which appears as in nature. The objects are of small size, 1 to 2 or 3 inches in diameter or length.

Several water-colour illustrations of visual objects were exhibited by Mr. Henslow.

Colonel Yule, C.B.: I am afraid my experiences in this way are less striking and vivid than those described by the gentlemen who have spoken. The diagram representing the form in which I see the series of numbers is on the wall, and will be seen to be of a very simple kind compared with theirs. With me, too, the impressions have become sensibly weaker of late years, and in describing them

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it is not always quite easy to say how far I am speaking from surviving impressions, and how far from memories of the past. I must say, too, that I have found that under the effort to fix and describe these impressions in writing for Mr. Galton, they have become, as it were, thinner, and hard to catch; and in this experience I do not stand alone.

Though I could respond to much that was said of their own impressions by Mr. Bidder and Mr. Henslow, there is one point in which their experiences raise in me strong dissent. They actually describe not only the procession of numbers as seen by them, but that of the days of the week and the months of the year as advancing from *right to left!* Now, so strong with me is the opposite impression that their description seems to me quite anomalous, and in fact if I said all I felt I should say—"Why, everybody *knows* that they go the other way."

I may mention that the procession of numbers as I see them, rising vertically from 1 to 20, and from 20 going off to the right in a tolerably straight line up to 100, applies strictly also to my retrospect of the history of the centuries. Every event in the first 20 years of a century (*e.g.*, the Union with Scotland, the Rebellion of 1715 in the last century; or the Regency, the battle of Waterloo, etc., in the present century), I see as in the vertical part of the series, every event in the remaining decades of the century falls into the horizontal procession.

Colonel Yule then spoke of the form and different colours of the days of the week as they appeared to him; and in conclusion said that in being called up to speak on this subject, he could not but feel a good deal like M. Jourdain, who was so astonished at learning that he had been speaking prose for 40 years without knowing it. So he (the speaker) had been *visualising* for a good deal more than 40 years, and but for their friend Mr. Galton he should never have become aware of the fact.

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Statistics of Mental Imagery,

BY

FRANCIS GALTON, F.R.S.

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STATISTICS OF MENTAL IMAGERY.

An outline is given in the following memoir of some of the earlier results of an inquiry which I am still prosecuting, and a comparatively new statistical process will be used in it for the first time in dealing with psychological data. It is that which I described under the title of "Statistics by Intercomparison" in the *Philosophical Magazine* of Jany., 1875.

The larger object of my inquiry is to elicit facts that shall define the natural varieties of mental disposition in the two sexes and in different races, and afford trustworthy data as to the relative frequency with which different faculties are inherited in different degrees. The particular branch of the inquiry to which this memoir refers, is Mental Imagery; that is to say, I desire to define the different degrees of vividness with which different persons have the faculty of recalling familiar scenes under the form of mental pictures, and the peculiarities of the mental visions of different persons. The first questions that I put referred to the illumination, definition and colouring of the mental image, and they were framed as follows (I quote from my second and revised schedule of questions):—

"Before addressing yourself to any of the Questions on the opposite page, think of some definite object—suppose it is your breakfast-table as you sat down to it this morning—and consider carefully the picture that rises before your mind's eye.

1. *Illumination.*—Is the image dim or fairly clear? Is its brightness comparable to that of the actual scene?

2. *Definition.*—Are all the objects pretty well defined at the same time, or is the place of sharpest definition at any one moment more contracted than it is in a real scene?

3. *Colouring.*—Are the colours of the china, of the toast, bread-crust, mustard, meat, parsley, or whatever may have been on the table, quite distinct and natural?"

There were many other questions besides these, of which I defer mention for the moment.

The first results of my inquiry amazed me. I had begun by questioning friends in the scientific world, as they were the most likely class of men to give accurate answers concerning this faculty of visualising, to which novelists and poets continually allude, which has left an abiding mark on the vocabularies of every language, and which supplies the material out of which dreams and the well-known hallucinations of sick people are built up.

To my astonishment, I found that the great majority of the men of science to whom I first applied, protested that mental imagery was unknown to them, and they looked on me as fanciful and fantastic in supposing that the words 'mental imagery' really expressed what I believed everybody supposed them to mean. They had no more notion of its true nature than a colour-blind man who has not discerned his defect has of the nature of colour. They had a mental deficiency of which they were unaware, and naturally enough supposed that those who were normally endowed, were romancing. To illustrate their mental attitude it will be sufficient to quote a few lines from the letter of one of my correspondents, who writes:—

"These questions presuppose assent to some sort of a proposition regarding the 'mind's eye' and the 'images' which it sees. . . . . This points to some initial fallacy. . . . . . It is only by a figure of speech that I can describe my recollection of a scene as a 'mental image' which I can 'see' with my 'mind's eye'. . . . . I do not see it . . . any more than a man sees the thousand lines of Sophocles which under due pressure he is ready to repeat. The memory possesses it, &c."

Much the same result followed some inquiries made for me by a friend among members of the French Institute.

On the other hand, when I spoke to persons whom I met in general society, I found an entirely different disposition to prevail. Many men and a yet larger number of women, and many boys and girls, declared that they habitually saw mental imagery, and that it was perfectly distinct to them and full of colour.

The more I pressed and cross-questioned them, professing myself to be incredulous, the more obvious was the truth of their first assertions. They described their imagery in minute detail, and they spoke in a tone of surprise at my apparent hesitation in accepting what they said. I felt that I myself should have spoken exactly as they did if I had been describing a scene that lay before my eyes, in broad daylight, to a blind man who persisted in doubting the reality of vision. Reassured by this, I recommenced to inquire among scientific men, and soon found scattered instances of what I sought, though in by no means the same abundance as elsewhere. I then circulated my questions more generally among my friends, and so obtained the replies that are the main subject of this memoir. The replies were from persons of both sexes and of various ages, but I shall confine my remarks in this necessarily brief memoir to the experiences derived from the male sex alone.

I have also received batches of answers from various educational establishments, and shall here make use of those sent by the Science Master of the Charterhouse, Mr. W. H. Poole, which he obtained from all the boys who attended his classes, after fully explaining the meaning of the questions, and interesting the boys in them. They have the merit of returns derived from a general census, which my other data lack, because I cannot for a moment suppose that the writers of them are a haphazard proportion of those to whom they were sent. Indeed, I know some men who, disavowing all possession of the power, cared to send no returns at all, and many more who possessed it in too faint a degree to enable them to express what their experiences really were, in a manner satisfactory to themselves. Considerable similarity in the general style of the replies will however be observed between the two sets of returns, and I may add that they accord in this respect with the oral information I have elsewhere obtained. The conformity of replies from so many different sources, the fact of their apparent trustworthiness being on the whole much increased by cross-examination (though I could give one or two amusing instances of break-down), and the evident effort made to give accurate answers, have convinced me that it is a much easier matter than I had anticipated to obtain trustworthy replies to psychological questions. Many persons, especially women and intelligent children, take pleasure in introspection, and strive their very best to explain their mental processes. I think that a delight in self-dissection must be a strong ingredient in the pleasure that many are said to take in confessing themselves to priests.

Here then are two rather notable results: the one is the proved facility of obtaining statistical insight into the processes of other

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persons' minds; and the other is that scientific men as a class have feeble powers of visual representation. There is no doubt whatever on the latter point, however it may be accounted for. My own conclusion is, that an over-readiness to perceive clear mental pictures is antagonistic to the acquirement of habits of highly generalised and abstract thought, and that if the faculty of producing them was ever possessed by men who think hard, it is very apt to be lost by disuse. The highest minds are probably those in which it is not lost, but subordinated, and is ready for use on suitable occasions. I am however bound to say, that the missing faculty seems to be replaced so serviceably by other modes of conception, chiefly I believe connected with the motor sense, that men who declare themselves entirely deficient in the power of seeing mental pictures can nevertheless give life-like descriptions of what they have seen, and can otherwise express themselves as if they were gifted with a vivid visual imagination. They can also become painters of the rank of Royal Academicians.

The facts I am now about to relate, are obtained from the returns of 100 adult men, of whom 19 are Fellows of the Royal Society, mostly of very high repute, and at least twice, and I think I may say three times, as many more are persons of distinction in various kinds of intellectual work. As already remarked, these returns taken by themselves, do not profess to be of service in a *general* statistical sense, but they are of much importance in showing how men of exceptional accuracy express themselves when they are speaking of mental imagery. They also testify to the variety of experiences to be met with in a moderately large circle. I will begin by giving a few cases of the highest, of the medium, and of the lowest order of the faculty of visualising. The hundred returns were first classified according to the order of the faculty, as judged from the whole of what was said in them, and all I knew from other sources of the writers; and the number prefixed to each quotation shows its place in the class-list.

Vividness of Mental Imagery.

(From returns furnished by 100 men, at least half of whom are distinguished in science or in other fields of intellectual work.)

Cases where the faculty is very high.

1. Brilliant, distinct, never blotchy.
2. Quite comparable to the real object. I feel as though I was dazzled, *e.g.*, when recalling the sun to my mental vision.
3. In some instances quite as bright as an actual scene.
4. Brightness as in the actual scene.
5. Thinking of the breakfast table this morning, all the objects in my mental picture are as bright as the actual scene.

6. The image once seen is perfectly clear and bright.

7. Brightness at first quite comparable to actual scene.

8. The mental image appears to correspond in all respects with reality. I think it is as clear as the actual scene.

9. The brightness is perfectly comparable to that of the real scene.

10. I think the illumination of the imaginary image is nearly equal to that of the real one.

11. All clear and bright; all the objects seem to me well defined at the same time.

12. I can see my breakfast table or any equally familiar thing with my mind's eye, quite as well in all particulars as I can do if the reality is before me.

Cases where the faculty is mediocre.

46. Fairly clear and not incomparable in illumination with that of the real scene, especially when I first catch it. Apt to become fainter when more particularly attended to.

47. Fairly clear, not quite comparable to that of the actual scene. Some objects are more sharply defined than others, the more familiar objects coming more distinctly in my mind.

48. Fairly clear as a general image; details rather misty.

49. Fairly clear, but not equal to the scene. Defined, but not sharply; not all seen with equal clearness.

50. Fairly clear. Brightness probably at least one-half to two-thirds of original. [The writer is a physiologist.] Definition varies very much, one or two objects being much more distinct than the others, but the latter come out clearly if attention be paid to them.

51. Image of my breakfast table fairly clear, but not quite so bright as the reality. Altogether it is pretty well defined; the part where I sit and its surroundings are pretty well so.

52. Fairly clear, but brightness not comparable to that of the actual scene. The objects are sharply defined; some of them are salient, and others insignificant and dim, but by separate efforts I can take a visualised inventory of the whole table.

53. Details of breakfast table *when the scene is reflected on*, are fairly defined and complete, but I have had a familiarity of many years with my own breakfast table, and the above would not be the case with a table seen casually unless there were some striking peculiarity in it.

54. I can recall any single object or group of objects, but not the whole table at once. The things recalled are generally clearly defined. Our table is a long one; I can in my mind pass my eyes all down the table and see the different things distinctly, but not the whole table at once.

Cases where the faculty is at the lowest.

89. Dim and indistinct, yet I can give an account of this morning's breakfast table;—split herrings, broiled chickens, bacon, rolls, rather light coloured marmalade, faint green plates with stiff pink flowers, the girls' dresses, &c., &c. I can also tell where all the dishes were, and where the people sat (I was on a visit). But my imagination is seldom pictorial except between sleeping and waking, when I sometimes see rather vivid forms.

90. Dim and not comparable in brightness to the real scene. Badly defined with blotches of light; very incomplete.

91. Dim, poor definition; could not sketch from it. I have a difficulty in seeing two images together.

92. Usually very dim. I cannot speak of its brightness, but only of its faintness. Not well defined and very incomplete.

93. Dim, imperfect.

94. I am very rarely able to recall any object whatever with any sort of distinctness. Very occasionally an object or image will recall itself, but even then it is more like a generalised image than an individual image. I seem to be almost destitute of visualising power, as under control.

95. No power of visualising. Between sleeping and waking, in illness and in health, with eyes closed, some remarkable scenes have occasionally presented themselves, but I cannot recall them when awake with eyes open, and by daylight, or under any circumstances whatever when a copy could be made of them on paper. I have drawn both men and places many days or weeks after seeing them, but it was by an effort of memory acting on study at the time, and assisted by trial and error on the paper or canvas, whether in black, yellow or colour, afterwards.

96. It is only as a figure of speech that I can describe my recollection of a scene as a 'mental image' which I can 'see' with my 'mind's eye.' . . The memory possesses it, and the mind can at will roam over the whole, or study minutely any part.

97. No individual objects, only a general idea of a very uncertain kind.

98. No. My memory is not of the nature of a spontaneous vision, though I remember well where a word occurs in a page, how furniture looks in a room, &c. The ideas are not felt to be mental pictures, but rather the symbols of facts.

99. Extremely dim. The impressions are in all respects so dim, vague and transient, that I doubt whether they can reasonably be called images. They are incomparably less than those of dreams.

100. My powers are zero. To my consciousness there is almost no association of memory with objective visual impressions. I recollect the breakfast table, but do not see it.

These quotations clearly show the great variety of natural powers of visual representation. I will proceed to examine the subject more closely, and to compare the returns from the 100 men with those from the Charterhouse boys, on the principle of my "Statistics by Intercomparison," which I must first explain at sufficient length.

There are many who deny to statistics the title of a science, and say that it is a mere collection of facts. For my part I think that there is such a thing as a science of statistics, though its field is narrowed almost to a point. Its object is to *discover methods* of epitomising a great, even an infinite, amount of variation in a compact form. To fix the ideas, it is well to take as an example the heights of men, in which case the science of statistics enables us to specify, by means of a very few figures, the conditions of stature that characterise the whole of the adult male inhabitants, say of the British Isles. These figures will suffice to inform us that there are so many per cent. between such and such heights, and so many between such other heights, giving us material whence we can answer any such question as this:— Out of 1000 men how many are we likely to find between 5 feet and 6 feet in height? If the figures do not give the answer directly, we can find it by interpolation and easy calculation from them. So again, if we wish to compare the heights of

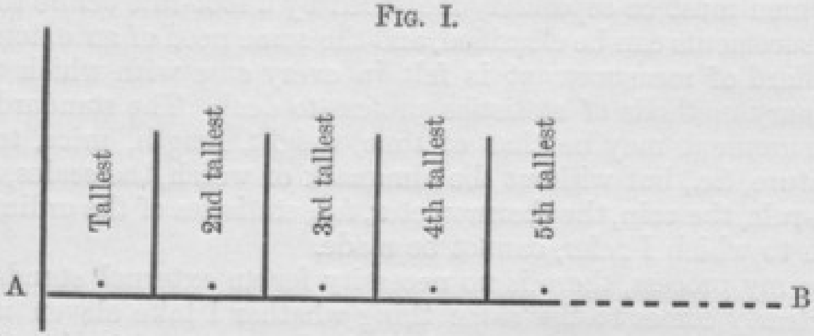
Englishmen and Frenchmen, statistics show how to obtain the average height of the two races, and the two averages may be readily compared, which goes a considerable way towards answering the question; or, if we wish it, we may compare very much more in detail, all the facts that are needed for the purpose being contained in the few figures of which I spoke.

But all these operations require the use of an *external standard.* The men must be separately measured by a foot-rule before their measurements can be classified, and the same need of an external standard of measurement is felt in every case with which the ordinary methods of statistics profess to deal. The standard of measurement may be that of time, weight, length, price, temperature, &c., but without the almanack or watch, the scales, the foot-rule, the coin, the thermometer, &c., statistics of the ordinary form to which I refer, cannot be made.

In my process, there is no necessity for an external standard. It clearly comes to the same thing whether I take eleven men and, measuring them one against another, range them in order, beginning with the highest and ending with the lowest, or if I measure them separately with a foot-measure, and range them in the order of the magnitude of the measurements recorded in my note-book. In each case the tallest man will stand first, the next tallest second, and so on to the last. In each case the same man will occupy the sixth or *middlemost* place, and will therefore represent the *medium* height of the whole of them. I do not wish to imply that 'medium' is identical with 'mean' or 'average,' for it is not necessarily so. But I do say that the word medium may be strictly defined, and therefore if we wish to compare the heights of Englishmen with Frenchmen, we shall proceed just as scientifically if we compare their medium heights as if we compare their average heights. Now it will be observed that we have got the medium heights without a foot-rule or any external standard; we have done so altogether by the method of intercomparison. In the particular question with which we are dealing I have classified the answers according to the degree of vividness of mental imagery to which they depose, and I pick out the middlemost answer and say that the description given in it describes the medium vividness of mental imagery in the group under discussion. If I want to compare two such groups I compare their respective middlemost answers, and judge which of the two implies the higher faculty.

Thus much is a great gain, yet I claim to effect more; but in order to explain what that is I must return to the illustration of heights of men. Suppose them as before to be all arranged in order of their stature, at equal distances apart on a long line A B, with their backs turned towards us. If there be a thousand men, we must suppose A B to be divided into 1000 equal parts, and a man to be set in the middle of each part. The tallest man will have A close to his left, and the shortest man will have B close to his right. They will form a series as shewn in Fig. I., where the subdivisions of A B are indicated by the vertical lines, and the positions where the men are standing are shown by the dots half-way between those lines.

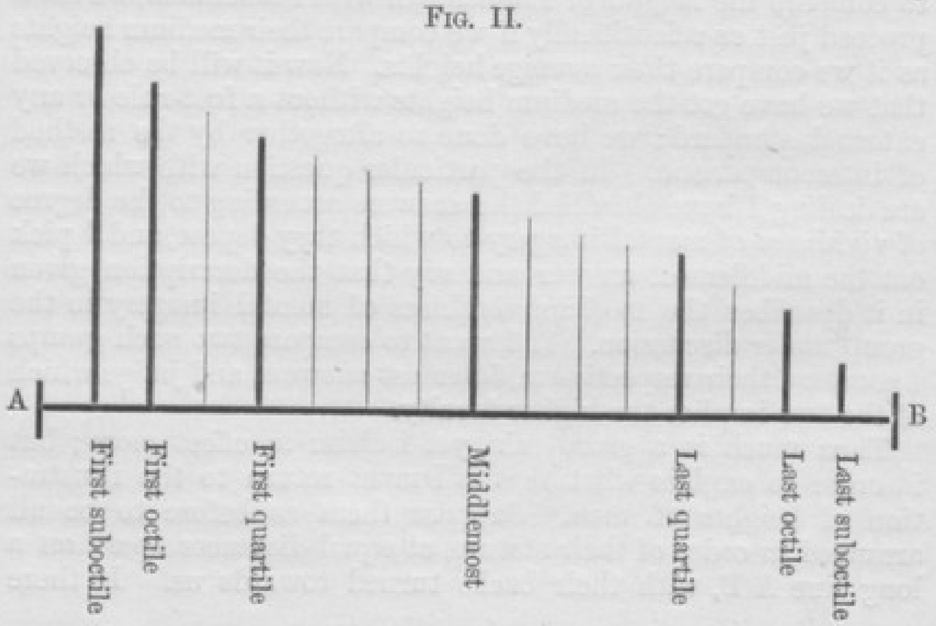
FIG. I.



Owing to the continuity of every statistical series, the imaginary line drawn along the tops of the heads of the men will form a regular curve, and if we can record this curve we shall be furnished with data whereby to ascertain the height of every man in the whole series. Drawing such a curve for Englishmen and another for Frenchmen, and superimposing the two, we should be able to compare the statures of the two nations in the minutest particulars.

A curve is recorded by measuring its ordinates. If we divide A B by a sufficient number of equi-distant subdivisions and measure the ordinates at each of them as has been done in Fig.

FIG. II.



II. (where the ordinates only are shewn, and not the curve), we can at any time plot them to scale, and by tracing a free line touching their tops, we can with more or less precision, reproduce the curve. It happens, however, from the peculiar character of all statistical curves, that ordinates at equal distances apart are by no means the most suitable. The mediocre cases are always so numerous that the curve flows in a steady and almost straight line about its middle, and it becomes a waste of effort to take many measurements thereabouts. On the other hand its shape varies rapidly at either end, and there the observations ought to be numerous. The most suitable stations are those which correspond to ordinates that differ in height by *equal degrees*, and these places admit of being discovered by *à priori* considerations on certain general suppositions.[1]

We shall however do well to ignore those minutiæ on which I laid much stress in the Memoir, and adopt the simpler plan of successive subdivisions of A B, and of measuring the ordinates shown by darkened lines in Fig. II., and severally named there as 'middlemost,' first and last 'quartile,' first and last 'octile,' and first and last 'suboctile'. This is far enough for our present wants, though the system admits of indefinite extension. By measuring the 'ordinate,' I mean measuring the 'man' whose place in the series is nearest to the true position of that ordinate. Absolute coincidence is not needed in such rude work as this; thus in a series of 100 men either the 50th or the 51st will do duty for the middlemost. The places I have actually taken in the series of 100 men for the several stations, are, the 6th and 94th for the first and last suboctiles, the 12th and 88th for the octiles, the 25th and 75th for the quartiles, and the 50th for the middlemost.

Seven men thus become the efficient representatives of a very large class. It will be found as a general rule that these seven selected representatives will differ each from the next by approximately *equal intervals*, the difference between the suboctile and the octile being usually about the same as that between the octile and quartile, and between the quartile and the middlemost.

[1] These are discussed in the Memoir already referred to, "Statistics by Intercomparison," by myself, in *Phil. Mag.*, Jan., 1875, but there are some errors, and also some appearances of error owing to faults of expression, in that article, which were first pointed out to me by Mr. J. W. L. Glaisher. There is a full mathematical discussion bearing on the matter in a memoir by Mr. D. McAlister in the *Proceedings of the Royal Society*, 1879, on the "Law of the Geometric Mean," to which and to the immediately preceding paper by myself on the "Geometric Mean," I would refer the mathematical reader. Mr. J. W. L. Glaisher has also taken the subject in hand and calculated tables, and I trust that his memoir thereon may before long be published.

As a matter of interest, and for the chance of finding very exceptional cases, I also record the highest and the lowest of the series, but it must be clearly understood that these have no solid value for purposes of comparison. In the first place, their position as ordinates is uncertain unless the number of the group of cases is given, for when the number is large the position of the highest and lowest will be nearer to A and B respectively than when it is small. In the second place, the highest and lowest being outside cases, they are more liable to be of an exceptional character than any of those which stand between neighbours, one on either hand of it.

The comparison of any two groups is made by collating their seven representatives each to each, the first suboctile of the one with the first suboctile of the other, the first octile with the first octile, the first quartile with the first quartile, and so on. I also collate the highest of each, and again the lowest of each, as a mere matter of interest, but not as an accurate statistical operation, for the reasons already given.

It is possible that I may be thought to have somewhat loosely expressed myself under the necessity of foregoing the use of technical terms, but the mathematical reader who demands precision of statement will understand me, while it would require a treatise and much study to make the mathematical substratum of my method perfectly intelligible to a person who was not familiar with the laws of 'Probabilities' and 'Frequency of Error'.

In the following comparison between the 100 Adult Englishmen and the 172 Charterhouse boys, I have divided the latter into two groups, to serve as a check upon one another. Group A includes boys of the four upper classes in the school, group B those of the five lower classes. I have combined their replies as to Illumination and Definition under the single head of 'Vividness,' and have taken no editorial liberties whatever except of the most pardonable description. It is wonderful how well and graphically the boys write, and how much individual character is shown in their answers.

Vividness of Imagery.

HIGHEST.

Adult Males.—Brilliant, distinct, never blotchy.

Charterhouse A.—The image is perfectly clear. I can see every feature in every one's face and everything on the table with great clearness. The light is quite as bright as reality.

Charterhouse B.—The image that arises in my mind is perfectly clear. The brightness is decidedly comparable to that of the real scene, for I can see in my mind's eye just as well as if I was beholding the scene with my real eye.

FIRST SUBOCTILE.

Adult Males.—The image once seen is perfectly clear and bright.

Charterhouse A.—It is very clear and is as bright as it actually was. Everything occurs most distinctly. I can imagine everything at once, but can think a great deal more clearly by thinking more on a particular object.

Charterhouse B.—I see it exactly as it was, all clearly defined just as it was.

FIRST OCTILE.

Adult Males.—I can see my breakfast table or any equally familiar thing with my mind's eye quite as well in all particulars as I can do if the reality is before me.

Charterhouse A.—To me the picture seems quite clear and the brightness equal to the real scene. I cannot see the whole scene at the same instant, but I see one thing at once and can turn my eye mentally to another object very quickly, so that I soon get the whole scene before my mind.

Charterhouse B.—Fairly clear. I cannot see everything at the same time, but what I do see seems almost real.

FIRST QUARTILE.

Adult Males.—Fairly clear; illumination of actual scene is fairly represented. Well defined. Parts do not obtrude themselves, but attention has to be directed to different points in succession to call up the whole.

Charterhouse A.—The image is fairly clear, but its brightness is dimmer than the actual. The objects are mostly defined clearly and at the same time.

Charterhouse B.—Fairly clear, the objects are pretty well defined at the same time.

MIDDLEMOST.

Adult Males.—Fairly clear. Brightness probably at least from one-half to two-thirds of the original. Definition varies very much, one or two objects being much more distinct than the others, but the latter come out clearly if attention be paid to them.

Charterhouse A.—The image is fairly clear, but its brightness is not comparable to that of the actual scene. The objects are pretty well defined at the same time.

Charterhouse B.—The image is pretty clear, but not so clear as the actual thing. I cannot take in the whole table at once, and I cannot see more than three plates at once, and when I try to see both ends of the table I cannot see anything of the middle. I can see nothing beyond the table, but the table itself seems to stand out from the distance beyond.

LAST QUARTILE.

Adult Males.—Dim, certainly not comparable to the actual scene. I have to think separately of the several things on the table to bring them clearly before the mind's eye, and when I think of some things the others fade away in confusion.

Charterhouse A.—The image is fairly clear. I cannot see everything at once, but as I think of them they come clearly before me. The objects are not all defined at the same time, and the place of sharpest definition is more contracted than in real scene.

Charterhouse B.—If I think of any particular thing without the others, it seems clear; all at once, are not clear.

LAST OCTILE.

Adult Males.—Dim and not comparable in brightness to the real scene.

Badly defined with blotches of light; very incomplete; very little of one object is seen at one time.

Charterhouse A.—I can call up to my mind the picture of the breakfast table in every detail, but seem to see everything through a darkened pane of glass. I see just the same number of people, plates, &c., the whole time, provided of course that I do not change my idea of the scene to any great degree.

Charterhouse B.—Rather dim; the objects are pretty well defined.

LAST SUBOCTILE.

Adult Males.—I am very rarely able to recall any object whatever with any sort of distinctness. Very occasionally an object or image will recall itself, but even then it is more like a generalised image than an individual one. I seem to be almost destitute of visualising power as under control.

Charterhouse A.—The image is dim, dark, and smaller than the actual scene, and the objects nearest to me show most distinctly. The whole picture is more or less of a dark green tint.

Charterhouse B.—Dim. The place of sharpest definition is more contracted than in a real scene.

LOWEST.

Adult Males.—My powers are zero. To my consciousness there is almost no association of memory with objective visual impressions. I recollect the table, but do not see it.

Charterhouse A.—Image dim, the brightness much less than in the real scene. Only one object is very clearly visible at the same time.

Charterhouse B.—Very dim. I can only see one part at a time.

I gather from the foregoing paragraphs that the A and B boys are alike in mental imagery, and that the adult males are not very dissimilar to them; but the latter do not seem to form so regular a series as the boys. They are avowedly not members of a true statistical group, being an aggregate of one class of persons who replied because they had remarkable powers of imagery and had much to say, of another class of persons, the scientific, who on the whole are very deficient in that gift, and of a third class who may justly be considered as fair samples of adult males.

I next proceed to colour, and annex the returns to the third of the above questions, which I have classified on the same principle as before.

Colour Representation.

HIGHEST.

Adult Males.—Perfectly distinct, bright, and natural.

Charterhouse A.—Yes, *perfectly* distinct and natural.

Charterhouse B.—The colours look more clear than they really are.

FIRST SUBOCTILE.

Adult Males.—White cloth, blue china, argand coffee pot, buff stand with sienna drawing, toast,—all clear.

Charterhouse A.—I see the colours just as if they were before me, and perfectly natural.

Charterhouse B.—The colours are especially distinct in every case.

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FIRST OCTILE.

Adult Males.—All details seen perfectly.

Charterhouse A.—Quite distinct and natural.

Charterhouse B.—All colours are perfectly distinct to me in my mind's eye, in whatever scene or shape they appear to me.

FIRST QUARTILE.

Adult Males.—Colours distinct and natural till I begin to puzzle over them.

Charterhouse A.—Quite distinct and natural.

Charterhouse B.—The colours of the china, &c., are quite distinct and natural.

MIDDLEMOST.

Adult Males.—Fairly distinct, though not certain that they are accurately recalled.

Charterhouse A.—They are all distinct after a little thought, and are natural.

Charterhouse B.—Yes, quite distinct and natural.

LAST QUARTILE.

Adult Males.—Natural, but very indistinct.

Charterhouse A.—The colours of the most pronounced things on the table are distinct, as the white tablecloth and yellow mustard.

Charterhouse B.—Some are ; china, mustard, toast,—the others are not.

LAST OCTILE.

Adult Males.—Faint, can only recall colours by a special effort for each.

Charterhouse A.—Colours not very distinct.

Charterhouse B.—They are natural, but not very distinct.

LAST SUBOCTILE.

Adult Males.—(Power is nil.)

Charterhouse A.—The colours are very dim.

Charterhouse B.—The colours seem to be more like shades, but they have some colour in them.

LOWEST.

Adult Males.—(Power is nil.)

Charterhouse A.—(Power is nil.)

Charterhouse B.—(Power is nil.)

The same general remarks may be made about the distribution of the faculty of colour representation as about that of the vividness of imagery. It seems that on the whole, colour is more easily recalled than form, and especially so by the young. As the faculty of visual representation is being dropped by disuse, colour disappears earlier than form. This I may remark, was the case with the often quoted hallucinations of Nicolai, which, in his progress to recovery, faded in colour before they faded in outline.

One of my correspondents, an eminent engineer, who has a highly developed power of recalling form, but who described himself as deficient in the power of recalling colour, tells me that since receiving and answering my questions he has practised himself in visualising colours and has succeeded perfectly in doing so. It now gives him great pleasure to recall them.

It will be of interest to extract the few instances from the returns of the Adult Males in which peculiarities were noticed in connexion with colour representation, other than in its degree of vividness. Each sentence is taken from a different return.

Light colours quite distinct, darker ones less so.
Patchy.
Generally hueless, unless excited.
Mostly neutral.
Brown colour, *e.g.* of the gravy, is difficult to visualise.

Another question that I put was as follows:—

"*Extent of field of view.*—Call up the image of some panoramic view (the walls of your room might suffice); can you force yourself to see mentally a wider range of it than could be taken in by any single glance of the eyes? Can you mentally see more than three faces of a die, or more than one hemisphere of a globe at the same instant of time?"

It would have been possible to classify the Charterhouse returns, but the answers were not so generally good as to make it advisable to spend pains upon them. I therefore content myself with the replies of the Adult Males, but shall subsequently add a few facts taken from those of the boys, in a separate paragraph.

Extent of Field of Mental View.

Highest.—My mental field of vision is larger than the normal one. In the former I appear to see everything from some commanding point of view, which at once embraces every object and all sides of every object.

First Suboctile.—A wider range. A faint perception *I think* of more than three sides of a room. Rather more *I think* than one hemisphere, but am not quite sure about this.

First Octile.—Field of view corresponding to reality.

First Quartile.—Field of view corresponding to reality.

Middlemost.—Field of view corresponding to reality.

Last Quartile.—I think the field of view is distinctly smaller than the reality. The object I picture starts out distinct with a hazy outline.

Last Octile.—Much smaller than the real. I seem only to see what is straight in front as it were.

Last Suboctile. }
Lowest. } No field of view at all.

It may seem strange to some that the field of mental vision should occasionally be wider than reality, but I have sufficient testimony to the fact from correspondents of unquestionable accuracy. Here are cases from the returns:—

I seem to see the whole room as though my eye was everywhere. I can see all around objects that I have handled.

I can see three walls of a room easily, and with an effort the fourth. I can see all the faces of a die and the whole globe, but die and globe seem transparent.

[An eminent mineralogist told me that familiarity with crystals gave him the power of mentally seeing all their facets simultaneously.]

This subject is of interest to myself on account of a weird nightmare by which I am occasionally plagued. In my dream, a small ball appears inside my eye. I speak in the singular, because the two eyes then seem fused into a single organ of vision, and I see by a kind of touch-sight all round the ball at once. Then the ball grows, and still my vision embraces the whole of it; it continues growing to an enormous size, and at the instant when the brain is ready to burst, I awake in a fright. Now, what I see in an occasional nightmare, others may be able to represent to themselves when awake and in health.

From the foregoing statistical record it will be seen that in one quarter of the cases, that is to say, in the last quartile and in all below, the field of mental view is decidedly contracted. The Charterhouse returns (A and B combined) give a higher ratio. They show that in at least 74 out of the 172 cases, or in 43 per cent. of them, it is so; indeed, the ratio may be much larger, as I hardly know what to say about 51 cases, owing to insufficient description. I am inclined to believe that habits of thought render the mental field of view more *comprehensive* in the man than in the boy, though at the same time it causes the images contained in it to become fainter.

A few of the boys' answers are much to the point. I append some of them:—

The part I look at is much smaller than reality, with a haze of black all round it. It is like a small picture.

I have to fix my eyes on one spot in my imagination, and that alone is fairly defined.

I cannot see anything unless I look specially at it, which is not the case with my real eyes.

I have to move my mental eyes a good deal about. The objects are not defined at the same time, but I think of them one at a time; also, if I am thinking of anything, as a map for instance, I can only imagine one name at a time.

The next question that I put referred to the apparent position of the image. It was as follows:—

"*Distance of images.*—Where do mental images appear to be situated? within the head, within the eye-ball, just in front of the eyes, or at a distance corresponding to reality? Can you project an image upon a piece of paper?"

Unfortunately this question was not included among those that I first issued, and I have not a sufficient number of answers to it from adult males to justify a statistical dependence on them even on that ground alone. It is better in this case to rely on the Charterhouse boys, of whom only twelve failed to answer the question. Reducing to percentages, I find:—

Position of Mental Images.

	Per Cent.
Further than the real scene	9
Corresponding to reality	39
Just in front of the eyes	22
In eye-ball	6
In head	15
Partly at one distance, partly at another	9
	100

The more closely the image resembles in its vividness the result of actual vision the more nearly should we expect its distance to appear to coincide with that of the real object, and this as a matter of fact I find to be the case. The meaning of the word *reflection* is bending backwards, and those who reflect have the sense of a turning back from without to within the head. When a mental scene arises vividly and without any effort, the position of the vision is more frequently external, as it is in an hallucination.

I will next give the results of the latter part of the question, about the ability to project images on paper.

For the same reason as in the last case the returns from the adult males are insufficient. I have five clear cases only among them of an affirmative answer, out of which I will quote the following:—

Ability to Project an Image.

Holding a blank piece of paper in my hand, I can imagine on it a photograph or any object that it will hold.

The Charterhouse boys in at least 18 cases, or in ten per cent. of them, appear to have this power. The following are a few of their answers:—

I can think things to be upon a blank piece of paper.

I can place a mental image wherever I like, outside the head, either in the air or upon any substance.

After looking at a blank wall for some time, I can imagine what I am thinking of.

I can half project an image upon paper, but could not draw round it, it being too indistinct. I see the effect, but not the details of it.

I find it very hard to project an image on a piece of paper, but if I think for some time and look very hard at the paper, I sometimes can.

I can project an image on to anything, but the longer I keep it the fainter it gets, and I don't think I could keep it long enough to trace it.

I find indirectly from the answers to other questions that visual representations are by no means invariably of the same apparent size as the real objects. The change is usually on the side of reduction, not of enlargement. Among the Charterhouse boys there are thirteen of the one to two cases of the other, and I think, but I have not yet properly worked it out, that the returns from adults generally, male and female, show somewhat similar results. The following are extracts from the reports of the boys:—

Images Larger than Reality.

The place and objects in a mental picture seem to be larger altogether than the reality; thus a room seems loftier and broader, and the objects in it taller.

They look larger than the objects [? such objects as may be handled] really are, and seem much further off, . . they look about five yards off.

Images Smaller than Reality.

Very small and close.

Much smaller and very far off.

All the objects are clearly defined, but the image appears much smaller.

The difference that I see is, that everything I call up in my mind seems to be a long way off.

The difference is that it is much smaller.

Space does not admit, neither is this the most suitable opportunity of analysing more of the numerous data which I have in hand, but before concluding I would say a few words on the "Visualised Numerals" which I described first in *Nature*, Jan. 15, 1880, but very much more fully and advisedly in a memoir read before the Anthropological Institute in March, 1880, which will be published in its *Transactions* a few weeks later than the present memoir. It will contain not only my own memoir and numerous illustrations, but the remarks made on it at the meeting by gentlemen who had this curious habit of invariably associating numbers with definite forms of mental imagery. It is a habit that is quite automatic, the form is frequently very vivid and sometimes very elaborate and highly coloured, and its origin is always earlier than those who see it can recollect. Those who visualise numerals in number-forms are apt to see the letters of the alphabet, the months of the year, dates, &c., also in forms; but whereas they nearly always can suggest some clue to the origin of the latter, they never can, or hardly ever can, to that of the numerals. I have argued in the memoir just mentioned, partly from this fact and partly because some of the number-forms twist and plunge and run out of sight in the strangest ways, unlike anything the child has ever seen, that these are his natural, self-developed lines of mnemonic thought, and are survivals of the earliest of his mental processes, and a clue to much that is individual in the constitution of his mind. I found that only about one in thirty adult males saw these forms, but suspected that they were more common in early life, and subsequently lost by disuse. This idea is abundantly confirmed by the returns of the Charterhouse boys. Nearly one in four has the habit of referring numbers to some visual mental form or other; often it is only a straight line, sometimes more elaborate. No doubt as the years go by, most of these will be wholly forgotten as useless and even cumbrous, but the rest will serve some useful turn in arithmetic and become fixed by long habit, and will gradually and insensibly develop themselves. For want of space I must here close my statement of facts; and, my data being thus imperfectly before the reader, it would be premature in me to generalise. I trust, however, that what has been adduced is enough to give a fair knowledge of the variability of the visualising faculty in the English male sex, and I hope that the examples of the use of my "Statistics by Intercomparison" will convince psychologists that the relative development of various mental qualities in different races admits of being pretty accurately defined.

FRANCIS GALTON.

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STATISTICS OF MENTAL IMAGERY,

BY

FRANCIS GALTON, F.R.S.

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STATISTICS OF MENTAL IMAGERY.

An outline is given in the following memoir of some of the earlier results of an inquiry which I am still prosecuting, and a comparatively new statistical process will be used in it for the first time in dealing with psychological data. It is that which I described under the title of "Statistics by Intercomparison" in the *Philosophical Magazine* of Jany., 1875.

The larger object of my inquiry is to elicit facts that shall define the natural varieties of mental disposition in the two sexes and in different races, and afford trustworthy data as to the relative frequency with which different faculties are inherited in different degrees. The particular branch of the inquiry to which this memoir refers, is Mental Imagery; that is to say, I desire to define the different degrees of vividness with which different persons have the faculty of recalling familiar scenes under the form of mental pictures, and the peculiarities of the mental visions of different persons. The first questions that I put referred to the illumination, definition and colouring of the mental image, and they were framed as follows (I quote from my second and revised schedule of questions):—

"Before addressing yourself to any of the Questions on the opposite page, think of some definite object—suppose it is your breakfast-table as you sat down to it this morning—and consider carefully the picture that rises before your mind's eye.

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1. *Illumination.*—Is the image dim or fairly clear? Is its brightness comparable to that of the actual scene?

2. *Definition.*—Are all the objects pretty well defined at the same time, or is the place of sharpest definition at any one moment more contracted than it is in a real scene?

3. *Colouring.*—Are the colours of the china, of the toast, bread-crust, mustard, meat, parsley, or whatever may have been on the table, quite distinct and natural?"

There were many other questions besides these, of which I defer mention for the moment.

The first results of my inquiry amazed me. I had begun by questioning friends in the scientific world, as they were the most likely class of men to give accurate answers concerning this faculty of visualising, to which novelists and poets continually allude, which has left an abiding mark on the vocabularies of every language, and which supplies the material out of which dreams and the well-known hallucinations of sick people are built up.

To my astonishment, I found that the great majority of the men of science to whom I first applied, protested that mental imagery was unknown to them, and they looked on me as fanciful and fantastic in supposing that the words 'mental imagery' really expressed what I believed everybody supposed them to mean. They had no more notion of its true nature than a colour-blind man who has not discerned his defect has of the nature of colour. They had a mental deficiency of which they were unaware, and naturally enough supposed that those who were normally endowed, were romancing. To illustrate their mental attitude it will be sufficient to quote a few lines from the letter of one of my correspondents, who writes:—

"These questions presuppose assent to some sort of a proposition regarding the 'mind's eye' and the 'images' which it sees. . . . . This points to some initial fallacy. . . . . . It is only by a figure of speech that I can describe my recollection of a scene as a 'mental image' which I can 'see' with my 'mind's eye'. . . . . I do not see it . . . any more than a man sees the thousand lines of Sophocles which under due pressure he is ready to repeat. The memory possesses it, &c."

Much the same result followed some inquiries made for me by a friend among members of the French Institute.

On the other hand, when I spoke to persons whom I met in general society, I found an entirely different disposition to prevail. Many men and a yet larger number of women, and many boys and girls, declared that they habitually saw mental imagery, and that it was perfectly distinct to them and full of colour.

The more I pressed and cross-questioned them, professing myself to be incredulous, the more obvious was the truth of their first assertions. They described their imagery in minute detail, and they spoke in a tone of surprise at my apparent hesitation in accepting what they said. I felt that I myself should have spoken exactly as they did if I had been describing a scene that lay before my eyes, in broad daylight, to a blind man who persisted in doubting the reality of vision. Reassured by this, I recommenced to inquire among scientific men, and soon found scattered instances of what I sought, though in by no means the same abundance as elsewhere. I then circulated my questions more generally among my friends, and so obtained the replies that are the main subject of this memoir. The replies were from persons of both sexes and of various ages, but I shall confine my remarks in this necessarily brief memoir to the experiences derived from the male sex alone.

I have also received batches of answers from various educational establishments, and shall here make use of those sent by the Science Master of the Charterhouse, Mr. W. H. Poole, which he obtained from all the boys who attended his classes, after fully explaining the meaning of the questions, and interesting the boys in them. They have the merit of returns derived from a general census, which my other data lack, because I cannot for a moment suppose that the writers of them are a haphazard proportion of those to whom they were sent. Indeed, I know some men who, disavowing all possession of the power, cared to send no returns at all, and many more who possessed it in too faint a degree to enable them to express what their experiences really were, in a manner satisfactory to themselves. Considerable similarity in the general style of the replies will however be observed between the two sets of returns, and I may add that they accord in this respect with the oral information I have elsewhere obtained. The conformity of replies from so many different sources, the fact of their apparent trustworthiness being on the whole much increased by cross-examination (though I could give one or two amusing instances of break-down), and the evident effort made to give accurate answers, have convinced me that it is a much easier matter than I had anticipated to obtain trustworthy replies to psychological questions. Many persons, especially women and intelligent children, take pleasure in introspection, and strive their very best to explain their mental processes. I think that a delight in self-dissection must be a strong ingredient in the pleasure that many are said to take in confessing themselves to priests.

Here then are two rather notable results: the one is the proved facility of obtaining statistical insight into the processes of other persons' minds; and the other is that scientific men as a class have feeble powers of visual representation. There is no doubt whatever on the latter point, however it may be accounted for. My own conclusion is, that an over-readiness to perceive clear mental pictures is antagonistic to the acquirement of habits of highly generalised and abstract thought, and that if the faculty of producing them was ever possessed by men who think hard, it is very apt to be lost by disuse. The highest minds are probably those in which it is not lost, but subordinated, and is ready for use on suitable occasions. I am however bound to say, that the missing faculty seems to be replaced so serviceably by other modes of conception, chiefly I believe connected with the motor sense, that men who declare themselves entirely deficient in the power of seeing mental pictures can nevertheless give life-like descriptions of what they have seen, and can otherwise express themselves as if they were gifted with a vivid visual imagination. They can also become painters of the rank of Royal Academicians.

The facts I am now about to relate, are obtained from the returns of 100 adult men, of whom 19 are Fellows of the Royal Society, mostly of very high repute, and at least twice, and I think I may say three times, as many more are persons of distinction in various kinds of intellectual work. As already remarked, these returns taken by themselves, do not profess to be of service in a *general* statistical sense, but they are of much importance in showing how men of exceptional accuracy express themselves when they are speaking of mental imagery. They also testify to the variety of experiences to be met with in a moderately large circle. I will begin by giving a few cases of the highest, of the medium, and of the lowest order of the faculty of visualising. The hundred returns were first classified according to the order of the faculty, as judged from the whole of what was said in them, and all I knew from other sources of the writers; and the number prefixed to each quotation shows its place in the class-list.

Vividness of Mental Imagery.

(From returns furnished by 100 men, at least half of whom are distinguished in science or in other fields of intellectual work.)

Cases where the faculty is very high.

1. Brilliant, distinct, never blotchy.

2. Quite comparable to the real object. I feel as though I was dazzled, *e.g.*, when recalling the sun to my mental vision.

3. In some instances quite as bright as an actual scene.

4. Brightness as in the actual scene.

5. Thinking of the breakfast table this morning, all the objects in my mental picture are as bright as the actual scene.

6. The image once seen is perfectly clear and bright.
7. Brightness at first quite comparable to actual scene.
8. The mental image appears to correspond in all respects with reality. I think it is as clear as the actual scene.
9. The brightness is perfectly comparable to that of the real scene.
10. I think the illumination of the imaginary image is nearly equal to that of the real one.
11. All clear and bright; all the objects seem to me well defined at the same time.
12. I can see my breakfast table or any equally familiar thing with my mind's eye, quite as well in all particulars as I can do if the reality is before me.

Cases where the faculty is mediocre.

46. Fairly clear and not incomparable in illumination with that of the real scene, especially when I first catch it. Apt to become fainter when more particularly attended to.
47. Fairly clear, not quite comparable to that of the actual scene. Some objects are more sharply defined than others, the more familiar objects coming more distinctly in my mind.
48. Fairly clear as a general image; details rather misty.
49. Fairly clear, but not equal to the scene. Defined, but not sharply; not all seen with equal clearness.
50. Fairly clear. Brightness probably at least one-half to two-thirds of original. [The writer is a physiologist.] Definition varies very much, one or two objects being much more distinct than the others, but the latter come out clearly if attention be paid to them.
51. Image of my breakfast table fairly clear, but not quite so bright as the reality. Altogether it is pretty well defined; the part where I sit and its surroundings are pretty well so.
52. Fairly clear, but brightness not comparable to that of the actual scene. The objects are sharply defined; some of them are salient, and others insignificant and dim, but by separate efforts I can take a visualised inventory of the whole table.
53. Details of breakfast table *when the scene is reflected on*, are fairly defined and complete, but I have had a familiarity of many years with my own breakfast table, and the above would not be the case with a table seen casually unless there were some striking peculiarity in it.
54. I can recall any single object or group of objects, but not the whole table at once. The things recalled are generally clearly defined. Our table is a long one; I can in my mind pass my eyes all down the table and see the different things distinctly, but not the whole table at once.

Cases where the faculty is at the lowest.

89. Dim and indistinct, yet I can give an account of this morning's breakfast table;—split herrings, broiled chickens, bacon, rolls, rather light coloured marmalade, faint green plates with stiff pink flowers, the girls' dresses, &c., &c. I can also tell where all the dishes were, and where the people sat (I was on a visit). But my imagination is seldom pictorial except between sleeping and waking, when I sometimes see rather vivid forms.
90. Dim and not comparable in brightness to the real scene. Badly defined with blotches of light; very incomplete.
91. Dim, poor definition; could not sketch from it. I have a difficulty in seeing two images together.
92. Usually very dim. I cannot speak of its brightness, but only of its faintness. Not well defined and very incomplete.

93. Dim, imperfect.

94. I am very rarely able to recall any object whatever with any sort of distinctness. Very occasionally an object or image will recall itself, but even then it is more like a generalised image than an individual image. I seem to be almost destitute of visualising power, as under control.

95. No power of visualising. Between sleeping and waking, in illness and in health, with eyes closed, some remarkable scenes have occasionally presented themselves, but I cannot recall them when awake with eyes open, and by daylight, or under any circumstances whatever when a copy could be made of them on paper. I have drawn both men and places many days or weeks after seeing them, but it was by an effort of memory acting on study at the time, and assisted by trial and error on the paper or canvas, whether in black, yellow or colour, afterwards.

96. It is only as a figure of speech that I can describe my recollection of a scene as a 'mental image' which I can 'see' with my 'mind's eye.' . . The memory possesses it, and the mind can at will roam over the whole, or study minutely any part.

97. No individual objects, only a general idea of a very uncertain kind.

98. No. My memory is not of the nature of a spontaneous vision, though I remember well where a word occurs in a page, how furniture looks in a room, &c. The ideas are not felt to be mental pictures, but rather the symbols of facts.

99. Extremely dim. The impressions are in all respects so dim, vague and transient, that I doubt whether they can reasonably be called images. They are incomparably less than those of dreams.

100. My powers are zero. To my consciousness there is almost no association of memory with objective visual impressions. I recollect the breakfast table, but do not see it.

These quotations clearly show the great variety of natural powers of visual representation. I will proceed to examine the subject more closely, and to compare the returns from the 100 men with those from the Charterhouse boys, on the principle of my "Statistics by Intercomparison," which I must first explain at sufficient length.

There are many who deny to statistics the title of a science, and say that it is a mere collection of facts. For my part I think that there is such a thing as a science of statistics, though its field is narrowed almost to a point. Its object is to *discover methods* of epitomising a great, even an infinite, amount of variation in a compact form. To fix the ideas, it is well to take as an example the heights of men, in which case the science of statistics enables us to specify, by means of a very few figures, the conditions of stature that characterise the whole of the adult male inhabitants, say of the British Isles. These figures will suffice to inform us that there are so many per cent. between such and such heights, and so many between such other heights, giving us material whence we can answer any such question as this:—Out of 1000 men how many are we likely to find between 5 feet and 6 feet in height? If the figures do not give the answer directly, we can find it by interpolation and easy calculation from them. So again, if we wish to compare the heights of

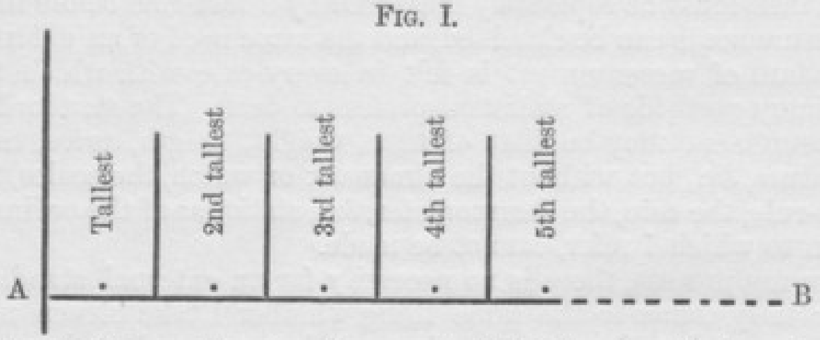
Englishmen and Frenchmen, statistics show how to obtain the average height of the two races, and the two averages may be readily compared, which goes a considerable way towards answering the question; or, if we wish it, we may compare very much more in detail, all the facts that are needed for the purpose being contained in the few figures of which I spoke.

But all these operations require the use of an *external standard.* The men must be separately measured by a foot-rule before their measurements can be classified, and the same need of an external standard of measurement is felt in every case with which the ordinary methods of statistics profess to deal. The standard of measurement may be that of time, weight, length, price, temperature, &c., but without the almanack or watch, the scales, the foot-rule, the coin, the thermometer, &c., statistics of the ordinary form to which I refer, cannot be made.

In my process, there is no necessity for an external standard. It clearly comes to the same thing whether I take eleven men and, measuring them one against another, range them in order, beginning with the highest and ending with the lowest, or if I measure them separately with a foot-measure, and range them in the order of the magnitude of the measurements recorded in my note-book. In each case the tallest man will stand first, the next tallest second, and so on to the last. In each case the same man will occupy the sixth or *middlemost* place, and will therefore represent the *medium* height of the whole of them. I do not wish to imply that 'medium' is identical with 'mean' or 'average,' for it is not necessarily so. But I do say that the word medium may be strictly defined, and therefore if we wish to compare the heights of Englishmen with Frenchmen, we shall proceed just as scientifically if we compare their medium heights as if we compare their average heights. Now it will be observed that we have got the medium heights without a foot-rule or any external standard; we have done so altogether by the method of intercomparison. In the particular question with which we are dealing I have classified the answers according to the degree of vividness of mental imagery to which they depose, and I pick out the middlemost answer and say that the description given in it describes the medium vividness of mental imagery in the group under discussion. If I want to compare two such groups I compare their respective middlemost answers, and judge which of the two implies the higher faculty.

Thus much is a great gain, yet I claim to effect more; but in order to explain what that is I must return to the illustration of heights of men. Suppose them as before to be all arranged in order of their stature, at equal distances apart on a long line A B, with their backs turned towards us. If there be a thousand men, we must suppose A B to be divided into 1000 equal parts, and a man to be set in the middle of each part. The tallest man will have A close to his left, and the shortest man will have B close to his right. They will form a series as shewn in Fig. I., where the subdivisions of A B are indicated by the vertical lines, and the positions where the men are standing are shown by the dots half-way between those lines.

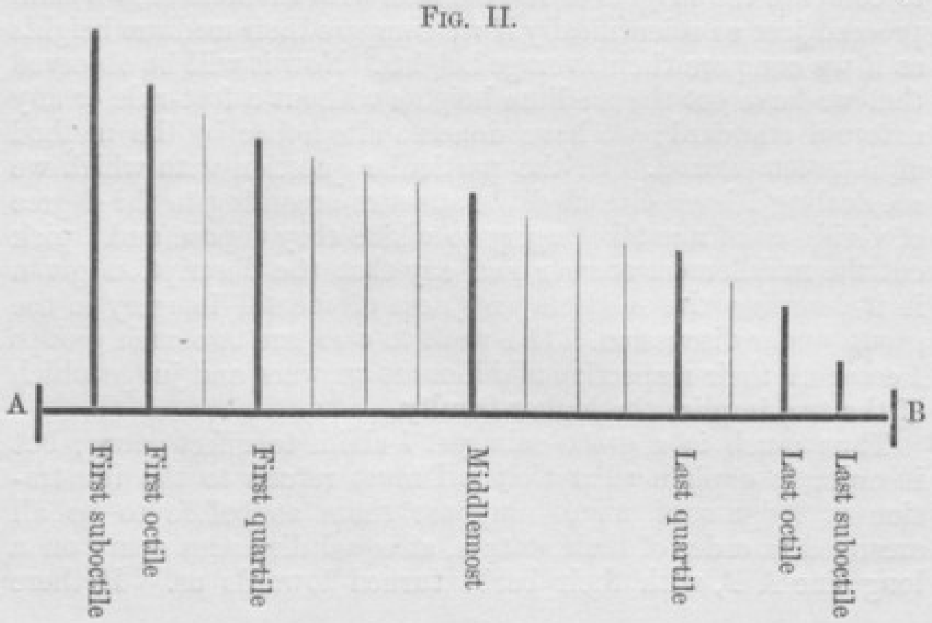
Fig. I.



Owing to the continuity of every statistical series, the imaginary line drawn along the tops of the heads of the men will form a regular curve, and if we can record this curve we shall be furnished with data whereby to ascertain the height of every man in the whole series. Drawing such a curve for Englishmen and another for Frenchmen, and superimposing the two, we should be able to compare the statures of the two nations in the minutest particulars.

A curve is recorded by measuring its ordinates. If we divide A B by a sufficient number of equi-distant subdivisions and measure the ordinates at each of them as has been done in Fig.

Fig. II.



II. (where the ordinates only are shewn, and not the curve), we can at any time plot them to scale, and by tracing a free line touching their tops, we can with more or less precision, reproduce the curve. It happens, however, from the peculiar character of all statistical curves, that ordinates at equal distances apart are by no means the most suitable. The mediocre cases are always so numerous that the curve flows in a steady and almost straight line about its middle, and it becomes a waste of effort to take many measurements thereabouts. On the other hand its shape varies rapidly at either end, and there the observations ought to be numerous. The most suitable stations are those which correspond to ordinates that differ in height by *equal degrees,* and these places admit of being discovered by *à priori* considerations on certain general suppositions.[1]

We shall however do well to ignore those minutiæ on which I laid much stress in the Memoir, and adopt the simpler plan of successive subdivisions of A B, and of measuring the ordinates shown by darkened lines in Fig. II., and severally named there as 'middlemost,' first and last 'quartile,' first and last 'octile,' and first and last 'suboctile'. This is far enough for our present wants, though the system admits of indefinite extension. By measuring the 'ordinate,' I mean measuring the 'man' whose place in the series is nearest to the true position of that ordinate. Absolute coincidence is not needed in such rude work as this; thus in a series of 100 men either the 50th or the 51st will do duty for the middlemost. The places I have actually taken in the series of 100 men for the several stations, are, the 6th and 94th for the first and last suboctiles, the 12th and 88th for the octiles, the 25th and 75th for the quartiles, and the 50th for the middlemost.

Seven men thus become the efficient representatives of a very large class. It will be found as a general rule that these seven selected representatives will differ each from the next by approximately *equal intervals,* the difference between the suboctile and the octile being usually about the same as that between the octile and quartile, and between the quartile and the middlemost.

[1] These are discussed in the Memoir already referred to, "Statistics by Intercomparison," by myself, in *Phil. Mag.*, Jan., 1875, but there are some errors, and also some appearances of error owing to faults of expression, in that article, which were first pointed out to me by Mr. J. W. L. Glaisher. There is a full mathematical discussion bearing on the matter in a memoir by Mr. D. McAlister in the *Proceedings of the Royal Society*, 1879, on the "Law of the Geometric Mean," to which and to the immediately preceding paper by myself on the "Geometric Mean," I would refer the mathematical reader. Mr. J. W. L. Glaisher has also taken the subject in hand and calculated tables, and I trust that his memoir thereon may before long be published.

As a matter of interest, and for the chance of finding very exceptional cases, I also record the highest and the lowest of the series, but it must be clearly understood that these have no solid value for purposes of comparison. In the first place, their position as ordinates is uncertain unless the number of the group of cases is given, for when the number is large the position of the highest and lowest will be nearer to A and B respectively than when it is small. In the second place, the highest and lowest being outside cases, they are more liable to be of an exceptional character than any of those which stand between neighbours, one on either hand of it.

The comparison of any two groups is made by collating their seven representatives each to each, the first suboctile of the one with the first suboctile of the other, the first octile with the first octile, the first quartile with the first quartile, and so on. I also collate the highest of each, and again the lowest of each, as a mere matter of interest, but not as an accurate statistical operation, for the reasons already given.

It is possible that I may be thought to have somewhat loosely expressed myself under the necessity of foregoing the use of technical terms, but the mathematical reader who demands precision of statement will understand me, while it would require a treatise and much study to make the mathematical substratum of my method perfectly intelligible to a person who was not familiar with the laws of 'Probabilities' and 'Frequency of Error'.

In the following comparison between the 100 Adult Englishmen and the 172 Charterhouse boys, I have divided the latter into two groups, to serve as a check upon one another. Group A includes boys of the four upper classes in the school, group B those of the five lower classes. I have combined their replies as to Illumination and Definition under the single head of 'Vividness,' and have taken no editorial liberties whatever except of the most pardonable description. It is wonderful how well and graphically the boys write, and how much individual character is shown in their answers.

Vividness of Imagery.

HIGHEST.

Adult Males.—Brilliant, distinct, never blotchy.

Charterhouse A.—The image is perfectly clear. I can see every feature in every one's face and everything on the table with great clearness. The light is quite as bright as reality.

Charterhouse B.—The image that arises in my mind is perfectly clear. The brightness is decidedly comparable to that of the real scene, for I can see in my mind's eye just as well as if I was beholding the scene with my real eye.

FIRST SUBOCTILE.

Adult Males.—The image once seen is perfectly clear and bright.

Charterhouse A.—It is very clear and is as bright as it actually was. Everything occurs most distinctly. I can imagine everything at once, but can think a great deal more clearly by thinking more on a particular object.

Charterhouse B.—I see it exactly as it was, all clearly defined just as it was.

FIRST OCTILE.

Adult Males.—I can see my breakfast table or any equally familiar thing with my mind's eye quite as well in all particulars as I can do if the reality is before me.

Charterhouse A.—To me the picture seems quite clear and the brightness equal to the real scene. I cannot see the whole scene at the same instant, but I see one thing at once and can turn my eye mentally to another object very quickly, so that I soon get the whole scene before my mind.

Charterhouse B.—Fairly clear. I cannot see everything at the same time, but what I do see seems almost real.

FIRST QUARTILE.

Adult Males.—Fairly clear; illumination of actual scene is fairly represented. Well defined. Parts do not obtrude themselves, but attention has to be directed to different points in succession to call up the whole.

Charterhouse A.—The image is fairly clear, but its brightness is dimmer than the actual. The objects are mostly defined clearly and at the same time.

Charterhouse B.—Fairly clear, the objects are pretty well defined at the same time.

MIDDLEMOST.

Adult Males.—Fairly clear. Brightness probably at least from one-half to two-thirds of the original. Definition varies very much, one or two objects being much more distinct than the others, but the latter come out clearly if attention be paid to them.

Charterhouse A.—The image is fairly clear, but its brightness is not comparable to that of the actual scene. The objects are pretty well defined at the same time.

Charterhouse B.—The image is pretty clear, but not so clear as the actual thing. I cannot take in the whole table at once, and I cannot see more than three plates at once, and when I try to see both ends of the table I cannot see anything of the middle. I can see nothing beyond the table, but the table itself seems to stand out from the distance beyond.

LAST QUARTILE.

Adult Males.—Dim, certainly not comparable to the actual scene. I have to think separately of the several things on the table to bring them clearly before the mind's eye, and when I think of some things the others fade away in confusion.

Charterhouse A.—The image is fairly clear. I cannot see everything at once, but as I think of them they come clearly before me. The objects are not all defined at the same time, and the place of sharpest definition is more contracted than in real scene.

Charterhouse B.—If I think of any particular thing without the others, it seems clear; all at once, are not clear.

LAST OCTILE.

Adult Males.—Dim and not comparable in brightness to the real scene.

Badly defined with blotches of light; very incomplete; very little of one object is seen at one time.

Charterhouse A.—I can call up to my mind the picture of the breakfast table in every detail, but seem to see everything through a darkened pane of glass. I see just the same number of people, plates, &c., the whole time, provided of course that I do not change my idea of the scene to any great degree.

Charterhouse B.—Rather dim; the objects are pretty well defined.

LAST SUBOCTILE.

Adult Males.—I am very rarely able to recall any object whatever with any sort of distinctness. Very occasionally an object or image will recall itself, but even then it is more like a generalised image than an individual one. I seem to be almost destitute of visualising power as under control.

Charterhouse A.—The image is dim, dark, and smaller than the actual scene, and the objects nearest to me show most distinctly. The whole picture is more or less of a dark green tint.

Charterhouse B.—Dim. The place of sharpest definition is more contracted than in a real scene.

LOWEST.

Adult Males.—My powers are zero. To my consciousness there is almost no association of memory with objective visual impressions. I recollect the table, but do not see it.

Charterhouse A.—Image dim, the brightness much less than in the real scene. Only one object is very clearly visible at the same time.

Charterhouse B.—Very dim. I can only see one part at a time.

I gather from the foregoing paragraphs that the A and B boys are alike in mental imagery, and that the adult males are not very dissimilar to them; but the latter do not seem to form so regular a series as the boys. They are avowedly not members of a true statistical group, being an aggregate of one class of persons who replied because they had remarkable powers of imagery and had much to say, of another class of persons, the scientific, who on the whole are very deficient in that gift, and of a third class who may justly be considered as fair samples of adult males.

I next proceed to colour, and annex the returns to the third of the above questions, which I have classified on the same principle as before.

Colour Representation.

HIGHEST.

Adult Males.—Perfectly distinct, bright, and natural.

Charterhouse A.—Yes, *perfectly* distinct and natural.

Charterhouse B.—The colours look more clear than they really are.

FIRST SUBOCTILE.

Adult Males.—White cloth, blue china, argand coffee pot, buff stand with sienna drawing, toast,—all clear.

Charterhouse A.—I see the colours just as if they were before me, and perfectly natural.

Charterhouse B.—The colours are especially distinct in every case.

FIRST OCTILE.

Adult Males.—All details seen perfectly.

Charterhouse A.—Quite distinct and natural.

Charterhouse B.—All colours are perfectly distinct to me in my mind's eye, in whatever scene or shape they appear to me.

FIRST QUARTILE.

Adult Males.—Colours distinct and natural till I begin to puzzle over them.

Charterhouse A.—Quite distinct and natural.

Charterhouse B.—The colours of the china, &c., are quite distinct and natural.

MIDDLEMOST.

Adult Males.—Fairly distinct, though not certain that they are accurately recalled.

Charterhouse A.—They are all distinct after a little thought, and are natural.

Charterhouse B.—Yes, quite distinct and natural.

LAST QUARTILE.

Adult Males.—Natural, but very indistinct.

Charterhouse A.—The colours of the most pronounced things on the table are distinct, as the white tablecloth and yellow mustard.

Charterhouse B.—Some are; china, mustard, toast,—the others are not.

LAST OCTILE.

Adult Males.—Faint, can only recall colours by a special effort for each.

Charterhouse A.—Colours not very distinct.

Charterhouse B.—They are natural, but not very distinct.

LAST SUBOCTILE.

Adult Males.—(Power is nil.)

Charterhouse A.—The colours are very dim.

Charterhouse B.—The colours seem to be more like shades, but they have some colour in them.

LOWEST.

Adult Males.—(Power is nil.)

Charterhouse A.—(Power is nil.)

Charterhouse B.—(Power is nil.)

The same general remarks may be made about the distribution of the faculty of colour representation as about that of the vividness of imagery. It seems that on the whole, colour is more easily recalled than form, and especially so by the young. As the faculty of visual representation is being dropped by disuse, colour disappears earlier than form. This I may remark, was the case with the often quoted hallucinations of Nicolai, which, in his progress to recovery, faded in colour before they faded in outline.

One of my correspondents, an eminent engineer, who has a highly developed power of recalling form, but who described himself as deficient in the power of recalling colour, tells me that since receiving and answering my questions he has practised himself in visualising colours and has succeeded perfectly in doing so. It now gives him great pleasure to recall them.

It will be of interest to extract the few instances from the returns of the Adult Males in which peculiarities were noticed in connexion with colour representation, other than in its degree of vividness. Each sentence is taken from a different return.

Light colours quite distinct, darker ones less so.
Patchy.
Generally hueless, unless excited.
Mostly neutral.
Brown colour, *e.g.* of the gravy, is difficult to visualise.

Another question that I put was as follows:—

"*Extent of field of view.*—Call up the image of some panoramic view (the walls of your room might suffice); can you force yourself to see mentally a wider range of it than could be taken in by any single glance of the eyes? Can you mentally see more than three faces of a die, or more than one hemisphere of a globe at the same instant of time?"

It would have been possible to classify the Charterhouse returns, but the answers were not so generally good as to make it advisable to spend pains upon them. I therefore content myself with the replies of the Adult Males, but shall subsequently add a few facts taken from those of the boys, in a separate paragraph.

Extent of Field of Mental View.

Highest.—My mental field of vision is larger than the normal one. In the former I appear to see everything from some commanding point of view, which at once embraces every object and all sides of every object.

First Suboctile.—A wider range. A faint perception *I think* of more than three sides of a room. Rather more *I think* than one hemisphere, but am not quite sure about this.

First Octile.—Field of view corresponding to reality.

First Quartile.—Field of view corresponding to reality.

Middlemost.—Field of view corresponding to reality.

Last Quartile.—I think the field of view is distinctly smaller than the reality. The object I picture starts out distinct with a hazy outline.

Last Octile.—Much smaller than the real. I seem only to see what is straight in front as it were.

Last Suboctile. } No field of view at all.
Lowest. }

It may seem strange to some that the field of mental vision should occasionally be wider than reality, but I have sufficient testimony to the fact from correspondents of unquestionable accuracy. Here are cases from the returns:—

I seem to see the whole room as though my eye was everywhere. I can see all around objects that I have handled.

I can see three walls of a room easily, and with an effort the fourth. I can see all the faces of a die and the whole globe, but die and globe seem transparent.

[An eminent mineralogist told me that familiarity with crystals gave him the power of mentally seeing all their facets simultaneously.]

This subject is of interest to myself on account of a weird nightmare by which I am occasionally plagued. In my dream, a small ball appears inside my eye. I speak in the singular, because the two eyes then seem fused into a single organ of vision, and I see by a kind of touch-sight all round the ball at once. Then the ball grows, and still my vision embraces the whole of it; it continues growing to an enormous size, and at the instant when the brain is ready to burst, I awake in a fright. Now, what I see in an occasional nightmare, others may be able to represent to themselves when awake and in health.

From the foregoing statistical record it will be seen that in one quarter of the cases, that is to say, in the last quartile and in all below, the field of mental view is decidedly contracted. The Charterhouse returns (A and B combined) give a higher ratio. They show that in at least 74 out of the 172 cases, or in 43 per cent. of them, it is so; indeed, the ratio may be much larger, as I hardly know what to say about 51 cases, owing to insufficient description. I am inclined to believe that habits of thought render the mental field of view more *comprehensive* in the man than in the boy, though at the same time it causes the images contained in it to become fainter.

A few of the boys' answers are much to the point. I append some of them:—

The part I look at is much smaller than reality, with a haze of black all round it. It is like a small picture.

I have to fix my eyes on one spot in my imagination, and that alone is fairly defined.

I cannot see anything unless I look specially at it, which is not the case with my real eyes.

I have to move my mental eyes a good deal about. The objects are not defined at the same time, but I think of them one at a time; also, if I am thinking of anything, as a map for instance, I can only imagine one name at a time.

The next question that I put referred to the apparent position of the image. It was as follows:—

"*Distance of images.*—Where do mental images appear to be situated? within the head, within the eye-ball, just in front of the eyes, or at a distance corresponding to reality? Can you project an image upon a piece of paper?"

Unfortunately this question was not included among those that I first issued, and I have not a sufficient number of answers to it from adult males to justify a statistical dependence on them even on that ground alone. It is better in this case to rely on the Charterhouse boys, of whom only twelve failed to answer the question. Reducing to percentages, I find:—

POSITION OF MENTAL IMAGES.

	Per Cent.
Further than the real scene	9
Corresponding to reality	39
Just in front of the eyes	22
In eye-ball	6
In head	15
Partly at one distance, partly at another	9
	100

The more closely the image resembles in its vividness the result of actual vision the more nearly should we expect its distance to appear to coincide with that of the real object, and this as a matter of fact I find to be the case. The meaning of the word *reflection* is bending backwards, and those who reflect have the sense of a turning back from without to within the head. When a mental scene arises vividly and without any effort, the position of the vision is more frequently external, as it is in an hallucination.

I will next give the results of the latter part of the question, about the ability to project images on paper.

For the same reason as in the last case the returns from the adult males are insufficient. I have five clear cases only among them of an affirmative answer, out of which I will quote the following:—

ABILITY TO PROJECT AN IMAGE.

Holding a blank piece of paper in my hand, I can imagine on it a photograph or any object that it will hold.

The Charterhouse boys in at least 18 cases, or in ten per cent. of them, appear to have this power. The following are a few of their answers:—

I can think things to be upon a blank piece of paper.

I can place a mental image wherever I like, outside the head, either in the air or upon any substance.

After looking at a blank wall for some time, I can imagine what I am thinking of.

I can half project an image upon paper, but could not draw round it, it being too indistinct. I see the effect, but not the details of it.

I find it very hard to project an image on a piece of paper, but if I think for some time and look very hard at the paper, I sometimes can.

I can project an image on to anything, but the longer I keep it the fainter it gets, and I don't think I could keep it long enough to trace it.

I find indirectly from the answers to other questions that visual representations are by no means invariably of the same apparent size as the real objects. The change is usually on the side of reduction, not of enlargement. Among the Charterhouse boys there are thirteen of the one to two cases of the other, and I think, but I have not yet properly worked it out, that the returns from adults generally, male and female, show somewhat similar results. The following are extracts from the reports of the boys:—

Images Larger than Reality.

The place and objects in a mental picture seem to be larger altogether than the reality; thus a room seems loftier and broader, and the objects in it taller.

They look larger than the objects [? such objects as may be handled] really are, and seem much further off, . . they look about five yards off.

Images Smaller than Reality.

Very small and close.

Much smaller and very far off.

All the objects are clearly defined, but the image appears much smaller.

The difference that I see is, that everything I call up in my mind seems to be a long way off.

The difference is that it is much smaller.

Space does not admit, neither is this the most suitable opportunity of analysing more of the numerous data which I have in hand, but before concluding I would say a few words on the "Visualised Numerals" which I described first in *Nature*, Jan. 15, 1880, but very much more fully and advisedly in a memoir read before the Anthropological Institute in March, 1880, which will be published in its *Transactions* a few weeks later than the present memoir. It will contain not only my own memoir and numerous illustrations, but the remarks made on it at the meeting by gentlemen who had this curious habit of invariably associating numbers with definite forms of mental imagery. It is a habit that is quite automatic, the form is frequently very vivid and sometimes very elaborate and highly coloured, and its origin is always earlier than those who see it can recollect. Those who visualise numerals in number-forms are apt to see the letters of the alphabet, the months of the year, dates, &c., also in forms; but whereas they nearly always can suggest some clue to the origin of the latter, they never can, or hardly ever can, to that of the numerals. I have argued in the memoir just mentioned, partly from this fact and partly because some of the number-forms twist and plunge and run out of sight in the strangest ways, unlike anything the child has ever seen, that these are his natural, self-developed lines of mnemonic thought, and are survivals of the earliest of his mental processes, and a clue to much that is individual in the constitution of his mind. I found that only about one in thirty adult males saw these forms, but suspected that they were more common in early life, and subsequently lost by disuse. This idea is abundantly confirmed by the returns of the Charterhouse boys. Nearly one in four has the habit of referring numbers to some visual mental form or other; often it is only a straight line, sometimes more elaborate. No doubt as the years go by, most of these will be wholly forgotten as useless and even cumbrous, but the rest will serve some useful turn in arithmetic and become fixed by long habit, and will gradually and insensibly develop themselves. For want of space I must here close my statement of facts; and, my data being thus imperfectly before the reader, it would be premature in me to generalise. I trust, however, that what has been adduced is enough to give a fair knowledge of the variability of the visualising faculty in the English male sex, and I hope that the examples of the use of my "Statistics by Intercomparison" will convince psychologists that the relative development of various mental qualities in different races admits of being pretty accurately defined.

FRANCIS GALTON.

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ABERDEEN, 1885

ADDRESS

TO THE

SECTION OF ANTHROPOLOGY

OF THE

BRITISH ASSOCIATION

BY

FRANCIS GALTON, F.R.S., &c.

PRESIDENT OF THE ANTHROPOLOGICAL INSTITUTE

PRESIDENT OF THE SECTION

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ADDRESS

TO THE

SECTION OF ANTHROPOLOGY

OF THE

BRITISH ASSOCIATION.

BY

FRANCIS GALTON, F.R.S., &c., President of the Anthropological Institute,

PRESIDENT OF THE SECTION.

The object of the Anthropologist is plain. He seeks to learn what mankind really are in body and mind, how they came to be what they are, and whither their races are tending; but the methods by which this definite inquiry has to be pursued are extremely diverse. Those of the geologist, the antiquarian, the jurist, the historian, the philologist, the traveller, the artist, and the statistician are all employed, and the Science of Man progresses through the help of specialists. Under these circumstances, I think it best to follow an example occasionally set by presidents of sections, by giving a lecture rather than an address, selecting for my subject one that has long been my favourite pursuit, on which I have been working with fresh data during many recent months, and about which I have something new to say.

My data were the Family Records entrusted to me by persons living in all parts of the country, and I am now glad to think that the publication of some first-fruits of their analysis will show to many careful and intelligent correspondents that their painstaking has not been thrown away. I shall refer to only a part of the work already completed, which in due time will be published, and must be satisfied if, when I have finished this address, some few ideas that lie at the root of heredity shall have been clearly apprehended, and their wide bearings more or less distinctly perceived. I am the more desirous of speaking on heredity, because, judging from private conversations and inquiries that are often put to me, the popular views of what may be expected from inheritance seem neither clear nor just.

The subject of my remarks will be Types and their Inheritance. I shall discuss the conditions of the stability and instability of types, and hope in doing so to place beyond doubt the existence of a simple and far-reaching law that governs hereditary transmission, and to which I once before ventured to draw attention, on far more slender evidence than I now possess.

It is some years since I made an extensive series of experiments on the produce of seeds of different size but of the same species. They yielded results that seemed very noteworthy, and I used them as the basis of a lecture before the Royal Institution on February 9, 1877. It appeared from these experiments that the offspring did *not* tend to resemble their parent seeds in size, but to be always more mediocre than they—to be smaller than the parents, if the parents were large;

to be larger than the parents, if the parents were very small. The point of convergence was considerably below the average size of the seeds contained in the large bagful I bought at a nursery-garden, out of which I selected those that were sown.

The experiments showed further that the mean filial regression towards mediocrity was directly proportional to the parental deviation from it. This curious result was based on so many plantings, conducted for me by friends living in various parts of the country, from Nairn in the north to Cornwall in the south, during one, two, or even three generations of the plants, that I could entertain no doubt of the truth of my conclusions. The exact ratio of regression remained a little doubtful, owing to variable influences; therefore I did not attempt to define it. After the lecture had been published, it occurred to me that the grounds of my misgivings might be urged as objections to the general conclusions. I did not think them of moment, but as the inquiry had been surrounded with many small difficulties and matters of detail, it would be scarcely possible to give a brief and yet a full and adequate answer to such objections. Also, I was then blind to what I now perceive to be the simple explanation of the phenomenon, so I thought it better to say no more upon the subject until I should obtain independent evidence. It was anthropological evidence that I desired, caring only for the seeds as means of throwing light on heredity in man. I tried in vain for a long and weary time to obtain it in sufficient abundance, and my failure was a cogent motive, together with others, in inducing me to make an offer of prizes for family records, which was largely responded to, and furnished me last year with what I wanted. I especially guarded myself against making any allusion to this particular inquiry in my prospectus, lest a bias should be given to the returns. I now can securely contemplate the possibility of the records of height having been frequently drawn up in a careless fashion, because no amount of unbiassed inaccuracy can account for the results, contrasted in their values but concurrent in their significance, that are derived from comparisons between different groups of the returns.

An analysis of the records fully confirms and goes far beyond the conclusions I obtained from the seeds. It gives the numerical value of the regression towards mediocrity as from 1 to $\frac{2}{3}$ with unexpected coherence and precision, and it supplies me with the class of facts I wanted to investigate—the degrees of family likeness in different degrees of kinship, and the steps through which special family peculiarities become merged into the typical characteristics of the race at large.

The subject of the inquiry on which I am about to speak was Hereditary Stature. My data consisted of the heights of 930 adult children and of their respective parentages, 205 in number. In every case I transmuted the female statures to their corresponding male equivalents and used them in their transmuted form, so that no objection grounded on the sexual difference of stature need be raised when I speak of averages. The factor I used was 1·08, which is equivalent to adding a little less than one-twelfth to each female height. It differs a very little from the factors employed by other anthropologists, who, moreover, differ a trifle between themselves; anyhow it suits my data better than 1·07 or 1·09. The final result is not of a kind to be affected by these minute details, for it happened that, owing to a mistaken direction, the computer to whom I first entrusted the figures used a somewhat different factor, yet the result came out closely the same.

I shall explain with fulness why I chose stature for the subject of inquiry, because the peculiarities and points to be attended to in the investigation will manifest themselves best by doing so. Many of its advantages are obvious enough, such as the ease and frequency with which its measurement is made, its practical constancy during thirty-five years of middle life, its small dependence on differences of bringing up, and its inconsiderable influence on the rate of mortality. Other advantages which are not equally obvious are no less great. One of these lies in the fact that stature is not a simple element, but a sum of the accumulated lengths or thicknesses of more than a hundred bodily parts, each so distinct from the rest as to have earned a name by which it can be specified. The list of them includes about fifty separate bones, situated in the skull, the spine, the pelvis, the two legs, and the two ankles and feet. The bones in both the lower limbs are counted, because it is the average length of these two limbs that contributes to the general stature. The cartilages interposed between the bones, two at each joint, are rather more numerous than the bones themselves. The fleshy parts of the scalp of the head and of the soles of the feet conclude the list. Account should also be taken of the shape and set of many of the bones which conduce to a more or less arched instep, straight back, or high head. I noticed in the skeleton of O'Brien, the Irish giant, at the College of Surgeons, which is, I believe, the tallest skeleton in any museum, that his extraordinary stature of about 7 feet 7 inches would have been a trifle increased if the faces of his dorsal vertebræ had been more parallel and his back consequently straighter.

The beautiful regularity in the statures of a population, whenever they are statistically marshalled in the order of their heights, is due to the number of variable elements of which the stature is the sum. The best illustrations I have seen of this regularity were the curves of male and female statures that I obtained from the careful measurements made at my Anthropometric Laboratory in the International Health Exhibition last year. They were almost perfect.

The multiplicity of elements, some derived from one progenitor, some from another, must be the cause of a fact that has proved very convenient in the course of my inquiry. It is that the stature of the children depends closely on the average stature of the two parents, and may be considered in practice as having nothing to do with their individual heights. The fact was proved as follows:—After transmuting the female measurements in the way already explained, I sorted the children of parents who severally differed 1, 2, 3, 4, and 5 or more inches into separate groups. Each group was then divided into similar classes, showing the number of cases in which the children differed 1, 2, 3, &c. inches from the common average of the children in their respective families. I confined my inquiry to large families of six children and upwards, that the common average of each might be a trustworthy point of reference. The entries in each of the different groups were then seen to run in the same way, except that in the last of them the children showed a faint tendency to fall into two sets, one taking after the tall parent, the other after the short one. Therefore, when dealing with the transmission of stature from parents to children, the average height of the two parents, or, as I prefer to call it, the 'mid-parental' height, is all we need care to know about them.

It must be noted that I use the word parent without specifying the sex. The methods of statistics permit us to employ this abstract term, because the cases of a tall father being married to a short mother are balanced by those of a short father being married to a tall mother. I use the word parent to save a complication due to a fact brought out by these inquiries, that the height of the children of both sexes, but especially that of the daughters, takes after the height of the father more than it does after that of the mother. My present data are insufficient to determine the ratio satisfactorily.

Another great merit of stature as a subject for inquiries into heredity is that marriage selection takes little or no account of shortness or tallness. There are undoubtedly sexual preferences for moderate contrast in height, but the marriage choice appears to be guided by so many and more important considerations that questions of stature exert no perceptible influence upon it. This is by no means my only inquiry into this subject, but, as regards the present data, my test lay in dividing the 205 male parents and the 205 female parents each into three groups—tall, medium, and short (medium being taken as 67 inches and upwards to 70 inches), and in counting the number of marriages in each possible combination between them. The result was that men and women of contrasted heights, short and tall or tall and short, married just about as frequently as men and women of similar heights, both tall or both short; there were 32 cases of the one to 27 of the other. In applying the law of probabilities to investigations into heredity of stature, we may regard the married folk as couples picked out of the general population at haphazard.

The advantages of stature as a subject in which the simple laws of heredity may be studied will now be understood. It is a nearly constant value that is frequently measured and recorded, and its discussion is little entangled with considerations of nurture, of the survival of the fittest, or of marriage selection. We have only to consider the mid-parentage and not to trouble ourselves about the parents separately. The statistical variations of stature are extremely regular, so much so that their general conformity with the results of calculations based on the abstract law of frequency of error is an accepted fact by anthropologists. I have made much use of the properties of that law in cross-testing my various conclusions, and always with success.

The only drawback to the use of stature is its small variability. One-half of the population with whom I dealt varied less than 1·7 inch from the average of all of them, and one-half of the offspring of similar mid-parentages varied less than 1·5 inch from the average of their own heights. On the other hand, the precision of my data is so small, partly due to the uncertainty in many cases whether the height was measured with the shoes on or off, that I find by means of an independent inquiry that each observation, taking one with another, is liable to an error that as often as not exceeds $\frac{2}{3}$ of an inch.

It must be clearly understood that my inquiry is primarily into the inheritance of different degrees of tallness and shortness. That is to say, of measurements made from the crown of the head to the level of mediocrity, upwards or downwards as the case may be, and not from the crown of the head to the ground. In the population with which I deal the level of mediocrity is $68\frac{1}{4}$ inches (without shoes). The same law applying with sufficient closeness both to tallness and shortness, we may include both under the single head of deviations, and I shall call any particular deviation a 'deviate.' By the use of this word and that of 'mid-parentage' we can define the law of regression very briefly. It is that the height-deviate of the offspring is, on the average, two-thirds of the height-deviate of its mid-parentage.

If this remarkable law had been based only on experiments on the diameters of the seeds, it might well be distrusted until confirmed by other inquiries. If it were corroborated merely by the observations on human stature, of which I am about to speak, some hesitation might be expected before its truth could be recognised in opposition to the current belief that the child tends to resemble its parents. But more can be urged than this. It is easily to be shown that we ought to expect filial regression, and that it should amount to some constant fractional part of the value of the mid-parental deviation. It is because this explanation confirms the previous observations made both on seeds and on men that I feel justified on the present occasion in drawing attention to this elementary law.

The explanation of it is as follows. The child inherits partly from his parents, partly from his ancestry. Speaking generally, the further his genealogy goes back, the more numerous and varied will his ancestry become, until they cease to differ from any equally numerous sample taken at haphazard from the race at large. Their mean stature will then be the same as that of the race; in other words, it will be mediocre. Or, to put the same fact into another form, the most probable value of the mid-ancestral deviates in any remote generation is zero.

For the moment let us confine our attention to the remote ancestry and to the mid-parentages, and ignore the intermediate generations. The combination of the zero of the ancestry with the deviate of the mid-parentage is that of nothing with something, and the result resembles that of pouring a uniform proportion of pure water into a vessel of wine. It dilutes the wine to a constant fraction of its original alcoholic strength, whatever that strength may have been.

The intermediate generations will each in their degree do the same. The mid-deviate of any one of them will have a value intermediate between that of the mid-parentage and the zero value of the ancestry. Its combination with the mid-parental deviate will be as if, not pure water, but a mixture of wine and water in some definite proportion had been poured into the wine. The process throughout is one of proportionate dilutions, and therefore the joint effect of all of them is to weaken the original wine in a constant ratio.

We have no word to express the form of that ideal and composite progenitor, whom the offspring of similar mid-parentages most nearly resemble, and from whose stature their own respective heights diverge evenly, above and below. He, she, or it, may be styled the 'generant' of the group. I shall shortly explain what my notion of a generant is, but for the moment it is sufficient to show that the parents are not identical with the generant of their own offspring.

The average regression of the offspring to a constant fraction of their respective mid-parental deviations, which was first observed in the diameters of seeds, and then confirmed by observations on human stature, is now shown to be a perfectly reasonable law which might have been deductively foreseen. It is of so simple a character that I have made an arrangement with one movable pulley and two fixed ones by which the probable average height of the children of known parents can be mechanically reckoned. This law tells heavily against the full hereditary transmission of any rare and valuable gift, as only a few of many children would resemble their mid-parentage. The more exceptional the gift, the more exceptional will be the good fortune of a parent who has a son who equals, and still more if he has a son who overpasses him. The law is even-handed; it levies the same heavy succession-tax on the transmission of badness as well as of goodness. If it discourages the extravagant expectations of gifted parents that their children will inherit all their powers, it no less discountenances extravagant fears that they will inherit all their weaknesses and diseases.

The converse of this law is very far from being its numerical opposite. Because the most probable deviate of the son is only two-thirds that of his mid-parentage, it does not in the least follow that the most probable deviate of the mid-parentage is $\frac{3}{2}$, or $1\frac{1}{2}$ that of the son. The number of individuals in a population who differ little from mediocrity is so preponderant that it is more frequently the case that an exceptional man is the somewhat exceptional son of rather mediocre parents, than the average son of very exceptional parents. It appears from the very same table of observations by which the value of the filial regression was determined, when it is read in a different way, namely, in vertical columns instead of in horizontal lines, that the most probable mid-parentage of a man is one that deviates only one-third as much as the man does. There is a great difference between this value of $\frac{1}{3}$ and the numerical converse mentioned above of $\frac{3}{2}$; it is four and a half times smaller, since $4\frac{1}{2}$, or $\frac{9}{2}$, being multiplied into $\frac{1}{3}$, is equal to $\frac{3}{2}$.

Let it not be supposed for a moment that these figures invalidate the general doctrine that the children of a gifted pair are much more likely to be gifted than the children of a mediocre pair. What it asserts is that the ablest child of one gifted pair is not likely to be as gifted as the ablest of all the children of very many mediocre pairs. However, as, notwithstanding this explanation, some suspicion may remain of a paradox lurking in these strongly contrasted results, I will explain the form in which the table of data was drawn up, and give an anecdote connected with it. Its outline was constructed by ruling a sheet into squares, and writing a series of heights in inches, such as 60 and under 61, 61 and under 62, &c., along its top, and another similar series down its side. The former referred to the height of offspring, the latter to that of mid-parentages. Each square in the table was formed by the intersection of a vertical column with a horizontal one, and in each square was inserted the number of children out of the 930 who were of the height indicated by the heading of the vertical column, and who at the same time were born of mid-parentages of the height indicated at the side of the horizontal column. I take an entry out of the table as an example. In the square where the vertical column headed[1] 69– is intersected by the horizontal column by whose side 67– is marked, the entry 38 is found; this means that out of the 930 children 38 were born of mid-parentages of 69 and under 70 inches who also were 67 and under 68 inches in height. I found it hard at first to catch the full significance of the entries in the table, which had curious relations that were very interesting to investigate. Lines drawn through entries of the same value formed a series of concentric and similar ellipses. Their common centre lay at the intersection of the vertical and horizontal lines, that corresponded to $68\frac{1}{4}$ inches. Their axes were similarly inclined. The points where each ellipse in succession was touched by a horizontal tangent, lay in a straight line inclined to the vertical in the ratio of $\frac{2}{3}$; those where they were touched by a vertical tangent,

[1] A matter of detail is here ignored which has nothing to do with the main principle, and would only serve to perplex if I described it.

lay in a straight line inclined to the horizontal in the ratio of $\frac{1}{3}$. These ratios confirm the values of average regression already obtained by a different method, of $\frac{2}{3}$ from mid-parent to offspring and of $\frac{1}{3}$ from offspring to mid-parent. These and other relations were evidently a subject for mathematical analysis and verification. They were all clearly dependent on three elementary data, supposing the law of frequency of error to be applicable throughout; these data being (1) the measure of racial variability, (2) that of co-family variability (counting the offspring of like mid-parentages as members of the same co-family), and (3) the average ratio of regression. I noted these values, and phrased the problem in abstract terms such as a competent mathematician could deal with, disentangled from all reference to heredity, and in that shape submitted it to Mr. J. Hamilton Dickson, of St. Peter's College, Cambridge. I asked him kindly to investigate for me the surface of frequency of error that would result from these three data, and the various particulars of its sections, one of which would form the ellipses to which I have alluded.

I may be permitted to say that I never felt such a glow of loyalty and respect towards the sovereignty and magnificent sway of mathematical analysis as when his answer reached me, confirming, by purely mathematical reasoning, my various and laborious statistical conclusions with far more minuteness than I had dared to hope, for the original data ran somewhat roughly, and I had to smooth them with tender caution. His calculation corrected my observed value of mid-parental regression from $\frac{1}{3}$ to $\frac{6}{17 \cdot 6}$, the relation between the major and minor axis of the ellipses was changed 3 per cent., their inclination was changed less than 2°. It is obvious, then, that the law of error holds throughout the investigation with sufficient precision to be of real service, and that the various results of my statistics are not casual determinations, but strictly interdependent.

In the lecture at the Royal Institution to which I have referred, I pointed out the remarkable way in which one generation was succeeded by another that proved to be its statistical counterpart. I there had to discuss the various agencies of the survival of the fittest, of relative fertility, and so forth; but the selection of human stature as the subject of investigation now enables me to get rid of all these complications and to discuss this very curious question under its simplest form. How is it, I ask, that in each successive generation there proves to be the same number of men per thousand, who range between any limits of stature we please to specify, although the tall men are rarely descended from equally tall parents, or the short men from equally short? How is the balance from other sources so nicely made up? The answer is that the process comprises two opposite sets of actions, one concentrative and the other dispersive, and of such a character that they necessarily neutralise one another, and fall into a state of stable equilibrium. By the first set, a system of scattered elements is replaced by another system which is less scattered; by the second set, each of these new elements becomes a centre whence a third system of elements are dispersed. The details are as follows:—In the first of these two stages, the units of the population group themselves, as it were by chance, into married couples, whence the mid-parentages are derived, and then by a regression of the values of the mid-parentages the true generants are derived. In the second stage each generant is a centre whence the offspring diverge. The stability of the balance between the opposed tendencies is due to the regression being proportionate to the deviation; it acts like a spring against a weight.

A simple equation connects the three data of race variability, of the ratio of regression, and of co-family variability, whence, if any two are given, the third may be found. My observations give separate measures of all three, and their values fit well into the equation, which is of the simple form—

$$v^2 \frac{p^2}{2} + f^2 = p^2,$$

where $v = \frac{2}{3}$, $p = 1 \cdot 7$, $f = 1 \cdot 5$.

It will therefore be understood that a complete table of mid-parental and filial heights may be calculated from two simple numbers.

It will be gathered from what has been said, that a mid-parental deviate of one unit implies a mid-grandparental deviate of $\frac{1}{3}$, a mid-ancestral unit in the next generation of $\frac{1}{9}$, and so on. I reckon from these and other data, by methods that I cannot stop to explain, that the heritage derived on an average from the mid-parental deviate, independently of what it may imply or of what may be known concerning the previous ancestry, is only $\frac{1}{2}$. Consequently, that similarly derived from a single parent is only $\frac{1}{4}$, and that from a single grandparent is only $\frac{1}{16}$.

The most elementary data upon which a complete table of mid-parental and filial heights admits of being constructed, are (1) the ratio between the mid-parental and the rest of the ancestral influences, and (2) the measure of the co-family variability.

I cannot now pursue the numerous branches that spring from the data I have given, as from a root. I will not speak of the continued domination of one type over others, nor of the persistency of unimportant characteristics, nor of the inheritance of disease, which is complicated in many cases by the requisite concurrence of two separate heritages, the one of a susceptible constitution, the other of the germs of the disease. Still less can I enter upon the subject of fraternal characteristics, which I have also worked out. It will suffice for the present to have shown some of the more important conditions associated with the idea of race, and how the vague word type may be defined by peculiarities in hereditary transmission, at all events when that word is applied to any single quality, such as stature. To include those numerous qualities that are not strictly measurable, we must omit reference to number and proportion, and frame the definition thus:—'The type is an ideal form towards which the children of those who deviate from it tend to regress.'

The stability of a type would, I presume, be measured by the strength of its tendency to regress; thus a mean regression from 1 in the mid-parents to $\frac{2}{3}$ in the offspring would indicate only half as much stability as if it had been to $\frac{1}{3}$.

The mean regression in stature of a population is easily ascertained, but I do not see much use in knowing it. It has already been stated that half the population vary less than 1·7 inch from mediocrity, this being what is technically known as the 'probable' deviation. The mean deviation is, by a well-known theory, 1·18 times that of the probable deviation, therefore in this case it is 1·9 inch. The mean loss through regression is $\frac{1}{3}$ of that amount, or a little more than 0·6 inch. That is to say, taking one child with another, the mean amount by which they fall short of their mid-parental peculiarity of stature is rather more than six-tenths of an inch.

With respect to these and the other numerical estimates, I wish emphatically to say that I offer them only as being serviceably approximate, though they are mutually consistent, and with the desire that they may be reinvestigated by the help of more abundant and much more accurate measurements than those I have had at command. There are many simple and interesting relations to which I am still unable to assign numerical values for lack of adequate material, such as that to which I referred some time back, of the superior influence of the father over the mother on the stature of their sons and daughters.

The limits of deviation beyond which there is no regression, but a new condition of equilibrium is entered into, and a new type comes into existence, have still to be explored. Let us consider how much we can infer from undisputed facts of heredity regarding the conditions amid which any form of stable equilibrium such as is implied by the word type must be established, or might be disestablished and superseded by another. In doing so I will follow cautiously along the same path by which Darwin started to construct his provisional theory of pangenesis; but it is not in the least necessary to go so far as that theory or to entangle ourselves in any questioned hypothesis.

There can be no doubt that heredity proceeds to a considerable extent, perhaps principally, in a piecemeal or piebald fashion, causing the person of the child to be to that extent a mosaic of independent ancestral heritages, one part coming with more or less variation from this progenitor, and another from that. To express this aspect of inheritance, where particle proceeds from particle, we may conveniently describe it as 'particulate.'

So far as the transmission of any feature may be regarded as an example of particulate inheritance, so far (it seems little more than a truism to assert) the element from which that feature was developed must have been particulate also. Therefore, wherever a feature in a child was not personally possessed by either parent, but transmitted through one of them from a more distant progenitor, the element whence that feature was developed must have existed in a particulate, though impersonal and latent form, in the body of the parent. The total heritage of that parent will have included a greater variety of material than was utilised in the formation of his own personal structure. Only a portion of it became developed; the survival of at least a small part of the remainder is proved, and that of a larger part may be inferred by his transmitting it to the person of his child. Therefore the organised structure of each individual should be viewed as the fulfilment of only one out of an indefinite number of mutually exclusive possibilities. It is the development of a single sample drawn out of a group of elements. The conditions under which each element in the sample became selected are, of course, unknown, but it is reasonable to expect they would fall under one or other of the following agencies: first, self-selection, where each element selects its most suitable neighbour, as in the theory of pangenesis; secondly, general co-ordination, or the influence exerted on each element by many or all of the remaining ones, whether in its immediate neighbourhood or not; finally, a group of diverse agencies, alike only in the fact that they are not uniformly helpful or harmful, that they influence with no constant purpose—in philosophical language, that they are not teleological; in popular language, that they are accidents or chances. Their inclusion renders it impossible to predict the peculiarities of individual children, though it does not prevent the prediction of average results. We now see something of the general character of the conditions amid which the stable equilibrium that characterises each race must subsist.

Political analogies of stability and change of type abound, and are useful to fix the ideas, as I pointed out some years ago. Let us take that which is afforded by the government of a colony which has become independent. The individual colonists rank as particulate representatives of families or other groups in the parent country. The organised colonial government ranks as the personality of the colony, being its mouthpiece and executive. The government is evolved amid political strife, one element prevailing here and another there. The prominent victors band themselves into the nucleus of a party, additions to their number and revisions of it ensue, until a body of men are associated capable of conducting a completely organised administration. The kinship between the form of government of the colony and that of the parent state is far from direct, and resembles in a general way that which I conceive to subsist between the child and his mid-parentage. We should expect to find many points of resemblance between the two, and many instances of great dissimilarity, for our political analogy teaches us only too well on what slight accidents the character of the government may depend when parties are nearly balanced.

The appearance of a new and useful family peculiarity is a boon to breeders, who by selection in mating gradually reduce the preponderance of those ancestral elements that endanger reversion. The appearance of a new type is due to causes that lie beyond our reach, so we ought to welcome every useful one as a happy chance, and do our best to domicile and perpetuate it. When heredity shall have become much better and more generally understood than now, I can believe that we shall look upon a neglect to conserve any valuable form of family type as a wrongful waste of opportunity. The appearance of each new natural peculiarity is a faltering step in the upward journey of evolution, over which, in outward appearance, the whole living world is blindly blundering and stumbling, but whose general direction man has the intelligence dimly to discern, and whose progress he has power to facilitate.

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THE object of the Anthropologist is plain. He seeks to learn what mankind really are in body and mind, how they came to be what they are, and whither their races are tending; but the methods by which this definite inquiry has to be pursued are extremely diverse. Those of the geologist, the antiquarian, the jurist, the historian, the philologist, the traveller, the artist, and the statistician are all employed, and the Science of Man progresses through the help of specialists. Under these circumstances, I think it best to follow an example occasionally set by presidénts of sections, by giving a lecture rather than an address, selecting for my subject one that has long been my favourite pursuit, on which I have been working with fresh data during many recent months, and about which I have something new to say.

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It is some years since I made an extensive series of experiments on the produce of seeds of different size but of the same species. They yielded results that seemed very noteworthy, and I used them as the basis of a lecture before the Royal Institution on February 9, 1877. It appeared from these experiments that the offspring did *not* tend to resemble their parent seeds in size, but to be always more mediocre than they—to be smaller than the parents, if the parents were large;

to be larger than the parents, if the parents were very small. The point of convergence was considerably below the average size of the seeds contained in the large bagful I bought at a nursery-garden, out of which I selected those that were sown.

The experiments showed further that the mean filial regression towards mediocrity was directly proportional to the parental deviation from it. This curious result was based on so many plantings, conducted for me by friends living in various parts of the country, from Nairn in the north to Cornwall in the south, during one, two, or even three generations of the plants, that I could entertain no doubt of the truth of my conclusions. The exact ratio of regression remained a little doubtful, owing to variable influences; therefore I did not attempt to define it. After the lecture had been published, it occurred to me that the grounds of my misgivings might be urged as objections to the general conclusions. I did not think them of moment, but as the inquiry had been surrounded with many small difficulties and matters of detail, it would be scarcely possible to give a brief and yet a full and adequate answer to such objections. Also, I was then blind to what I now perceive to be the simple explanation of the phenomenon, so I thought it better to say no more upon the subject until I should obtain independent evidence. It was anthropological evidence that I desired, caring only for the seeds as means of throwing light on heredity in man. I tried in vain for a long and weary time to obtain it in sufficient abundance, and my failure was a cogent motive, together with others, in inducing me to make an offer of prizes for family records, which was largely responded to, and furnished me last year with what I wanted. I especially guarded myself against making any allusion to this particular inquiry in my prospectus, lest a bias should be given to the returns. I now can securely contemplate the possibility of the records of height having been frequently drawn up in a careless fashion, because no amount of unbiassed inaccuracy can account for the results, contrasted in their values but concurrent in their significance, that are derived from comparisons between different groups of the returns.

An analysis of the records fully confirms and goes far beyond the conclusions I obtained from the seeds. It gives the numerical value of the regression towards mediocrity as from 1 to $\frac{2}{3}$ with unexpected coherence and precision, and it supplies me with the class of facts I wanted to investigate—the degrees of family likeness in different degrees of kinship, and the steps through which special family peculiarities become merged into the typical characteristics of the race at large.

The subject of the inquiry on which I am about to speak was Hereditary Stature. My data consisted of the heights of 930 adult children and of their respective parentages, 205 in number. In every case I transmuted the female statures to their corresponding male equivalents and used them in their transmuted form, so that no objection grounded on the sexual difference of stature need be raised when I speak of averages. The factor I used was 1·08, which is equivalent to adding a little less than one-twelfth to each female height. It differs a very little from the factors employed by other anthropologists, who, moreover, differ a trifle between themselves; anyhow it suits my data better than 1·07 or 1·09. The final result is not of a kind to be affected by these minute details, for it happened that, owing to a mistaken direction, the computer to whom I first entrusted the figures used a somewhat different factor, yet the result came out closely the same.

I shall explain with fulness why I chose stature for the subject of inquiry, because the peculiarities and points to be attended to in the investigation will manifest themselves best by doing so. Many of its advantages are obvious enough, such as the ease and frequency with which its measurement is made, its practical constancy during thirty-five years of middle life, its small dependence on differences of bringing up, and its inconsiderable influence on the rate of mortality. Other advantages which are not equally obvious are no less great. One of these lies in the fact that stature is not a simple element, but a sum of the accumulated lengths or thicknesses of more than a hundred bodily parts, each so distinct from the rest as to have earned a name by which it can be specified. The list of them includes about fifty separate bones, situated in the skull, the spine, the pelvis, the two legs, and the two ankles and feet. The bones in both the lower limbs are counted, because it is the average length of these two limbs that contributes to the general stature. The cartilages interposed between the bones, two at each joint, are rather more numerous than the bones themselves. The fleshy parts of the scalp of the head and of the soles of the feet conclude the list. Account should also be taken of the shape and set of many of the bones which conduce to a more or less arched instep, straight back, or high head. I noticed in the skeleton of O'Brien, the Irish giant, at the College of Surgeons, which is, I believe, the tallest skeleton in any museum, that his extraordinary stature of about 7 feet 7 inches would have been a trifle increased if the faces of his dorsal vertebræ had been more parallel and his back consequently straighter.

The beautiful regularity in the statures of a population, whenever they are statistically marshalled in the order of their heights, is due to the number of variable elements of which the stature is the sum. The best illustrations I have seen of this regularity were the curves of male and female statures that I obtained from the careful measurements made at my Anthropometric Laboratory in the International Health Exhibition last year. They were almost perfect.

The multiplicity of elements, some derived from one progenitor, some from another, must be the cause of a fact that has proved very convenient in the course of my inquiry. It is that the stature of the children depends closely on the average stature of the two parents, and may be considered in practice as having nothing to do with their individual heights. The fact was proved as follows:—After transmuting the female measurements in the way already explained, I sorted the children of parents who severally differed 1, 2, 3, 4, and 5 or more inches into separate groups. Each group was then divided into similar classes, showing the number of cases in which the children differed 1, 2, 3, &c. inches from the common average of the children in their respective families. I confined my inquiry to large families of six children and upwards, that the common average of each might be a trustworthy point of reference. The entries in each of the different groups were then seen to run in the same way, except that in the last of them the children showed a faint tendency to fall into two sets, one taking after the tall parent, the other after the short one. Therefore, when dealing with the transmission of stature from parents to children, the average height of the two parents, or, as I prefer to call it, the 'mid-parental' height, is all we need care to know about them.

It must be noted that I use the word parent without specifying the sex. The methods of statistics permit us to employ this abstract term, because the cases of a tall father being married to a short mother are balanced by those of a short father being married to a tall mother. I use the word parent to save a complication due to a fact brought out by these inquiries, that the height of the children of both sexes, but especially that of the daughters, takes after the height of the father more than it does after that of the mother. My present data are insufficient to determine the ratio satisfactorily.

Another great merit of stature as a subject for inquiries into heredity is that marriage selection takes little or no account of shortness or tallness. There are undoubtedly sexual preferences for moderate contrast in height, but the marriage choice appears to be guided by so many and more important considerations that questions of stature exert no perceptible influence upon it. This is by no means my only inquiry into this subject, but, as regards the present data, my test lay in dividing the 205 male parents and the 205 female parents each into three groups—tall, medium, and short (medium being taken as 67 inches and upwards to 70 inches), and in counting the number of marriages in each possible combination between them. The result was that men and women of contrasted heights, short and tall or tall and short, married just about as frequently as men and women of similar heights, both tall or both short; there were 32 cases of the one to 27 of the other. In applying the law of probabilities to investigations into heredity of stature, we may regard the married folk as couples picked out of the general population at haphazard.

The advantages of stature as a subject in which the simple laws of heredity may be studied will now be understood. It is a nearly constant value that is frequently measured and recorded, and its discussion is little entangled with considerations

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of nurture, of the survival of the fittest, or of marriage selection. We have only to consider the mid-parentage and not to trouble ourselves about the parents separately. The statistical variations of stature are extremely regular, so much so that their general conformity with the results of calculations based on the abstract law of frequency of error is an accepted fact by anthropologists. I have made much use of the properties of that law in cross-testing my various conclusions, and always with success.

The only drawback to the use of stature is its small variability. One-half of the population with whom I dealt varied less than 1·7 inch from the average of all of them, and one-half of the offspring of similar mid-parentages varied less than 1·5 inch from the average of their own heights. On the other hand, the precision of my data is so small, partly due to the uncertainty in many cases whether the height was measured with the shoes on or off, that I find by means of an independent inquiry that each observation, taking one with another, is liable to an error that as often as not exceeds $\frac{2}{3}$ of an inch.

It must be clearly understood that my inquiry is primarily into the inheritance of different degrees of tallness and shortness. That is to say, of measurements made from the crown of the head to the level of mediocrity, upwards or downwards as the case may be, and not from the crown of the head to the ground. In the population with which I deal the level of mediocrity is $68\frac{1}{4}$ inches (without shoes). The same law applying with sufficient closeness both to tallness and shortness, we may include both under the single head of deviations, and I shall call any particular deviation a 'deviate.' By the use of this word and that of 'mid-parentage' we can define the law of regression very briefly. It is that the height-deviate of the offspring is, on the average, two-thirds of the height-deviate of its mid-parentage.

If this remarkable law had been based only on experiments on the diameters of the seeds, it might well be distrusted until confirmed by other inquiries. If it were corroborated merely by the observations on human stature, of which I am about to speak, some hesitation might be expected before its truth could be recognised in opposition to the current belief that the child tends to resemble its parents. But more can be urged than this. It is easily to be shown that we ought to expect filial regression, and that it should amount to some constant fractional part of the value of the mid-parental deviation. It is because this explanation confirms the previous observations made both on seeds and on men that I feel justified on the present occasion in drawing attention to this elementary law.

The explanation of it is as follows. The child inherits partly from his parents, partly from his ancestry. Speaking generally, the further his genealogy goes back, the more numerous and varied will his ancestry become, until they cease to differ from any equally numerous sample taken at haphazard from the race at large. Their mean stature will then be the same as that of the race; in other words, it will be mediocre. Or, to put the same fact into another form, the most probable value of the mid-ancestral deviates in any remote generation is zero.

For the moment let us confine our attention to the remote ancestry and to the mid-parentages, and ignore the intermediate generations. The combination of the zero of the ancestry with the deviate of the mid-parentage is that of nothing with something, and the result resembles that of pouring a uniform proportion of pure water into a vessel of wine. It dilutes the wine to a constant fraction of its original alcoholic strength, whatever that strength may have been.

The intermediate generations will each in their degree do the same. The mid-deviate of any one of them will have a value intermediate between that of the mid-parentage and the zero value of the ancestry. Its combination with the mid-parental deviate will be as if, not pure water, but a mixture of wine and water in some definite proportion had been poured into the wine. The process throughout is one of proportionate dilutions, and therefore the joint effect of all of them is to weaken the original wine in a constant ratio.

We have no word to express the form of that ideal and composite progenitor, whom the offspring of similar mid-parentages most nearly resemble, and from whose stature their own respective heights diverge evenly, above and below. He, she, or it, may be styled the 'generant' of the group. I shall shortly explain

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what my notion of a generant is, but for the moment it is sufficient to show that the parents are not identical with the generant of their own offspring.

The average regression of the offspring to a constant fraction of their respective mid-parental deviations, which was first observed in the diameters of seeds, and then confirmed by observations on human stature, is now shown to be a perfectly reasonable law which might have been deductively foreseen. It is of so simple a character that I have made an arrangement with one movable pulley and two fixed ones by which the probable average height of the children of known parents can be mechanically reckoned. This law tells heavily against the full hereditary transmission of any rare and valuable gift, as only a few of many children would resemble their mid-parentage. The more exceptional the gift, the more exceptional will be the good fortune of a parent who has a son who equals, and still more if he has a son who overpasses him. The law is even-handed; it levies the same heavy succession-tax on the transmission of badness as well as of goodness. If it discourages the extravagant expectations of gifted parents that their children will inherit all their powers, it no less discountenances extravagant fears that they will inherit all their weaknesses and diseases.

The converse of this law is very far from being its numerical opposite. Because the most probable deviate of the son is only two-thirds that of his mid-parentage, it does not in the least follow that the most probable deviate of the mid-parentage is $\frac{3}{2}$, or $1\frac{1}{2}$ that of the son. The number of individuals in a population who differ little from mediocrity is so preponderant that it is more frequently the case that an exceptional man is the somewhat exceptional son of rather mediocre parents, than the average son of very exceptional parents. It appears from the very same table of observations by which the value of the filial regression was determined, when it is read in a different way, namely, in vertical columns instead of in horizontal lines, that the most probable mid-parentage of a man is one that deviates only one-third as much as the man does. There is a great difference between this value of $\frac{1}{3}$ and the numerical converse mentioned above of $\frac{3}{2}$; it is four and a half times smaller, since $4\frac{1}{2}$, or $\frac{9}{2}$, being multiplied into $\frac{1}{3}$, is equal to $\frac{3}{2}$.

Let it not be supposed for a moment that these figures invalidate the general doctrine that the children of a gifted pair are much more likely to be gifted than the children of a mediocre pair. What it asserts is that the ablest child of one gifted pair is not likely to be as gifted as the ablest of all the children of very many mediocre pairs. However, as, notwithstanding this explanation, some suspicion may remain of a paradox lurking in these strongly contrasted results, I will explain the form in which the table of data was drawn up, and give an anecdote connected with it Its outline was constructed by ruling a sheet into squares, and writing a series of heights in inches, such as 60 and under 61, 61 and under 62, &c., along its top, and another similar series down its side. The former referred to the height of offspring, the latter to that of mid-parentages. Each square in the table was formed by the intersection of a vertical column with a horizontal one, and in each square was inserted the number of children out of the 930 who were of the height indicated by the heading of the vertical column, and who at the same time were born of mid-parentages of the height indicated at the side of the horizontal column. I take an entry out of the table as an example. In the square where the vertical column headed[1] 69– is intersected by the horizontal column by whose side 67– is marked, the entry 38 is found; this means that out of the 930 children 38 were born of mid-parentages of 69 and under 70 inches who also were 67 and under 68 inches in height. I found it hard at first to catch the full significance of the entries in the table, which had curious relations that were very interesting to investigate. Lines drawn through entries of the same value formed a series of concentric and similar ellipses. Their common centre lay at the intersection of the vertical and horizontal lines, that corresponded to $68\frac{1}{4}$ inches. Their axes were similarly inclined. The points where each ellipse in succession was touched by a horizontal tangent, lay in a straight line inclined to the vertical in the ratio of $\frac{2}{3}$; those where they were touched by a vertical tangent,

[1] A matter of detail is here ignored which has nothing to do with the main principle, and would only serve to perplex if I described it.

lay in a straight line inclined to the horizontal in the ratio of $\frac{1}{3}$. These ratios confirm the values of average regression already obtained by a different method, of $\frac{2}{3}$ from mid-parent to offspring and of $\frac{1}{3}$ from offspring to mid-parent. These and other relations were evidently a subject for mathematical analysis and verification. They were all clearly dependent on three elementary data, supposing the law of frequency of error to be applicable throughout; these data being (1) the measure of racial variability, (2) that of co-family variability (counting the offspring of like mid-parentages as members of the same co-family), and (3) the average ratio of regression. I noted these values, and phrased the problem in abstract terms such as a competent mathematician could deal with, disentangled from all reference to heredity, and in that shape submitted it to Mr. J. Hamilton Dickson, of St. Peter's College, Cambridge. I asked him kindly to investigate for me the surface of frequency of error that would result from these three data, and the various particulars of its sections, one of which would form the ellipses to which I have alluded.

I may be permitted to say that I never felt such a glow of loyalty and respect towards the sovereignty and magnificent sway of mathematical analysis as when his answer reached me, confirming, by purely mathematical reasoning, my various and laborious statistical conclusions with far more minuteness than I had dared to hope, for the original data ran somewhat roughly, and I had to smooth them with tender caution. His calculation corrected my observed value of mid-parental regression from $\frac{1}{3}$ to $\frac{6}{17{\cdot}6}$, the relation between the major and minor axis of the ellipses was changed 3 per cent., their inclination was changed less than 2°. It is obvious, then, that the law of error holds throughout the investigation with sufficient precision to be of real service, and that the various results of my statistics are not casual determinations, but strictly interdependent.

In the lecture at the Royal Institution to which I have referred, I pointed out the remarkable way in which one generation was succeeded by another that proved to be its statistical counterpart. I there had to discuss the various agencies of the survival of the fittest, of relative fertility, and so forth; but the selection of human stature as the subject of investigation now enables me to get rid of all these complications and to discuss this very curious question under its simplest form. How is it, I ask, that in each successive generation there proves to be the same number of men per thousand, who range between any limits of stature we please to specify, although the tall men are rarely descended from equally tall parents, or the short men from equally short? How is the balance from other sources so nicely made up? The answer is that the process comprises two opposite sets of actions, one concentrative and the other dispersive, and of such a character that they necessarily neutralise one another, and fall into a state of stable equilibrium. By the first set, a system of scattered elements is replaced by another system which is less scattered; by the second set, each of these new elements becomes a centre whence a third system of elements are dispersed. The details are as follows:—In the first of these two stages, the units of the population group themselves, as it were by chance, into married couples, whence the mid-parentages are derived, and then by a regression of the values of the mid-parentages the true generants are derived. In the second stage each generant is a centre whence the offspring diverge. The stability of the balance between the opposed tendencies is due to the regression being proportionate to the deviation; it acts like a spring against a weight.

A simple equation connects the three data of race variability, of the ratio of regression, and of co-family variability, whence, if any two are given, the third may be found. My observations give separate measures of all three, and their values fit well into the equation, which is of the simple form—

$$v^2 \frac{p^2}{2} + f^2 = p^2,$$

where $v = \frac{2}{3}$, $p = 1{\cdot}7$, $f = 1{\cdot}5$.

It will therefore be understood that a complete table of mid-parental and filial heights may be calculated from two simple numbers.

It will be gathered from what has been said, that a mid-parental deviate of one unit implies a mid-grandparental deviate of $\frac{1}{3}$, a mid-ancestral unit in the next generation of $\frac{1}{9}$, and so on. I reckon from these and other data, by methods that I cannot stop to explain, that the heritage derived on an average from the mid-parental deviate, independently of what it may imply or of what may be known concerning the previous ancestry, is only $\frac{1}{2}$. Consequently, that similarly derived from a single parent is only $\frac{1}{4}$, and that from a single grandparent is only $\frac{1}{16}$.

The most elementary data upon which a complete table of mid-parental and filial heights admits of being constructed, are (1) the ratio between the mid-parental and the rest of the ancestral influences, and (2) the measure of the co-family variability.

I cannot now pursue the numerous branches that spring from the data I have given, as from a root. I will not speak of the continued domination of one type over others, nor of the persistency of unimportant characteristics, nor of the inheritance of disease, which is complicated in many cases by the requisite concurrence of two separate heritages, the one of a susceptible constitution, the other of the germs of the disease. Still less can I enter upon the subject of fraternal characteristics, which I have also worked out. It will suffice for the present to have shown some of the more important conditions associated with the idea of race, and how the vague word type may be defined by peculiarities in hereditary transmission, at all events when that word is applied to any single quality, such as stature. To include those numerous qualities that are not strictly measurable, we must omit reference to number and proportion, and frame the definition thus:—'The type is an ideal form towards which the children of those who deviate from it tend to regress.'

The stability of a type would, I presume, be measured by the strength of its tendency to regress; thus a mean regression from 1 in the mid-parents to $\frac{2}{3}$ in the offspring would indicate only half as much stability as if it had been to $\frac{1}{3}$.

The mean regression in stature of a population is easily ascertained, but I do not see much use in knowing it. It has already been stated that half the population vary less than 1·7 inch from mediocrity, this being what is technically known as the 'probable' deviation. The mean deviation is, by a well-known theory, 1·18 times that of the probable deviation, therefore in this case it is 1·9 inch. The mean loss through regression is $\frac{1}{3}$ of that amount, or a little more than 0·6 inch. That is to say, taking one child with another, the mean amount by which they fall short of their mid-parental peculiarity of stature is rather more than six-tenths of an inch.

With respect to these and the other numerical estimates, I wish emphatically to say that I offer them only as being serviceably approximate, though they are mutually consistent, and with the desire that they may be reinvestigated by the help of more abundant and much more accurate measurements than those I have had at command. There are many simple and interesting relations to which I am still unable to assign numerical values for lack of adequate material, such as that to which I referred some time back, of the superior influence of the father over the mother on the stature of their sons and daughters.

The limits of deviation beyond which there is no regression, but a new condition of equilibrium is entered into, and a new type comes into existence, have still to be explored. Let us consider how much we can infer from undisputed facts of heredity regarding the conditions amid which any form of stable equilibrium such as is implied by the word type must be established, or might be disestablished and superseded by another. In doing so I will follow cautiously along the same path by which Darwin started to construct his provisional theory of pangenesis; but it is not in the least necessary to go so far as that theory or to entangle ourselves in any questioned hypothesis.

There can be no doubt that heredity proceeds to a considerable extent, perhaps principally, in a piecemeal or piebald fashion, causing the person of the child to be to that extent a mosaic of independent ancestral heritages, one part coming with more or less variation from this progenitor, and another from that. To express this aspect of inheritance, where particle proceeds from particle, we may conveniently describe it as 'particulate.'

So far as the transmission of any feature may be regarded as an example of particulate inheritance, so far (it seems little more than a truism to assert) the element from which that feature was developed must have been particulate also. Therefore, wherever a feature in a child was not personally possessed by either parent, but transmitted through one of them from a more distant progenitor, the element whence that feature was developed must have existed in a particulate, though impersonal and latent form, in the body of the parent. The total heritage of that parent will have included a greater variety of material than was utilised in the formation of his own personal structure. Only a portion of it became developed; the survival of at least a small part of the remainder is proved, and that of a larger part may be inferred by his transmitting it to the person of his child. Therefore the organised structure of each individual should be viewed as the fulfilment of only one out of an indefinite number of mutually exclusive possibilities. It is the development of a single sample drawn out of a group of elements. The conditions under which each element in the sample became selected are, of course, unknown, but it is reasonable to expect they would fall under one or other of the following agencies: first, self-selection, where each element selects its most suitable neighbour, as in the theory of pangenesis; secondly, general co-ordination, or the influence exerted on each element by many or all of the remaining ones, whether in its immediate neighbourhood or not; finally, a group of diverse agencies, alike only in the fact that they are not uniformly helpful or harmful, that they influence with no constant purpose—in philosophical language, that they are not teleological; in popular language, that they are accidents or chances. Their inclusion renders it impossible to predict the peculiarities of individual children, though it does not prevent the prediction of average results. We now see something of the general character of the conditions amid which the stable equilibrium that characterises each race must subsist.

Political analogies of stability and change of type abound, and are useful to fix the ideas, as I pointed out some years ago. Let us take that which is afforded by the government of a colony which has become independent. The individual colonists rank as particulate representatives of families or other groups in the parent country. The organised colonial government ranks as the personality of the colony, being its mouthpiece and executive. The government is evolved amid political strife, one element prevailing here and another there. The prominent victors band themselves into the nucleus of a party, additions to their number and revisions of it ensue, until a body of men are associated capable of conducting a completely organised administration. The kinship between the form of government of the colony and that of the parent state is far from direct, and resembles in a general way that which I conceive to subsist between the child and his mid-parentage. We should expect to find many points of resemblance between the two, and many instances of great dissimilarity, for our political analogy teaches us only too well on what slight accidents the character of the government may depend when parties are nearly balanced.

The appearance of a new and useful family peculiarity is a boon to breeders, who by selection in mating gradually reduce the preponderance of those ancestral elements that endanger reversion. The appearance of a new type is due to causes that lie beyond our reach, so we ought to welcome every useful one as a happy chance, and do our best to domicile and perpetuate it. When heredity shall have become much better and more generally understood than now, I can believe that we shall look upon a neglect to conserve any valuable form of family type as a wrongful waste of opportunity. The appearance of each new natural peculiarity is a faltering step in the upward journey of evolution, over which, in outward appearance, the whole living world is blindly blundering and stumbling, but whose general direction man has the intelligence dimly to discern, and whose progress he has power to facilitate.

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EXPERIMENTS ON "PREHENSION".

By Joseph Jacobs.[1]

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It is obvious that there is a limit to the power of reproducing sounds accurately. Anyone can say *Bo* after once hearing it: few could catch the name of the Greek statesman M. Papamichalopoulos without the need of a repetition. It is here attempted to ascertain the normal limits of such reproduction in various circumstances and under varying conditions. At first experiments were made with nonsense-syllables like *cral-forg-mul-tal-nop*, as suggested by Ebbinghaus's experiments. It was found, however, that the syllables used varied greatly in relative difficulty of pronunciation and in relative facility of rhythm. After a few trials they were abandoned for letters (omitting "double *u*") and numerals (omitting "seven" as dissyllabic). It was found on the whole that the facility of reproducing the different kinds of sounds, after once hearing them, went together in a tolerably constant ratio. Thus a number of schoolgirls who could repeat on an average 6·1 nonsense-syllables could repeat 7·3 letters and 9·3 numerals. The explanation for this order of difficulty is not far to seek. The syllables, as contrasted with numerals and letters, are new to the hearer, have to be learnt, and absorb more energy; then, again, their grotesqueness would distract the attention more. The comparative difficulty of reproducing letters as compared with numerals is not so obvious. Reading accustoms us to take letters in groups having a phonetic value, and collocations of consonants like *bsvlrtm* strike us in a minor degree with the same sense of incongruity which prevents our minds from easily assimilating a conjunction like *dak-mil-tak-bin-roz*. Numerals, on the other hand, have few, if any, associations of contiguity, and we are accustomed to find them in haphazard order. Finally, our expectant attention has only to search among nine numerals, whereas it has to be ready to select from twenty-five letters. School-habits, however, might modify these conditions, and the cases were not infrequent in which the limit for letters was higher than for numerals: thus in one set of schoolboys no less than 14 boys out of 88 could repeat more letters than they could numerals, while 33 of the remainder had the same limit for both.

[1] The following investigation was made with the co-operation and advice of a circle of inquirers interested in psychological science, among whom should be mentioned, in the present connexion, Mr. J. Sully and Mr. Carveth Read but especially Mrs. S. Bryant, D.Sc., who obtained the results from the North London Collegiate School and made many valuable suggestions both in the part of the investigation now presented and that still in hand.

Numerals have the further advantage that school-children are accustomed to take them down from dictation, and this leads us to deal with the *modus operandi* adopted in obtaining our results. It was necessary, in the first place, to adopt some uniform rate at which the dictation should be given, as the power of apprehension varied with the rate of utterance. A sound every half-second was found to be a convenient rate, and a little practice with a metronome beating twice a second gives the experimenter a sense of the proper interval. The repetition was in the first experiments oral, but afterwards was taken in written form. If possible, two sets of the series of sounds should be given, and the highest number correctly reproduced is to be regarded as the limit which we wish to find, and which we term here the *span*. The reading should be in a monotonous tone, so as not to give any perceptible accent or rhythm, either of which, it appeared, assists the power of repetition in a considerable degree. The papers, when handed in, were marked with the names of the " subjects," to which it was found useful to add their ages and, if possible, their places in form.

Early in the inquiry it became evident that the power of reproducing a number of sounds increased steadily with *age*. Our materials enable us to draw up the following Table, which clearly shows the increasing power of school-girls in mastering nonsense-syllables as they grow older :—

Age, ...	11	12	13	14	17	18	19	20
Number of " Subjects," .	3	7	11	9	12	13	6	2
Average of Syllables, .	5·3	5·3	5·7	5·2	5·7	6·1	7·2	7·0

Here there is a distinct rise from 11 to 13, and from 17 to 19, and a marked progress in the whole series from 5·3 at 11 to 7·0 at 20. The same gradual increase of span is also shown in the following results for boys and girls of various ages in reproducing numerals and letters :—

	Boys.			Girls.				
Age, . .	11	12	13	13	17	18	19	20
" Subjects," .	70	57	47	60	32	28	4	3
Av. of Numerals,	6·5	6·8	8·8	8·3	9·1	9·9	9·4	9·0
Av. of Letters,	5·5	5·7	6·9	7·3	8·7	8·8	8·1	8·0 [1]

[1] These are summaries of results by different observers and under varying conditions. Later on a more extended and trustworthy set of observations were made on the girls of the North London Collegiate School, with the following results :—

Age, . . .	8	9	10	11	12	13	14	15	16	17	18	19
" Subjects," .	8	13	19	36	41	42	42	72	66	50	30	14
Av. of Numerals,	6·6	6·7	6·8	7·2	7·4	7·3	7·3	7·7	8	8	8·6	8·6
Av. of Letters, .	6	7	6·6	4·6	6·5	6·7	6·7	7·4	7·9	7·3	8·2	7·9

The answers were here written down, not taken orally as in the cases tabulated above. The uniform reduction of span at the corresponding ages

f. 2r

Steady advance is shown on the average throughout this Table except in the highest ages of the girls, where, however, the numbers are too small to allow us to draw any definite conclusions. The progress must, however, stop at some time, and the familiar fact of minds getting 'stale' after a certain age suggests the possibility that the increase in the span ceases with the increase in the bodily growth. The most noteworthy result of the table is the sudden leap of two syllables in the cyphering powers of the boys between the ages of 12 and 13. This may be due to greater practice in arithmetic. At any rate it raises them above the average for the girls of the same age, though they hold the reverse position as regards letters. No conclusions can be drawn as to the relative spans of the two sexes at the age of 13, as the subjects were drawn from two entirely different grades of society, and in the case of the boys (who were of the Jews Free School, Bell Lane,)[1] racial influences may have been at work in producing earlier maturity.

If, then, the span increases normally with age up to a certain point, it follows that in any class of the population, and in the population generally, below that age there will be a fixed number of syllables, letters and numbers which can on the average be seized after once hearing by persons of each age. This number can be determined by the means referred to above, and might easily form an addition to the usual items of anthropometric inquiries. If this were done we should obtain a *standard* span for the various ages and conditions just as we do for height, weight, &c., a standard relative and not absolute, but still enabling us to ascertain whether a boy or girl were above or below the average, and even the rate of growth in this particular. Another fact came out with equal clearness as our materials accumulated. This was that, as a rule, high span went with high place in form. Thus, selecting 30 boys of 12 years old out of a class and taking the average of their span as regards numerals, this was found to be 9·1 for the first ten, 8·3 for the next ten, and 7·9 in the remainder. In another class, also of 30 boys of the same age, the averages of the three sets of ten were in order 7·6, 7·1 and 6·3 respectively. Eight girls of the same age, taken in their order in class, gave for the first four an average of 8·2 for numerals against 8·0 for the last four, while the span for letters remained constant. With 12 girls of 13 years of age the first six had an average span of 8·3 against 7·8 for the last six in the case of numerals, while for letters the two sets were again equal. But the generality of the relation comes out clearly in the following

(of the girls) may perhaps be taken as a mark—or even as a measure—of the cerebral process involved in translating sounds into their visual symbols.

[1] The experiments were made by Mr. Louis Cohen, one of the masters of the school.

Table, giving the averages for the first and second halves of the various classes at the North London Collegiate School for Girls:—

	Numerals.		*Letters.*	
Form.	1st half.	2nd half.	1st half.	2nd half.
VI.	10·5	9·1	9	8·1
Up. V.	9·8	9·1	8·8	8·2
V.	7·9	8·6	8·1	7·8
L.V.R.	8·2	8·1	8	8·1
Low V.	8·5	9	8·2	8
Up. IV. R.	8·4	8	8·4	7·5
Up. IV.	8·4	7·8	7·4	6·5
IV. R	8·6	7·6	7·2	6·9
IV.	8	6·6	7	6·5
L. IV. R.	8	6·7	7·1	7·5
L. IV.	7·5	7·5	7	6·3
Up. III.	7·4	6·4	6·4	5·4
III.	7·8	8·5	6·7	6·4
II.	6·8	4·9	6·5	6
I.	7·4	7·1	6·8	7

Here the general superiority of the averages for the first half of the class comes out distinctly, though with exceptions which in many cases allow of special explanation. The only difficulty is the very small extent of variability: in order to get a wider range, and also to test the obvious deduction to be made from these figures, it was suggested by Mr. Francis Galton that experiments should be tried on idiots, and he kindly undertook the inquiry in conjunction with Prof. Bain and Mr. Sully. The detailed results are given below. At Earlswood the average span was as low as 4, and much the same at Darenth. 'Idiots' differ so much as to make it, indeed, hardly possible to speak of average results; but it appears that few, if any, attain to the normal span, and that a good number of those who can 'speak' at all are unable to reproduce more than 2 numerals.

This notable concomitance of high span and high place in form, though at first sight surprising, is perhaps nothing more than a corollary of the one previously shown. If the span rises with age, and is thus seemingly a measure of a pupil's relation to the standard of his or her age, it should not be surprising that a pupil with a span higher than the normal should take rank above those of the same age. At any rate, whatever be the cause, the above facts are too consistent and widespread to leave much doubt as to there being a definite connexion between high span and high place in form. And, so far as high place in form can be said to measure ability, the span may serve as some indication of ability.

This at once raises the question as to what is the exact power of the mind which is involved in reproducing these sounds. In our experiments we have simply tested the power of temporarily retaining sounds long enough to reproduce them correctly. We propose to call this power *Prehension* from the analogy of Apprehension and Comprehension, to both of which it is clearly related as a simpler process. It may be described as the mind's power of *taking on* certain material; in this case auditory sensations. Now, of course, this power of taking on need not necessarily go with that of *taking in*, but, on the other hand, we clearly cannot take in without first taking on, and the mental operation we have been testing thus seems a necessary preliminary to all obtaining of mental material, *i.e.*, through auditory presentations. Under these circumstances we might expect that "span of prehension" should be an important factor in determining mental grasp, and its determination one of the tests of mental capacity. The results given above, as far as they go, seem to confirm in no slight degree the theoretical probability.

Supplementary Notes on "Prehension" in Idiots.

By Francis Galton, F.R.S.

Prof. Bain and myself paid a visit of $4\frac{1}{4}$ hours' duration to the Earlswood Asylum for Idiots, on June 18, 1886, where we were received by Dr. Cobbold, who gave us every assistance. There were 566 idiots in the asylum, and he picked out those who were the most suitable for our inquiries.

He told us, and we had abundant evidence of the truth of the statement, that, as a general rule, idiots are incapable of the simplest arithmetic. Usually they cannot even add two figures together, though they may know the multiplication table by rote. On the other hand, a very few cases are to be met with in which idiots have a tenacious memory for dates. We determined to apply the test of the number of figures that can be orally repeated after having heard them read out once distinctly, to (1) the better class of idiots generally; (2) those who had the special power of recollecting dates, and to test the latter in other ways as well.

I. Nine of the best girls were selected by Dr. Cobbold out of the class-room. They could all read and write a little, and were intelligent enough to do some house work. They were aged apparently from 16 or 17 to 25. They all failed in adding two figures together, such as 3 to 5, 4 to 7, &c. Their performances in the numeral-test are given below at A.

Six other girls were then taken by Dr. Cobbold from the same class not quite indiscriminately, as our wish at that moment was to find girls who were intelligent enough to answer quickly, and who were nevertheless unable to repeat many figures. The result was, however, that given at B.

	Number of cases.	Greatest number of Figures that could be recollected.		Number of Figures at which the memory first wholly broke down.
		Perfectly.	Imperfectly.	
A	1	2		3
	1	3		4
	1	4		5
	2	4	5	6
	4	5		6
B	1	2		3
	2	3		4
	1	4		5
	1	5		6
	1	6		7

Having thus obtained two girls, one from each batch, who could not repeat more than two figures without mistake, 23 trials were made with them with three figures in each, and their errors were classified. In 17 cases the last figure was rightly repeated; in 10, the second; and in 7, the first. The last uttered figure is therefore most easily repeated.

There was no obvious tendency to transposition. One of the girls had a peculiar trick of duplicating a numeral and giving an answer of 4 instead of 3 figures, thus 1216 for 216, 0808 for 408.

II. Three men idiots were brought to us who were remarkable for their memory of dates; their initials were J. M., W. C. and G. M.

The speciality of J. M. was his acquaintance with Magnall's *History*. I had seen him some years ago when I visited the Asylum in company with Mr. Romanes, previous to Dr. Cobbold's appointment. He had then a well-thumbed volume, printed to the best of my recollection in small type; he now has a new volume of 419 pages, small 8vo, and in large type, but does not profess to know the whole of it by heart. He was tested at the lives of Copernicus, Columbus and elsewhere, and repeated with considerable exactitude. Where he substituted words they made good sense, and where he omitted words or passages the omissions did not spoil the sense. He repeated much that we did not find in the book, but which I ascribed to his recollection of the more diffuse edition of the work. He was asked about astronomical measures and gave abundance of correct numerical data, and when questioned as to their signification answered sensibly enough. His memory cannot be visual, as he does not know in what part of the page the recollected passages lie. Of the sermons he had heard, he could remember the texts of many and the dates when they were preached, but not the sermons themselves. His power of learning new sentences seemed small; he was tried with one of three lines out of a local guide-book that lay on the table, which was written in much the same magniloquent language as Magnall's *History*, but after five readings he failed to recall more than a few words.

On trying the numeral-test, he was right four times out of six with three figures, but wholly broke down at four.

W. C. has a minute recollection of dates of deaths, visits, holidays and other events in the asylum. He was tried in many cases familiar to Dr. Cobbold and in others verified by his journal, and his answers were pronounced to be exact. He also had a considerable knowledge of the day of the week on which any day of a month would fall in the present or in recent years, and was particular about leap years. I tried him from my pocket almanac. He correctly gave Monday as the day on which May 10 fell this year. The 13th of April puzzled him a little; he recollected that the 12th was a Wednesday, but calculated at first wrongly from that premiss; however he at last got the answer out correctly. When I pronounced the names of a month, day and year to him, as "October the twelfth, 1883," he could not recollect it, apparently from want of interest in abstract figures.

The numeral-test was a complete failure with him. We could not get him to repeat even three figures by rote. He seemed unable to understand what was wanted, and gave some fancy results.

G. M. had a memory for dates resembling that of W. C., but less good. They often conferred together about them. He was quite unable to add, saying that 2 and 3 made 4, 3 and 2 made 6, &c.

The numeral-test was a complete failure; he did not seem to understand what was wanted.

The impression left by these three men, based on what they said, and otherwise confirmed, was that their memory was chiefly due to their habit of mentally reiterating certain events and phrases that happened to interest them, so that their memory was peculiar in its limitations rather than strong. It would follow that if they happened to take a fancy to the numeral-tests, future results might not be so complete a failure as these were.

Prof. Bain has read the rough draft of this, and approves.

On June 30, 1886, Mr. Sully and I spent four hours at the Asylum for Idiots at Darenth, near Dartford. Dr. Fletcher Beach had kindly made preliminary experiments there for us, and when we arrived he gave us every assistance.

Most of the Darenth inmates are merely imbecile. Those reckoned as "first-class" struck me as far superior in intellect to any I had seen at Earlswood, and those of the second-class as distinctly superior to the first-class at Earlswood. They were taught some simple arithmetic. In the lower classes it seemed that the children were better able to seize what was wanted when tested with the names of letters than with those of numerals, so in the later experiments letters were employed; otherwise the mode of testing was exactly the same as that used at Earlswood. The names of the numerals (or letters) were distinctly uttered at estimated intervals of half a second, and after I had quite done the child began to repeat them.

Below, the figures *on* lines are intermediate estimates; thus in the case of one idiot who was not successful with 3 figures, we had reason to think the mistake possibly due to other causes than incapacity, so the entry was made on the line dividing 2 from 3.

	Span of Prehension.											
	2	2–3	3	3–4	4	4–5	5	6	7	8	9	10
Class I. The four sharpest children; ages 9, 12, 13 and 15. The quickest of these, who repeated 9 figures, was only "morally imbecile".							1		2		1	
Class II. Ages, 9-16.		1	1	1	1	2	1	1				
Claas III. Three of those whose span was only 2 had been removed from school for nearly 12 months. Their ages are 18, 18, and 19. The others range from 11 to 15.	4		2		5	2						
Class IV. Ages 11-15.			2		1							

It was very noticeable that the last uttered word was the best repeated, and after this the first. Also that there was much tendency to the transposition of adjacent words. The children were usually slow of utterance and apparently of thought. They tired very quickly; sometimes after only three or four attempts. In other cases there was an improvement within brief limits, due apparently to their better understanding what was required. They did not show signs of inattention (by looking away, &c.), but upon this Dr. Fletcher Beach remarks that the faculty of attention is one of the first to be trained. If the children should be made familiar with these experiments, and be tested when quite fresh, at and a little beyond the limits of their previously ascertained span, it is probable that better results could be obtained. They seemed to take pleasure in the tests and to show emulation.

I submitted a rough draft of the foregoing to Mr. Sully, and afterwards to Dr. Fletcher Beach, whose remarks are now incorporated in it.

NOTES ON PERMANENT COLOUR TYPES IN MOSAIC.

BY

F. GALTON, M.A., F.R.S., President.

LONDON:
HARRISON AND SONS, ST. MARTIN'S LANE
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1886.

NOTES *on* PERMANENT COLOUR TYPES *in* MOSAIC.

By F. GALTON, M.A., F.R.S., President.

DURING a brief stay in Rome, I recently made such inquiries as I could, into the suitability of the material used in the manufactory of mosaics, for affording permanent specimens of standard colours for the description of tints of skin. The original paintings by Broca, as well as the lithographs from them, have already changed colour, and some more permanent standard is needed; this I have little doubt, could be best obtained by means of the material used for making mosaics. The general result of what I am about to describe is that about a dozen identical slabs should be made, each containing six small pieces of mosaic material, lettered respectively, A, B, C, D, E, and F, and severally brought into relation with corresponding tints on Broca's scale. These slabs which need not be larger than letter-weights, could be distributed among the existing Anthropological Institutions and Museums, and would form practically unalterable standards of reference whence painted copies might be made from time to time, as often as desired, for the use of travellers.

The mosaic material is glass rendered opaque by oxides of tin and lead, and is manufactured in flat cakes, circular or otherwise, of usually about six inches in diameter, and a quarter of an inch thick. Each cake is a hard vitreous mass, from which pieces are chipped, of approximately the required shape, and which are then ground on a lapidary's wheel to the exact size; next they are polished on the exposed side, and are afterwards cemented into their proper places. Each cake is of uniform tint throughout, except in rare cases where, possibly from over baking, I noticed a rind of a lighter color. The material is inexpensive, costing a very few shillings per pound weight. If I am not mistaken, it is a very difficult matter to produce an exact tint to order. The method employed appears to be to make a large number of trial tints, and to sort and classify according to results.

There are upwards of forty thousand bins in the Vatican manufactory, containing the proceeds of different attempts. Out of these no less than 10,752 are classified; they occupy 24 cases in each of which are 16 rows of 28 samples. The flesh tints appropriate to European nations (such as those which are found in the second of the two pages of selections from Broca's tints, which appear in the "Anthropological Notes and Queries") are about 500 in number. We may therefore conclude, that a superabundance of material exists in the Vatican manufactory, whence a series of standard tints, such as anthropologists desire, admit of being selected.

There can be no question as to the persistence of the colours of mosaic. I examined carefully some in St. Peter's that were more than a century old, and was astonished at their freshness throughout. They seemed to be brand-new. If the surface of mosaic is dirty, it can be freely washed. If stained in any way, the stain can be ground off. If the surface is roughened it can be repolished.

M. Topinard informs me that as the original tints of Broca have already changed colour, he is engaged in preparing a new and much smaller series of only five or six tints, for hair-color to serve as a fresh departure. These will of course be correlated with Broca's numbers. I have written to M. Topinard, explaining about the mosaics, and inviting him to send me the five or six tints that he provisionally selects, in order that I may ascertain how nearly they may be matched by existing mosaic material, and I hope that if the difference is in no case considerable, it may be found possible to make a compromise by adopting the mosaic tints as the final standards. I would willingly charge myself with the trouble and such small cost as there may be in obtaining the mosaic material. At the same time I fear it is possible from some former experience that an application to the Vatican may not prove successful; that experience, which I may as well put upon record is as follows:

Many years ago, having been much impressed by a visit to the Vatican manufactory, and being equally impressed by the then faulty nomenclature of colour, I wrote to the authorities at South Kensington, suggesting that they should make application to the Vatican for samples of their large collection of mosaic material, and select therefrom a considerable scale of standard tints. Also that a small and second selection from these tints should be supplied to schools of art. This scheme, which I need not now describe more minutely, was taken up by the South Kensington authorities, and the late Lord Ampthill, then Mr. Odo Russell, our semi-official representative at the Papal Court, was asked to inquire into the feasibility of bringing it into effect. It was perfectly feasible in all respects save one, namely, that the price asked by the Papal government was altogether excessive, and so the matter dropped. Now, however, resulting not improbably from my then abortive suggestions, I find that such samples are being produced. I saw one set in process of being made.

If it should not be found easy to procure samples from the manufactory in the Vatican, it may be possible to obtain them from private dealers in mosaics, but after my inquiries at Rome, I doubt if any of the private dealers possesses a collection of tints comparable in variety and quantity to that in the Vatican, and it might prove difficult to obtain from them the exact tints that will be required. Anyhow, I propose to try what can be done towards putting anthropologists in possession of standard sets of permanent tints, and I shall of course communicate the results, if they prove favourable, to the Anthropological Institute.

Discussion.

Mr. Rudler exhibited some cakes of Roman and Venetian enamels, and called attention to the permanence of their colours. The enamels may be regarded as opaque varieties of glass, consisting of various silicates, borates, and boro-silicates. The opacity of an enamel is commonly obtained by the use of stannic oxide ("putty powder," or binoxide of tin), which, being infusible, is mechanically suspended in a finely comminated condition through the substance of the glass, producing, if the vitrified base be colourless, a dense white enamel. Colour is obtained by the use of various metallic oxides, some of which remain suspended in the vitreous vehicle, while others enter into chemical combination with some of the constituents of the glassy flux and form metallic silicates and borates. Many of the colours which are of interest to anthropologists, such as the browns and reds, used to denote tints of skin and hair, belong to the former class, being due to the presence of peroxide of iron; while the blue tints for eyes, being furnished by the oxides of cobalt or of copper, belong to the latter group. In either case the stability of the colours is beyond doubt, fugitive pigments being quite unable to withstand the temperature necessary for the fusion of the enamel.

Professor Meldola suggested that if, on account of expense or other difficulties, it was not found convenient to get the mosaics from abroad he had no doubt that some of our English manufacturers, such as Messrs. Doulton of Lambeth, might be found willing to take the matter up.

f. 3v

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Harrison and Sons, Printers in Ordinary to Her Majesty, St. Martin's Lane.

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Reproduce the plates between pp 247 & 248 might make one plate of these two K.P.

REGRESSION TOWARDS MEDIOCRITY IN HEREDITARY STATURE.

BY

FRANCIS GALTON, F.R.S., &c.

LONDON:
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1885.

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ANTHROPOLOGICAL MISCELLANEA.

REGRESSION *towards* MEDIOCRITY *in* HEREDITARY STATURE.

By FRANCIS GALTON, F.R.S., &c.

[WITH PLATES IX AND X.]

THIS memoir contains the data upon which the remarks on the Law of Regression were founded, that I made in my Presidential Address to Section H, at Aberdeen. That address, which will appear in due course in the Journal of the British Association, has already been published in "Nature," September 24th. I reproduce here the portion of it which bears upon regression, together with some amplification where brevity had rendered it obscure, and I have added copies of the diagrams suspended at the meeting, without which the letterpress is necessarily difficult to follow. My object is to place beyond doubt the existence of a simple and far-reaching law that governs the hereditary transmission of, I believe, every one of those simple qualities which all possess, though in unequal degrees. I once before ventured to draw attention to this law on far more slender evidence than I now possess.

It is some years since I made an extensive series of experiments on the produce of seeds of different size but of the same species. They yielded results that seemed very noteworthy, and I used them as the basis of a lecture before the Royal Institution on February 9th, 1877. It appeared from these experiments that the offspring did *not* tend to resemble their parent seeds in size, but to be always more mediocre than they—to be smaller than the parents, if the parents were large; to be larger than the parents, if the parents were very small. The point of convergence was considerably below the average size of the seeds contained in the large bagful I bought at a nursery garden, out of which I selected those that were sown, and I had some reason to believe that the size of the seed towards which the produce converged was similar to that of an average seed taken out of beds of self-planted specimens.

The experiments showed further that the mean filial regression towards mediocrity was directly proportional to the parental deviation from it. This curious result was based on so many plantings, conducted for me by friends living in various parts of the country, from Nairn in the north to Cornwall in the south, during one, two, or even three generations of the plants, that I could entertain no doubt of the truth of my conclusions. The exact ratio of regression remained a little doubtful, owing to variable influences; therefore I did not attempt to define it. But as it seems a pity that no

a

f. 2v

record should exist in print of the general averages, I give them, together with a brief account of the details of the experiment, in Appendix I to the present memoir.

After the lecture had been published, it occurred to me that the grounds of my misgivings might be urged as objections to the general conclusions. I did not think them of moment, but as the inquiry had been surrounded with many small difficulties and matters of detail, it would be scarcely possible to give a brief and yet a full and adequate answer to such objections. Also, I was then blind to what I now perceive to be the simple explanation of the phenomenon, so I thought it better to say no more upon the subject until I should obtain independent evidence. It was anthropological evidence that I desired, caring only for the seeds as means of throwing light on heredity in man. I tried in vain for a long and weary time to obtain it in sufficient abundance, and my failure was a cogent motive, together with others, in inducing me to make an offer of prizes for Family Records, which was largely responded to, and furnished me last year with what I wanted. I especially guarded myself against making any allusion to this particular inquiry in my prospectus, lest a bias should be given to the returns. I now can securely contemplate the possibility of the records of height having been frequently drawn up in a careless fashion, because no amount of unbiassed inaccuracy can account for the results, contrasted in their values but concurrent in their significance, that are derived from comparisons between different groups of the returns.

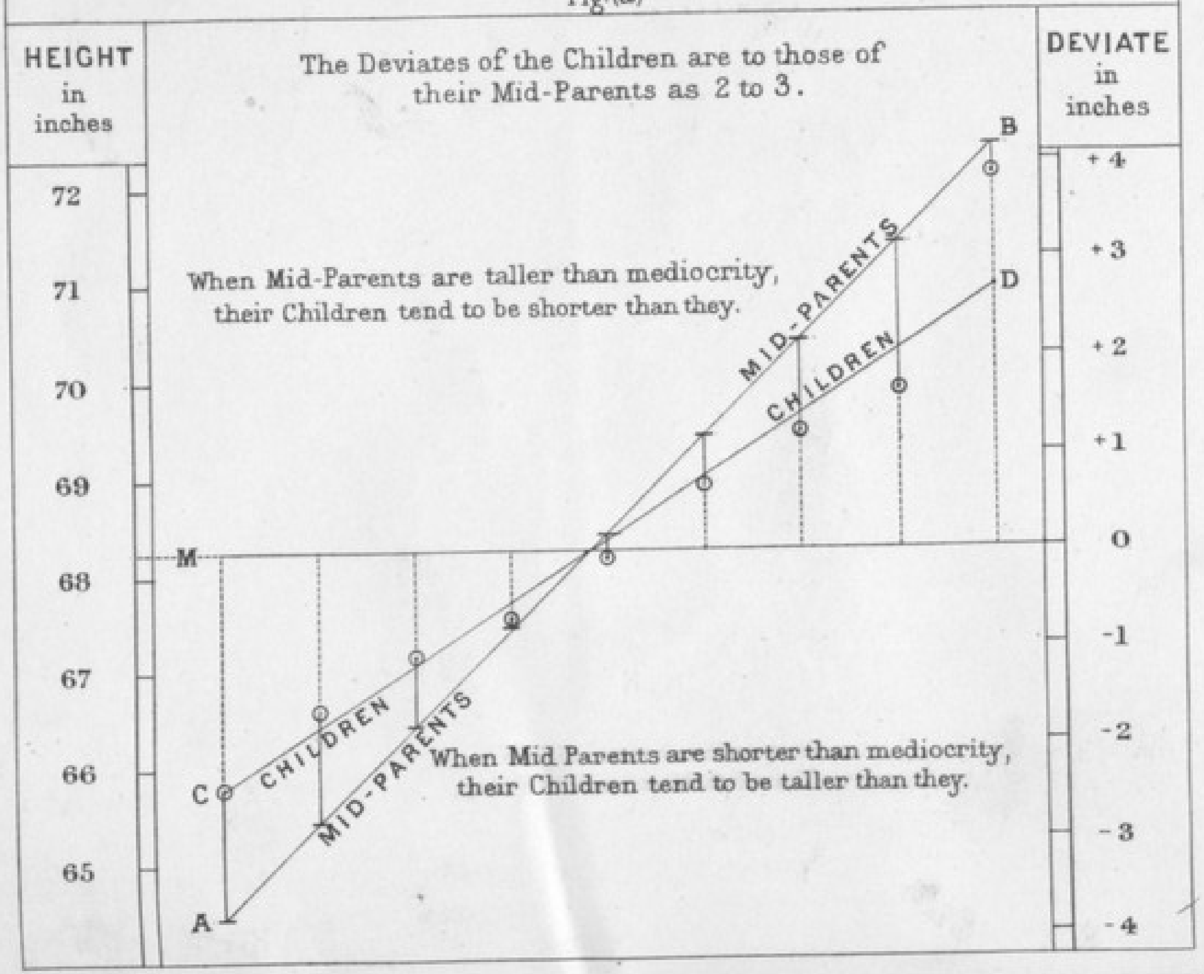
An analysis of the Records fully confirms and goes far beyond the conclusions I obtained from the seeds. It gives the numerical value of the regression towards mediocrity in the case of human stature, as from 1 to $\frac{2}{3}$ with unexpected coherence and precision [*see* Plate IX, fig. (*a*)], and it supplies me with the class of facts I wanted to investigate—the degrees of family likeness in different degrees of kinship, and the steps through which special family peculiarities become merged into the typical characteristics of the race at large.

My data consisted of the heights of 930 adult children and of their respective parentages, 205 in number. In every case I transmuted the female statures to their corresponding male equivalents and used them in their transmuted form, so that no objection grounded on the sexual difference of stature need be raised when I speak of averages. The factor I used was 1·08, which is equivalent to adding a little less than one-twelfth to each female height. It differs a very little from the factors employed by other anthropologists, who, moreover, differ a trifle between themselves; anyhow, it suits my data better than 1·07 or 1·09. The final result is not of a kind to be affected by these minute details, for it happened that, owing to a mistaken direction, the computer to whom I first entrusted the figures used a somewhat different factor, yet the result came out closely the same.

I shall now explain with fulness why I chose stature for the

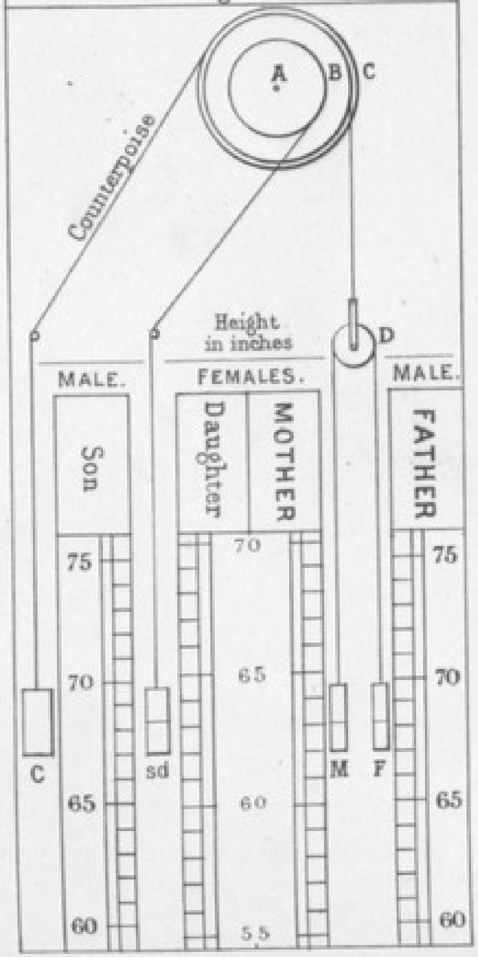
RATE OF REGRESSION IN HEREDITARY STATURE.

Fig. (a)



FORECASTER OF STATURE

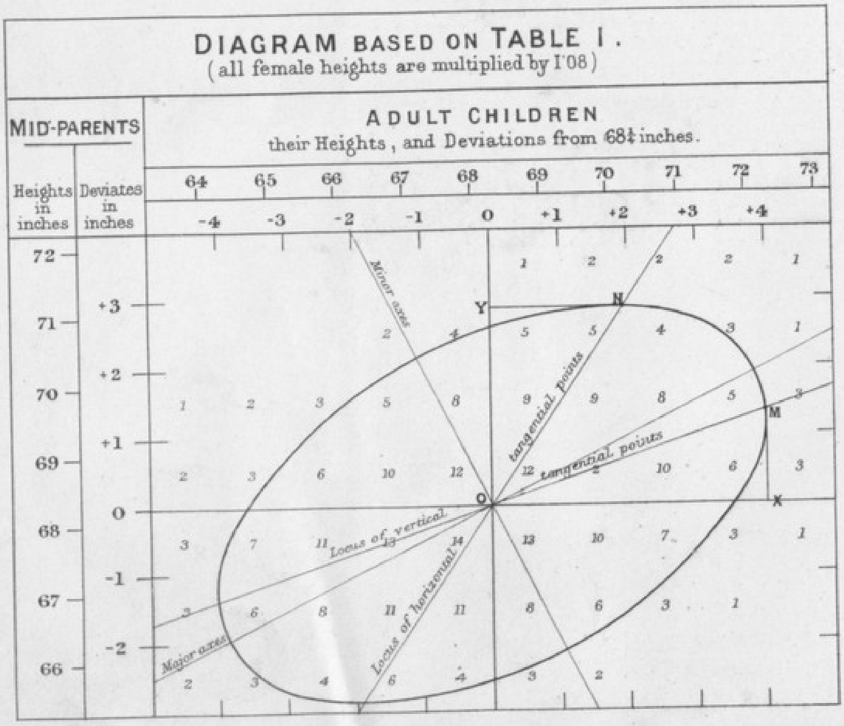
Fig (b)

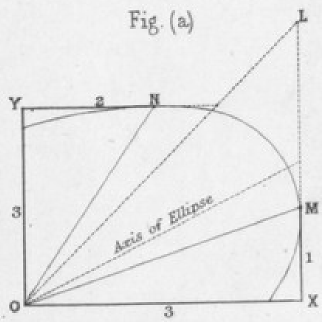


Journ. Anthropolog. Inst., Vol. XV, Pl. IX.

J.P. & W.R. Emslie, lith.

Plate X.





J.P. & W.R. Emslie, lith.

TABLE I.

NUMBER OF ADULT CHILDREN OF VARIOUS STATURES BORN OF 205 MID-PARENTS OF VARIOUS STATURES.
(All Female heights have been multiplied by 1·08).

Heights of the Mid-parents in inches.	Heights of the Adult Children.														Total Number of		Medians.
	Below	62·2	63·2	64·2	65·2	66·2	67·2	68·2	69·2	70·2	71·2	72·2	73·2	Above	Adult Children.	Mid-parents.	
Above ..	..	..	..	..	..	..	..	..	..	..	..	1	3	..	4	5	..
72·5	..	..	..	..	..	..	..	1	2	1	2	7	2	4	19	6	72·2
71·5	..	..	..	..	1	3	4	3	5	10	4	9	2	2	43	11	69·9
70·5	1	..	1	..	1	1	3	12	18	14	7	4	3	3	68	22	69·5
69·5	..	..	1	16	4	17	27	20	33	25	20	11	4	5	183	41	68·9
68·5	1	..	7	11	16	25	31	34	48	21	18	4	3	..	219	49	68·2
67·5	..	3	5	14	15	36	38	28	38	19	11	4	..	..	211	33	67·6
66·5	..	3	3	5	2	17	17	14	13	4	..	..	..	..	78	20	67·2
65·5	1	..	9	5	7	11	11	7	7	5	2	1	..	..	66	12	66·7
64·5	1	1	4	4	1	5	5	..	2	..	..	..	..	..	23	5	65·8
Below ..	1	..	2	4	1	2	2	1	1	..	..	..	..	..	14	1	..
Totals ..	5	7	32	59	48	117	138	120	167	99	64	41	17	14	928	205	..
Medians ..	..	..	66·3	67·8	67·9	67·7	67·9	68·3	68·5	69·0	69·0	70·0	..	..	..	..	..

NOTE.—In calculating the Medians, the entries have been taken as referring to the middle of the squares in which they stand. The reason why the headings run 62·2, 63·2, &c., instead of 62·5, 63·5, &c., is that the observations are unequally distributed between 62 and 63, 63 and 64, &c., there being a strong bias in favour of integral inches. After careful consideration, I concluded that the headings, as adopted, best satisfied the conditions. This inequality was not apparent in the case of the Mid-parents.

subject of inquiry, because the peculiarities and points to be attended to in the investigation will manifest themselves best by doing so. Many of its advantages are obvious enough, such as the ease and frequency with which its measurement is made, its practical constancy during thiry-five years of middle life, its small dependence on differences of bringing up, and its inconsiderable influence on the rate of mortality. Other advantages which are not equally obvious are no less great. One of these lies in the fact that stature is not a simple element, but a sum of the accumulated lengths or thicknesses of more than a hundred bodily parts, each so distinct from the rest as to have earned a name by which it can be specified. The list of them includes about fifty separate bones, situated in the skull, the spine, the pelvis, the two legs, and the two ankles and feet. The bones in both the lower limbs are counted, because it is the average length of these two limbs that contributes to the general stature. The cartilages interposed between the bones, two at each joint, are rather more numerous than the bones themselves. The fleshy parts of the scalp of the head and of the soles of the feet conclude the list. Account should also be taken of the shape and set of many of the bones which conduce to a more or less arched instep, straight back, or high head. I noticed in the skeleton of O'Brien, the Irish giant, at the College of Surgeons, which is, I believe, the tallest skeleton in any museum, that his extraordinary stature of about 7 feet 7 inches would have been a trifle increased if the faces of his dorsal vertebræ had been more parallel and his back consequently straighter.

The beautiful regularity in the statures of a population, whenever they are statistically marshalled in the order of their heights, is due to the number of variable elements of which the stature is the sum. The best illustrations I have seen of this regularity were the curves of male and female statures that I obtained from the careful measurements made at my Anthropometric Laboratory in the International Health Exhibition last year. They were almost perfect.

The multiplicity of elements, some derived from one progenitor, some from another, must be the cause of a fact that has proved very convenient in the course of my inquiry. It is that the stature of the children depends closely on the average stature of the two parents, and may be considered in practice as having nothing to do with their individual heights. The fact was proved as follows:—After transmuting the female measurements in the way already explained, I sorted the adult children of those parents who severally differed 1, 2, 3, 4, and 5 or more inches, into separate lines (see Table II). Each line was then divided into similar classes, showing the number of cases in which the children differed 1, 2, 3, &c., inches from the common average of the children in their respective families. I confined my inquiry to large families of six children and upwards, that the common average of each might be a trustworthy point of reference. The entries in each of the different lines were then seen to run in the same way, except that in the last of them the children showed a faint tendency to fall into two sets, one taking after the tall parent, the other after the short one; this, however, is not visible in the summary Table II that I annex. Therefore, when dealing with the transmission of stature from parents to children, the average height of the two parents, or, as I prefer to call it, the "mid-parental" height, is all we need care to know about them.

TABLE II.

EFFECT UPON ADULT CHILDREN OF DIFFERENCES IN HEIGHT OF THEIR PARENTS.

Difference between the Heights[1] of the Parents in inches.	Proportion per 50 of cases in which the Heights[1] of the Children deviated to various amounts from the Mid-filial Stature of their respective families.						Number of Children whose Heights were observed.
	Less than 1 inch.	Less than 2 inches.	Less than 3 inches.	Less than 4 inches.	Less than 5 inches.	Within Extreme Limit.	
Under 1 ..	21	35	43	46	48	50	105
1 and under 2..	23	37	46	49	50	..	122
2 " 3..	16	34	41	45	49	50	112
3 " 5..	24	35	41	47	49	50	108
5 and above ..	18	30	40	47	49	50	78

[1] Every female height has been transmuted to its male equivalent by multiplying it by 1·08, and only those families have been included in which the number of adult children amounted to six, at least.

NOTE.—When these figures are protracted into curves, it will be seen—(1) that they run much alike; (2) that their peculiarities are not in sequence; and (3) that the curve corresponding to the first line occupies a medium position. It is therefore certain that differences in the heights of the parents have on the whole an inconsiderable effect on the heights of their offspring.

It must be noted that I use the word parent without specifying the sex. The methods of statistics permit us to employ this abstract term, because the cases of a tall father being married to a short mother are balanced by those of a short father being married to a tall mother. I use the word parent to save any complication due to a fact apparently brought out by these inquiries, that the height of the children of both sexes, but especially that of the daughters, takes after the height of the father more than it does after that of the mother. My present data are insufficient to enable me to speak with any confidence on this point, much less to determine the ratio satisfactorily.

Another great merit of stature as a subject for inquiries into heredity is that marriage selection takes little or no account of shortness or tallness. There are undoubtedly sexual preferences for moderate contrast in height, but the marriage choice is guided by so many and more important considerations that questions of stature appears to exert no perceptible influence upon it. This is by no means my only inquiry into this subject, but, as regards the present data, my test lay in dividing the 205 male parents and the 205 female parents each into three groups—T, M, and S—that is, tall, medium, and short (medium male measurement being taken as 67 inches and upwards to 70 inches), and in counting the number of marriages in each possible combination between them (see Table III). The result was that men and women of contrasted heights, short and tall or tall and short, married just about as frequently as men and women of similar heights, both tall or both short; there were 32 cases of the one to 27 of the other.

TABLE III.

S., t. 12 cases.	M., t. 20 cases.	T., t. 18 cases.
S., m. 25 cases.	M., m. 51 cases.	T., m. 28 cases.
S., s. 9 cases.	M., s. 28 cases.	T., s. 14 cases.

Short and tall, 12 + 14 = 32 cases.
Short and short, 9 }
Tall and tall, 18 } = 27 cases.

In applying the law of probabilities to investigations into heredity of stature, we may therefore regard the married folk as couples picked out of the general population at haphazard.

The advantages of stature as a subject in which the simple laws of heredity may be studied will now be understood. It is a nearly constant value that is frequently measured and recorded, and its discussion is little entangled with considerations of nurture, of the survival of the fittest, or of marriage selection. We have only to consider the mid-parentage and not to trouble ourselves about the parents separately. The statistical variations of stature are extremely regular, so much so that their general conformity with the results of calculations based on the abstract law of frequency of error is an accepted fact by anthropologists. I have made much use of the properties of that law in cross-testing my various conclusions, and always with success. For example, the measure of variability (say the "probable error") of the system of mid-parental heights, ought, on the suppositions justified in the preceding paragraphs, to be equal to that of the system of adult male heights, multiplied into the square root of 2; this inference is shown to be correct by direct observation.

The only drawback to the use of stature is its small variability. One-half of the population with whom I dealt, varied less than 1·7 inch from the average of all of them, and one-half of the offspring of similar mid-parentages varied less than 1·5 inch from the average of their own heights. On the other hand, the precision of my data is so small, partly due to the uncertainty in many cases whether the height was measured with the shoes on or off, that I find by means of an independent inquiry that each observation, taking one with another, is liable to an error that as often as not exceeds $\frac{2}{3}$ of an inch.

The law that I wish to establish refers primarily to the inheritance of different degrees of tallness and shortness, and only secondarily to that of absolute stature. That is to say, it refers to measurements made from the crown of the head to the level of mediocrity, upwards or downwards as the case may be, and not from the crown of the head to the ground. In the population with which I deal the level of mediocrity is $68\frac{1}{4}$ inches (without shoes). The same law applying with sufficient closeness both to tallness and shortness, we may include both under the single head of deviations, and I shall call any particular deviation a "deviate." By the use of this word and that of "mid-parentage" we can define the law of regression very briefly. It is that the height-deviate of the offspring is, on the average, two-thirds of the height-deviate of its mid-parentage.

Plate IX, fig. *a*, gives a graphic expression of the data upon which this law is founded. It will there be seen that the relations between the statures of the children and their mid-parents, which are perfectly simple when referred to the scale of deviates at the right hand of the plate, do not admit of being briefly phrased when they are referred to the scale of statures at its left.

If this remarkable law had been based only on experiments on the diameters of the seeds, it might well be distrusted until confirmed by other inquiries. If it were corroborated merely by a comparatively small number of observations on human stature, some hesitation might be expected before its truth could be recognised in opposition to the current belief that the child tends to resemble its parents. But more can be urged than this. It is easily to be shown that we ought to expect filial regression, and that it should amount to some constant fractional part of the value of the mid-parental deviation. It is because this explanation confirms the previous observations made both on seeds and on men that I feel justified on the present occasion in drawing attention to this elementary law.

The explanation of it is as follows. The child inherits partly from his parents, partly from his ancestry. Speaking generally, the further his genealogy goes back, the more numerous and varied will his ancestry become, until they cease to differ from any equally numerous sample taken at haphazard from the race at large. Their mean stature will then be the same as that of the race; in other words, it will be mediocre. Or, to put the same fact into another form, the most probable value of the mid-ancestral deviates in any remote generation is zero.

For the moment let us confine our attention to the remote ancestry and to the mid-parentages, and ignore the intermediate generations. The combination of the zero of the ancestry with the deviate of the mid-parentage is the combination of nothing with something, and the result resembles that of pouring a uniform proportion of pure water into a vessel of wine. It dilutes the wine to a constant fraction of its original alcoholic strength, whatever that strength may have been.

The intermediate generations will each in their degree do the same. The mid-deviate in any one of them will have a value intermediate between that of the mid-parentage and the zero value of the ancestry. Its combination with the mid-parental deviate will be as if, not pure water, but a mixture of wine and water in some definite proportion, had been poured into the wine. The process throughout is one of proportionate dilutions, and therefore the joint effect of all of them is to weaken the original wine in a constant ratio.

We have no word to express the form of that ideal and composite progenitor, whom the offspring of similar mid-parentages most nearly resemble, and from whose stature their own respective heights diverge evenly, above and below. If he, she, or it, is styled the "generant" of the group, then the law of regression makes it clear that parents are not identical with the generants of their own offspring.

The average regression of the offspring to a constant fraction of their respective mid-parental deviations, which was first observed in the diameters of seeds, and then confirmed by observations on human stature, is now shown to be a perfectly reasonable law which might have been deductively foreseen. It is of so simple a character that I have made an arrangement with pulleys and weights by which the probable average height of the children of known parents can be mechanically reckoned (see Plate IX, fig. *b*). This law tells heavily against the full hereditary transmission of any gift, as only a few of many children would resemble their mid-parentage. The more exceptional the amount of the gift, the more exceptional will be the good fortune of a parent who has a son who equals, and still more if he has a son who overpasses him in that respect. The law is even-handed; it levies the same heavy succession-tax on the transmission of badness as well as of goodness. If it discourages the extravagant expectations of gifted parents that their children will inherit all their powers, it no less discountenances extravagant fears that they will inherit all their weaknesses and diseases.

The converse of this law is very far from being its numerical opposite. Because the most probable deviate of the son is only two-thirds that of his mid-parentage, it does not in the least follow that the most probable deviate of the mid-parentage is $\frac{3}{2}$, or $1\frac{1}{2}$ that of the son. The number of individuals in a population who differ little from mediocrity is so preponderant that it is more frequently the case that an exceptional man is the somewhat exceptional son of rather mediocre parents, than the average son of very exceptional parents. It appears from the very same table of observations by which the value of the filial regression was determined when it is read in a different way, namely, in vertical columns instead of in horizontal lines, that the most probable mid-parentage of a man is one that deviates only one-third as much as the man does. There is a great difference between this value of $\frac{1}{3}$ and the numerical converse mentioned above of $\frac{3}{2}$; it is four and a half times smaller, since $4\frac{1}{2}$, or $\frac{9}{2}$, being multiplied into $\frac{1}{3}$, is equal to $\frac{3}{2}$.

It will be gathered from what has been said, that a mid-parental deviate of one unit implies a mid-grandparental deviate of $\frac{1}{3}$, a mid-ancestral unit in the next generation of $\frac{1}{9}$, and so on. I reckon from these and other data, by methods that I cannot stop now to explain, but will do so in the Appendix, that the heritage derived on an average from the mid-parental deviate, independently of what it may imply, or of what may be known concerning the previous ancestry, is only $\frac{1}{2}$. Consequently, that similarly derived from a single parent is only $\frac{1}{4}$, and that from a single grandparent is only $\frac{1}{16}$.

Let it not be supposed for a moment that any of these statements invalidate the general doctrine that the children of a gifted pair are much more likely to be gifted than the children of a mediocre pair. What they assert is that the ablest child of one gifted pair is not likely to be as gifted as the ablest of all the children of very many mediocre pairs. However, as, notwithstanding this explanation, some suspicion may remain of a paradox lurking in my strongly contrasted results, I will call attention to the form in which the table of data (Table I) was drawn up, and give an anecdote connected with it.

It is deduced from a large sheet on which I entered every child's height, opposite to its mid-parental height, and in every case each was entered to the nearest tenth of an inch. Then I counted the number of entries in each square inch, and copied them out as they appear in the table. The meaning of the table is best understood by examples. Thus, out of a total of 928 children who were born to the 205 mid-parents on my list, there were 18 of the height of 69·2 inches (counting to the nearest inch), who were born to mid-parents of the height of 70·5 inches (also counting to the nearest inch). So again there were 25 children of 70·2 inches born to mid-parents of 69·5 inches. I found it hard at first to catch the full significance of the entries in the table, which had curious relations that were very interesting to investigate. They came out distinctly when I "smoothed" the entries by writing at each intersection of a horizontal column with a vertical one, the sum of the entries in the four adjacent squares, and using these to work upon. I then noticed (see Plate X) that lines drawn through entries of the same value formed a series of concentric and similar

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ellipses. Their common centre lay at the intersection of the vertical and horizontal lines, that corresponded to $68\frac{1}{4}$ inches. Their axes were similarly inclined. The points where each ellipse in succession was touched by a horizontal tangent, lay in a straight line inclined to the vertical in the ratio of $\frac{2}{3}$; those where they were touched by a vertical tangent lay in a straight line inclined to the horizontal in the ration of $\frac{1}{3}$. These ratios confirm the values of average regression already obtained by a different method, of $\frac{2}{3}$ from mid-parent to offspring, and of $\frac{1}{3}$ from offspring to mid-parent, because it will be obvious on studying Plate X that the point where each horizontal line in succession is touched by an ellipse, the greatest value in that line must occur at the point of contact. The same is true in respect to the vertical lines. These and other relations were evidently a subject for mathematical analysis and verification. They were all clearly dependent on three elementary data, supposing the law of frequency of error to be applicable throughout; these data being (1) the measure of racial variability, whence that of the mid-parentages may be inferred as has already been explained, (2) that of co-family variability (counting the offspring of like mid-parentages as members of the same co-family), and (3) the average ratio of regression. I noted these values, and phrased the problem in abstract terms such as a competent mathematician could deal with, disentangled from all reference to heredity, and in that shape submitted it to Mr. J. Hamilton Dickson, of St. Peter's College, Cambridge. I asked him kindly to investigate for me the surface of frequency of error that would result from these three data, and the various particulars of its sections, one of which would form the ellipses to which I have alluded.

I may be permitted to say that I never felt such a glow of loyalty and respect towards the sovereignty and magnificent sway of mathematical analysis as when his answer reached me, confirming, by purely mathematical reasoning, my various and laborious statistical conclusions with far more minuteness than I had dared to hope, for the original data ran somewhat roughly, and I had to smooth them with tender caution. His calculation corrected my observed value of mid-parental regression from $\frac{1}{3}$ to $\frac{6}{17 \cdot 6}$, the relation between the major and minor axis of the ellipses was changed 3 per cent. (it should be as $\sqrt{7} : \sqrt{2}$), their inclination was changed less than 2° (it should be to an angle whose tangent is $\frac{1}{2}$). It is obvious, then, that the law of error holds throughout the investigation with sufficient precision to be of real service, and that the various results of my statistics are not casual and disconnected determinations, but strictly interdependent.

In the lecture at the Royal Institution to which I have referred, I pointed out the remarkable way in which one generation was succeeded by another that proved to be its statistical counterpart. I there had to discuss the various agencies of the survival of the fittest, of relative fertility, and so forth; but the selection of human stature as the subject of investigation now enables me to get rid of all these complications and to discuss this very curious question under its simplest form. How is it, I ask, that in each successive generation there proves to be the same number of men per thousand, who range between any limits of stature we please to specify, although the tall men are rarely descended from equally tall parents, or the short men from equally short? How is the balance from other sources so nicely made up? The answer is that the process comprises two opposite sets of actions, one concentrative and the other dispersive, and of such a character that they necessarily neutralise one another, and fall into a state of stable equilibrium (see Table IV). By the first set, a system of scattered elements is replaced by another system which is less scattered; by the second set, each of these new elements becomes a centre whence a third system of elements are dispersed.

The details are as follows:—In the first of these two stages we start from the population generally, in the first generation; then the units of the population group themselves, as it were by chance, into married couples, whence the more compact system of mid-parentages is derived, and then by a regression of the values of the mid-parentages the still more compact system of the generants is derived. In the second stage each generant is a centre whence the offspring diverge upwards and downwards to form the second generation. The stability of the balance between the opposed tendencies is due to the regression being proportionate to the deviation. It acts like a spring against a weight; the spring stretches until its resilient force balances the weight, then the two forces of spring and weight are in stable equilibrium; for if the weight be lifted by the hand, it will obviously fall down again when the hand is withdrawn, and, if it be depressed by the hand, the resilience of the spring will be thereby increased, so that the weight will rise when the hand is withdrawn.

A simple equation connects the three data of race variability, of the ratio of regression, and of co-family variability, whence, if any two are given, the third may be found. My observations give separate measures of all three, and their values fit well into the equation, which is of the simple form—

$$v^2 \frac{p^2}{2} + f^2 = p^2,$$

where $v=\frac{2}{3}$, $p=1{\cdot}7$, $f=1{\cdot}5$.

It will therefore be understood that the complete table of mid-parental and filial heights may be calculated from two simple numbers, and that the most elementary data upon which it admits of being constructed are—(1) the ratio between the mid-parental and the rest of the ancestral influences, and (2) the measure of the co-family variability.

The mean regression in stature of a population is easily ascertained; I do not see much use in knowing it, but will give the work merely as a simple example. It has already been stated that half

TABLE IV.

PROCESS THROUGH WHICH THE DISTRIBUTION OF STATURES, IN SUCCESSIVE GENERATIONS OF THE SAME PEOPLE, REMAINS UNCHANGED.

Height in inches.	Deviation in inches.	Statistical Distribution of Statures in the several Systems of: GENERATION I.		MID-PARENTS.		GENERANTS.		GENERATION II.	
73		2	Concentrates.		Concentrates.		Disperses.	2	
72	+ 4	6		2		1		6	
71	+ 3	10		6		7		10	Upper Quartile.
70	+ 2	15		16		13		15	
69	+ 1	17		26		29		17	Median.
68	0	17		26		29		17	
67	− 1	15		16		13		15	Lower Quartile.
66	− 2	10	Concentrates.	6	Concentrates.	7	Disperses.	10	
65	− 3	6		2		1		6	
64	− 4	2						2	
Total		100		100		100		100	
Probable derivation ...		1·7		1·2		0·8		1·7	

NOTE.—The cases are symmetrically disposed above and below the common mean value of 68¼ inches. The Upper and Lower Quartiles are the values that in each case divide the number of cases above the Median or mean value, and those below it, respectively into equal parts. Thus in each column there are (1) 25 cases per cent. above the Upper Quartile, (2) 25 cases between the Upper Quartile and the Median, (3) 25 cases between the Median and the Lower Quartile, (4) 25 cases below the Lower Quartile. The difference between either Quartile and the Median is technically called the "Probable" deviation.

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the population vary less than 1·7 inch from mediocrity, this being what is technically known as the "probable" deviation. The mean deviation is, by a well-known theory, 1·18 times that of the probable deviation, therefore in this case it is 1·9 inch. The mean loss through regression is $\frac{1}{3}$ of that amount, or a little more than 0·6 inch. That is to say, taking one child with another, the mean amount by which they fall short of their mid-parental peculiarity of stature is rather more than six-tenths of an inch.

The stability of a Type, which I should define as "an ideal form towards which the children of those who deviate from it tend to regress," would, I presume, be measured by the strength of its tendency to regress; thus a mean regression from 1 in the mid-parents to $\frac{2}{3}$ in the offspring would indicate only half as much stability as if it had been to $\frac{1}{3}$.

The limits of deviation beyond which there is no regression, but a new condition of equilibrium is entered into, and a new type comes into existence, have still to be explored.

With respect to my numerical estimates, I wish emphatically to say that I offer them only as being serviceably approximate, though they are mutually consistent, and with the desire that they may be reinvestigated by the help of more abundant and much more accurate measurements than those I have had at command. There are many simple and interesting relations to which I am still unable to assign numerical values for lack of adequate material, such as that to which I referred some time back, of the relative influence of the father and the mother on the stature of their sons and daughters.

I do not now pursue the numerous branches that spring from the data I have given, as from a root. I do not speak of the continued domination of one type over others, nor of the persistency of unimportant characteristics, nor of the inheritance of disease, which is complicated in many cases by the requisite concurrence of two separate heritages, the one of a susceptible constitution, the other of the germs of the disease. Still less do I enter upon the subject of fraternal deviation and collateral descent, which I have also worked out.

Appendix.

I.—*Experiments on Seeds bearing on the Law of Regression.*

I sent a set of carefully selected sweet pea seeds to each of several country friends, who kindly undertook to help me. The advantage of sweet peas over other seeds is that they do not cross fertilise, that they are spherical, and that all the seeds in the same pod are of much the same size. They are also hardy and prolific. I selected them as the subject of experiment after consulting eminent botanists. Each set contained seven packets, numbered K, L, M, N, O, P, and Q. Each packet contained ten seeds of exactly the same weight; those in K being the heaviest, L the next heaviest, and so on down to Q, which was the lightest. The precise weights are given in Table V, together with the corresponding diameter, which I ascertained by laying 100 peas of the same sort in a row. The weights run in an arithmetic series, having a common average difference of 0·172 grain. I do not of course profess to work to thousandths of a grain, though I did to less than tenths of a grain; therefore the third decimal place represents no more than an arithmetical working value, which has to be regarded in multiplications, lest an error of sensible importance should be introduced by its neglect. Curiously enough, the diameters were found to run approximately in an arithmetic series also, owing, I suppose, to the misshape and corrugations of the smaller seeds, which gave them a larger diameter than if they had been plumped out into spheres. The results are given in Table V, which show that I was justified in sorting the seeds by the convenient method of the balance and weights, and of accepting the weights as directly proportional to the mean diameters, which can hardly be measured satisfactorily except in spherical seeds.

In each experiment seven beds were prepared in parallel rows; each was $1\frac{1}{2}$ feet wide and 5 feet long. Ten holes of 1 inch deep were dibbled at equal distances apart along each bed, and one seed was put into each hole. They were then bushed over to keep off the birds. Minute instructions were given and followed to ensure uniformity, which I need not repeat here. The end of all was that the seeds as they became ripe were collected from time to time in bags that I sent, lettered from K to Q, the same letters being stuck at the ends of the beds, and when the crop was coming to an end the whole foliage of each bed was torn up, tied together, labelled, and sent to me. I measured the foliage and the pods, both of which gave results confirmatory of those of the peas, which will be found in Table VI, the first and last columns of which are those that especially interest us; the remaining columns showing clearly enough how these two were obtained. It will be seen that for each increase of one unit on the part of the parent seed, there is a mean increase of only one-third of a unit in the filial seed; and again that the mean filial seed resembles the parental when the latter is about 15·5 hundredths of an inch in diameter. Taking then 15·5 as the point towards which filial regression points, whatever may be the parental deviation (within the tabular limits) from that point, the mean filial deviation will be in the same direction, but only one-third as much.

This point of regression is so low that I possessed less evidence than I desired to prove the bettering of the produce of very small seeds. The seeds smaller than Q were such a miserable set that I could hardly deal with them. Moreover, they were very infertile. It did, however, happen that in a few of the sets some of the Q seeds turned out very well.

If I desired to lay much stress on these experiments, I could make my case considerably stronger by going minutely into the details of the several experiments, foliage and length of pod included, but I do not care to do so.

TABLE V.

WEIGHTS AND DIAMETERS OF SEEDS (SWEET PEA).

Letter of seed.	Weight of one seed in grains.	Length of row of 100 seeds in inches.	Diameter of one seed in hundredths of inch.
K	1·750	21·0	21
L	1·578	20·2	20
M	1·406	19·2	19
N	1·234	17·9	18
O	1·062	17·0	17
P	·890	16·1	16
Q	·718	15·2	15

TABLE VI.

PARENT SEEDS AND THEIR PRODUCE.

Table showing the proportionate number of seeds (sweet peas) of different sizes, produced by parent seeds also of different sizes. The measurements are those of mean diameter, in hundredths of an inch.

Diameter of Parent Seed.	Diameters of Filial Seeds.								Total.	Mean Diameter of Filial Seeds.	
	Under 15	15–	16–	17–	18–	19–	20–	Above 21–		Observed.	Smoothed.
21	22	8	10	18	21	13	6	2	100	17·5	17·3
20	23	10	12	17	20	13	3	2	100	17·3	17·0
19	35	16	12	13	11	10	2	1	100	16·0	16·6
18	34	12	13	17	16	6	2	0	100	16·3	16·3
17	37	16	13	16	13	4	1	0	100	15·6	16·0
16	34	15	18	16	13	3	1	0	100	16·0	15·7
15	46	14	9	11	14	4	2	0	100	15·3	15·4

II.—*Separate Contribution of each Ancestor to the Heritage of the Offspring.*

When we say that the mid-parent contributes two-thirds of his peculiarity of height to the offspring, it is supposed that nothing is known about the previous ancestor. We now see that though nothing is known, something is implied, and that something must be eliminated if we desire to know what the parental bequest, pure and simple, may amount to. Let the deviate of the mid-parent be a, then the implied deviate of the mid-grandparent will be $\frac{1}{3}a$, of the mid-ancestor in the next generation $\frac{1}{9}a$, and so on. Hence the sum of the deviates of all the mid-generations that contribute to the heritage of the offspring is $a(1+\frac{1}{3}+\frac{1}{9}+\&c.)=a\frac{3}{2}$.

Do they contribute on equal terms, or otherwise? I am not prepared as yet with sufficient data to yield a direct reply, therefore we must try the effects of limiting suppositions. First, suppose they contribute equally; then as an accumulation of ancestral deviates whose sum amounts to $a\frac{3}{2}$, yields an effective heritage of only $a\frac{2}{3}$, it follows that each piece of property, as it were, must be reduced by a sucession tax to $\frac{4}{9}$ of its original amount, because $\frac{3}{2}\times\frac{4}{9}=\frac{2}{3}$.

Another supposition is that of successive diminution, the property being taxed afresh in each transmission, so that the effective heritage would be—

$$a\left(\frac{1}{r}+\frac{1}{3r^2}+\frac{1}{3^2r^3}+-\right)=a\left(\frac{3}{3r-1}\right)$$

and this must, as before, be equal to a $\frac{2}{3}$, whence $\frac{1}{r}=\frac{6}{11}$.

The third limiting supposition of a mid-ancestral deviate in any one remote generation contributing more than a mid-parental deviate, is notoriously incorrect. Thus the descendants of "pedigree-wheat" in the (say) twentieth generation show no sign of their mid-ancestral magnitude, but those in the first generation do so most unmistakably.

The results of our two valid limiting suppositions are therefore (1) that the mid-parental deviate, pure and simple, influences the offspring to $\frac{4}{9}$ of its amount; (2) that it influences it to the $\frac{6}{11}$ of its amount. These values differ but slightly from $\frac{1}{2}$, and their mean is closely $\frac{1}{2}$, so we may fairly accept that result. Hence the influence, pure and simple, of the mid-parent may be taken as $\frac{1}{2}$, of the mid-grandparent $\frac{1}{4}$, of the mid-great-grandparent $\frac{1}{8}$, and so on. That of the individual parent would therefore be $\frac{1}{4}$, of the individual grandparent $\frac{1}{16}$, of an individual in the next generation $\frac{1}{64}$, and so on.

Explanation of Plates IX and X.

Plate IX, fig. *a*. Rate of Regression in Hereditary Stature.

The short horizontal lines refer to the stature of the mid-parents as given on the scale to the left. These are the same values as those in the left hand column of Table I.

The small circles, one below each of the above, show the mean stature of the children of each of those mid-parents. These are the values in the right hand column of Table I, headed "Medians." [The Median is the value that half the cases exceed, and the other fall short of it. It is practically the same as the mean, but is a more convenient value to find, in the way of working adopted throughout in the present instance.]

f. 12r

The sloping line AB passes through all possible mid-parental heights.

The sloping line CD passes through all the corresponding mean heights of their children. It gives the "smoothed" results of the actual observations.

The ratio of CM to AM is as 2 to 3, and this same ratio connects the deviate of every mid-parental value with the mean deviate of its offspring.

The point of convergence is at the level of mediocrity, which is $68\frac{1}{4}$ inches.

The above data are derived from the 928 adult children of 205 mid-parents, female statures having in every case been converted to their male equivalents by multiplying each of them by 1·08.

Fig. *b*. Forecasts of stature. This is a diagram of the mechanism by which the most probable heights of the sons and daughters can be foretold, from the data of the heights of each of their parents.

The weights M and F have to be set opposite to the heights of the mother and father on their respective scales; then the weight sd will show the most probable heights of a son and daughter on the corresponding scales. In every one of these cases it is the fiducial mark in the middle of each weight by which the reading is to be made. But, in addition to this, the length of the weight sd is so arranged that it is an equal chance (an even bet) that the height of each son or each daughter will lie within the range defined by the upper and lower edge of the weight, on their respective scales. The length of sd is 3 inches $= 2f$; that is, $2 \times 1{\cdot}50$ inch.

A, B, and C are three thin wheels with grooves round their edges. They are screwed together so as to form a single piece that turns easily on its axis. The weights M and F are attached to either end of a thread that passes over the movable pulley D. The pulley itself hangs from a thread which is wrapped two or three times round the groove of B and is then secured to the wheel. The weight sd hangs from a thread that is wrapped in the same direction two or three times round the groove of A, and is then secured to the wheel. The diameter of A is to that of B as 2 to 3. Lastly, a thread wrapped in the opposite direction round the wheel C, which may have any convenient diameter, is attached to a counterpoise.

It is obvious that raising M will cause F to fall, and *vice versâ*, without affecting the wheels AB, and therefore without affecting sd; that is to say, the parental differences may be varied indefinitely without affecting the stature of the children, so long as the mid-parental height is unchanged. But if the mid-parental height is changed, then that of sd will be changed to $\frac{2}{3}$ of the amount.

The scale of female heights differs from that of the males, each female height being laid down in the position which would be occupied by its male equivalent. Thus 56 is written in the position of 60·48 inches, which is equal to $56 \times 1{\cdot}08$. Similarly, 60 is written in the position of 64·80, which is equal to $60 \times 1{\cdot}08$.

In the actual machine the weights run in grooves. It is also

b

f. 12v

taller and has a longer scale than is shown in the figure, which is somewhat shortened for want of space.

Plate X. This is a diagram based on Table I. The figures in it were first "smoothed" as described in the memoir, then lines were drawn through points corresponding to the same values, just as isobars or isotherms are drawn. These lines, as already stated, formed ellipses. I have also explained how calculation showed that they were true ellipses, and verified the values I had obtained of the relation of their major to their minor axes, of the inclination of these to the coordinates passing through their common centre, and so forth. The ellipse in the figure is one of these. The numerals are not directly derived from the smoothed results just spoken of, but are rough interpolations so as to suit their present positions. It will be noticed that each horizontal line grows to a maximum and then symmetrically diminishes, and that the same is true of each vertical line. It will also be seen that the loci of maxima in these follow the lines *ON* and *OM*, which are respectively inclined to their adjacent coordinates at the gradients of 2 to 3, and of 1 to 3. If there had been no regression, but if like bred like, then *OM* and *ON* would both have coincided with the diagonal *OL*, in fig. *a*, as shown by the dotted lines.

I annex a comparison between calculated and observed results. The latter are inclosed in brackets.

Given—

"Probable error" of each system of mid-parentages = 1·22.
Ratio of mean filial regression = $\frac{2}{3}$.
"Probable error" of each system of regressed values = 1·50.

Sections of surface of frequency parallel to XY are true ellipses.
[Obs.—Apparently true ellipses.]
$MX : YO = 6 : 17{\cdot}5$, or nearly 1 : 3.
[Obs.—1 : 3.]
Major axes to minor axes = $\sqrt{7} : \sqrt{2} = 10 : 5{\cdot}35$.
[Obs.—10 : 5·1.]
Inclination of major axes to $OX = 26^\circ\ 36'$.
[Obs.—25°.]
Section of surface parallel to XY is a true curve of frequency.
[Obs.—Apparently so.]
"Probable error" of that curve = 1·07.
[Obs.—1·0 or a little more.]

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ON THE ANTHROPOMETRIC LABORATORY

AT THE LATE

INTERNATIONAL HEALTH EXHIBITION.

BY

FRANCIS GALTON, M.A., F.R.S.

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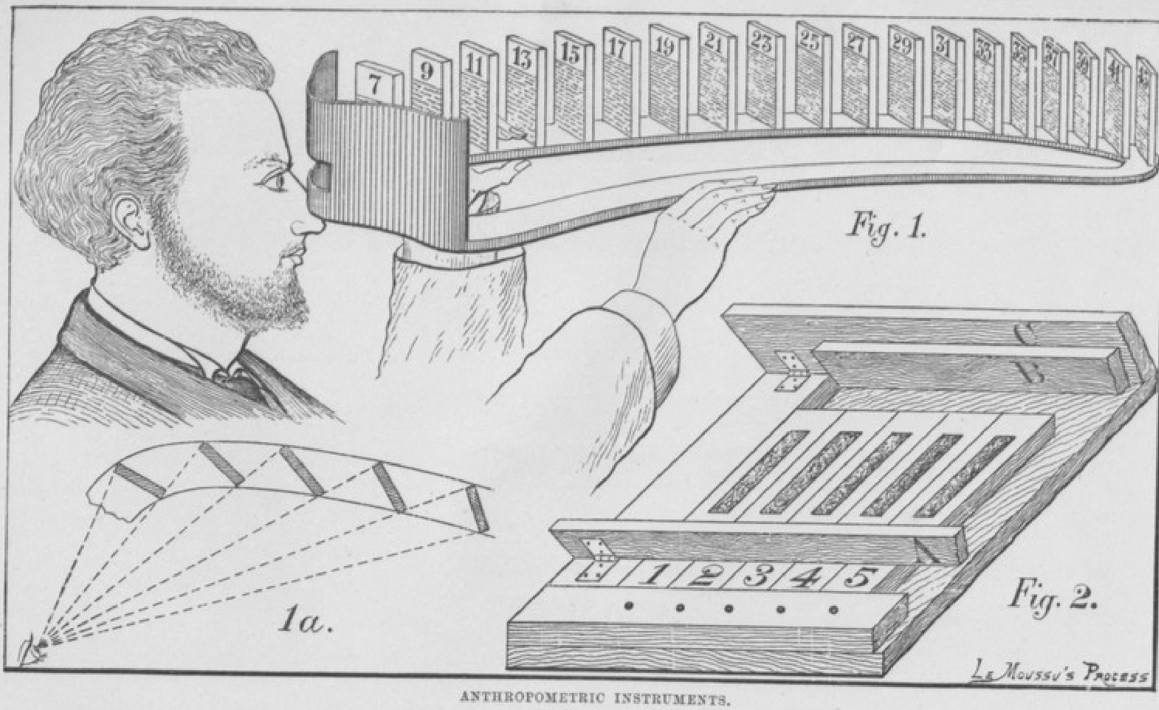
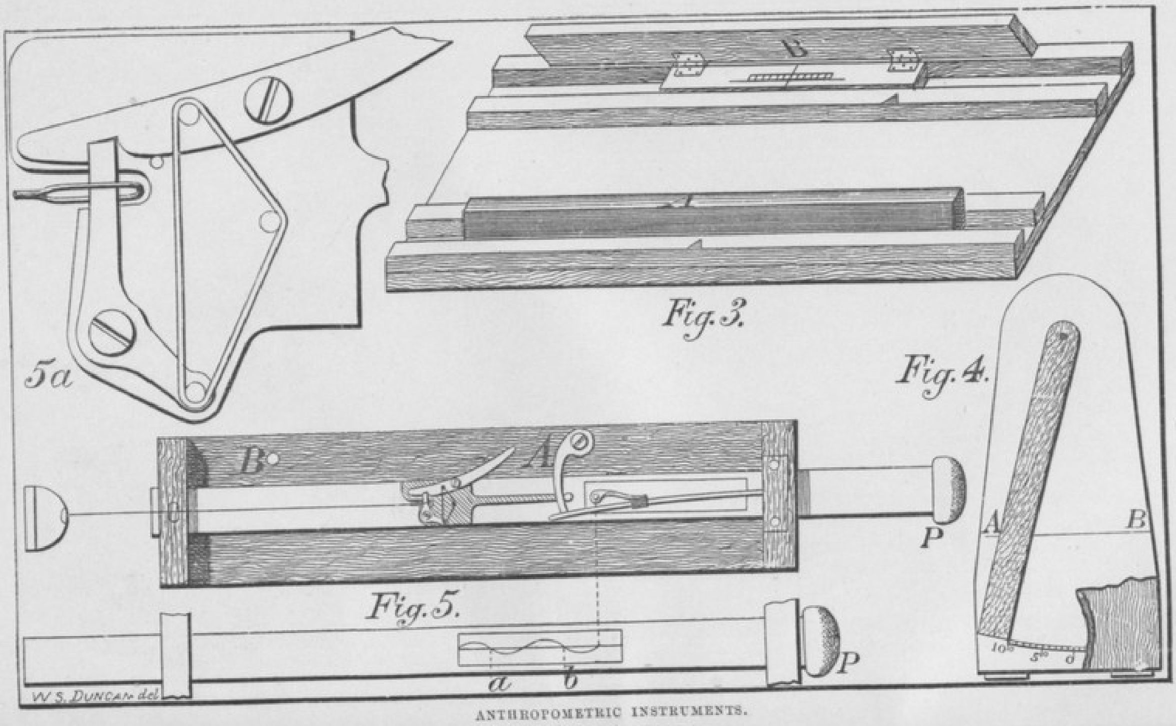


Journ. Anthrop. Inst., Vol. XIV, Pl. XII.

ANTHROPOMETRIC INSTRUMENTS.

f. 2

Journ. Anthrop. Inst., Vol. XIV, Pl. XIII.



ANTHROPOMETRIC INSTRUMENTS.

f. 3

On the ANTHROPOMETRIC LABORATORY *at the late* INTERNATIONAL HEALTH EXHIBITION. By FRANCIS GALTON, M.A., F.R.S.

[WITH PLATES XII AND XIII.]

Now that the International Health Exhibition is over, and the Anthropometric Laboratory there established has done its appointed work, it is desirable to put on record its methods and experiences. As for the statistical results they are still under discussion and I shall not speak of them now, but I hope before long to communicate these also to the Institute.

The object of the laboratory was to show to the public the simplicity of the instruments and methods by which the chief physical characteristics of man may be measured and recorded. The instruments in action dealt with keenness of sight; colour-sense; judgment of eye; hearing; highest audible note; breathing power; strength of pull and squeeze; swiftness of blow; span of arms; height, standing and sitting; and weight. Some other apparatus not in actual use was also exhibited.

The chief motive of this memoir is to invite criticism and suggestions. Duplicates of the instruments have been ordered by executive officers in foreign countries, and considerable interest has been expressed in the collection by the authorities of many places of education in this country, as well as by numerous private individuals. It seems, therefore, well to lose no time in considering whether any and what improvements should be made in their scope and design before they or any others may be so widely used that it would become difficult to make a change. We want a set of standard apparatus of as appropriate a pattern as can be devised, for the sake of uniformity in the methods of measurement and facility in statistical comparisons. I have therefore brought all my instruments to this room, together with the attendants who had charge of them in the Exhibition, availing myself very gladly of the opportunity afforded by this meeting of submitting my method and appliances to discussion.

The number of persons measured in the laboratory from first to last was no less than 9,337, and each of them in 17 different ways. The only attendants were Serjeant Williams, who was permanently on duty, Mr. Gammage (optical instrument maker, 172, Brompton Road), who came for some hours every evening to assist and supervise, and who maintained the instruments in efficiency, and a doorkeeper provided by the executive, who admitted visitors, received the admission fee of 3*d*., supplied the blank forms, and saw that the required particulars were written down by them. The doorkeeper also made himself useful in many other details. With this small staff, and in a compartment only 6 feet wide and 36 feet long, about ninety persons were measured daily in an elaborate manner.

It was not possible to work so rapidly at first, but the process gradually improved. Thus it was found best to take two persons through the laboratory together at the same time, and to keep parents and their children apart, as the old did not like to be outdone by the young, and insisted on repeated trials.

Hardly any trouble occurred with the visitors, though on some few occasions rough persons entered the laboratory who were apparently not altogether sober. On the whole, the laboratory worked with astonishing smoothness, and its popularity was extraordinary. Its door was thronged by applicants waiting patiently for their turn, or after a while turning away seeing that it was almost a hopeless task to wait. If there had been more accommodation there would have been a large increase in the number measured. The small admission fee of 3*d*. did more than cover every charge connected with the maintenance of the laboratory and I have therefore little doubt that a smaller number of careful measurements might be made periodically at large schools under skilled supervision for a very minute charge per head, if the system of doing so was well methodised, and if the masters, older pupils, and school attendants gave willing help.

There is a vast field for work among the millions of school boys and girls of all degrees, with the object of keeping an adequate oversight upon their physical well-being by a judicious series of physical measurements. I do not see why it should be either difficult or costly to the schools of the upper and middle classes, to whom a charge of two or three pence per head is a matter of no moment whatever, to institute periodical measurements even of a somewhat elaborate character under skilful itinerant supervision, and to register them in a methodical and uniform manner. It should, I think, become a recognised part of school discipline to have this regularly done; the more so as the experience of this laboratory, confirmed by those of many American colleges, makes it certain that the innovation would be popular. One of the conditions that a standard set of instruments ought to fulfil is, that it should admit of being readily packed, carried about from place to place, and quickly set up anywhere for temporary use by a professional measurer.

We have now to consider what we should measure. One object is to ascertain what may be called the personal constants of mature life. This phrase must not be taken in too strict a sense, because there is nothing absolutely constant in a living body. Life is a condition of perpetual change. Men are about half an inch shorter when they go to bed than when they rise in the morning. Their weight is affected by diet and habit of life. All our so-called personal constants are really variables, though a large proportion of their actual variations may lie between narrow limits. Our first rule then is, that the trouble of measurement is best repaid when it is directed upon the least variable faculties.

There are many faculties that may be said to be potentially constant in adults though they are not developed, owing to want of exercise. After adequate practice, a limit of efficiency would in each case be attained, and this would be the personal constant; but it is obviously impossible to guess what that constant would be from the results of a single trial. No test professes to do more than show the efficiency of the faculty at the time it was applied, and many tests do even less than this, being so novel to the person experimented on that he is maladroit, and fails to do himself justice; consequently the results of earlier trials with ill-devised tests may differ considerably from those of later ones. The second rule then is, that the actions required by the tests should be as familiar as possible.

For example, in testing the delicacy of the various senses I think we should do wrong if we pursued the strict methods appropriate to psycho-physical investigations. We do not want to analyse how much of our power of discriminating between two objects is due to this, that, or the other of the many elementary perceptions called into action. It is the total result that chiefly interests us. Thus in measuring the delicacy with which a person can estimate the difference between weights, I think he ought to be allowed to handle them in the way he prefers and that we may disregard the fact that his judgment rests on a blend of many different data, such as pressure, muscular exertion, and appreciation of size.

There is some hope that we may in time learn to eliminate the effect of an unknown amount of previous practice by three or more distinct sets of trials. There exists a rough relation between practice and proficiency which ought to be apparent wherever progress is not due to acquiring a succession of new knacks, but proceeds regularly. When no practice has previously taken place, the progressive improvement will be very rapid; then its rate will smoothly decrease until it comes to an entire stop. I suspect that a curve might be drawn, representing the relation between proficiency and practice, and that the data afforded by at least three successive series of tests would roughly determine the position in the curve of the person who was being tested. They would show what he was capable of at the time, and approximately how much conscious or unconscious practice he had already gone through, and the maximum efficiency to which his faculty under test admitted of being educated.

An ideally perfect laboratory, whether a plain or an elaborate one, would admit of a stream of persons passing continuously through it. There would be no gaps and no blocks by the way, because the number of such instruments as might necessitate two, three, or more units of delay would be multiplied in that proportion. Again, there would be no waste of the attendant's time in idly watching examinees puzzling over tests that required a prolonged judgment, because those tests would be so contrived that the examinee might be left to himself until he had performed the specified act, after which the attendant would return and note the result. To exemplify what I mean, I will describe the test (Plate XII, fig. 2) for colour-sense by the use of wools, which is further explained on p. 215. A set of Holmgren's patterns were wound each through two holes in a separate rod, much as a net maker winds string on his netting needle, and each rod had a separate number stamped on it. A row of these rods were laid in any order side by side in a frame, with a long narrow flap above and below.

When the flaps were shut, the rods were nipped fast and their numbers were hid; when the lower flap was opened the numbers were exposed. The test consisted in telling the examinee that there were four tints of green, and he was required to point them out. Then the lower flap was opened, and the truth of his choice was tested by the correctness of the exposed numbers.

If this had been the process pure and simple, the test would have occupied an undue amount of the attendant's time, who would have had to stand by doing nothing while the examinee was hesitating. It is probable that two minutes might have been so wasted, in which case the ninety persons who daily required between them about thirteen hours of direct supervision in performing seventeen tests, would have required twice ninety minutes, or three hours, for this test alone. Such a sacrifice would have been inadmissible and it was easily avoided by a simple contrivance. Holes were bored below the bottom flap, one opposite to each rod, and four pegs were tied to the instrument. The attendant directed the examinee to put a peg into the hole opposite to each of the four greens, and then left him to ponder over his task at leisure, while he attended to others. After awhile the attendant returned, found the pegs set, and noted the result in a couple of seconds.

A similar plan was adopted in two instruments (Plate XIII, figs. 3 & 4) that I used, less for the intrinsic value of their results than as examples of the way in which a large class of tests might be methodised. They were to test the judgment of the eye in dividing a line into equal parts and in estimating squareness. The accuracy of the result was in each case measured by graduations that were hidden under a closed flap, while the examinee was left by himself to make the required adjustments. Here, again, the examiner returned after awhile, and noted the results of a prolonged pondering in a very few seconds.

On this principle very elaborate tests might be introduced into a well furnished laboratory without adding to the cost of the course by taking up the valuable time of a skilled supervisor, or of diminishing the rate at which applicants might be admitted. The stream of them would still pass regularly through, but the length of the stream included between the entrance and the exit doors would be longer.

It will be remembered that in the laboratory at the Exhibition, ninety persons passed through daily, and that the amount of skilled attendance given to them amounted in the aggregate to about thirteen hours: that is, seven minutes to each. But the time each person was occupied in the laboratory was fully twenty-one minutes, and often half-an-hour. In the first place, the persons to be tested were taken in pairs, that one explanation and illustration might suffice for both, and since the promptest minded man of the two was usually the one who presented himself first, the less prompt man had the advantage of seeing his companion perform the test before he was called upon to do so himself. This duplex system changed the seven minutes into fourteen. Then there was the time occupied by each examinee in reading notices, writing down particulars of his age, state, occupation, and birth-place, in puzzling alone over set tasks, and in amusing himself by watching others.

I have dwelt at length on this because the necessity of labour-saving arrangements must be carefully borne in mind when devising a standard laboratory outfit, in which a large number of persons may be elaborately measured at a minimum of cost.

In the Appendix to this paper will be found a brief but sufficient description of the instruments used in the laboratory. I will now call attention only to those points which appear especially in need of criticism.

One omission in the laboratory has been noticed by many. I had decided, perhaps wrongly, after much hesitation, not to measure the head. My reason was, that the results would, under the peculiar circumstances of a mixed crowd of persons, each measured only once, be of little or no profit, and I feared it would be troublesome to perform on most women on account of their bonnets, and the bulk of their hair, and that it would lead to objections and difficulties. In the case of periodical measurements at schools, the head measurement would be of primary importance, and I should propose to take its maximum length and breadth with graduated calipers, and its maximum height above the plane that passes through the upper edges of the orbits and the orifices of the ears.[1]

I measured the chief dimensions of the body, the weight and the breathing capacity, but could devise no good method, other than what these implicitly afford, of ascertaining the bulk, and its distribution in muscle or fat. Stripping was of course inadmissible, and measurements of girth, whether of body or limb, taken over the clothes, are rather fallacious. The excess due to the presence of the clothes, and supposing no wrinkles, is six times their thickness, taking the circumference of the limb as equal to six times its mean radius. The wrinkles add an unknown amount to the error.

For the first time such a thing has been attempted, I measured swiftness of blow as distinguished from force of blow, the latter

[1] I have designed and made the necessary instruments since this memoir was read. They are now being constructed solidly for me by the Scientific Instrument Company at Cambridge, and they will be in use at Cambridge in the beginning of 1885.

of which is a compound result of swiftness, weight, and knack. The instrument was based upon a very pretty principle first applied by Exner in his little apparatus for measuring reaction-time. It was a matter of surprise to myself, who was born in the days of pugilism, to find that the art of delivering a clean hit, straight from the shoulder, as required by this instrument, is nearly lost to the rising generation. My instrument (Plate XIII, fig. 5) consisted of a rod, padded at one end, and running quite freely between guides. The person to be tested was asked to hit the pad which fronted him, and to drive the bar forwards with as much swiftness as he could. The rate of progress of the rod was marked by a pencil attached to a vibrating spring that had been bent to one side and was retained by a catch to be set free by the moving rod. Notwithstanding the simplicity of the test, a large proportion of persons bungled absurdly over it. They could not or would not strike straight at the pad, but punched its side, and often broke the rod and hurt their knuckles. I had the deal rod replaced by an oaken one, and they still broke it and hurt their knuckles all the more. I then, in despair, reversed the action, by passing the looped end of a string round a catch (fig. 5*a*), forming part of an apparatus that was fixed to the opposite end of the rod, and I attached a stirrup to the other end of the string which the examinee held in his hand while he struck out into space, pulling the rod after him. While the rod was in motion, and before it was pulled home, the free end of the lever that retained the catch struck against a peg B in the frame of the apparatus; the catch was thereby released and the string (or rather the steel wire, which I used at last) was disengaged, and there was nothing left to break. On this plan all went well. This instrument has given beautifully accordant results in successive trials, but I propose to supersede it by another pattern, not yet quite complete in details, but primarily consisting of a light hoop turning round a horizontal axis, the string disengaging itself as it does from a humming-top.

I employed only a few tests for the delicacy of the various senses, but many others might be added with advantage to a fully equipped laboratory if they were constructed on the labour-saving principle I have described.

The construction of an absolute and convenient test for delicacy of hearing, quite baffles me. I mean an apparatus that any instrument maker might construct from description, every specimen of which should emit a sound always of the same loudness and quality. Identity in the striking bodies may be ensured by using coins, and the arrangement of two pennies (that is two short cylinders) striking crossways is theoretically perfect as ensuring that the locus of contact shall be a point. But the trouble is to hold the pence firmly and conveniently by rods, too slight to increase the sound either by echoes or by their own vibrations caused by the concussion. The rods should nip the pence at their nodal points so as not to hinder the vibrations. I should be very grateful for useful suggestions.

The sickle-shaped hand instrument (Plate XII, fig. 1) used for reading small test type, first with one eye and then with the other, acted excellently, but the light in the laboratory was often bad. I used pages cut out of the shilling diamond edition of the Prayer Book, because it was easily accessible, and to enable persons who had been tested at the laboratory to repeat the identical experiment at home with their friends. But printed sentences, especially when they are so generally well known as those in the Prayer Book, are objectionable: a page of logarithms would be much better.

I exhibited, but did not use, a model of a test for delicacy of touch, so far as pressure is concerned, which has merits, but would I feared have occupied too much time. It is a "Roberval" balance, like a common letter weigher; the finger is laid on one scale pan, and the object of the instrument is to increase or diminish the weight in the other pan with perfect smoothness and at any desired rate. I effected this by placing a light cylindical glass vessel, half filled with water, in the other scale pan, and suspended a broad plunger above it on the "Roberval" principle.

When the plunger was depressed, the water rose in the graduated glass cylinder, and the effect was exactly the same as if an equivalent amount of water had been poured in; conversely, the water sank when the plunger was raised. The action of the instrument seems perfect, but it exists as yet only as a working model.

A useful set of tests of judgment of absolute weights might be added, such as by requiring vessels to be filled with sand, till in the judgment of the examinee the one should weigh a pound, and the other an ounce, and then setting them in scales and recording the percentage of error. Similarly as regards absolute length, as by pulling out slides until they measured respectively a yard, a foot and an inch, and then opening a flap and displaying the test graduations in percentages of the yard, foot, and inch.

I will not take up time by describing other contrivances more or less promising that I have thought of but not actually used, and will now conclude by submitting the points on which I have dwelt to discussion, adding that I should also feel sincerely obliged by any helpful remarks that may be sent to me in writing.

f. 8r

APPENDIX

(Chiefly extracted from the 1*d. book sold by Authority at the Exhibition).*

The object of the Anthropometric Laboratory is to show to the public the great simplicity of the instruments and method by which the chief physical characteristics may be measured and recorded. The instruments at present in action deal with Keenness of Sight; Colour-Sense; Judgment of Eye; Hearing; Highest Audible Note; Breathing Power; Strength of Pull and Squeeze; Swiftness of Blow; Span of Arms; Height, standing and sitting; and Weight.

Such is the ease of working the instruments that a person can be measured in all these respects, and a card containing the results furnished to him, while a duplicate is made and preserved for statistical purposes, at a total cost of 3*d*.

The use of periodical measurements is two-fold, personal and statistical. The one shows the progress of the individual; the other that of portions of the nation, or of the nation as a whole.

Description of the Laboratory.

A space 36 feet long by 6 feet wide is fenced off from the side of a gallery by open lattice work. It is entered by a door at one end, and is quitted by a second door at the other. The public can easily see through the lattice work, while they are prevented from crowding too close. A narrow table runs half-way down the side of the laboratory, on which the smaller instruments are placed. The measurements with the larger ones take place in the open space beyond the table.

The successive stations for the various operations lie in the following order:—

1. Desk at which the newly-entered person writes down certain data concerning himself.
2. Standard specimens for colour of eyes and hair.
3. Sight: (*a*) its keenness; (*b*) the colour-sense; (*c*) judgment of the eye in estimating length and squareness.
4. Hearing: (*a*) its keenness (scarcely practicable on account of the noise and echoes); (*b*) highest audible note.
5. Touch (exhibition of various apparatus).
6. Breathing capacity.
7. Swiftness of blow with fist.
8. Strength: (*a*) of pull; (*b*) of squeeze with right and with left hands.
9. Height: (*a*) when sitting, measured from the seat of the chair; (*b*) standing in shoes; (*c*) the thickness of the heel of the shoe.
10. Span of the arms.
11. Weight.

Process gone through.

1. THE DESK.—On payment of 3*d.* at the door, the applicant is admitted to the desk, and given a frame which contains a card, over which thin transfer paper is stretched. Carbonised paper is placed between them. Thus a duplicate copy of the entries is obtained, to be kept for statistical purposes. The card with the entries upon it is given to the person measured.

No names are asked for. The following plan is adopted to secure such data for the duplicate copy as are needful for its use as a statistical document, without annoying the applicant, who may be disinclined to parade his or her age, &c., on the card. The transfer paper is doubled over the back of the card, and no carbonized paper is put behind the flap; consequently what may be written upon it will not appear on the card. The particulars required on the flap are: Age last birthday; birthplace; state (married, unmarried, or widowed); residence, whether urban, suburban or country; occupation. All this takes place at the first station, which is partially curtained for the sake of privacy.

When these data have been written, the frame is turned over, and the other side is henceforth uppermost. On this the attendant marks the sex, and the applicant writes his initials or other distinguishing mark, to guard against any accidental interchange of the frames belonging to different persons who are simultaneously undergoing measurement.

At this same station is suspended a card, with specimens of wool of various shades of green worked upon it. Attention is directed to these specimens, that the applicant may clearly understand what will be required of him a few stations on, when his colour-sense is tested by his being asked to pick out all the green shades from among many wools of different colour. It is important that he should appreciate the wide variety of shades that are used, otherwise he may fail in the test, owing to a misunderstanding of what he is wanted to do.

2. COLOUR OF EYES AND HAIR.—Artificial eyes of standard colours are exhibited, together with the following descriptive names—dark-blue, blue, grey, dark-grey, brown-grey, (green, light hazel), brown, dark-brown, black. The attendant will note the colour of the eyes, but no entry will be made regarding the colour of the hair, for the reason that what with the darkening effects of pomades, and of dyes, and the misleading appearances of false hair, no useful results could be arrived at. However, for the convenience of the visitor, samples of standard colour of hair are exhibited, and the names are attached by which the chief varieties of colour are usually described. They are flaxen, light-brown, brown, dark-brown, fair red (golden), red, dark red (chestnut auburn), black.

3. SIGHT.—(*a*) *Keenness of Eye-sight* is measured by the greatest distance at which the small print known as "diamond" type can be read.

The eyes are tested separately, as it often occurs that they differ considerably in efficiency without the person being aware of the fact, who ought in that case to use appropriate glasses.

The apparatus (Plate XII, fig. 1) is a long and light frame with a single eye-hole. Blocks of wood about $1\frac{1}{4}$ inch wide and $2\frac{1}{2}$ inches high, each with a sentence in diamond print pasted upon its face, are fastened square to the line of sight at distances of 7, 9, 11, and so on up to 41 inches. The number of inches is painted in bold figures on the upper part of the face of each block. The blocks are disposed in a curve, so that when viewed from the eye-hole each stands just clear of the preceding one (see fig. 1*a*); the curve of the frame is, in fact, an equiangular spiral. First the right eye is tested, and then the left eye, and the greatest distance at which the type can be read by each of them is recorded. If the print cannot be read at all by the unaided eye, a cross is marked on the schedule.

b. Colour-Sense.—A series of bars are packed closely side by side in a frame, looking something like the keys of a pianoforte. Fig. 2, Plate XII, shows only a portion of the instrument, as the right hand part has been broken off in order to exhibit its construction more distinctly. The two flaps are half opened for the same reason. When the upper flap is closed, the part B keeps the bars in an even row, and the part C nips their tops. When the lower flap is closed, the numbers on the bars are hidden. Along the middle part of each of these bars a differently coloured wool is wound lengthways, and the foot of each bar is stamped with a separate number. In the frame there are as many peg-holes as there are bars, one hole to each bar. The order of the bars can be changed when the instrument is unlocked. The frame is placed before the person to be tested, the numbers are hidden by the flap A, and he is required to insert a peg opposite each of the bars that has any shade of green wound round it. After he has leisurely done this to his satisfaction the attendant lifts up the flap and displays the numbers of the chosen colours, and records the fact of his having judged rightly or wrongly as the case may be.

c. Judgment of Eye as regards Length.—A board (Plate XIII, fig. 3) has two pairs of parallel strips of wood fastened across it, between each of which a bar slides freely. In each case a square rod, 15 inches long and somewhat longer than the bar, is hinged to it along its edges, and when closed down upon it, hides it altogether. There is a movable pointer attached to the lower of each pair of strips. The position of the pointers is shown in the figure, but the scale of the drawing is too small to show the slot and the rest of the easily-to-be-imagined arrangement by which they are rendered movable. In the one pair, the pointer is set somewhere about midway, and the person to be tested is desired to slide the rod until its middle is brought as nearly as he can judge opposite the pointer. When he has done this, the hinged rod is lifted and the face of the bar is exposed. This has a central fiducial mark, and bears graduations on either side of it each equal to $\frac{1}{100}$ of the total length of the rod. The error of adjustment is thus determined in percentage.

The second rod has to be set so that the pointer shall correspond to one-third of its length, and the error of adjustment is similarly read off in units, each equal to a hundreth part of the total length of the rod.

As regards Squareness.—A board (Plate XIII, fig. 4), including a sector of a circle, has an arm movable about the centre of the circle, while a broad flap of which the last part is supposed in the figure to have been broken off, hides its free ends. A black line AB is drawn across the board. The person tested is desired to set the arm as squarely as he can to the black line. When he has done this, the attendant lifts the flap and exposes a scale of degrees graduated on the foot of the board, and reads off the error of the setting of the arm in degrees.

HEARING.—(*a*) *Its Keenness.*—Some apparatus is exhibited by which at least the relative acuteness of hearing can be tested; but it will not be used, as the noises and echoes of the building render such determinations untrustworthy.

(*b*) *Highest audible Note.*—An india-rubber tube communicates through 5 others with 5 fixed whistles of small bore, and of depths that will give 50, 40, 30, 20, and 10 thousand air vibrations in a second respectively—that is, of the several depths of 0·067, 0·084, 0·113, 0·169, and 0·380 inch. Each tube is nipped by a separate clamp. These are numbered in order, 5, 4, 3, 2, 1, and serve as keys. When any one of them is depressed, air is blown through the corresponding whistle, and is thrown into vibrations which can be heard by some as a shrill and pure note, while others hear merely a puff or nothing at all. Every person has his limits of power of hearing high notes, quite independently of the general acuteness of his hearing. The test lies in ascertaining which is the shrillest of the five notes that is audible. The precise limit of audible sound may be found by using a whistle that has a movable plug for its base. The larger of the small whistles are made by Messrs. Tisley & Co., 172, Brompton Road; the smaller and more delicate ones are made by Mr. Hawkesley, 357, Oxford Street.

TOUCH, &c.—Several instruments are exhibited, but it is not proposed to test with them, as the requisite time cannot be spared.

BREATHING CAPACITY.—A spirometer is used, made by a counterpoised vessel suspended in water. When the air is breathed into it through a tube, the vessel rises, and the scale at its side shows the number of cubic inches of displacement. The person to be tested fills his chest and expires deeply three or four times for practice, then, after a few seconds' rest, he tries the spirometer. Spirometers are usually furnished with a stop-cock to the breathing tube, which is intended to be closed when the expiration has ceased. An inverted glass syphon with a little water in it is connected with the breathing tube beyond the stop-cock. If the water does not stand at the same level in the two arms of the syphon it would show that the air in the spirometer was somewhat compressed or dilated as the case might be, and the air cylinder would have to be slightly adjusted before reading off. However, the error caused by neglecting this manometer rarely exceeds 4 cubic inches, and may be disregarded.

Swiftness of Blow.—A flat bar (Plate XIII, fig. 5) with a pad, P, at one end runs freely between guides. The blow is delivered with the fist straight at the pad, driving the rod nearly or quite home, or else the blow is converted into a pull by holding a stirrup attached to a string, and striking out into space. The stirrup is attached to a string or, better, to a piece of steel pianoforte wire which is looped round a catch that forms part of a little apparatus attached to the bar, and which is shown enlarged in fig. 5*a*. When the bar is in full motion the catch releases the string or wire, so that there is nothing to break. The swiftness of the motion of the bar is registered as follows:—Across its path a bridge is fixed and a flat steel rod projects from the bridge, lying above the bar and parallel to it. Its free end points in the same direction as that towards which the bar is driven by the fist. When the bar is set back ready for use, an arm, A, turning round a pin fixed in the framework is set so as to push the spring forcibly to one side, but as soon as the bar begins to move, a stud that is fixed to the bar strikes the arm from before it, and so releases the spring, which thereupon vibrates transversely to the moving bar. A pencil is attached to the spring, and the upper face of the bar carries a strip of the prepared cardboard used for white flexible slates. The pencil leaves a sinuous trace on the strip as shown in the lower figure, and the points where the trace crosses its own median line can be measured with precision. The spring that is used makes twenty-five complete vibrations in a second. Hence, if the interval between any two alternate crossing-points is 0·48 inch in length, the bar is travelling 1 foot per second. A scale is constructed of which the unit is 0·48 of an inch, and the graduations upon it are in feet per second. By applying this scale to the curve, the swiftness of the corresponding blow is immediately read off.

Strength (*a*) *of pull.*—The well-known instrument with a spring, dial, and pointer, made by Salter, is held as an archer holds his bow when in the act of drawing it, and the strength of the pull is given by the index.

(*b*) *Of squeeze.*—The instrument, also made by Salter, is tried first in the right hand, secondly, in the left hand.

Span of Arms.—A pair of rods, sliding over each other and with projections at either end, is held so that the tips of the fingers press against those projections; then the arms are extended to their full stretch. The graduations show the span.

Height (*a*) *above seat of chair.*—A quickly acting measuring-rod is fastened upright to the back of a solid and narrow chair.

(*b*) *Standing in shoes.*—This is taken by a measuring-rod fixed against the wall.

(*c*) The thickness of the heel of the shoe is measured.

Lastly *c* is subtracted from *b*, which gives—

(*d*) The height without shoes.

Weight.—A simple commercial balance is used, as cheaper, more accurate, and much more capable of bearing hard usage than the lever balances. Its sole disadvantage lies in the necessity of handling heavy weights during its use. Overcoats should be taken off, the weight required being that of ordinary indoor clothing.

Most of the instruments in use at the laboratory are wholly or in large part of my own designing. Those that are not are the spirometer, the instruments for testing strength of pull and of squeeze, and the weighing machine.

On the opposite page is a *fac simile* of the Schedule which was retained at the Anthropometric Laboratory. The card that was presented to each person examined was a duplicate of all the entries in the Schedule, except those printed crosswise at the right hand side.

Explanation of Plates XII and XIII.

(*For description, see both the Memoir and the Appendix to it.*)

Fig. 1. Instrument for testing keenness of sight.

„ 1*a*. Diagram showing how each of the blocks appears to stand just free of the preceding one when they are viewed through the eye-hole.

„ 2. Part of the apparatus for testing colour-sense by various samples of coloured wools. The right hand portion is supposed to be broken off.

„ 3. Apparatus for testing the accuracy of the judgment of the eye, in dividing a rod into two, and into three equal parts.

„ 4. Apparatus for testing the judgment of the eye as regards squareness. The left hand portion of a flap that conceals graduations is supposed to be broken off.

„ 5. Apparatus for testing swiftness of blow or pull.

„ 5*a*. Shows the mechanism of a self-acting catch, which releases the string by which the rod is pulled just before the rod comes home.

INTERNATIONAL HEALTH EXHIBITION, 1884.

ANTHROPOMETRIC LABORATORY,

Arranged by Francis Galton, F.R.S.

Sex — Colour of eyes

EYESIGHT.

	right eye	left eye
Greatest distance in inches, of reading "Diamond" type		

Colour-sense, goodness of

JUDGMENT OF EYE.

	in three parts	in two parts
Error per cent. in dividing a line of 15 inches		

Error in degrees of estimating squareness

HEARING.

Keenness can hardly be tested here owing to the noises and echoes.

Highest audible note — between { 0·000 and 0·000 } vibrations per second.

BREATHING POWER.

Greatest expiration in cubic inches

Date — Initials

SWIFTNESS

of blow of hand in feet per second

STRENGTH

of squeeze in lbs. of { right hand / left „ } of pull in lbs.

SPAN OF ARMS

From finger tips of opposite hands — feet, — inches.

HEIGHT

Sitting, measured from seat of chair — feet, — inches.

Standing in shoes — feet, — inches.

less height of heel.. inches.

Height without shoes — feet, — inches.

WEIGHT

in ordinary in-door clothing in lbs.

Age last birthday? ______________________

Married or unmarried? ______________________

Birthplace? ______________________

Occupation? ______________________

Residence in town, suburb, or country? ______________________

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APPENDIX II.

THE value of the results obtained at the laboratory may be questioned on the ground that the persons who applied to be measured were not random specimens of the crowd who visited the Exhibition, and that the latter themselves were no fair sample of the British population, nor of any well-defined section of it. I have no reply to make to this objection, except that it should not be pushed unreasonably far. On the other hand, it may justly be claimed that results which, taken each by itself, have no great value as absolute determinations, may nevertheless be of considerable importance relatively to one another, by affording materials for testing the relations between various bodily faculties and the influences of occupation and birthplace. Their discussion in any form is a laborious task, and the portion of it that I now submit is very far indeed from exhausting the uses to which the laboratory records can be put. It deals mostly with the very results that I have just spoken of as being the least valuable; but I have taken them in hand first, because it was a necessary preliminary to any further discussion. I also wished to utilise the copious material at my disposal to exemplify what I trust will be found a convenient development of a method of statistical treatment I have long advocated, by presenting in a compact and methodical form (Table II) a great deal more concerning the distribution of the measurements of man than has hitherto been attempted in a numerical form.

The following brief summary of maximum measurements will be interesting:—

9,337 persons were measured, of whom 4,726 were adult males, and 1,657 adult females. The highest records during the whole time that the laboratory was open were those shown in Table I.

TABLE I.

—	Highest recorded cases among	
	4,726 Adult Males.	1,657 Adult Females.
Height without shoes, in inches	79·5	70·3
Weight, in lbs.	308	222
Breathing capacity, in cubic inches.. ..	354	270
Strength of pull, in lbs.	148	89
Strength of squeeze, in lbs.	112	86
Swiftness of blow, in feet per second ..	29	20
Sight distance, in inches, of reading diamond test-type	39	40

The meaning of Table II, and that of the new word "per-centile" which is defined in the heading to that Table, will be understood by the help of a single example, for which I will take the line referring to Strength of Squeeze among males. We see that a discussion was made of 519 measurements in that respect, of men whose ages ranged between 23 and 26; that 95 per cent. of them were able to exert a squeeze with their strongest hand (the squeeze was measured by a spring dynamometer) that surpassed 67 lbs. of pressure; that 90 per cent. could exert one that surpassed 71; 80 per cent. one that surpassed 76; and so on. The value which 50 per cent. exceeded, and 50 per cent. fell short of, is the Median Value, or the 50th per-centile, and this is practically the same as the Mean Value; its amount is 85 lbs. This line of the Table consequently presents an exact and very complete account of the distribution of strength in one respect among the middle 90 per cent. of any group of males of the tabular ages similar to those who were measured at the laboratory. The 5 per cent. lowest and the 5 per cent. highest cannot be derived directly from it, but their values may be approximately inferred from the run of the tabular figures, supplemented by such deductions as the Law of Error may encourage us to draw. Those who wish to apply this law will note that the "probable error" is half the difference between the 25th and the 75th per-centile, which can easily be found by interpolation, and they will draw the per-centiles that correspond respectively to the median value *minus* twice, three times, and three-and-a-half times the probable error, at the graduations 8·7, 2·4, 0·8, and those that correspond to the median value *plus* those amounts, at the graduations 91·3, 97·6, and 99·2. The Table is a mere statement of observed fact; there is no theory whatever involved in its construction, beyond simple interpolations between values that differ little from one another and which have been found to run in very regular series.

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Table II.—ANTHROPOMETRIC PER-CENTILES.

Values surpassed, and Values unreached, by various percentages of the persons measured at the Anthropometric Laboratory in the late International Health Exhibition.

*(The value that is unreached by n per cent. of any large group of measurements, and surpassed by 100-n of them, is called its n*th *per-centile.)*

Subject of measurement.	Age.	Unit of measurement.	Sex.	No. of persons in the group.	Values surpassed by per-cents. as below: 95 / Values unreached by per-cents. as below: 5	90 / 10	80 / 20	70 / 30	60 / 40	**50 / 50**	40 / 60	30 / 70	20 / 80	10 / 90	5 / 95
Height, standing, without shoes ...	23–51	Inches	M.	811	63·2	64·5	65·8	66·5	67·3	67·9	68·5	69·2	70·0	71·3	72·4
			F.	770	58·9	59·9	61·3	62·1	62·7	63·3	63·9	64·6	65·3	66·4	67·3
Height, sitting, from seat of chair ...	23–51	Inches	M.	1013	33·6	34·2	34·9	35·3	35·4	36·0	36·3	36·7	37·1	37·7	38·2
			F.	775	31·8	32·3	32·9	33·3	33·6	33·9	34·2	34·6	34·9	35·6	36·0
Span of arms	23–51	Inches	M.	811	65·0	66·1	67·2	68·2	69·0	69·9	70·6	71·4	72·3	73·6	74·8
			F.	770	58·6	59·5	60·7	61·7	62·4	63·0	63·7	64·5	65·4	66·7	68·0
Weight in ordinary in-door clothes ...	23–26	Pounds	M.	520	121	125	131	135	139	143	147	150	156	165	172
			F.	276	102	105	110	114	118	122	129	132	136	142	149
Breathing capacity ...	23–26	Cubic inches	M.	212	161	177	187	199	211	219	226	236	248	277	290
			F.	277	92	102	115	124	131	138	144	151	164	177	186
Strength of pull as archer with bow	23–26	Pounds	M.	519	56	60	64	68	71	74	77	80	82	89	96
			F.	276	30	32	34	36	38	40	42	44	47	51	54
Strength of squeeze with strongest hand	23–26	Pounds	M.	519	67	71	76	79	82	85	88	91	95	100	104
			F.	276	36	39	43	47	49	52	55	58	62	67	72
Swiftness of blow... ...	23–26	Feet per second	M.	516	13·2	14·1	15·2	16·2	17·3	18·1	19·1	20·0	20·9	22·3	23·6
			F.	271	9·2	10·1	11·3	12·1	12·8	13·4	14·0	14·5	15·1	16·3	16·9
Sight, keenness of—by distance of reading diamond test-type ...	23–26	Inches	M.	398	13	17	20	22	23	25	26	28	30	32	34
			F.	433	10	12	16	19	22	24	26	27	29	31	32

It may be used in many ways. Suppose, for example, that a man of the tabular age, viz., above 23 and under 26, and who could exert a squeeze of 80 lbs., desired to know his rank among the rest, the Table tells him at once that his strength in this respect certainly exceeds that of 30 per cent. of those who were measured, because if it had been only 79 lbs. it would have done so. It also tells him that his strength does not exceed that of 40 per cent. of the rest, since it would have required a pressure of 82 lbs. to have done this. He therefore ranks between the 30th and the 40th per-centile, and a very simple mental sum in proportion shows his place to be about the 33rd or 34th in a class of 100.

The Table exhibits in a very striking way the differences between the two sexes. The 5th male per-centile of strength of squeeze is equal to the 90th female per-centile, which is nearly but not quite the same as saying that the man who ranks 5th from the bottom of a class of 100 males would rank 10th from the top in a class of 100 females. The small difference between the two forms of expression will be explained further on. If the male per-centiles of strength of squeeze are plotted on ruled paper, beginning with the lowest, and if the female per-centiles are plotted on the same paper, beginning with the highest, the curves joining their respective tops will be found to intersect at the 7th per-centile, which is the value that 7 of the females and 93 of the males just surpass. Therefore, if we wished to select the 100 strongest individuals out of two groups, one consisting of 100 males chosen at random, and the other of 100 females, we should take the 100 males and draft out the 7 weakest of them, and draft in the 7 strongest females. Very powerful women exist, but happily perhaps for the repose of the other sex, such gifted women are rare. Out of 1,657 adult females of various ages measured at the laboratory, we have already seen that the strongest could only exert a squeeze of 86 lbs., or about that of a medium man. The population of England hardly contains enough material to form even a few regiments of efficient Amazons.

The various measurements of males surpass those of females in very different degrees, but in nearly every particular. A convenient way of comparing them in each case is that which I have just adopted, of finding the per-centile which has the same value when reckoned from the lower end of the male series, and from the higher end of the female series. When this has been done, the position of the per-centiles arranged in order of their magnitude are as follows:—Pull, 4; Squeeze, 7; Breathing capacity, 10; Height, 14; Weight, 26; Swiftness of blow, 26; Keenness of sight, 37. We conclude from them that the female differs from the male more conspicuously in strength than in any other particular, and therefore that the commonly used epithet of "the weaker sex" is peculiarly appropriate.

The Table was constructed as follows:—I had groups of appropriate cases extracted for me from the duplicate records by Mr. J. Henry Young, of the General Register Office. I did not care to have the records exhausted, but requested him to take as many as seemed in each case to be sufficient to give a trustworthy result for these and certain other purposes to which I desired to apply them. The precise number was determined by accidental matters of detail that in no way implied a selection of the measurements. The summarised form in which I finally took them in hand is shown in the two upper lines of the following specimen:—

Height, Sitting, of Female Adults, aged 23–50, *in inches.*

29–	30–	31–	32–	33–	34–	35–	36–	37–	
2	8	52	116	226	227	108	31	5	Total 775
2	10	62	178	404	631	739	770	775	Abscissæ 0 to 775
30	31	32	33	34	35	36	37	38	Corresponding Ordinates.

The meaning of the two upper lines is that in a total of 775 observations there were 2 cases measuring 29 and under 30 inches, 8 cases measuring 30 and under 31 inches, and so on. The third line contains the sums of the entries in the second line reckoned from the beginning, and is to be read as follows:—2 cases under 30 inches, 10 cases (=2+8) under 31 inches, 62 cases (=2+8+52) under 32 inches, and so on.

I plotted these 775 cases on French "sectional" paper, which is procurable in long and inexpensive rolls, ruled crossways by lines 1 millimetre apart. I counted the first line as 0° and the 776th as 775°. Supposing the measurements to have been plotted in the order of their magnitude, in succession between these lines, the first would stand between 0° and 1°, the second between 1° and 2°, and so on. Now we see from the Table that the second measurement was just short of 30 inches, consequently the third measurement was presumably just beyond it, therefore the abscissa whose value is 2°, and which separates the second from the third measurement, may fairly be taken to represent the abscissa of the ordinate that is equal to 30 inches exactly. Similarly, the abscissa whose value is 10° divides the measurement that is just under 31 inches from that which is presumably just above it, and may be taken as the abscissa to that ordinate whose precise value is 31°, and so on for the rest. The fourth line of the Table gives the ordinates thus determined for the abscissæ whose values are entered above them in the third line. I dotted the values of these ordinates in their right places on the sectional paper, and joined the dots with a line, which in every case, except the breathing capacity, fell into a strikingly regular curve. (I shall speak further on about this one partial exception.) Per-centiles were then drawn to the curve, corresponding to abscissæ that were respectively 5 per cent., 10 per cent., 20 per cent., &c., of the length of the base line. As the length of the base line was 275, these per-centiles stood at the graduations 13·8°, 27·5°, 55·0°, &c. Their values, as read off on the sectional paper, are those which I have given in the Table.

It will be understood after a little reflection that the 9th rank in a row of 10, the 90th rank in a row of 100, and the 900th rank in a row of 1000, are not identical, and that none of them are identical with the 90th per-centile. There must always be the difference of one half-place between the post which each person occupies in a row of n individuals, numbered from 1 to n, and that of the corresponding graduation of the base on which he stands, and which bears the same nominal value, because the graduations are numbered from 0 to n and begin at a point one half-place short of the first man, and end at one half-place beyond the last man. Consequently the graduations corresponding to the posts of the 9th, 90th, and 900th man in the above example, refer to the distance of those posts from the beginning at 0 of their several base lines, and those distances are related to the lengths of the base lines in the proportions of 8·5 : 10, of 89·5 : 100, and of 899·5 : 1000, which when reckoned in per-cents. of the several base lines are 85, 89·5, and 89·95 respectively. The larger the number of places in the series, the more insignificant does this half-place become. Moreover, the intrusion of each fresh observation into the series separates its neighbours by almost double that amount, and propagates a disturbance that reaches to either end, though it is diminished to almost nothing by the time it has arrived there. We may therefore ignore the existence of this theoretically troublesome half-place in our ordinary statistical work.

There is a latent source of error that might affect such statistics as these, as well as many others that are drawn up in the usual way, which has not, so far as I know, been recognised, and which deserves attention. It is due to uncertainty as to the precise meaning of such headings as 30–, 31–, &c. If the measurements, no matter whether they were made carefully or carelessly, are read off from the instruments with great nicety, then a reading such as 30·99 would fall in the column 30–, and the mean of all the entries in such a column might fairly be referred to a mean value of 3·50.

But if the instruments are roughly read, say to the nearest half inch, the reading of a real instrumental value of 30·99, and even that of a real value of 30·76, would both be entered in the column 31–. The column 30– would then contain measurements whose real instrumental values ranged between 29·75 and 30·75, and the column 31– would contain those that ranged between 30·75 and 31·75; consequently, the means of all the entries in those columns respectively should be referred, not to 30·5 and 31·5, but to 30·25 and to 31·25. Thus an error of a quarter of an inch in the final results might easily be occasioned by the neglect to note and allow for the degree of minuteness with which the instruments were read. No multiplication of measurements would get rid of it, neither would any increase of care in setting the instruments nor any increase in their accuracy. The error of which I speak is purely dependent on the degree of minuteness with which the instruments are read off. I strongly suspect that many statistical tables are affected by this generally unrecognised cause of error. The measurements at my laboratory were read to the nearest tenth of an inch and to a fraction of a pound, so I can afford to disregard this consideration. There was, however, a slight bias in favour of entering round numbers, which should have been, but were not (because I neglected to give the necessary instructions), rateably divided between the columns on either side.

I will now make a few remarks upon the measurements severally, and give some extracts from the numerous MS. tables already prepared, which I propose ultimately to present to the Anthropological Institute, together with the original laboratory records. They will form a valuable addition to those now in their possession, made by the Anthropometric Committee of the British Association, if utilised in connection with future inquiries into the influences of occupation and birthplace.

HEIGHT, STANDING *and* SITTING, *and* SPAN *of* ARMS *in* ADULTS.

A compendious view of the chief linear measurements of the persons examined is afforded by the three data: (1) height standing (without shoes); (2) height when sitting, measured upwards from the seat of the chair; (3) the span of the extended arms measured from the extreme finger tips. From these we can infer with approximate and adequate accuracy the lengths of the trunk, legs, and arms, and the proportion they severally bear to the total stature.

Height Sitting, and Span.

The ratio between height sitting and span varies as is well known during the period of growth, and is different in tall and short adults. The following table shows the relation between the two in persons of both sexes of approximately medium stature, who are between the ages of 23 and 51.

Height Sitting.			Span.		
Inches.	Males. Height 5 feet 8½ inches.	Females. Height 5 feet 3½ inches.	Inches.	Males. Height 5 feet 8½ inches.	Females. Height 5 feet 3½ inches.
31–	..	1	60–	..	3
32–	..	7	61–	..	4
33–	..	39	62–	..	31
34–	4	42	63–	..	23
35–	31	11	64–	..	19
36–	44	..	65–	1	10
37–	19	..	66–	2	3
38–	2	..	67–	3	..
..	..	..	68–	12	..
..	..	..	69–	18	..
..	..	..	70–	27	..
..	..	..	71–	22	..
..	..	..	72–	10	..
..	..	..	73–	3	..
..	..	..	74–	1	..
..	..	..	75–	1	..
	100	100		100	100

Height, Sitting and Standing.

As regards the ratio between height sitting and standing, it does not appear that a moderate increase of tallness in males is associated with a disproportionate increase of length of legs, the ratio of height sitting to height standing being uniform up to 6 feet or more. Its value is 54 : 100; in other words, the ratio of their legs to their trunk is 46 to 54 or thereabouts. When the stature exceeds 6 feet, the length of the legs as compared to that of the trunk increases notably; but my cases are too few to warrant a numerical estimate. As regards females, the case is curiously different. Here an increase of stature is from first to last accompanied by an increase of the length of legs as compared to that of trunk. The data calculated as above are as follows:—For a female stature of 4 feet 10½ inches it is as 45 : 55, for 5 feet 2½ inches 46 : 50, and for 5 feet 6½ inches it is as 47 : 53. As regards taller females, my data distinctly point to a rapid progression in the rate of increase of the relation in question.

Weight.

As regards weight, I have nothing more to say at present.

Breathing Capacity.

The returns show a remarkable regularity in the alteration of the breathing capacity as life advances. It increases rapidly in early youth, and becomes stationary between the ages of 20 and 30 or a little later, and thenceforward steadily declines. I have already alluded to the existence of some irregularity in the run of the per-centiles of breathing capacity, in adults aged from 23 to 51. This is chiefly due, I think, to an unequal representation of the various ages between those limits, and to the somewhat irregular mixture of town and country folk, and of sedentary and active professions among the persons measured.

The following brief abstract gives a very fair epitome of the returns:—

AVERAGE BREATHING CAPACITY

(in cubic inches).

Ages.	Males.	Females.
10	135	121
15	199	138
20	216	142
25	217	137
30	213	137
35	211	136
40	203	123
45	194	119
50	191	118
55	178	111

The superior breathing capacity of the male is partly related to his stature and bulk; it is little in excess of that of females in early life, but becomes half as great again at the age of 20, and that large ratio is more than maintained throughout the whole of the after life.

Strength of Pull and Squeeze in Adults.

The strength of squeeze, as indicated by the instrument, does not keep ahead of that of pull at the highest end of the scale. The difference between them falls off, and is even reversed in the higher figures. I ascribe this wholly to the fault of the instrument, which does not permit the hand to act throughout with the same advantage. The more nearly it squeezes the bars together, the more it closes upon itself, and the less advantageously do the muscles act. It is easy to contrive an adjustment that might offer a similar grip in all cases, but it is not easy to construct one that shall act without additional loss of time. I have thought of a grip that should be forced by a steady increasing pressure, the strain at the moment of forcing it being registered automatically.

Strength of the Right and Left Hands.

I had a batch of about 500 cases of males between the ages of 23 and 51 analysed to determine the relative strength of the right and left hands. Out of every 100 cases about 50 had the right hand strongest; 20, or rather more, had the left hand strongest; and in 30 the strength was the same. A single line out of the table will give a good idea of the whole. The total of the cases to which it refers was only 82, but for the convenience of percentages I have raised it to 100.

	Squeeze of the left hand (in lbs.)										Total cases.
Squeeze of the right hand (in lbs.)	50–	55–	60–	65–	70–	75–	80–	85–	90–	95–	
75–80.	1	1	6	9	26	27	21	6	2	1	100

On the average of all the cases the left hand appears to be about 6 per cent. weaker than the right hand.

While the figures were accessible, I thought it as well to see if by chance there existed any relation between the superior strength of the right or left hand and the superior reading power, as explained farther on, of the right or left eye. There was absolutely none. Had I had means to compare the inferior skill of the right or left hand, which I had not, the existence of some relation would be less improbable.

Strength of Squeeze and Breathing Capacity.

I was surprised to find that there is no close relation between strength of squeeze and breathing capacity. As the measurements are peculiarly trustworthy, being all made with the same instruments and by the same observers, I give the records in full to establish the fact. The importance of a large breathing capacity to a man who expends force rapidly, as to a runner or a mountain climber, is undoubted, but for a strain of short duration it seems comparatively non-essential. Still, I should have thought it to have been more nearly connected with every form of strength than it is. The table shows that an increase of breathing capacity from 150 to 300 inches is accompanied by an increase of strength of squeeze from an average of 75 lbs. to not more than 92 lbs. That is to say, when the breathing capacity is double, the strength of squeeze is on the average only one quarter greater.

BREATHING CAPACITY AND STRONGEST SQUEEZE.—Males, age 23, 24, and 25.

Breathing capacity.	STRONGEST SQUEEZE IN LBS.														
	45–	50–	55–	60–	65–	70–	75–	80–	85–	90–	95–	100–	105–	110–	115–	Total.
Cubic inches.																
70–80	..	..	..	..	1	..	..	..	..	..	..	..	..	..	..	1
100	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
110	..	..	..	..	..	..	..	1	..	..	..	..	..	..	..	1
120	..	..	..	..	1	..	1	..	..	..	..	..	..	..	..	2
130	..	..	..	..	1	..	1	..	..	..	..	..	..	..	..	2
140	..	..	1	..	1	..	1	..	..	..	..	..	..	..	..	3
150	..	..	1	..	1	3	1	1	..	1	..	1	..	..	..	9
160	..	..	..	1	2	3	4	2	1	2	..	..	..	..	..	15
170	..	..	..	..	1	3	7	6	3	1	1	..	..	..	..	22
180	..	..	1	3	3	9	10	6	6	3	1	1	1	..	..	44
190	..	..	1	..	..	4	6	10	3	2	3	2	..	1	..	32
200	..	..	..	1	1	10	7	9	8	9	3	..	1	..	..	49
210	..	..	..	1	4	2	9	11	7	6	5	3	3	..	..	51
220	..	..	..	1	4	6	11	12	11	6	10	4	..	..	..	65
230	..	..	1	1	3	1	6	6	13	5	9	1	2	1	..	49
240	..	..	..	..	1	5	4	10	10	8	7	2	1	..	..	48
250	..	..	..	1	..	1	2	4	8	4	4	3	2	..	..	29
260	..	..	..	..	1	2	3	1	7	4	4	1	..	..	..	23
270	..	..	..	..	..	..	2	3	5	1	2	1	..	1	..	15
280	..	..	..	..	..	..	2	2	1	2	4	3	2	1	..	17
290	..	..	..	..	..	..	..	2	3	7	4	2	1	1	..	20
300	..	..	..	..	1	..	3	2	3	6	2	1	..	..	..	18
310	..	..	..	..	..	..	..	..	1	..	1	1	..	..	..	3
320	..	..	..	..	..	..	..	..	1	..	..	1	1	..	..	3
330	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
340	..	..	..	..	..	..	..	..	..	..	..	1	..	..	..	1
..	..	..	5	9	26	49	80	88	91	67	60	28	14	5	..	522

Eyesight.

The light at the laboratory was rarely sufficient, and it was very variable, since the tests were carried on partly in the daytime, partly when the light was waning, and partly during the evening illumination. The absolute results are therefore of little importance, though they are worth recording, namely, that one quarter of the males of various ages who were able to read small print at all without glasses, were able with one eye to read pages taken from the well-known little shilling prayer-book, printed in diamond type, at a greater distance than 27 inches, one-half of them at a greater distance than 22 inches, and three-quarters at a greater distance than 18 inches. No person at the laboratory succeeded in reading a page further off than 38 inches, though one lady at my own house, probably under better light and using both her eyes, unmistakably exceeded 41 inches.

Though the tests are of little importance absolutely, they are of much value relatively in comparing the power of the two eyes, as to whether on the whole the right eye is stronger than the left, or *vice versâ*, and what is the average difference between their powers. It appeared from an examination of about 850 cases that the number of those whose two eyes were equally effective bore the ratio of 2 to 3 (or a very little more than 3) to the number of those in whom the powers of the two eyes differed to a notable degree. It also appeared, on taking the average of all the 850 cases, that the difference between the greatest reading distance of the two eyes with the above test type, was just 2 inches (or the merest trifle less). And lastly, it appeared that the average strength of the right and of the left eyes was almost exactly the same. Thus with the right eye there were 253 cases in which the greatest reading distance lay between 20 and 24 inches, and with the left eye there were 256 such cases; again, when the greatest distances lay between 25 and 29 inches, the cases were 229 and 224 respectively.

I have nothing of novelty to say regarding the colour sense, as the data, although they have been tabulated, have not yet been discussed.

Highest Note Audible.

The measurements were made with five whistles set to emit 10, 20, 30, 40, and 50 thousand vibrations per second respectively. Notwithstanding the roughness of the measurements, the results fall into a very fair curve; however, it would be hardly justifiable to give per-centiles, because the values on which the curve is based are wide apart. I therefore limit myself to giving a table of the actual observations reduced to percentages for the convenience of comparison. It will be seen here, as in every other faculty that has been discussed, the male surpasses the female; 18 per cent. of the males hear the shrillest test-note, as against 11 per cent. of the

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females, and 34 per cent. of the males hear the next shrillest test-note, as against 28 per cent. of the females.

	Ages.	Percentage of cases in which the under-mentioned number of vibrations were perceived as a musical note. Number of vibrations per second. 20,000	30,000	40,000	50,000	Number of Cases.
Males	23–26	99	96	34	18	206
	40–50	100	70	13	4	317
Females	23–26	100	94	28	11	176
	40–50	100	63	8	1	284

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f. 1

[*FROM THE AUTHOR.*

REMARKS ON REPLIES BY TEACHERS

TO

QUESTIONS RESPECTING MENTAL FATIGUE.

[*Reprinted from the Journ. of the Anthrop. Inst., November*, 1888.]

BY

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f. 2

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REMARKS *on* REPLIES *by* TEACHERS *to* QUESTIONS RESPECTING MENTAL FATIGUE.

By FRANCIS GALTON, F.R.S., President.

THE question of over fatigue of the brain in schools was discussed some months ago with much heat, and the arguments on either side were supported by experiences that so flatly contradicted each other, as to make it difficult to arrive at just conclusions. After the heat of discussion had somewhat cooled down, it happened that I was asked to occupy the chair at a meeting of the Educational Section of the Teachers' Guild, and while doing so I was much impressed by the eager and sustained attention of the large audience to the memoir read on that evening. It occurred to me that the Teachers' Guild might become a powerful instrument for the solution of statistical problems, if the interest of its intelligent members could be excited in inquiries bearing on Education, and if their good will and confidence could be gained. I determined to make a trial, and selected questions bearing on fatigue for the purpose. The Council of the Guild kindly assisted me by circulating my questions, together with a covering letter from their Vice-Chairman, Dr. Morse. The replies to those questions form the basis of the following remarks. Let me say at once, that I was somewhat disappointed in respect to the number and fullness of the replies—so much so, that I long hesitated to publish anything before supplementing them with other materials, to be gained gradually through my own observation, but having much else on hand, it seemed on the whole best to work off this matter at once, without admixture. I have 116 replies from teachers, many of large experience, concerning both themselves and their pupils, and as this is just sufficient to deserve a separate discussion, I shall not travel beyond the bounds of what may be called my brief, I will not enter upon other materials, and barely into the psycho-physics of fatigue, but shall merely set forward in an orderly way the statements contained in the 116 replies.

The objects of my questions were first to determine the signs and effects of incipient fatigue in as *measureable* a form as possible; for it is obviously most desirable to know what the tests of fatigue should be, in consequence of the contradictory opinions above alluded to. There ought to be no room for doubt as to whether the pupils in a particular school or class, and at a particular time, were or were not over fatigued. Secondly, I wished to hear from the teachers whether they had themselves ever broken down from over work, and what their own experiences might be concerning their pupils and friends. The actual questions are subjoined: numbers 1, 2, and 3 regard the person addressed; 4, 5, 6, regard their pupils and acquaintances.

1. What particular mental work can you perform easily, when your mind is fresh, that you find difficult or impossible when your mind is somewhat fatigued? 2. Has illness, due solely to mental overwork, independent of domestic anxiety and worry ever incapacitated you for more than a month at a time from ordinary school work? If so, give dates and symptoms. Do you consider your present health to be in any way affected by that illness? 3. Has experience discovered to you any warning signs, bodily or mental, distinct or obscure, of the imminent approach of mental fatigue, other than the growing sense of becoming fatigued? If so describe them. 4. What particular intellectual work do you find your pupils perform with ease when their minds are fresh, in which they fail more or less when they are mentally fatigued, even though they are still interested in their work? 5. Have you known cases of more or less serious prostration from mental overwork, as distinguished from the effects of domestic or other anxiety? If so, give initials and dates, and a very brief notice of the severity and duration of the illness? 6. Has experience discovered to you any warning signs of imminent mental fatigue among over zealous pupils?

The upshot of the replies to the questions is as follows:—

General Aspect.—Experienced teachers place most dependence on the general aspect of their classes, due to a variety of small indications, such as jaded expression and abnormal skin colour. They more especially speak of a strange look in the eye, which is variously described as dazed, weary, fixed, or lack lustre, as being a peculiarly characteristic indication that work should be slackened at once.

Nervous Irregularities.—Restlessness appears to be the commonest sign of partial fatigue: that is, of the attention being wearied while the muscles are craving to be employed. I may here for one moment break my plan of not travelling beyond my brief by alluding to a short account I wrote in "Nature" three years ago, Vol. xxxii, p. 174, but signed only with my initials, entiled "Measure of Fidget," describing how I had succeeded in counting the varying rate of fidget of a section of a large audience during the reading of a wearisome memoir. I have since frequently tried this method; it is an amusing way of passing an otherwise dull evening, but in drawing conclusions from the number of movements the average age of the audience and their habits of thought have to be taken into account. Children are extraordinary mobile, and those adults who are little accustomed to concentrate their attention, are rarely still except when spell-bound by eloquence. On the other hand I have frequently noticed at meetings of the Royal Society, that as many of the persons present as I could hold in a glance, were all as rigid as statutary for many seconds together. Yawning and lolling are common among tired children, and twitchings and grimaces, which in serious cases culminate in St. Vitus' dance. Here are some extracts from the various replies.

1. Sudden muscular movements. 2. Grimaces, frowning, or compression of the lips are marked signs. 3. The fingers sometimes twitch and the whole nervous system seems affected. 4. Twitching of the face. 5. Twitching, blinking the eyes. 6. Fluttering of the eyelids. 7. Tendency to nervous laughter or movements. One correspondent has fits of sneezing in the early morning when he has been fatigued over night.

General unsteadiness of muscular co-ordination is shown by bad and shaky handwriting: this is sometimes specifically mentioned, but more often implied by such phrases as—8. Careless writing; or, 9. "Failure in all work requiring neatness." 10. Sometimes a loss of power to continue writing, the pen going crooked, &c. Fatigue is also very frequently indicated by disordered utterance as—11. Tendency to stumble over words when speaking. 12. Refusal of the tongue to obey the will, so that in speaking or reading I substitute one word for another.

Irregularity of nervous action is further shown by conditions of pallor or of flushings in the face. They sometimes alternate; testifying to a depression of general nerve power, combined with morbid excitability. Allusions to abnormal skin colour are frequent in the replies. One teacher goes so far as to lay particular stress on the colour of the tips of the ears in deciding whether and in what way the girls of her class are suffering. If the tips are white, flaccid, and drooping she concludes the girls are thoroughly weary in mind. If they are relaxed but purplish, she concludes that they are "tired not with study but from struggling with their nerves, which the average school girl of 14 or 15 very rarely has completely under control."

Headaches.—The frequent occurrence of headaches in varied forms and in every degree of severity may be accepted as a matter of course. Similarly as regards cold feet, faintness and actual faintings. Sleeplessness in a very serious degree is another well-known sign; much more rarely somnolence. Grinding the teeth at night and talking in the sleep are frequently mentioned; somnambulism occasionally so. I do not propose to enter into details respecting any of the matters just mentioned, as they are all of them well known signs of over fatigue. It may, however, perhaps interest the meeting to see a drawing I hold in my hand made in sleep not many weeks ago, by a young friend and connection of my own, who was studying rather too hard for a Government examination. He awoke in the night, and found himself in his nightgown, sitting at his table with the gas burning and with this grotesque sketch of an elephant's head and of some other animals just completed. The ink was still wet. He had not the slightest recollection of anything previous to the act of awakening, but there had been conversation before he went to bed that probably suggested the sketch.

Disposition.—Irritability is perhaps the commonest sign of incipient fatigue. My correspondents freely acknowledge it to be so with themselves and it is very easily noticed among their pupils, who become cross and peevish when tired. I shall not enter further into this, as the fact is a familiar one; it is also well-known that the nerves of sensitive people becomes so irritable by overwork as to be painfully jarred by what they wholly disregard when well, such as the ticking of clocks and the rattle of the street. A most pitiable amount of suffering is disclosed in these replies, due to nervous irritability. Much is said of the gloomy way of looking at life, that is brought on by overwork; of the sense of incapacity, of magnifying trifles, and of dread of society. Irritability is sometimes accompanied by a notable amount of ordinary excitability expressed by such remarks as—1. I get nervous and start at noises. 2. I start sometimes at a sudden noise or movement in the room.

It is, I need hardly say, known by experiment, that both the quickness and the magnitude of the reaction to any stimulus is greatly affected by fatigue.

There is an experiment, not so well known as it should be, that after a class had practice in performing it, can be repeated at any time in a few seconds, which gives an excellent measure of the varying amount of reaction time. The class take hands all round, the teacher being included in the circle, a watch with a seconds hand lies on the table before him. All the pupils shut their eyes. When the seconds hand of the watch comes over a division the teacher gives a squeeze with his left hand to the right hand of the pupil next to him. That pupil forthwith with his left hand squeezes the right hand of the next pupil, and so on. Thus the squeeze travels round the class and is finally received by the right hand of the teacher, who then records the elapsed time since he started it; or he may let it make many circuits before he does so. This interval divided by the numbers of pupils in the class and by the number of circuits, gives the average reaction time of each pupil. The squeeze takes usually about a second of time to pass through each dozen or fifteen persons. We should expect to find uniformity in successive experiments when the pupils are fresh; irregularity and prevalent delay when they are tired. I wish that teachers would often try this simple, amusing, and attractive experiment, and when they have assured themselves that their class enters into its performance with interest and curiosity, they might begin to make careful records at different periods of the day and see whether it admits of being used as a test of incipient fatigue. I should be exceedingly glad to receive accounts of their experiences. Deception must of course be guarded against.

Senses.—The frequency with which serious alteration in the power of hearing and of seeing is mentioned, and the feelings sometimes of intense sensitivity and sometimes of numbness, show that the delicacy of the senses is markedly affected by fatigue.

Hearing is often heightened in keenness, sometimes it is dulled. It is heightened in those numerous cases of irritability of which I have spoken, when the tired brain becomes almost maddened by an organ grinder. It is temporarily paralysed in others. The following is a mixed case:—1. My hearing had never been very acute, and I think the first symptom of fatigue is a feeling of deafness, but at the same time that I cannot hear the voices I want to hear, the outside noises of traffic, bells, &c., become intolerable. Other cases of deafness from fatigue are—2. Inability to hear in school without a painful effort. 3. Increased deafness.

Vision is greatly affected by over fatigue, not only owing to the strain upon the eyes from much reading in a bad light, but apparently through more deeply-seated causes as well. It is difficult otherwise to account for the following interesting case in which colour-blindness was brought on by fatigue and disappeared after rest. It has much physiological interest and well deserves being placed on record. The lady allows me to mention her name for the sake of authenticity. She is Miss J. Beckett, Girls' Grammar School, Ripon.

"After several hard hours of continuous study I have been subject to attacks of colour-blindness, which leave me after resting. The first time I noticed that I was not able to distinguish one colour from another, was when I was reading for the London Matriculation years ago. I was at the same time etching for an American magazine and teaching most of the day. This lasted from Christmas to July, when I began to feel considerably worn out. One day I went to spend a few hours with a friend, and whilst there, began to paint some ivy leaves on a terra-cotta plaque. Imagine my distress when my friend told me the leaves were orange instead of green. On my return I went into my study and to my astonishment, the curtains which were blue in colour looked a kind of dingy yellow. However in a few hours I was quite well. Towards the end of the year I was obliged to give up work on account of my health. I got well, and took up my work again, still subject to temporary colour-blindness when tired." In answer to further inquiries, she adds: "I do not remember whether I have any difficulty in recalling colours when tired. From a little child I have been particularly fond of them, and can readily paint flowers, foliage, and neutral tints from memory."

The frequency and severity with which the sight is affected by fatigue is sufficiently shown by the following extracts:—

1. The eyes fail first. Sometimes after hurrying to a lesson, on my arrival I could not see a single note on the page of music for a few minutes. After writing and playing long, everything goes black or black spots dance up and down. 2. A time of great excitement or worry will so affect my sight that for about half-an-hour at a time, I can see nothing clearly. The outline of everything is deficient in some part, so that I only see half of a thing at a time. There seems to be a bright wheel of light whizzing in the corner of one or the other eye.

3. At first the lines of the page become indistinct, then at intervals they appear to vibrate; finally they merge into one mass.

4. The words appear to rise from the paper and frequently a double row of words are visible.

5. Lights and after images are distinct before my eye.

6. A confusion in the lettering of mathematical diagrams is sometimes an early symptom of fatigue among my pupils.

As regards sensations in the eye itself, beside such remarks as—7. A dazzling and burning sensation in the eye, the following is a case of an affection of the eye being subordinate to that of the brain, rather than *vice versâ*.

8. A nervous sensation in the eyes as though the eyeballs were loose in my head, and would fall which ever way the head is inclined. The sensation is worse on lying down. I am somewhat short-sighted and wear glasses, but only feel this disagreeable sensation when mentally weary, not necessarily through over-reading.

Memory.—A very common and early symptom of fatigue is failure of memory, using that word in the allied senses of recalling ideas at will, or else of former ideas presenting themselves readily by association, or else of the sure association of muscular movements engaged in utterance, with the idea of the words intended to be uttered. I have made extracts of no less than twenty-five cases of failure of memory, out of which I will select half-a-dozen.

1. My first indication of mental failure is an inability to spell common words; my second, an omission of words in writing; my third, sudden forgetfulness of what I am actually saying. 2. Tendency to forget the meaning of words in a foreign language which are usually well known or have been met with quite recently. Tendency to make stupid blunders in subjects in which, when the mind is in full vigour, it is accurate without effort. Simple and obvious mistakes are increased twofold in number, and that throughout the class. 3. Through days and weeks together, the utterance of wrong words or sentences, not intended or desired to be spoken, and the writing of wrong words. 4. Tendency to stumble over words in speaking, and to misplace letters in writing, generally putting them too soon as "Wesday" for "Wednesday." 5. Want of power of calling at will to memory, names and little matters connected with every day life. 6. Some of the pupils never spell correctly when tired.

Arithmetic and Mathematics.—The studies that are the first to fail under fatigue differ in different individuals, but in the majority of cases those of arithmetic and elementary mathematics go soonest. Though many of the 116 replies come from teachers who have little, if anything, to do with those subjects, no less than forty-seven specifically mention them. For example:—

1. The merely mechanical processes of arithmetic become bewildering at the end of a day in which I have been particularly engrossed with school work.

2. Arithmetic and algebra become impossible when fatigued, not as being disagreeable or painful, but because I then blunder so much that it is hardly any use attempting them.

3. Another correspondent speaks of the impossibility when fatigued of doing work that requires both accuracy of detail and a certain force of will to fix the attention, such as arithmetic.

4. Speaks of the difficulty to tired boys of working out any *common sense* problem in arithmetic.

Though very many similar answers could be quoted in corroboration of these, there are two that tell in an opposite direction. They are—

5. Whenever my mind is wearied, it affords me a certain amount of relief to do some work which involves the solving of arithmetical and algebraical problems, and by preference such as call for the use of logarithms or of the slide rule.

6. I find accounts a great rest when I cannot exert my mind usefully in any other way.

I may be permitted again to break my rule by adding a case

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from my own knowledge of a very distinguished man, now deceased, who having always found repose in his favourite mathematics when he was fagged and worried by multifarious duties, naively recommended the same remedy to a friend whose brain had so broken down for a time, that he shrank from the least mental exertion as from a fatal danger.

Languages.—A difficulty in translating is another of the noticeable effects of incipient fatigue, and is partly due to the lapses of memory already spoken of.

1. In translating, words and phrases do not occur readily to the mind.

2. Translation into or out of a foreign language with which I am not very familiar.

3. I have occasionally lost the power of speaking German when fatigued, though when in my ordinary condition I speak it without conscious effort.

The failure to translate *well* is due of course to much more than the simple failure of memory in small things and depends on the loss of power of grasp, and on depressed mental vigour generally. The following is an instructive case :—

When I taught young boys of ages 8 to 13, all day, I took arithmetic and Latin in the morning, and English reading, geography, &c., in the afternoon. On some occasions the Latin lesson got put off till the afternoon, and I was surprised to find that lesson, which was always a successful one in the morning, failed entirely in the afternoon. The boys wished to learn but could not. Their ordinary work, which made less demand on the intellect, they did in the afternoon well enough.

This and such like cases fall more properly in the next division.

Failure of Mental Grasp.—The evidences that the fatigued mind is unable to work up to its normal standard, and that it wastes itself in futile exertion, are very numerous. They are such as :—

1. Failure of ability to grasp the meaning of even simple things.

2. Failure of the *portative* memory. In reading complete inability to take in the matter whilst mechanically scanning the page. A curious incapacity to count the cups when serving tea.

3. Reading sentences without recognition of what was read.

4. Confusion alternating with excessive clearness of thought.

5. Tendency of thoughts to wander. Failure in pupils to grasp the meaning of what is said to them quickly and fully.

6. Before the actual sense of fatigue is distinctly felt, I am

conscious of a want of power to grasp ideas, and of an incapacity for conveying them clearly.

7. Inability to read the "Journal of Education."

8. Rapid disappearance of immediately preceding concepts, and hence difficulty in establishing connections between paragraphs, as in writing a Review article.

9. Tendency to *use long words.* (This strikes me as a very suggestive reply).

10. Any book in which the language is wanting in ease and simplicity, though its subject may be familiar or easily understood.

In short, to use a common and vigorous phrase, the mind ceases to *bite,* when it is fatigued.

Failure of Energy.—It requires no evidence to corroborate the well known fact that energy fails as fatigue increases. New subjects are distasteful; teaching dullards becomes almost an impossibility. Sustained effort, vigorous inspection, quick decision—all are impossible.

Possibility of Tests of Incipient Fatigue.—The replies I have received do not contain any distinct proposition of tests of incipient mental fatigue, and I am myself far too ignorant of the practice of education to venture to formulate any. On the other hand, the replies are not deficient in indications of what such tests might be directed to ascertain. They are principally as follows:—

1. The length of time during which neatness of execution can be sustained in performing a prolonged task.

2. Promptness and sureness of memory in simple things.

3. Common sense arithmetical problems.

4. Reaction time.

The measure of fatigue is inversely the measure of endurance, and this strikes me as being a faculty that well deserves investigation. Under the strain and exhausting calls of modern civilized life, the power of endurance is rising continually in importance. Men and women have now a-days to act rapidly and for many hours, and not only to act exceptionally well. It therefore seems very reasonable that teachers should direct their attention to some fair way of appraising the relative power of endurance among their pupils. It is of course incidentally discovered in the ordinary course of tuition, but one would like to see appropriate tests directly applied to determine it, and such as would show at any time in a definite and unmistakeable manner whether the minds of pupils were fagged or not.

Breaking Down.—I now come to the evidence given in these replies respecting the frequency with which both pupils and teachers are found to "break down." There is an intelligible and very transparent tendency in not a few of the respondents to say that such a thing as overwork is impossible in their respective schools. Some of them protest so much and so extravagantly as to raise not a little suspicion. There are even a few who say they have never heard of a case of breaking down.

Taking all the replies together, I find that out of my 116 correspondents no less than 23 of them have at some period of their lives broken down, and that 21 of these have never wholly recovered the effects. There are six other cases of a less serious kind, some of them slight. In other words one out of five teachers has, so far as the evidence before me goes, been severely stricken. As to the cases well known to my correspondents, there is vagueness in some of the replies where the word "several," and the like, are used, to which I am quite unable to assign a numerical value, but 59 sad cases are specified in detail in answer to the question 5 : "Have you known cases of more or less serious prostration from mental overwork, as distinguished from the effects of domestic or other anxiety ? If so, give initials and dates, and a very brief notice of the severity and duration of the illness."

In many other cases the writers express the difficulty they feel in distinguishing between worry and overwork. The latter is a consequence of the former, while the former often results from the gloom, anxiety, and sense of incapacity caused by the latter. It is a self regenerating circle of evil.

I draw two conclusions from the replies. The first is that the reason why mental fatigue leaves effects that are so much more serious than those of bodily fatigue is largely owing to the cause just mentioned. When a man is fatigued in body he has very similar symptoms to many of those mentioned above, but there is a great after difference. As soon as the bodily exertion has closed for the day, the man lies down and his muscles have rest; but when the mentally fatigued man lies down, his enemy continues to harass him during his weary hours of sleeplessness. He cannot quiet his thoughts and he wastes himself in a futile way.

The other conclusion is that breaks down usually occur amongst those who work by themselves, and not among pupils whose teachers keep a reasonable oversight. Over zealous pupils are rare, as many of my correspondents insist. But the danger is not so much at school, when the hours of study and those of play and exercise are fixed, as it is at the age when young persons are qualifying themselves for the profession of a teacher, and who have also to support themselves, and perhaps to endure domestic trials at the same time. Dull persons protect their own health of brain by refusing to overwork. It is among those who are zealous and eager, who have high aims and ideas, who know themselves to be mentally gifted, and are too generous to think much of their own health, that the most frequent victims of overwork are chiefly found.

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E. S. Galton

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[FROM THE AUTHOR.

P. 11

REMARKS ON REPLIES BY TEACHERS

TO

QUESTIONS RESPECTING MENTAL FATIGUE.

[*Reprinted from the Journ. of the Anthrop. Inst., November, 1888.*]

BY

FRANCIS GALTON, F.R.S.,
President of the Anthropological Institute.

LONDON:
HARRISON AND SONS, ST. MARTIN'S LANE,
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1888.

f. 12

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f. 13r

Remarks *on* Replies *by* Teachers *to* Questions respecting Mental Fatigue.

By Francis Galton, F.R.S., President.

The question of over fatigue of the brain in schools was discussed some months ago with much heat, and the arguments on either side were supported by experiences that so flatly contradicted each other, as to make it difficult to arrive at just conclusions. After the heat of discussion had somewhat cooled down, it happened that I was asked to occupy the chair at a meeting of the Educational Section of the Teachers' Guild, and while doing so I was much impressed by the eager and sustained attention of the large audience to the memoir read on that evening. It occurred to me that the Teachers' Guild might become a powerful instrument for the solution of statistical problems, if the interest of its intelligent members could be excited in inquiries bearing on Education, and if their good will and confidence could be gained. I determined to make a trial, and selected questions bearing on fatigue for the purpose. The Council of the Guild kindly assisted me by circulating my questions, together with a covering letter from their Vice-Chairman, Dr. Morse. The replies to those questions form the basis of the following remarks. Let me say at once, that I was somewhat disappointed in respect to the number and fullness of the replies—so much so, that I long hesitated to publish anything before supplementing them with other materials, to be gained gradually through my own observation, but having much else on hand, it seemed on the whole best to work off this matter at once, without admixture. I have 116 replies from teachers, many of large experience, concerning both themselves and their pupils, and as this is just sufficient to deserve a separate discussion, I shall not travel beyond the bounds of what may be called my brief, I will not enter upon other materials, and barely into the psycho-physics of fatigue, but shall merely set forward in an orderly way the statements contained in the 116 replies.

The objects of my questions were first to determine the signs and effects of incipient fatigue in as *measureable* a form as possible; for it is obviously most desirable to know what the tests of fatigue should be, in consequence of the contradictory opinions above alluded to. There ought to be no room for doubt as to whether the pupils in a particular school or class, and at a particular time, were or were not over fatigued. Secondly, I wished to hear from the teachers whether they had themselves ever broken down from over work, and what their own experiences might be concerning their pupils and friends. The actual questions are subjoined: numbers 1, 2, and 3 regard the person addressed; 4, 5, 6, regard their pupils and acquaintances.

1. What particular mental work can you perform easily, when your mind is fresh, that you find difficult or impossible when your mind is somewhat fatigued? 2. Has illness, due solely to mental overwork, independent of domestic anxiety and worry ever incapacitated you for more than a month at a time from ordinary school work? If so, give dates and symptoms. Do you consider your present health to be in any way affected by that illness? 3. Has experience discovered to you any warning signs, bodily or mental, distinct or obscure, of the imminent approach of mental fatigue, other than the growing sense of becoming fatigued? If so describe them. 4. What particular intellectual work do you find your pupils perform with ease when their minds are fresh, in which they fail more or less when they are mentally fatigued, even though they are still interested in their work? 5. Have you known cases of more or less serious prostration from mental overwork, as distinguished from the effects of domestic or other anxiety? If so, give initials and dates, and a very brief notice of the severity and duration of the illness? 6. Has experience discovered to you any warning signs of imminent mental fatigue among over zealous pupils?

The upshot of the replies to the questions is as follows:—

General Aspect.—Experienced teachers place most dependence on the general aspect of their classes, due to a variety of small indications, such as jaded expression and abnormal skin colour. They more especially speak of a strange look in the eye, which is variously described as dazed, weary, fixed, or lack lustre, as being a peculiarly characteristic indication that work should be slackened at once.

Nervous Irregularities.—Restlessness appears to be the commonest sign of partial fatigue: that is, of the attention being wearied while the muscles are craving to be employed. I may here for one moment break my plan of not travelling beyond my brief by alluding to a short account I wrote in "Nature" three years ago, Vol. xxxii, p. 174, but signed only with my initials, entiled "Measure of Fidget," describing how I had succeeded in counting the varying rate of fidget of a section of a large audience during the reading of a wearisome memoir. I have since frequently tried this method; it is an amusing way of passing an otherwise dull evening, but in drawing conclusions from the number of movements the average age of the audience and their habits of thought have to be taken into account. Children are extraordinary mobile, and those adults who are little accustomed to concentrate their attention, are rarely still except when spell-bound by eloquence. On the other hand I have frequently noticed at meetings of the Royal Society, that as many of the persons present as I could hold in a glance, were all as rigid as statutary for many seconds together. Yawning and lolling are common among tired children, and twitchings and grimaces, which in serious cases culminate in St. Vitus' dance. Here are some extracts from the various replies.

1. Sudden muscular movements. 2. Grimaces, frowning, or compression of the lips are marked signs. 3. The fingers sometimes twitch and the whole nervous system seems affected. 4. Twitching of the face. 5. Twitching, blinking the eyes. 6. Fluttering of the eyelids. 7. Tendency to nervous laughter or movements. One correspondent has fits of sneezing in the early morning when he has been fatigued over night.

General unsteadiness of muscular co-ordination is shown by bad and shaky handwriting: this is sometimes specifically mentioned, but more often implied by such phrases as—8. Careless writing; or, 9. "Failure in all work requiring neatness." 10. Sometimes a loss of power to continue writing, the pen going crooked, &c. Fatigue is also very frequently indicated by disordered utterance as—11. Tendency to stumble over words when speaking. 12. Refusal of the tongue to obey the will, so that in speaking or reading I substitute one word for another.

Irregularity of nervous action is further shown by conditions of pallor or of flushings in the face. They sometimes alternate; testifying to a depression of general nerve power, combined with morbid excitability. Allusions to abnormal skin colour are frequent in the replies. One teacher goes so far as to lay particular stress on the colour of the tips of the ears in deciding whether and in what way the girls of her class are suffering. If the tips are white, flaccid, and drooping she concludes the girls are thoroughly weary in mind. If they are relaxed but purplish, she concludes that they are "tired not with study but from struggling with their nerves, which the average school girl of 14 or 15 very rarely has completely under control."

Headaches.—The frequent occurrence of headaches in varied forms and in every degree of severity may be accepted as a matter of course. Similarly as regards cold feet, faintness and actual faintings. Sleeplessness in a very serious degree is another well-known sign; much more rarely somnolence. Grinding the teeth at night and talking in the sleep are frequently mentioned; somnambulism occasionally so. I do not propose to enter into details respecting any of the matters just mentioned, as they are all of them well known signs of over fatigue. It may, however, perhaps interest the meeting to see a drawing I hold in my hand made in sleep not many weeks ago, by a young friend and connection of my own, who was studying rather too hard for a Government examination. He awoke in the night, and found himself in his nightgown, sitting at his table with the gas burning and with this grotesque sketch of an elephant's head and of some other animals just completed. The ink was still wet. He had not the slightest recollection of anything previous to the act of awakening, but there had been conversation before he went to bed that probably suggested the sketch.

Disposition.—Irritability is perhaps the commonest sign of incipient fatigue. My correspondents freely acknowledge it to be so with themselves and it is very easily noticed among their pupils, who become cross and peevish when tired. I shall not enter further into this, as the fact is a familiar one; it is also well-known that the nerves of sensitive people becomes so irritable by overwork as to be painfully jarred by what they wholly disregard when well, such as the ticking of clocks and the rattle of the street. A most pitiable amount of suffering is disclosed in these replies, due to nervous irritability. Much is said of the gloomy way of looking at life, that is brought on by overwork; of the sense of incapacity, of magnifying trifles, and of dread of society. Irritability is sometimes accompanied by a notable amount of ordinary excitability expressed by such remarks as—1. I get nervous and start at noises. 2. I start sometimes at a sudden noise or movement in the room.

It is, I need hardly say, known by experiment, that both the quickness and the magnitude of the reaction to any stimulus is greatly affected by fatigue.

There is an experiment, not so well known as it should be, that after a class had practice in performing it, can be repeated at any time in a few seconds, which gives an excellent measure of the varying amount of reaction time. The class take hands all round, the teacher being included in the circle, a watch with a seconds hand lies on the table before him. All the pupils shut their eyes. When the seconds hand of the watch comes over a division the teacher gives a squeeze with his left hand to the right hand of the pupil next to him. That pupil forthwith with his left hand squeezes the right hand of the next pupil, and so on. Thus the squeeze travels round the class and is finally received by the right hand of the teacher, who then records the elapsed time since he started it; or he may let it make many circuits before he does so. This interval divided by the numbers of pupils in the class and by the number of circuits, gives the average reaction time of each pupil. The squeeze takes usually about a second of time to pass through each dozen or fifteen persons. We should expect to find uniformity in successive experiments when the pupils are fresh; irregularity and prevalent delay when they are tired. I wish that teachers would often try this simple, amusing, and attractive experiment, and when they have assured themselves that their class enters into its performance with interest and curiosity, they might begin to make careful records at different periods of the day and see whether it admits of being used as a test of incipient fatigue. I should be exceedingly glad to receive accounts of their experiences. Deception must of course be guarded against.

Senses.—The frequency with which serious alteration in the power of hearing and of seeing is mentioned, and the feelings sometimes of intense sensitivity and sometimes of numbness, show that the delicacy of the senses is markedly affected by fatigue.

Hearing is often heightened in keenness, sometimes it is dulled. It is heightened in those numerous cases of irritability of which I have spoken, when the tired brain becomes almost maddened by an organ grinder. It is temporarily paralysed in others. The following is a mixed case:—1. My hearing had never been very acute, and I think the first symptom of fatigue is a feeling of deafness, but at the same time that I cannot hear the voices I want to hear, the outside noises of traffic, bells, &c., become intolerable. Other cases of deafness from fatigue are—2. Inability to hear in school without a painful effort. 3. Increased deafness.

Vision is greatly affected by over fatigue, not only owing to the strain upon the eyes from much reading in a bad light, but apparently through more deeply-seated causes as well. It is difficult otherwise to account for the following interesting case in which colour-blindness was brought on by fatigue and disappeared after rest. It has much physiological interest and well deserves being placed on record. The lady allows me to mention her name for the sake of authenticity. She is Miss J. Beckett, Girls' Grammar School, Ripon.

"After several hard hours of continuous study I have been subject to attacks of colour-blindness, which leave me after resting. The first time I noticed that I was not able to distinguish one colour from another, was when I was reading for the London Matriculation years ago. I was at the same time etching for an American magazine and teaching most of the day. This lasted from Christmas to July, when I began to feel considerably worn out. One day I went to spend a few hours with a friend, and whilst there, began to paint some ivy leaves on a terra-cotta plaque. Imagine my distress when my friend told me the leaves were orange instead of green. On my return I went into my study and to my astonishment, the curtains which were blue in colour looked a kind of dingy yellow. However in a few hours I was quite well. Towards the end of the year I was obliged to give up work on account of my health. I got well, and took up my work again, still subject to temporary colour-blindness when tired." In answer to further inquiries, she adds: "I do not remember whether I have any difficulty in recalling colours when tired. From a little child I have been particularly fond of them, and can readily paint flowers, foliage, and neutral tints from memory."

The frequency and severity with which the sight is affected by fatigue is sufficiently shown by the following extracts:—

1. The eyes fail first. Sometimes after hurrying to a lesson, on my arrival I could not see a single note on the page of music for a few minutes. After writing and playing long, everything goes black or black spots dance up and down. 2. A time of great excitement or worry will so affect my sight that for about half-an-hour at a time, I can see nothing clearly. The outline of everything is deficient in some part, so that I only see half of a thing at a time. There seems to be a bright wheel of light whizzing in the corner of one or the other eye.

3. At first the lines of the page become indistinct, then at intervals they appear to vibrate; finally they merge into one mass.

4. The words appear to rise from the paper and frequently a double row of words are visible.

5. Lights and after images are distinct before my eye.

6. A confusion in the lettering of mathematical diagrams is sometimes an early symptom of fatigue among my pupils.

As regards sensations in the eye itself, beside such remarks as—7. A dazzling and burning sensation in the eye, the following is a case of an affection of the eye being subordinate to that of the brain, rather than *vice versâ*.

8. A nervous sensation in the eyes as though the eyeballs were loose in my head, and would fall which ever way the head is inclined. The sensation is worse on lying down. I am somewhat short-sighted and wear glasses, but only feel this disagreeable sensation when mentally weary, not necessarily through over-reading.

Memory.—A very common and early symptom of fatigue is failure of memory, using that word in the allied senses of recalling ideas at will, or else of former ideas presenting themselves readily by association, or else of the sure association of muscular movements engaged in utterance, with the idea of the words intended to be uttered. I have made extracts of no less than twenty-five cases of failure of memory, out of which I will select half-a-dozen.

1. My first indication of mental failure is an inability to spell common words; my second, an omission of words in writing; my third, sudden forgetfulness of what I am actually saying. 2. Tendency to forget the meaning of words in a foreign language which are usually well known or have been met with quite recently. Tendency to make stupid blunders in subjects in which, when the mind is in full vigour, it is accurate without effort. Simple and obvious mistakes are increased twofold in number, and that throughout the class. 3. Through days and weeks together, the utterance of wrong words or sentences, not intended or desired to be spoken, and the writing of wrong words. 4. Tendency to stumble over words in speaking, and to misplace letters in writing, generally putting them too soon as "Wesday" for "Wednesday." 5. Want of power of calling at will to memory, names and little matters connected with every day life. 6. Some of the pupils never spell correctly when tired.

Arithmetic and Mathematics.—The studies that are the first to fail under fatigue differ in different individuals, but in the majority of cases those of arithmetic and elementary mathematics go soonest. Though many of the 116 replies come from teachers who have little, if anything, to do with those subjects, no less than forty-seven specifically mention them. For example:—

1. The merely mechanical processes of arithmetic become bewildering at the end of a day in which I have been particularly engrossed with school work.

2. Arithmetic and algebra become impossible when fatigued, not as being disagreeable or painful, but because I then blunder so much that it is hardly any use attempting them.

3. Another correspondent speaks of the impossibility when fatigued of doing work that requires both accuracy of detail and a certain force of will to fix the attention, such as arithmetic.

4. Speaks of the difficulty to tired boys of working out any *common sense* problem in arithmetic.

Though very many similar answers could be quoted in corroboration of these, there are two that tell in an opposite direction. They are—

5. Whenever my mind is wearied, it affords me a certain amount of relief to do some work which involves the solving of arithmetical and algebraical problems, and by preference such as call for the use of logarithms or of the slide rule.

6. I find accounts a great rest when I cannot exert my mind usefully in any other way.

I may be permitted again to break my rule by adding a case from my own knowledge of a very distinguished man, now deceased, who having always found repose in his favourite mathematics when he was fagged and worried by multifarious duties, naively recommended the same remedy to a friend whose brain had so broken down for a time, that he shrank from the least mental exertion as from a fatal danger.

Languages.—A difficulty in translating is another of the noticeable effects of incipient fatigue, and is partly due to the lapses of memory already spoken of.

1. In translating, words and phrases do not occur readily to the mind.

2. Translation into or out of a foreign language with which I am not very familiar.

3. I have occasionally lost the power of speaking German when fatigued, though when in my ordinary condition I speak it without conscious effort.

The failure to translate *well* is due of course to much more than the simple failure of memory in small things and depends on the loss of power of grasp, and on depressed mental vigour generally. The following is an instructive case:—

When I taught young boys of ages 8 to 13, all day, I took arithmetic and Latin in the morning, and English reading, geography, &c., in the afternoon. On some occasions the Latin lesson got put off till the afternoon, and I was surprised to find that lesson, which was always a successful one in the morning, failed entirely in the afternoon. The boys wished to learn but could not. Their ordinary work, which made less demand on the intellect, they did in the afternoon well enough.

This and such like cases fall more properly in the next division.

Failure of Mental Grasp.—The evidences that the fatigued mind is unable to work up to its normal standard, and that it wastes itself in futile exertion, are very numerous. They are such as:—

1. Failure of ability to grasp the meaning of even simple things.

2. Failure of the *portative* memory. In reading complete inability to take in the matter whilst mechanically scanning the page. A curious incapacity to count the cups when serving tea.

3. Reading sentences without recognition of what was read.

4. Confusion alternating with excessive clearness of thought.

5. Tendency of thoughts to wander. Failure in pupils to grasp the meaning of what is said to them quickly and fully.

6. Before the actual sense of fatigue is distinctly felt, I am

conscious of a want of power to grasp ideas, and of an incapacity for conveying them clearly.

7. Inability to read the "Journal of Education."

8. Rapid disappearance of immediately preceding concepts, and hence difficulty in establishing connections between paragraphs, as in writing a Review article.

9. Tendency to *use long words.* (This strikes me as a very suggestive reply).

10. Any book in which the language is wanting in ease and simplicity, though its subject may be familiar or easily understood.

In short, to use a common and vigorous phrase, the mind ceases to *bite,* when it is fatigued.

Failure of Energy.—It requires no evidence to corroborate the well known fact that energy fails as fatigue increases. New subjects are distasteful; teaching dullards becomes almost an impossibility. Sustained effort, vigorous inspection, quick decision—all are impossible.

Possibility of Tests of Incipient Fatigue.—The replies I have received do not contain any distinct proposition of tests of incipient mental fatigue, and I am myself far too ignorant of the practice of education to venture to formulate any. On the other hand, the replies are not deficient in indications of what such tests might be directed to ascertain. They are principally as follows:—

1. The length of time during which neatness of execution can be sustained in performing a prolonged task.
2. Promptness and sureness of memory in simple things.
3. Common sense arithmetical problems.
4. Reaction time.

The measure of fatigue is inversely the measure of endurance, and this strikes me as being a faculty that well deserves investigation. Under the strain and exhausting calls of modern civilized life, the power of endurance is rising continually in importance. Men and women have now a-days to act rapidly and for many hours, and not only to act exceptionally well. It therefore seems very reasonable that teachers should direct their attention to some fair way of appraising the relative power of endurance among their pupils. It is of course incidentally discovered in the ordinary course of tuition, but one would like to see appropriate tests directly applied to determine it, and such as would show at any time in a definite and unmistakeable manner whether the minds of pupils were fagged or not.

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protect their own health of brain by refusing to overwork. It is among those who are zealous and eager, who have high aims and ideas, who know themselves to be mentally gifted, and are too generous to think much of their own health, that the most frequent victims of overwork are chiefly found.

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PERSONAL IDENTIFICATION

AND

DESCRIPTION.

BY

F. GALTON, F.R.S.

LONDON:
HARRISON AND SONS, ST. MARTIN'S LANE,
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1888.

f. 2r

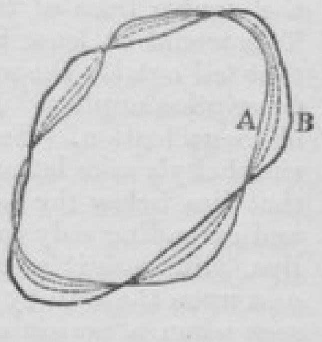
PERSONAL IDENTIFICATION *and* DESCRIPTION.

By FRANCIS GALTON, F.R.S.

Being the substance of a lecture delivered at the Royal Institution on Friday, May 25th, 1888; reprinted from "Nature" of June 21st and 28th, after some slight revision by the Author.[1]

IT is strange that we should not have acquired more power of describing form and personal features than we actually possess. For my own part I have frequently chafed under the sense of inability to verbally explain hereditary resemblances and types of features, and to describe irregular outlines of many different kinds, which I will not now particularise. At last I tried to relieve myself as far as might be from this embarrassment, and took considerable trouble, and made many experiments. The net result is that while there appear to be many ways of approximately effecting what is wanted, it is difficult as yet to select the best of them with enough assurance to justify a plunge into a rather serious undertaking. According to the French proverb, the better has thus far proved an enemy to the passably good, so I cannot go much into detail at present, but will chiefly dwell on general principles.

Fig. 1.



Measure of Resemblance.—We recognise different degrees of likeness and unlikeness, though I am not aware that attempts have as yet be made to measure them. This can be done if we take for our unit the *least discernible difference.* The application of this principle to irregular contours is particularly easy. Fig. 1 shows two such contours, A and B, which might be meteorological, geographical, or anything else. They are drawn with firm lines, but of different strengths for the sake of distinction. They contain the same area, and are so superimposed as to lie as fairly one over the other as may be. Now draw a broken contour which we will call C equally sub-dividing the intervals between A and B; then C will

[1] The Council is indebted to the Editor of "Nature" for the woodcuts illustrating this lecture.

f. 2v

be more like A than B was. Again draw a dotted contour, D, equally subdividing the intervals between C and A; the likeness of D to A will be again closer. Continue to act on the same principle until a stage is reached when the contour last drawn is undistinguishable from A. Suppose it to be the fourth stage; then as $2^4 = 16$, there are sixteen grades of least-discernible differences between A and B. If one of the contours differs greatly in a single or few respects from the other, reservation may be made of those peculiarities. Thus, if A has a deep notch in its lower right-hand border, we might either state that fact, and say that in other respects it differed from B by only sixteen grades of unlikeness, or we might make no reservation, and continue subdividing until all trace of the notch was smoothed away. It is purely a matter of convenience which course should be adopted in any given case. The measurement of resemblance by units of least-discernible differences is applicable to shades, colours, sounds, tastes, and to sense-indications generally. There is no such thing as infinite unlikeness, because the number of just discernible differences between any objects, however dissimilar, is always finite. A point as perceived by the sense of sight is not a mathematical point, but an object so small that its shape ceases to be discernible. Mathematically, it requires an infinitude of points to make a short line; sensibly, it requires a finite and not a large number of what the vision reckons as points, to do so. If from thirty to forty points were dotted in a row across the disk of the moon, they would appear to the naked eyes of most persons as a continuous line.

Description within Specified Limits.—It is impossible to verbally define an irregular contour with such precision that a drawing made from the description shall be undistinguishable from the original, but we may be content with a lower achievement. Much would be gained if we could refer to a standard collection of contours drawn with double lines, and say that the contour in question falls between the double lines of the contour catalogued as number so-and-so. This would at least tell us that none of the very many contours that fell outside the specified limits could be the one to which the description applied. It is an approximate and a negative method of identification. Suppose the contour to be a profile, and for simplicity's sake let us suppose it to be only the portion of a profile that lies below the notch that separates the brow from the nose, and extending only so far downwards as the parting between the lips. Suppose it also to be the mere outline of a shadow sharply cast upon the wall by a single source of light, such as is excellently seen when a person stands sideways between the electric lantern and the screen in a lecture-room. All human profiles of this kind, when they have been reduced to a uniform vertical scale, fall within a small space. I have taken those given by Lavater, which are in many cases of extreme shapes, and have added others of English faces, and find that they all fall within the space shown in Fig. 2. The outer and inner limits of the space are, of course, not the profiles of any real faces, but the limits of many profiles, some of which

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are exceptional at one point, and others at another. We can classify the great majority of profiles so that each of them shall be included between the double borders of one, two, or some small number of standard portraits, such as Fig. 3. I am as yet unprepared to say how near together the double borders of such standard portraits should be drawn; in other words, what is the smallest number of grades of unlikeness that we can satisfactorily deal with. The process of sorting profiles into their proper classes and of gradually building up a well-selected standard collection, is a laborious undertaking if attempted by any obvious way; but I believe it can be effected with comparative ease on the basis of measurements, as will be explained later on, and by an apparatus that will be described.

Fig. 2.

Fig. 3.

Classification of Sets of Measures.—Prisoners are now identified in France by the measures of their heads and limbs, the set of measures of each suspected person being compared with the sets that severally refer to each of many thousands of convicts. This idea, and the practical application of it, is due to M. Alphonse Bertillon. The actual method by which this is done is not all that could be theoretically desired, but it is said to be effective in action, and enables the authorities quickly to assure themselves whether the suspected person is or is not an old malefactor. The primary measures in the classification are four—namely, the head length, head breadth, foot length, and middle-finger length of the left foot and hand respectively. Each of these is classified according as it is large, medium, or small. There are thus three, and only three, divisions of head lengths, each of which is subdivided into three divisions of head breadth; again, each of these is further subdivided into three of foot length, and these again into three of middle-finger length; thus the number of primary classes is equal to three multiplied into itself four times—that is to say, their number is eighty-one, and a separate pigeon hole is assigned to each. All the exact measures and other notes on each criminal are written on the same card, and this card is stored in its appropriate pigeon-hole. The contents of each pigeon-hole are themselves sub-sorted on the same principle of three-fold classification in respect to other measures. This process can, of course, be extended indefinitely; but how far it admits of being carried on advantageously is another question. The fault of all hard-and-fast lines of classification, when variability is continuous, is the doubt where to place and where to look for values that are near the limits between two adjacent classes. Let us take Stature as an illustration of what must occur in every case, and let us represent its distribution by what I have called a "Scheme," as shown in Fig. 4.

Here the statures of any large group of male adults such as those whom I had measured by the thousand at the International

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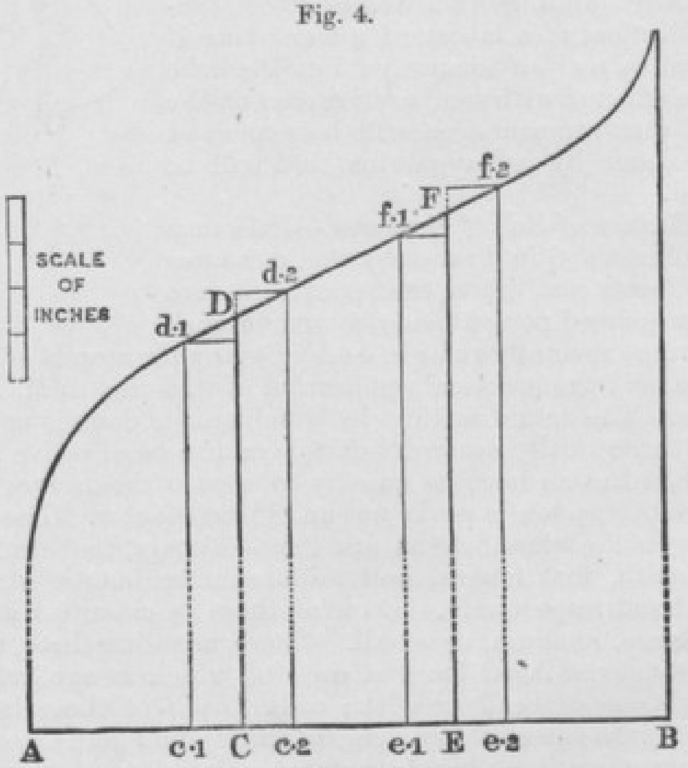
Health Exhibition of 1884, are represented by lines of proportionate length. The lines are arranged side by side at equal distances apart on a base, A B, of convenient length. A curve drawn through their tops gives the upper boundary of the scheme; the lines themselves are then wiped out, having served their purpose. If the base A B be divided into three equal parts, and perpendiculars, C D, E F, be erected at the divisions between them, reaching from the base upwards to the curve, then the lengths of those perpendiculars will be proportionate to the limiting values between the small and the medium group, and between those of the medium and the large group respectively. I find the difference between these perpendiculars in the case of stature to be about 2·3 inches. In other words, the shortest and tallest men in the medium class differ only by 2·3 inches. We have next to consider how much ought reasonably to be allowed for error of measurement. Considering that a man differs in height by a full third of an inch between the time of getting up in the morning and lying down at night: considering also that measures are recorded to the nearest tenth of an inch at the closest, also the many uncertainties connected with the measurement of stature, it would be rash not to allow for a possible (I do not say "probable") and not rare error of at least $\pm$ half-an-inch. Prolong C D, and note the points upon it at the distance of half-an-inch above and below D; draw horizontal lines from those points to meet the curve at *d*.1, *d*.2, and from

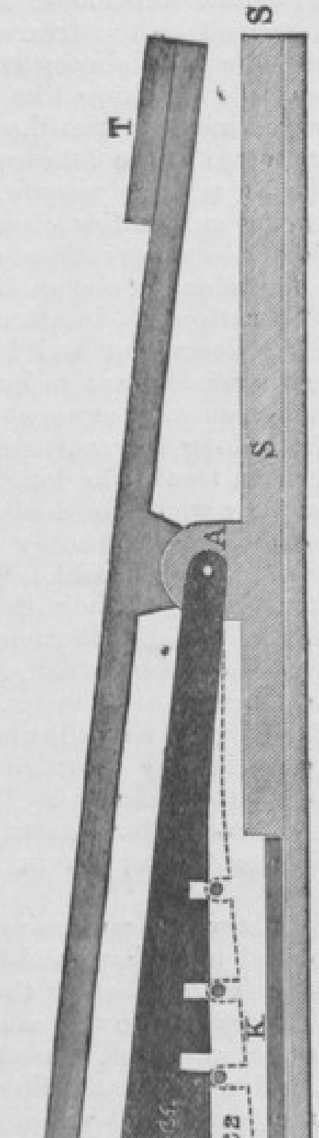
Fig. 4.



the points of intersection drop perpendiculars reaching the base at *c*.1, *c*.2. A similar figure is drawn at F. Then the ratio borne by the uncertain entries to the whole number of entries is as $c_1\ c_2 + e_1\ e_2$ to A B. This, as seen by the diagram, is a very large proportion. There is a dilemma from which those who adopt hard-and-fast lines of classification cannot escape: either the fringe of uncertainty must be dangerously wide, or else the delicacy with which measures are made cannot be turned to anything like its full account. If the delicacy of the measurement is small, each of the fringes of uncertainty must be very wide: if the delicacy is great, the summed widths of all the fringes will be narrow, if there are only a few classes; but, by having only a few classes, most of the advantages of possessing delicate observations are wasted. The bodily measurements are so dependent on one another that we cannot afford to neglect small distinctions in an attempt to make an effective classification. Thus long feet and long middle-fingers usually go together. We therefore want to know whether the long feet in some particular person are accompanied by very long, or moderately long, or barely long fingers, though the fingers may in all three cases have been treated as long in M. Bertillon's system of classes, because they would be long as compared with those of the general population. Certainly his eighty-one combinations seem far from being equally probable. The more numerous the measures the greater would be their interdependence, and the more unequal would be the distribution of cases among the various possible combinations of large, small, and medium values. No attempt has yet been made to estimate the degree of their interdependence. I am therefore having the above measurements (with a slight necessary variation) recorded at my anthropometric laboratory for the purpose of doing so. This laboratory, I may add, is now open to public use under reasonable restrictions. It is entered from the Science Collections in the Western Galleries at South Kensington.

Mechanical Selector.—Feeling the advantage of possessing a method of classification that did not proceed upon hard-and-fast lines, I contrived an apparatus that is quite independent of them, and which I call a mechanical selector. Its object is to find which set, out of a standard collection of many sets of measures, resembles any one given set within any given degree of unlikeness. No one measure in any of the sets selected by the instrument can differ from the corresponding measure in the given set by more than a specified value. The apparatus is very simple; it applies to sets of measures of very description, and ought to act on a large scale as well as it does on a small one, with great rapidity, and be able to test several hundred sets by each movement. It relieves the eye and brain from the intolerable strain of tediously comparing a set of many measures with each of a large number of successive sets, in doing which a mental allowance has to be made for a *plus* or *minus* deviation of a specified amount in every entry. It is not my business to look after prisoners, and I do not fully know what need may really exist for new methods of quickly identifying suspected persons. If there be any real need, I should think that this apparatus, which is contrived for other purposes, might, after obvious modifications, supply it.

Fig. 5.



Section of the apparatus, but the bridges and rods are not shown, only the section of the wires.

The apparatus consists, in principle, of a large number of strips of card or metal c 1, c 2 (fig. 5), say 8 or 9 inches long, and having a common axis A passing through all their smaller ends. A tilting-frame T, which turns on the same axis, has a front cross-bar F (whose section is seen in fig. 5), on which the tips of the larger ends of all the cards rest whenever the machine is left alone. In this condition a counterpoise at the other end of T suffices to overcome the weight of all the cards, and this heavier end of T lies on the base-board S. When the heavy end of T is lifted, as shown in fig. 5, its front-bar F is of course depressed, and the cards being individually acted on by their own weights, are free to descend with the cross-bar unless they are otherwise prevented. The lower edge of each card is variously notched to indicate the measures of the person it represents. Only four notches are shown in the figure, but six could be employed in a card of 8 or 9 inches long, allowing compartments of 1 inch in length to each of six different measures. The position of the notch in the compartment allotted to it, indicates the corresponding measure according to a suitable scale. When the notch is in the middle of a compartment, it means that the measure is of mediocre amount; when at one end of it, the measure is of some specified large value or of any other value above that; when at the other end, the measure is of some specified small value, or of any other value below it. Intermediate positions represent intermediate values according to the scale. Each of the cards corresponds to one of the sets of measures in the standard collection. The set of measures of the given person

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are indicated by the positions of parallel strings or wires, one for each measure, that are stretched between rods and across bridges at either end of a long board set cross-ways to the cards. Their positions on the bridges are adjusted by the same scale as that by which the notches were cut in the cards. Figs. 6*a* and 6*b* are views of this portion of the apparatus, which acts as a key, and is of about 30 inches in effective length. The whole is shown in working position in fig. 7. When the key is slid into its place, and the heavy end of the tilting-frame T is raised, all the cards are free to descend so far as the tilting frame is concerned, but they

Fig. 6*a*.

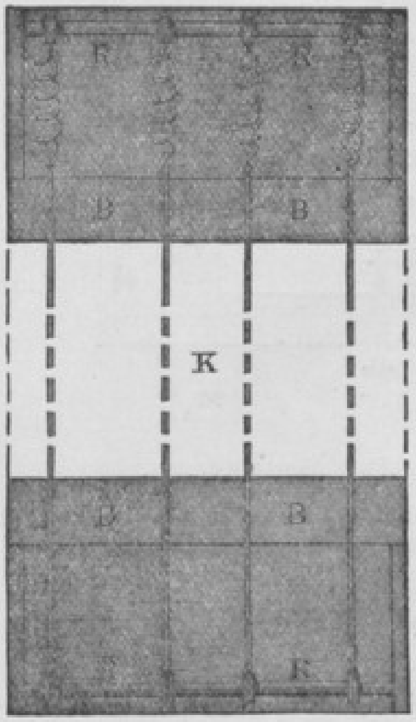
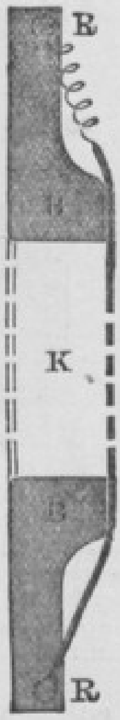


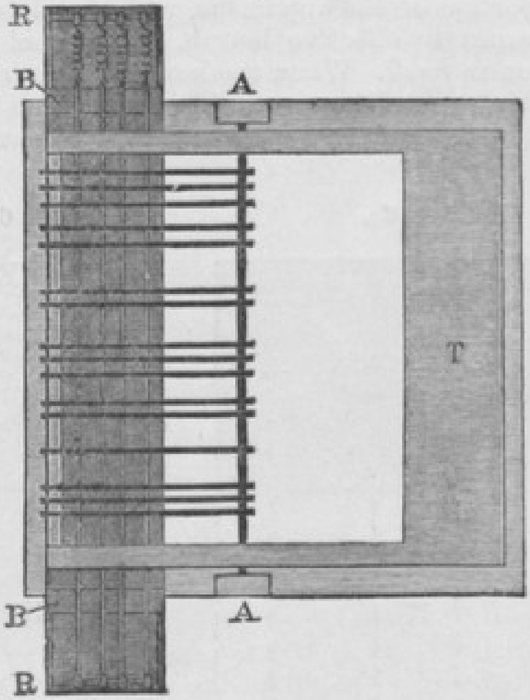
Fig. 6*b*.



Plan and section of the key-board K.

are checked by one or more of the wires from descending below a particular level, except those few, if any, whose notches correspond throughout to the positions of the underlying wires. This is the case with the card *c*2 (fig. 5), drawn with a dotted outline, but not with *c*1, which rests upon the third wire, counting from the axis. As the wires have to sustain the weight of all or nearly all the cards, frequent narrow bridges must be interposed between the main bridges to sustain the wires from point to point. The cards should be divided into batches by partitions corresponding to these interposed bridges, else they may press sideways with enough friction to interfere with their free independent action. The action is improved by interposing stretched threads between each pair of adjacent cards so that every card works in a separate compartment. None of these are shown in the figure. The method of adjusting the wires there shown, is simply by sliding the rings to which they are

Fig. 7.



Reduced plan of complete apparatus.

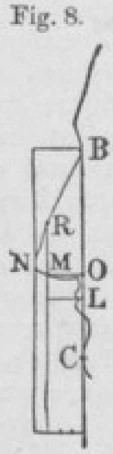
Explanation.—A, the common axis; c_1, c_2, the cards; T, tilting-frame, turning on A (the cards rest by their front ends on F, which is the front cross-bar of T, at the time when the heavy hinder end of T rests on the base-board S); K is the key-board; R R are the rods between which the wires are stretched; B B are the bridges at either end of the key-board, over which the wires pass. (The explanation refers to the other figs., as well as to this).

attached at either end, along the rod which passes through them. It is easy to arrange a more delicate method of effecting the adjustment if desired. Hitherto I have snipped out the notches in the cards with a cutter made on the same principle as that used by railway guards in marking the tickets of travellers. The width of the notch is greater than the width of the wire by an amount proportionate to the allowance intended to be made for error of measurement, and also for that due to mechanical misfit. There seems to be room for 500 cards or metal strips, and ample room for 200 or 300 of them, to be arranged in sufficiently loose order within the width of 30 inches. A key of that effective length would test all these by a single movement; it could also be applied in quick succession to any number of other sets of cards.

Measurement of Profiles.—The sharp outline of a photographed profile admits of more easy and precise measurement than the yielding outline of the face itself. The measurable distances between the profiles of different persons are small, but the available measures are much more numerous than might have been expected, and their variations are more independent of one another than those of the limbs. I suspect that measures of the profile may be nearly as trustworthy as those of the limbs for approximate identification, that is, for excluding a very large proportion of persons from the possibility of being mistaken for the individual whose measurements are given. The measurement of a profile enables us to use a mechanical selector for finding those in a large standard collection to which they nearly correspond. From the selection thus made, the eye could easily make a further selection of those that suited best in other respects. A mechanical selector also enables us to quickly build up a standard collection step by step, by telling us whether or no each fresh set of measures falls within the limits of any of those already collected. If it does, we know that it is already provided for; if not, a new card must be added to the collection. There will be no fear of duplications, as every fresh-added standard will differ from all its predecessors by more than the specified range of permitted differences.

As regards the most convenient measurements to be applied to a profile for use with the selector, I am unable as yet to speak decidedly. If we are dealing merely with a black silhouette, such as the shadow cast on a wall by a small and brilliant light, the best line from which to measure seems to be B C in fig. 8; namely, that which touches both the concavity of the notch between the brow and nose, and the convexity of the chin. It is not difficult to frame illustrated instructions to explain what should be done in the cases where no line can be drawn that strictly fulfils these conditions. I have taken a considerable number of measures from the line that touches the brow and chin, but am now inclined to prefer that which I have just described. A sharp unit of measurement is given by the distance between this line and another drawn parallel to it just touching the nose, as at N in the figure. A small uncertainty in the direction of B C has but a very trifling effect on this distance. By dividing the interval between these parallel lines into four parts, and drawing a line through the third of the divisions, parallel to B C, we obtain the two important points of reference, M and R. M is a particularly well-defined point, from which O is determined by dropping a perpendicular from M upon B C. O seems the best of all points from which to measure. It is excellently placed for defining the shape and position of the notch between the nose and the upper lip, which is perhaps the most distinctive feature in the profile. O L can be determined with some precision; O B and O C are but coarse measurements.

Fig. 8.



In addition to these and other obvious measures, such as one or more to define the projection of the lips, it would be well to measure the radius of the circle of curvature of the depression at B, also of that between the nose and the lip, for they are both very variable and very distinctive. So is the general slope of the base of the nose. The difficulty lies not in selecting a few measures that will go far towards negatively identifying a face, but in selecting the best—namely, those that can be most precisely determined, are most independent of each other, most variable, and most expressive of the general form of the profile. I have tried many different sets, and found all to be more or less efficient, but have not yet decided to my own satisfaction which to adopt.

We will now suppose that either by the above method or by any other, a standard collection of doubly outlined portraits such as that in fig. 3, has been made and come into use, so that a profile can be approximately described by referring it to number so-and-so in the catalogue. If the number it contained was less than 1,000, three figures would suffice to define any one of them. We will now consider how a yet closer description of the profile may be given by using a few additional figures. One way of doing so is to have short cross-lines drawn at critical positions between the two outlines of the standard, and to suppose each of them to be divided into eight equal parts. The intersection of the cross-lines with the outer border would count as 0; that with the inner border as 8; and the intermediate divisions from 1 to 7. As the cross-lines would be very short, a single numeral would thus define the position of a point in any one of them, with perhaps as much precision as the naked eye could utilise. By employing as many figures as there are cross-lines in the standard, each successive figure for each successive cross-line, a corresponding number of points in the profile would be fixed with great accuracy. Suppose a total of nine figures to be allowed, then the first three figures would specify the catalogue number of the portrait to be referred to, and the remaining six figures would determine six points in the outline of the portrait with greatly increased precision.

I may say that after numerous trials of different methods for comparing portraits successively by the eye, I have found none so handy and generally efficient as a double-image prism, which I largely used in my earlier attempts in making composite portraits.

I have not succeeded in contriving an instrument that shall directly compare a given profile with those in a standard collection, and which shall at the same time act with anything like the simplicity of a mechanical selector, and with the same quick decision in acceptance or rejection. Still, I recognise some waste of opportunity in not utilising the power of varying the depths of the notches in the cards, independently of their longitudinal position.

Personal Characteristics.—These are to be found in much more minute portions of the body than those just described. Leaving aside microscopic peculiarities, which are of unknown multitudes, such as might be studied in the 800,000,000 specimens cut by a microtome, say of one two-thousandth part of an inch in thickness, and one-tenth of an inch each way in area, out of the 4,000 cubic inches or so of the flesh, fat, and bone of a single average human body, there are many that are visible with or without the aid of a lens.

The markings in the iris of the eye are of the above kind. They have been never adequately studied, except by the makers of artificial eyes, who recognise thousands of varieties of them. These markings well deserve being photographed from life on an enlarged scale. I shall not dwell now upon these, nor on such peculiarities as those of handwriting, nor on the bifurcations and interlacements of the superficial veins, nor on the shape and convolutions of the external ear. They all admit of brief approximate description by the method just explained—namely, by reference to the number in a standard collection of the specimen that shall not differ from it by more than a specified number of units of unlikeness. I have already explained what is meant by a unit of unlikeness, and the mechanical means by which a given set of measures can be compared with great ease with every set in a standard collection of sets of measures.

Perhaps the most beautiful and characteristic of all superficial marks are the small furrows, with the intervening ridges and their pores, that are disposed in a singularly complex yet regular order on the under surfaces of the hands and the feet. I do not now speak of the the large wrinkles in which chiromantists delight, and which may be compared to the creases in an old coat, or to the deep folds in the hide of a rhinoceros, but of those fine lines of which the buttered fingers of children are apt to stamp impressions on the margins of the books they handle, that leave little to be desired on the score of distinctness. These lines are found to take their origin from various centres, one of which lies in the under surface of each finger-tip. They proceed from their several centres in spirals and whorls, and distribute themselves in beautiful patterns over the whole palmar surface. A corresponding system covers the soles of the feet. The same lines appear with little modification in the hands and feet of monkeys. They appear to have been carefully studied for the first time by Purkinje in 1822, and since then they have attracted the notice of many writers and physiologists, the fullest and latest of whom is Kollman, who has published a pamphlet, "Tastapparat der Hand" (Leipzig, 1883), in which their physiological significance is fully discussed. Into that part of the subject I am not going to enter here. It has occurred independently to many persons to propose finger-marks as a means of identification. In the last century, Bewick, in one of the vignettes in the "History of Birds," gave a woodcut of his own thumb-mark, which is the first clear impression I know of, and afterwards one of his finger-marks. Some of the latest specimens that I have seen are by Mr. Gilbert Thomson, an officer of the American Geological Survey, who, being in Arizona, and having to make his orders for payment on a camp suttler, hit upon the expedient of using his own thumb-mark to serve the same purpose as the elaborate scroll engraved on blank cheques—namely, to make the alteration of figures written on it impossible without detection. I possess copies of two of his cheques. A San Francisco photographer, Mr. Tabor, made enlarged photographs of the finger-marks of Chinese, and his proposal to employ them as a means of identifying Chinese immigrants, seems to have been seriously considered. I may say that I can obtain no verification of a common statement that the method is in actual use in the prisons in China. The thumb-mark has been used there as elsewhere to form a manual impression in attestation of deeds, such as a man might make with a common seal, not his own, and say, "This is my act and deed;" but I cannot hear of any elaborate system of finger-marks having ever been employed in China for the identification of prisoners. It was, however, largely used in India, by Sir William Herschel, many years ago, when he was an officer of the Bengal Civil Service. He found it to be most successful in preventing personation, and in putting an end to disputes about the authenticity of deeds. He described his method fully in "Nature," in 1880 (Vol. xxiii, p. 76), which should be referred to; also a paper by Mr. Faulds in the next volume. I may in addition allude to articles in the American journal "Science," 1886 (Vol. viii, pp. 166 and 212).

The question arises whether these finger-marks remain unaltered throughout the life of the same person. In reply to this, I am enabled to submit a most interesting piece of evidence, which thus far is

Fig. 9.

Enlarged impressions of the fore and middle finger tips of the right hand of Sir William Herschel, made in the year 1860.

unique, through the kindness of Sir Wm. Herschel. It consists of the imprints of the two first fingers of his own hand, made in 1860 and in 1888 respectively—that is, at periods separated by an interval of twenty-eight years. I have also two intermediate imprints, made by him in 1874 and in 1883 respectively. Figs. 9 and 11 are cut from photographs on an enlarged scale of the imprints of 1860 and 1888, which were made direct upon the engraver's block; these woodcuts may therefore be relied on as being very correct representations of the originals in my present possession. Fig. 10 refers to the portion of fig. 9 to which I am about to draw attention. On first examining these and other finger-marks, the eye wanders and becomes confused, not knowing where to fix itself; the points shown in fig. 10 are those which it ought to select. They are the places

Fig. 10.

Positions of furrow-heads and bifurcations of furrows, in Fig. 9.

Fig. 11.

Enlarged impressions of the fore and middle finger tips of the right hand of Sir William Herschel, made in the year 1888.

at which each new furrow makes its first appearance. The furrows may originate in two principal ways, which are not always clearly distinguishable: (1) the new furrow may arise in the middle of a ridge; (2) a single furrow may bifurcate and form a letter Y. The distinction between (1) and (2) is not greatly to be trusted, because one of the sides of the ridge in case (1) may become worn, or be narrow and low, and not always leave an imprint, thus converting it into case (2); conversely case (2) may be converted into case (1). The position of the origin of the new furrow is, however, none the less defined. I have noted the furrow-heads and bifurcations of furrows in fig. 9, and shown them separately in fig. 10. The reader will be able to identify these positions with the aid of a pair of compasses, and he will find that they persist unchanged in fig. 11, though there is occasionally some uncertainty between cases (1) and (2). Also there is a little confusion in the middle of the small triangular space that separate two distinct systems of furrows, much as eddies separate the stream lines of adjacent currents converging from opposite directions. A careful comparison of figs. 9 and 11 is a most instructive study of the effects of age. There is an obvious amount of wearing and of coarseness in the latter, but the main features of both are the same.

I happen to possess a little apparatus that proves very convenient for examining finger-marks and for recording the positions of furrow heads. It is a slight and small, but well-made wooden pentagraph, multiplying five-fold, in which a very low-power microscope, with *coarse* cross-wires, forms the axis of the short limb, and a pencil-holder forms the axis of the long limb. I contrived it for quite another use—namely, the measurements of the length of wings of moths in some rather extensive experiments that are now being made for me in pedigree moth-breeding. It has proved very serviceable in this inquiry also, and was much used in measuring the profiles spoken of in the last article. Without some moderate magnifying power the finger-marks cannot be properly studied. It is a convenient plan, in default of better methods, to prick holes with a needle through the furrow-heads into a separate piece of paper, where they can be studied without risk of confusing the eye. There are peculiarities often found in furrows that do not appear in these particular specimens, and to which I will not further refer. In fig. 10 the form of the origin of the spirals is just indicated. These forms are various; they may be in single or in multiple lines, and the earlier turns may form long loops or be nearly circular. My own ten fingers show at least four distinct varieties.

Notwithstanding the experience of others to the contrary, I find it not easy to make clear and perfect impressions of the fingers. The proper plan seems to be to cover a flat surface, like that of a piece of glass or zinc, with a thin and even coat of paint, whether it be printers' ink or Indian ink rubbed into a thick paste, and to press the finger lightly upon it so that the ridges only shall become inked, then the inked fingers are pressed on smooth and slightly damped paper. If a plate of glass be smoked over a paraffin lamp, a beautiful negative impression may be made on it by the finger,

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suitable for a lantern transparency. The blackened finger may afterwards be made to leave a positive impression on a piece of paper, that must be varnished if the impression is to be rendered permanent. All this is rather dirty work, but people do not seem to object to it; rivalry and the hope of making continually better impressions carries them on. It is troublesome to make plaster casts; modelling-clay has been proposed; hard wax, such as dentists use, acts fairly well; sealing-wax is excellent if the heat can be tolerated; I have some good impressions in it. For the mere study of the marks, no plan is better than that of rubbing a little thick paste of chalk ("prepared chalk") and water or sized water upon the finger. The chalk lies in the furrows, and defines them. They might then be excellently photographed on an enlarged scale. My own photographic apparatus is not at hand, or I should have experimented on this. When notes of the furrow-heads and of the initial shape of the spiral have been made, the measurements would admit of comparison with those in catalogued sets by means of a numerical arrangement, or even by the mechanical selector described above. If a cleanly and simple way could be discovered of taking durable impressions of the finger tips, there would be little doubt of their being serviceable in more than one way.

In concluding my remarks, I should say that one of the inducements to making these inquiries into personal identification has been to discover independent features suitable for hereditary investigation. It has long been my hope, though utterly without direct experimental corroboration thus far, that if a considerable number of variable and independent features could be catalogued, it might be possible to trace kinship with considerable certainty. It does not at all follow because a man inherits his main features from some one ancestor, that he may not also inherit a large number of minor and commonly overlooked features from many ancestors. Therefore it is not improbable, and worth taking pains to inquire, whether each person may not carry visibly about his body undeniable evidence of his parentage and near kinships.

Postscript.

Since delivering this lecture, I have had the opportunity of seeing M. Bertillon's system in operation at Paris, and was much impressed with the skill and celerity shown by his staff of assistants, with the large amount of work got through, and with the practical overcome of theoretical difficulties, especially in the method of subsorting cards in the movable trays, which are, in fact, the "pigeon holes." Numerous data methodically inscribed on each card, in addition to the primary measures and the free use of coloured tickets to aid in the subdivisions, make it easy to a practised person to hunt rapidly through the contents of any one tray. These additional data include a notice of scars and personal marks, and very good photographs of the profile and full face.

f. 9v

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f. 1

NOTE ON AUSTRALIAN MARRIAGE SYSTEMS.

BY

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Note *on* Australian Marriage Systems.

By Francis Galton, M.A., F.R.S.

A very simple way of understanding the peculiarly complicated system of Australian marriages has lately occurred to me, and I should be glad to bring it before the notice of the meeting. The well-known Kamilaroi system is as follows: I add the letters A., B., C., D., to the names of the sub-phratries for the purposes of the explanation to be offered:—

Table I.

A Male	marries a	and the children are
A. Muri	D. Kumbo	C. Ipai.
B. Kubi	C. Ipai	D. Kumbo.
C. Ipai	B. Kubi	A. Muri.
D. Kumbo	A. Muri	B. Kubi

I had often tried, in vain, to find an easy clue to this strange custom, feeling assured that no aboriginal Australian brain could acquire the accurate and almost instinctive knowledge they all have of it without one. At last, I think, or rather hope, that I have succeeded. We now know that the Muri and the Kubi are sub-phratries of the phratrie called Dilbi, let us designate this phratrie by the letter P.; also that the Ipai and the Kumbo are sub-phratries of the phratrie called Kupathin, and this phratrie we will designate by Q. More briefly, A. and B. are sub-phratries of P.; C. and D. are sub-phratries of Q.

Now if we suppose a cross division, such that A. and C. are both alike in some respect, which we will indicate by appending to them the numeral 1, and that B. and D. are also alike in some contrasted respect, which we will designate by appending to them the numeral 2, we shall convert Table I into Table II.

TABLE II.

Phratries.	A male	marries a female	their children are
P.	A. = P. 1	Q. 2	Q. 1
	B. = P. 2	Q. 1	Q. 2
Q.	C. = Q. 1	P. 2	P. 1
	D. = Q. 2	P. 1	P. 2

The last three columns are to read thus :—A male P. 1 marries a female Q. 2; their children are Q. 1. A male P. 2 marries a female Q. 1; their children are Q. 2; and so on. We see at once from this that a man may not marry a woman who has the same letter or the same numeral as himself, and that the children take after the letter of their mother and after the numeral of their father. Amongst the Kiabara the marriage rule is exactly the same, except that the children take after the numeral of their mother and after the letter of their father.

It is extremely difficult, if not impossible, to find good analogies in civilised life to these phratries and sub-phratries. I will, therefore, take an illustration that does not profess to be applicable, otherwise than by giving an adequate idea of the sort of function that is intended to be described by these numerals and letters. Suppose persons of both sexes to be educated, some at Oxford, and some at Cambridge. Again, suppose persons of both sexes to be members of one or other of two clubs to which members of either university are equally admissible, such as are the Oxford and Cambridge Club and the University Club. Then the Australian marriage rule is analogous to saying that a man may not marry a woman who is a member either of the same university or of the same club as himself. Also, that, if he be one of the Kamilaroi, the children will be entered at their mother's university and at his club; but if he be one of the Kiabara, the children will be entered at his university, and at their mother's club. A rule so simple as this could be understood by any savage, whose totem and other customs are quite as distinct, and affect a far larger part of their lives than the consequences of being an Oxford or a Cambridge man, and of belonging to this club or that, affect ours. Now comes the testing question, does such a cross division as that which I have supposed, really exist? I communicated with Mr. Frazer on this subject, whose recent volume on Totemism is very favourably known. He pointed out to me that Mr. Ridley called the Muri the highest grade and the Kubi the lowest, and that, he adds, "so every family passes in two or three or four generations, through the highest and lowest grades—a curious combination of the ideas of aristocracy and levelling—but the difference in rank is slight." Mr. Frazer also informs me that Prof. Müller, of Vienna, has quoted apparently from an early work of Mr. Ridley, whether by mistake or not, I do not know, in a different sense, making the Ipai and Kumbo patricians, and the Muri and Kubi plebians. It is reasonable to believe that the zealous inquirers into Australian totems and other distinctions have not yet got wholly to the bottom of them, and that an as-yet-undiscovered cross division, such as I have supposed, may be found on further inquiry to exist. Mr. Frazer has written on this subject to his Australian correspondents, and I await the result with much curiosity. If my expectations are falsified, I can at all events recommend my theory as a *memoria technica*, by which the complexities of the Australian marriage customs may easily be kept in mind.

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Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, May 25, 1888.

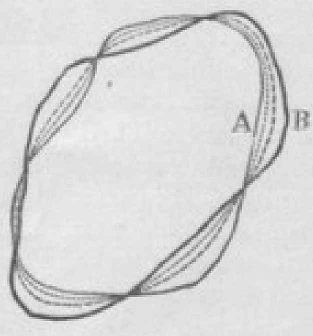
JOHN RAE, M.D. LL.D. F.R.S. Vice-President, in the Chair.

FRANCIS GALTON, Esq. M.A. F.R.S. *M.R.I.*

*Personal Identification and Description.**

IT is strange that we should not have acquired more power of describing form and personal features than we actually possess. For my own part I have frequently chafed under the sense of inability to verbally explain hereditary resemblances and types of features, and to describe irregular outlines of many different kinds, which I will not now particularise. At last I tried to relieve myself as far as might be from this embarrassment, and took considerable trouble, and made many experiments. The net result is that while there appear to be many ways of approximately effecting what is wanted, it is difficult as yet to select the best of them with enough assurance to justify a plunge into a rather serious undertaking. According to the French proverb, the better has thus far proved an enemy to the passably good, so I cannot go much into detail at present, but will chiefly dwell on general principles.

FIG. 1.



Measure of Resemblance.—We recognise different degrees of likeness and unlikeness, though I am not aware that attempts have as yet been made to measure them. This can be done if we take for our unit the *least discernible difference.* The application of this principle to irregular contours is particularly easy. Fig. 1 shows two such contours, A and B, which might be meteorological, geographical, or anything else. They are drawn with firm lines, but of different strengths for the sake of distinction. They contain the same area, and are so superimposed as to lie as fairly one over the other as may be. Now draw a broken contour which we will call C, equally subdividing the intervals between A and B; then C will be more like A than B was. Again draw a dotted contour, D, equally subdividing the intervals between C and A; the likeness of D to A will be again

* The substance of the lecture is here reprinted from 'Nature' of June 21 and 28, with the kind permission of the Editor, and after some slight revision by the author.

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closer. Continue to act on the same principle until a stage is reached when the contour last drawn is undistinguishable from A. Suppose it to be the fourth stage; then as $2^4 = 16$, there are sixteen grades of least-discernible differences between A and B. If one of the contours differs greatly in a single or few respects from the other, reservation may be made of those peculiarities. Thus, if A has a deep notch in its lower right-hand border, we might either state that fact, and say that in other respects it differed from B by only 16 grades of unlikeness, or we might make no reservation, and continue subdividing until all trace of the notch was smoothed away. It is purely a matter of convenience which course should be adopted in any given case. The measurement of resemblance by units of least-discernible differences is applicable to shades, colours, sounds, tastes, and to sense-indications generally. There is no such thing as infinite unlikeness, because the number of just discernible difference between any objects, however dissimilar, is always finite. A point as perceived by the sense of sight is not a mathematical point, but an object so small that its shape ceases to be discernible. Mathematically, it requires an infinitude of points to make a short line; sensibly, it requires a finite and not a large number of what the vision reckons as points, to do so. If from thirty to forty points were dotted in a row across the disk of the moon, they would appear to the naked eyes of most persons as a continuous line.

Description within Specified Limits.—It is impossible to verbally define an irregular contour with such precision that a drawing made from the description shall be undistinguishable from the original, but we may be content with a lower achievement. Much would be gained if we could refer to a standard collection of contours drawn with double lines, and say that the contour in question falls between the double lines of the contour catalogued as number so-and-so. This would at least tell us that none of the very many contours that fell outside the specified limits could be the one to which the description applied. It is an approximate and a negative method of identification. Suppose the contour to be a profile, and for simplicity's sake let us suppose it to be only the portion of a profile that lies below the notch that separates the brow from the nose, and extending only so far downwards as the parting between the lips. Suppose it also to be the mere outline of a shadow sharply cast upon the wall by a single source of light, such as is excellently seen when a person stands sideways between the electric lantern and the screen in a lecture-room. All human profiles of this kind, when they have been reduced to a uniform vertical scale, fall within a small space. I have taken those given by Lavater, which are in many cases of extreme shapes, and have added others of English faces, and find that they all fall within the space shown in Fig. 2. The outer and inner limits of the space are of course not the profiles of any real faces, but the limits of many profiles, some of which are exceptional at one point, and others at another. We can classify the great majority of profiles so that each of them shall be included between the double borders of one, two, or some small number of standard portraits, such as Fig. 3. I am as yet unprepared to say how near together the double borders of such standard portraits should be drawn; in other words, what is the smallest number of grades of unlikeness that we can satisfactorily deal with. The process of sorting profiles into their proper classes and of gradually building up a well-selected standard collection, is a laborious undertaking if attempted by any obvious way, but I believe it can be effected with comparative ease on the basis of measurements, as will be explained later on, and by an apparatus that will be described.

Fig. 2.

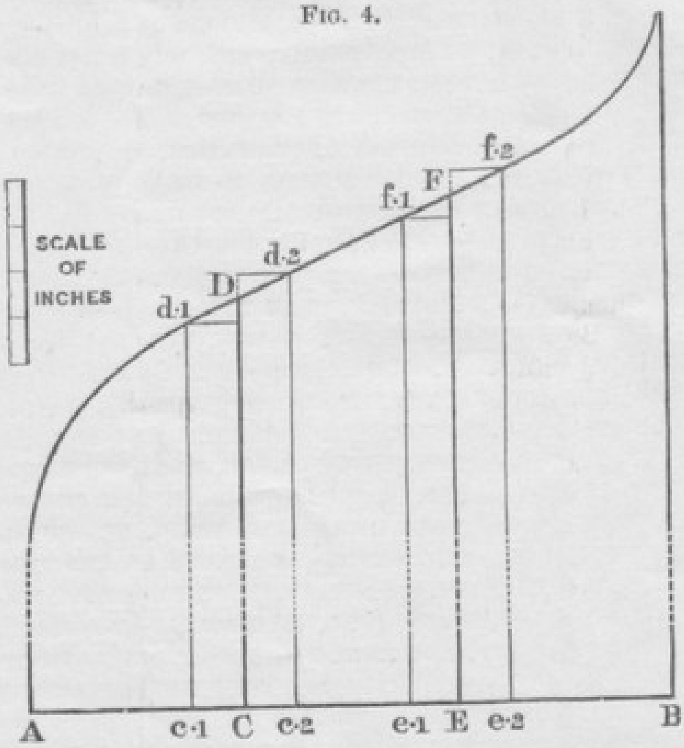
Fig. 3.

Classification of Sets of Measures.—Prisoners are now identified in France by the measures of their heads and limbs, the set of measures of each suspected person being compared with the sets that severally refer to each of many thousands of convicts. This idea, and the practical application of it, is due to M. Alphonse Bertillon. The actual method by which this is done is not all that could be theoretically desired, but it is said to be effective in action, and enables the authorities quickly to assure themselves whether the suspected person is or is not an old malefactor. The primary measures in the classification are four—namely, the head length, head breadth, foot length, and middle-finger length of the left foot and hand respectively. Each of these is classified according as it is large, medium or small. There are thus three, and only three, divisions of head lengths, each of which is subdivided into three divisions of head breadth; again, each of these is further subdivided into three of foot length, and these again into three of middle-finger length; thus the number of primary classes is equal to three multiplied into itself four times—that is to say, their number is eighty-one, and a separate pigeon-hole is assigned to each. All the exact measures and other notes on each criminal are written on the same card, and this card is stored in its appropriate pigeon-hole. The contents of each pigeon-hole are themselves sub-sorted on the same principle of three-fold classification in respect to other measures. This process can, of course, be extended indefinitely, but how far it admits of being carried on advantageously is another question. The fault of all hard-and-fast lines of classification, when variability is continuous, is the doubt where to place and where to look for values that are near the limits between two adjacent classes. Let us take Stature as an illustration of what must occur in every case, and let us represent its distribution by what I have called a "Scheme," as shown in Fig. 4.

Here the statures of any large group of persons are represented by lines of proportionate length. The lines are arranged side by side at equal distances apart on a base, A B, of convenient length.

A curve drawn through their tops gives the upper boundary of the scheme; the lines themselves are then wiped out, having served their purpose. If the base A B be divided into three equal parts and perpendiculars, C, D; E, F, be erected at the divisions between them, reaching from the base up to the curve, then the lengths of those

Fig. 4.



perpendiculars will be proportionate to the limiting values between the small and the medium group, and between those of the medium and the large group, respectively. The difference between these perpendiculars in the case of stature is about 2·3 inches. In other words, the shortest and tallest men in the medium class differ only by that amount. We have next to consider how much ought reasonably to be allowed for error of measurement. Considering that a man differs in height by a full third of an inch between the time of getting up in the morning and lying down at night; considering also that measures are recorded to the nearest tenth of an inch at the closest, also the many uncertainties connected with the measurement of stature, it would be rash not to allow for a possible (I do not say "probable") error of at least ± half an inch. Prolong C D, and note the points upon it at the distance of half an inch above and below D; draw horizontal lines from those points to meet the curve at *d*.1, *d*.2, and from the points of intersection drop perpendiculars reaching the base at *c*.1, *c*.2. A similar figure is drawn at F.

Then the ratio borne by the uncertain entries to the whole number of entries is as $c_1 c_2 + e_1 e_2$ to A B. This, as seen by the diagram, is a very large proportion. There is a dilemma from which those who adopt hard-and-fast lines of classification cannot escape: either the fringe of uncertainty must be dangerously wide, or else the delicacy with which measures are made cannot be turned to anything like its full account. If the delicacy is small, the fringe of uncertainty must be very wide; if the delicacy is great, the summed widths of all the fringes will be narrow, so long as there are only a few classes; but, on the other hand, by having only a few classes, most of the advantages of possessing delicate observations are wasted. The bodily measurements are so dependent on one another that we cannot afford to neglect small distinctions in an attempt to make an effective classification. Thus long feet and long middle-fingers usually go together. We therefore want to know whether the long feet in some particular person are accompanied by very long, or moderately long, or barely long fingers, though the fingers may in all three cases have been treated as long in M. Bertillon's system of classes, because they would be long as compared with those of the general population. Certainly his eighty-one combinations are far from being equally probable. The more numerous the measures the greater would be their interdependence, and the more unequal would be the distribution of cases among the various possible combinations of large, small, and medium values. No attempt has yet been made to estimate the degree of their interdependence. I am therefore having the above measurements (with slight necessary variation) recorded at my anthropometric laboratory for the purpose of doing so. This laboratory, I may add, is now open to public use under reasonable restrictions. It is entered from the Science Collections in the Western Galleries at South Kensington.

Mechanical Selector.—Feeling the advantage of possessing a method of classification that did not proceed upon hard and fast lines, I contrived an apparatus that is quite independent of them, and which I call a mechanical selector. Its object is to find which set, out of a standard collection of many sets of measures, resembles any one given set within any given degree of unlikeness. No one measure in any of the sets selected by the instrument can differ from the corresponding measure in the given set by more than a specified value. The apparatus is very simple; it applies to sets of measures of every description, and ought to act on a large scale as well as it does on a small one, with great rapidity, and be able to test several hundred sets by each movement. It relieves the eye and brain from the intolerable strain of tediously comparing a set of many measures with each of a large number of successive sets, in doing which a mental allowance has to be made for a *plus* or *minus* deviation of a specified amount in every entry. It is not my business to look after prisoners, and I do not fully know what need may really exist for new methods of quickly identifying suspected persons. If there be any real need, I should think that this apparatus, which is contrived for other purposes, might, after obvious modifications, supply it.

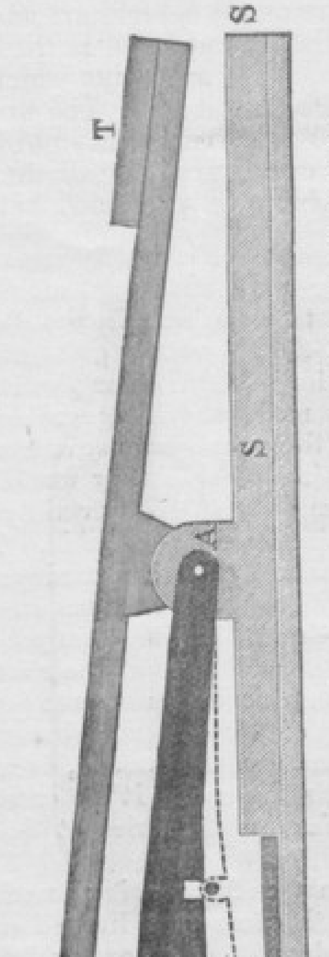


Fig. 5.

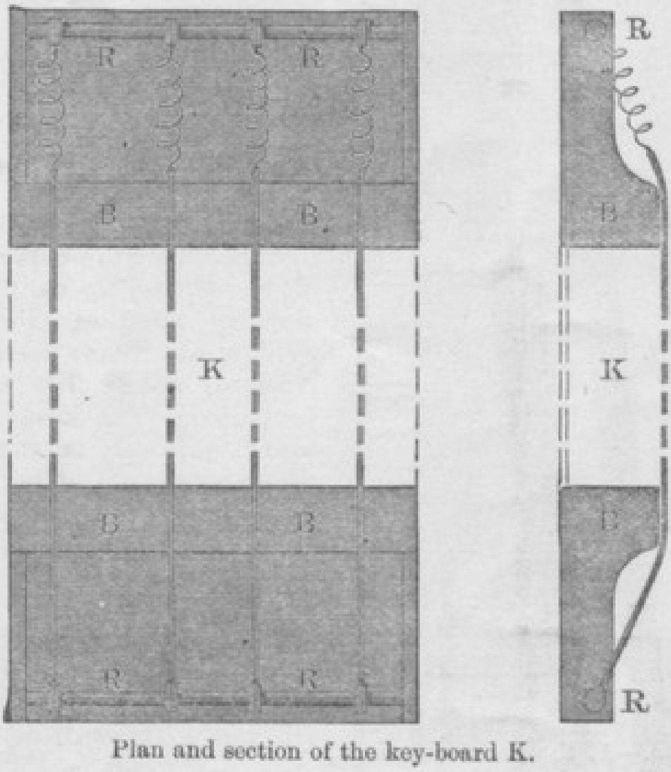
Section of the apparatus, but the bridges and rods are not shown, only the section of the wires.

The apparatus consists, in principle, of a large number of strips of card or metal *c* 1, *c* 2 (Fig. 5), say 8 or 9 inches long, and having a common axis A passing through all their smaller ends. A tilting-frame T, which turns on the same axis, has a front cross-bar F (whose section is seen in Fig. 5), on which the tips of the larger ends of all the cards rest whenever the machine is left alone. In this condition a counterpoise at the other end of T suffices to overcome the weight of all the cards, and this heavier end of T lies on the base-board S. When the heavy end of T is lifted, as shown in Fig. 5, its front-bar F is of course depressed, and the cards being individually acted on by their own weights, are free to descend with the cross-bar unless they are otherwise prevented. The lower edge of each card is variously notched to indicate the measures of the person it represents. Only four notches are shown in the figure, but six could be employed in a card of 8 or 9 inches long, allowing compartments of 1 inch in length to each of six different measures. The position of the notch in the compartment allotted to it, indicates the corresponding measure according to a suitable scale. When the notch is in the middle of a compartment, it means that the measure is of mediocre amount; when at one end of it, the measure is of some specified large value or of any other value above that; when at the other end the measure is of some specified small value or of any other value below it. Intermediate positions represent intermediate values according to the scale. Each of the cards corresponds to one of the sets of measures in the standard collection. The set of measures of the given person are indicated by the positions

f. 4r

of parallel strings or wires, one for each measure, that are stretched between Rods and across Bridges at either end of a long board set cross-ways to the cards. Their positions on the bridges are adjusted by the same scale as that by which the notches were cut in the cards. Figs. 6*a* and 6*b* are views of this portion of the apparatus, which acts as a key, and is of about 30 inches in effective length. The whole is shown in working position in Fig. 7. When the key is slid into its place, and the heavy end of the tilting-frame T is raised, all the cards are free to descend so far as the tilting-frame is concerned, but they

FIG. 6*a*. 6*b*.



Plan and section of the key-board K.

are checked by one or more of the wires from descending below a particular level, except those few, if any, whose notches correspond throughout to the positions of the underlying wires. This is the case with the card *c*2 (Fig. 5), drawn with a dotted outline, but not with *c*1, which rests upon the third wire, counting from the axis. As the wires have to sustain the weight of all or nearly all the cards, frequent narrow bridges must be interposed between the main bridges to sustain the wires from point to point. The cards should be divided into batches by partitions corresponding to these interposed bridges, else they may press sideways with enough friction to interfere with their free independent action. Neither these interposed bridges nor the partitions are drawn in the figure. The method of adjusting the wires there shown,

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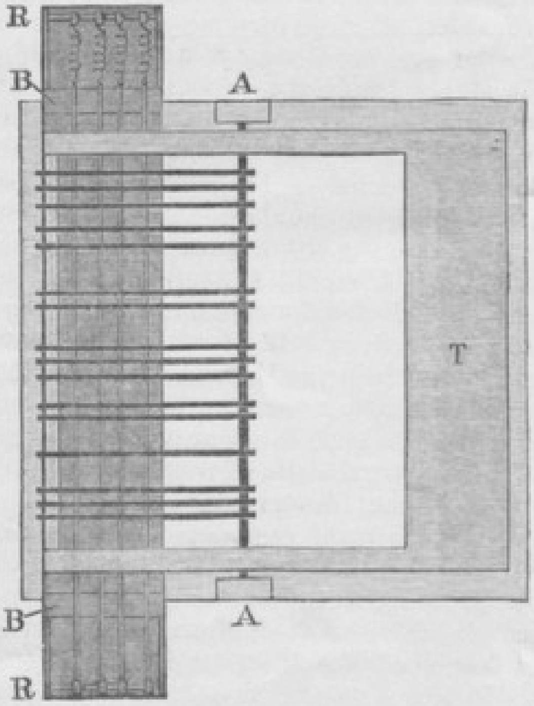
is simply by sliding the rings to which they are attached at either end along the rod which passes through them. It is easy to arrange a more delicate method of effecting the adjustment if desired. Hitherto I have snipped out the notches in the cards with a cutter made on the same principle as that used by railway guards in marking the tickets of travellers. The width of the notch is greater than the width of the wire by an amount proportionate to the allowance intended to be made for error of measurement, and also for that

Fig. 7.



Reduced plan of complete apparatus.

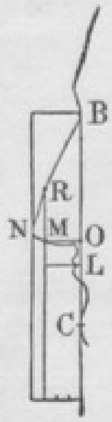
Explanation.—A, the common axis; c_1, c_2, the cards; T, tilting-frame, turning on A (the cards rest by their front ends on F, which is the front cross-bar of T, at the time when the heavy hinder end of T rests on the base-board S); K is the key-board; R R are the rods between which the wires are stretched; B B are the bridges at either end of the key-board, over which the wires pass. (The explanation refers to the other figs., as well as to this.)

due to mechanical misfit. There seems to be room for 500 cards or metal strips, and ample room for 300 of them, to be arranged in sufficiently loose order within the width of 30 inches, and a key of that effective length would test all these by a single movement. It could also be applied in quick succession to any number of other collections.

Measurement of Profiles.—The sharp outline of a photographed profile admits of more easy and precise measurement than the yielding outline of the face itself. The measurable distances between the profiles of different persons are small, but the available measures are much more numerous than might have been expected, and their variations are more independent of one another than those of the limbs. I suspect that measures of the profile may be nearly as trustworthy as those of the limbs for approximate identification, that is, for excluding a very large proportion of persons from the possibility of being mistaken for the one whose measurements are given. The measurement of a profile enables us to use a mechanical selector for finding those in a large standard collection to which they nearly correspond. From the selection thus made, the eye could easily make a further selection of those that suited best in other respects. A mechanical selector also enables us to quickly build up a standard collection step by step, by telling us whether or no each fresh set of measures falls within the limits of any of those already collected. If it does, we know that it is already provided for; if not, a new card must be added to the collection. There will be no fear of duplications, as every freshly-added standard will differ from all its predecessors by more than the specified range of permitted differences.

As regards the most convenient measurements to be applied to a profile for use with the selector, I am unable as yet to speak decidedly. If we are dealing merely with a black silhouette, such as the shadow cast on a wall by a small and brilliant light, the best line from which to measure seems to be B C in Fig. 8; namely, that which touches both the concavity of the notch between the brow and nose, and the convexity of the chin. It is not difficult to frame illustrated instructions to explain what should be done in the cases where no line can be drawn that strictly fulfils these conditions. I have taken a considerable number of measures from the line that touches the brow and chin, but am now inclined to prefer that which I have just described. A sharp unit of measurement is given by the distance between this line and another drawn parallel to it just touching the nose, as at N in the figure. A small uncertainty in the direction of B C has but a very trifling effect on this distance. By dividing the interval between these parallel lines into four parts, and drawing a line through the third of the divisions, parallel to B C, we obtain the two important points of reference, M and R. M is a particularly well-defined point, from which O is determined by dropping a perpendicular from M upon B C. O seems the best of all points from which to measure. It is excellently placed for defining the shape and position of the notch between the nose and the upper lip, which is perhaps the most distinctive feature in the profile. O L can be determined with some precision; O B and O C are but coarse measurements.

Fig. 8.



In addition to these and other obvious measures, such as one or more to define the projection of the lips, it would be well to measure

f. 5v

the radius of the circle of curvature of the depression at B, also of that between the nose and the lip, for they are both very variable and very distinctive. So is the general slope of the base of the nose. The difficulty lies not in selecting a few measures that will go far towards negatively identifying a face, but in selecting the best—namely, those that can be most precisely determined, are most independent of each other, most variable, and most expressive of the general form of the profile. I have tried many different sets, and found all to be more or less efficient, but have not yet decided to my own satisfaction which to adopt.

We will now suppose that either by the above method or by any other, a standard collection of doubly outlined portraits such as that in Fig. 3, has been made and come into use, so that a profile can be approximately described by referring it to number so-and-so in the catalogue. If the number it contained was less than 1000, three figures would suffice to define any one of them. We will now consider how a yet closer description of the profile may be given by using a few additional figures. One way of doing so is to have short cross-lines drawn at critical positions between the two outlines of the standard, and to suppose each of them to be divided into eight equal parts. The intersection of the cross-lines with the outer border would count as 0; that with the inner border as 8, and the intermediate divisions from 1 to 7. As the cross-lines would be very short, a single numeral would thus define the position of a point in any one of them, with perhaps as much precision as the naked eye could utilise. By employing as many figures as there are cross-lines in the standard, each successive figure for each successive cross-line, a corresponding number of points in the profile would be fixed with great accuracy. Suppose a total of nine figures to be allowed, then the first three figures would specify the catalogue number of the portrait to be referred to, and the remaining six figures would determine six points in the outline of the portrait with greatly increased precision.

I may say that after numerous trials of different methods for comparing portraits successively by the eye, I have found none so handy and generally efficient as a double-image prism, which I largely used in my earlier attempts in making composite portraits.

I have not succeeded in contriving an instrument that shall directly compare a given profile with those in a standard collection, and which shall at the same time act with anything like the simplicity of the mechanical selector, and with the same quick decision in acceptance or rejection. Still, I recognise some waste of opportunity in not utilising the power of varying the depths of the notches in the cards, independently of their longitudinal position.

Personal characteristics exist in much more minute particulars than those just described. Leaving aside microscopic peculiarities, which are of unknown multitudes, such as might be studied in the 800,000,000 specimens cut by a microtome, say of one two-thousandth part of an inch in thickness, and one-tenth of an inch each way in area, out of the 4000 cubic inches or so of the flesh, fat, and bone of a single average human body, there are many that are visible with or without the aid of a lens.

The markings in the iris of the eye are of the above kind. They have been never adequately studied except by the makers of artificial eyes, who recognise thousands of varieties of them. These markings well deserve being photographed from life on an enlarged scale. I shall not dwell now upon these, nor on such peculiarities as those of handwriting, nor on the bifurcations and interlacements of the superficial veins, nor on the shape and convolutions of the external ear. These all admit of brief approximate description by the method just explained—namely, by reference to the number in a standard collection of the specimen that shall not differ from it by more than a specified number of units of unlikeness. I have already explained what is meant by a unit of unlikeness, and the mechanical means by which a given set of measures can be compared with great ease and by a single movement with every set simultaneously, in a large standard collection of sets of measures.

Perhaps the most beautiful and characteristic of all superficial marks are the small furrows, with the intervening ridges and their pores, that are disposed in a singularly complex yet regular order on the under surfaces of the hands and the feet. I do not now speak of the large wrinkles in which chiromantists delight, and which may be compared to the creases in an old coat, or to the deep folds in the hide of a rhinoceros, but of those fine lines of which the buttered fingers of children are apt to stamp impressions on the margins of the books they handle, that leave little to be desired on the score of distinctness. These lines are found to take their origin from various centres, one of which lies in the under surface of each finger-tip. They proceed from their several centres in spirals and whorls, and distribute themselves in beautiful patterns over the whole palmar surface. A corresponding system covers the soles of the feet. The same lines appear with little modification in the hands and feet of monkeys. They appear to have been carefully studied for the first time by Purkinje in 1822, and since then they have attracted the notice of many writers and physiologists, the fullest and latest of whom is Kollman, who has published a pamphlet, 'Tastapparat der Hand' (Leipzig, 1883), in which their physiological significance is fully discussed. Into that part of the subject I am not going to enter here. It has occurred independently to many persons to propose finger-marks as a means of identification. In the last century, Bewick, in one of the vignettes in the 'History of Birds,' gave a woodcut of his own thumb-mark, which is the first clear impression I know of, and afterwards one of his finger-marks. Some of the latest specimens that I have seen are by Mr. Gilbert Thomson, an officer of the American Geological Survey, who, being in Arizona, and having to make his orders for payment on a camp suttler, hit upon the expedient of using his own thumb-mark to serve the same purpose as the elaborate scroll engraved on blank cheques—namely, to make the alteration of figures written on it impossible without detection. I possess copies of two of his cheques. A San Francisco photographer, Mr. Tabor, made enlarged photographs of the finger-marks of Chinese, and his proposal to employ them as a means of identifying Chinese immigrants, seems to have been seriously considered. I may say that I can obtain no verification of a common statement that the method is in actual use in the prisons of China. The thumb-mark has been used there as elsewhere in attestation of deeds, such as a man might make an impression with a common seal, not his own, and say, " This is my act and deed "; but I cannot hear of any elaborate system of finger-marks having ever been employed in China for the identification of prisoners. It was, however, largely used in India, by Sir William Herschel, many years ago, when he was an officer of the Bengal Civil Service. He found it to be most successful in preventing personation, and in putting an end to disputes about the authenticity of deeds. He described his method fully in ' Nature,' in 1880 (vol. xxiii. p. 76), which should be referred to; also a paper by Mr. Faulds in the next volume. I may also allude to articles in the American journal ' Science,' 1886 (vol. viii. pp. 166 and 212.)

The question arises whether these finger-marks remain unaltered throughout the life of the same person. In reply to this I am enabled to submit a most interesting piece of evidence, which thus far is

Fig. 9.

Enlarged impressions of the fore and middle finger tips of the right hand of Sir William Herschel, made in the year 1860.

unique, through the kindness of Sir Wm. Herschel. It consists of the imprints of the two first fingers of his own hand, made in 1860 and in 1888 respectively, that is, at periods separated by an interval of twenty-eight years. I have also two intermediate imprints, made by him in 1874 and in 1883 respectively. Figs. 9 and 11 are cut from photographs on an enlarged scale of the imprints of 1860 and 1888, which were made direct upon the engraver's block; these woodcuts may therefore be relied on as very correct representations of the originals in my present possession. Fig. 10 refers to the portion of Fig. 9 to which I am about to draw attention. On first examining these and other finger-marks, the eye wanders and becomes confused, not knowing where to fix itself; the points shown in Fig. 10 are

Fig. 10.

Positions of furrow-heads and bifurcations of furrows, in Fig. 9.

Fig. 11.

Enlarged impressions of the fore and middle finger tips of the right hand of Sir William Herschel, made in the year 1888.

those it should select. They are the places at which each new furrow makes its first appearance. The furrows may originate in two principal ways, which are not always clearly distinguishable: (1) the new furrow may arise in the middle of a ridge; (2) a single furrow may bifurcate and form a letter Y. The distinction between (1) and (2) is not greatly to be trusted, because one of the sides of the ridge in case (1) may become worn, or be narrow and low, and not always leave an imprint, thus converting it into case (2); conversely case (2) may be converted into case (1). The position of the origin of the new furrow is, however, none the less defined. I have noted the furrow-heads and bifurcations of furrows in Fig. 9, and shown them separately in Fig. 10. The reader will be able to identify these positions with the aid of a pair of compasses, and he will find that they persist unchanged in Fig. 11, though there is occasionally uncertainty between cases (1) and (2). Also there is a little confusion in the middle of the small triangular space that separates two distinct systems of furrows, much as eddies separate the stream lines of adjacent currents converging from opposite directions. A careful comparison of Figs. 9 and 11 is a most instructive study of the effects of age. There is an obvious amount of wearing and of coarseness in the latter, but the main features in both are the same.

I happen to possess a very convenient little apparatus for examining finger-marks and for recording the positions of furrow-heads. It is a slight and small, but well-made wooden pentagraph, multiplying five-fold, in which a very low-power Microscope, with *coarse* cross-wires, forms the axis of the short limb, and a pencil-holder forms the axis of the long limb. I contrived it for quite another use, namely, the measurement of the length of wings of moths in some rather extensive experiments that are now being made for me in pedigree moth-breeding. It has proved very serviceable in this inquiry also, and was much used in measuring the profiles spoken of in the last article. Without some moderate magnifying power the finger-marks cannot be properly studied. It is a convenient plan, in default of better methods, to prick holes with a needle through the furrow-heads into a separate piece of paper, where they can be studied without risk of confusing the eye. There are peculiarities often found in furrows that do not appear in these particular specimens, and to which I will not further refer. In Fig. 10 the form of the origin of the spirals is just indicated. These forms are various; they may be in single or in multiple lines, and the earlier turns may form long loops or be nearly circular. My own ten fingers show at least four distinct varieties.

Notwithstanding the experience of others to the contrary, I find it not easy to make clear and perfect impressions of the fingers. The proper plan seems to be to cover a flat surface, like that of a piece of glass or zinc, with a thin and even coat of paint, whether it be printers' ink or Indian ink rubbed into a thick paste, and to press the finger lightly upon it so that the ridges only shall become inked, then the

f. 8

inked fingers are pressed on smooth and slightly damped paper. If a plate of glass be smoked over a paraffin lamp, a beautiful negative impression may be made on it by the finger, suitable for a lantern transparency. The blackened finger may afterwards be made to leave a positive impression on a piece of paper, that requires to be varnished if it is to be rendered permanent. All this is rather dirty work, but people do not seem to object to it; rivalry and the hope of making continually better impressions carries them on. It is troublesome to make plaster casts; modelling-clay has been proposed; hard wax, such as dentists use, acts fairly well; sealing-wax is excellent if the heat can be tolerated; I have some good impressions in it. For the mere study of the marks, no plan is better than that of rubbing a little thick paste of chalk ("prepared chalk") and water or sized water upon the finger. The chalk lies in the furrows, and defines them. They might then be excellently photographed on an enlarged scale. My own photographic apparatus is not at hand, or I should have experimented in this. When notes of the furrow-heads and of the initial shape of the spiral have been made, the measurements would admit of comparison with those in catalogued sets by means of a numerical arrangement, or even by the mechanical selector described in the last article. If a cleanly and simple way could be discovered of taking durable impressions of the finger tips, there would be little doubt of their being serviceable in more than one way.

In concluding my remarks, I should say that one of the inducements to making these inquiries into personal identification has been to discover independent features suitable for hereditary investigation. It has long been my hope, though utterly without direct experimental corroboration thus far, that if a considerable number of variable and independent features could be catalogued, it might be possible to trace kinship with considerable certainty. It does not at all follow because a man inherits his main features from some one ancestor, that he may not also inherit a large number of minor and commonly overlooked features from many ancestors. Therefore it is not improbable, and worth taking pains to inquire whether each person may not carry visibly about his body undeniable evidence of his parentage and near kinships.

[F. G.]

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TABLES OF OBSERVATIONS.

BY

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1889.

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Tables *of* Observations.

By Francis Galton, F.R.S.

The following observations are printed for the use of those who desire to investigate the relations of the various measurements of the same individuals, whether among themselves or in connection with birthplace, occupation, &c. The observations were made at the International Exhibition in 1884, with the same instruments, the same methods, and mostly by the same operator. They refer to male adults of the ages 23–26 and have been tabulated for me by Mr. J. H. Young, of the General Register Office, from the original documents. There were 518 available returns altogether, but I thought it better to limit the publication to the round number of 400 and to take the opportunity thereby afforded of weeding out all foreigners, or persons of abnormal antecedents or occupation, so that the remainder should be as homogeneous a group as circumstances admitted. I may add that the data used in my recent memoir on *Correlation* (Proc. Royal Soc., December, 1888) were entirely different to these. Consequently the present publication will afford useful material for checking the results arrived at in that memoir.

I did not care to enlarge the table by including other measurements that had been made at the same time. As much is printed as the page can conveniently hold, and the omitted measurements seem to be of secondary importance from the present point of view.

Age last birth-day.	Married or un-married.	Eye Colour.	Birthplace.	Occupation.	Residence.	Breathg. Capacity. cubic inches.	Squeeze in lbs.		Span. inches.	Height from Seat. inches.	Height without shoes. inches.	Weight.* lbs.
							Rt. hand.	Left hand.				
23	Unmarr.	Brown	Fulwood, Lancashire	Clerk	Suburb	76	67	64	64·7	34·0	64·0	111·5
23	Unmarr.	Blue	Portsmouth, Hants	Shipbroker	Suburb	214	91	80	74·0	36·7	72·2	143·0
23	Unmarr.	Dark grey	Rochdale, Yorkshire	Hatter	Town	240	71	70	68·4	34·9	66·1	125·5
23	Unmarr.	Dark grey	Newport, Salop	Schoolmaster	Suburb	230	79	76	68·0	36·6	66·6	146·0
23	Unmarr.	Grey	Cambridge	Draper's assistant	Town	234	79	77	71·7	36·8	69·8	143·5
23	Unmarr.	Blue grey	Oxford	Messenger	Town	238	87	81	69·3	36·6	67·5	143·0
23	Unmarr.	Dark grey	Woolwich, Kent	Builder	Town	254	71	64	71·8	38·3	68·7	156·25
23	Unmarr.	Brown	St. Thomas, North Devon	Clerk	Town	278	82	80	67·7	36·1	67·1	147·0
23	Unmarr.	Blue	New Cross, Kent	Mercantile	Suburb	300	104	103	71·0	37·0	71·5	179·5
23	Unmarr.	Blue	Paddington, Middlesex	Clerk	Town	260	87	88	72·6	37·1	71·8	145·0
23	Married	Blue	Paddington, Middlesex	Clerk	Town	252	91	90	72·8	37·6	71·2	143·75
23	Unmarr.	Blue grey	Ealing, Middlesex	Clerk	Suburb	230	93	93	70·0	35·8	68·1	177·5
23	Unmarr.	Brown grey	Knutsford, Cheshire	Surgeon	Country	250	73	82	70·3	34·8	68·3	155·5
23	Unmarr.	Dark grey	Birmingham, Warwickshire	Schoolmaster	Suburb	164	86	75	68·2	35·5	67·2	142·0
23	Unmarr.	Grey	London	Electrical engineer	Suburb	260	95	91	71·3	35·5	69·0	141·5
23	Unmarr.	Blue grey	Middlesex	Engineer	Suburb	270	112	105	68·0	41·7	79·5	203·25
23	Unmarr.	Blue grey	London	Land agent	Country	234	81	67	68·8	36·5	67·3	139·0
23	Unmarr.	Grey	Surrey	Barrister	Town	238	94	81	70·2	37·0	69·3	156·5
23	Unmarr.	Blue grey	Derby	Clerk	Town	238	61	60	66·6	33·5	64·5	118·0
23	Unmarr.	Brown	Kent	Cook	Suburb	208	80	75	69·5	35·0	67·2	127·0
23	Unmarr.	Green grey	Plymouth, Devonshire	Gentleman	Various	248	96	72	72·8	36·2	68·3	156·0
23	Unmarr.	Hazel	West Lynn, Norfolk	Chemist's assistant	Suburb	204	78	88	65·9	35·3	65·4	129·5
23	Unmarr.	Grey brown	Chaddesley, Worcestershire	Chemist and druggist	Town	234	88	87	72·0	37·1	70·7	143·25
23	Unmarr.	Brown	York	Assistant in manufactory	Town	220	78	70	65·5	35·4	64·0	126·75
23	Unmarr.	Brown	Camberwell, Surrey	Builder	Town	290	91	86	76·0	37·6	72·7	145·0
23	Unmarr.	Grey brown	London	Student	Country	208	90	86	76·0	39·1	73·8	151·5
23	Unmarr.	Grey	London	Builder	Suburb	278	80	80	77·0	38·2	72·0	164·0
23	Unmarr.	Blue	Derby	Pattern maker	Country	180	68	69	63·5	35·3	62·6	121·0
23	Unmarr.	Brown	City of London	Accountant	Town	174	75	64	61·25	33·3	62·3	129·0
23	Unmarr.	Blue grey	Devonshire	Carpenter	Town	222	77	77	67·75	35·2	67·8	128·25
23	Unmarr.	Brown grey	Westminster, Middlesex	Tailor	Town	206	72	68	70·0	34·2	66·0	132·25
23	Unmarr.	Brown	London, Surrey	Builder	Town	292	86	80	76·0	37·7	72·3	147·0
23	Unmarr.	Blue grey	London	Analyt. Chem. Teacher	Town	290	105	95	69·5	37·2	69·6	141·75
23	Unmarr.	Brown	Birkenhead, Cheshire	Pawnbroker	Town	158	68	49	66·2	32·6	62·1	121·0
23	Married	Brown	Manchester, Lancashire	Teacher	...	234	91	97	71·1	37·0	67·2	141·5
23	Unmarr.	Blue	Boston, Lincolnshire	Bank clerk	Country	270	75	72	71·6	35·6	67·6	146·25
23	Unmarr.	Blue	London	Compositor	Suburb	210	88	79	69·25	36·2	66·3	143·0
23	Unmarr.	Grey	Dunstable, Beds	Carpenter	Town	212	84	84	73·5	36·0	68·5	148·75
23	Unmarr.	Blue	Sheffield, Yorkshire	Architect's assistant	Town	206	76	85	66·75	35·8	65·2	123·0
23	Unmarr.	Grey	Sheffield, Yorkshire	Architect's assistant	Town	174	77	77	65·0	34·8	62·9	110·5

* The weight was taken in ordinary indoor clothing.

Age last birth-day.	Married or un-married.	Eye Colour.	Birthplace.	Occupation.	Residence.	Breathg. Capacity. cubic inches.	Squeeze in lbs. Rt. hand.	Squeeze in lbs. Left hand.	Span. inches.	Height from Seat. inches.	Height without shoes. inches.	Weight. lbs.
23	Unmarr.	Brown ...	Liverpool, Lancashire ...	Tinman	Suburb ...	218	82	72	66·5	35·4	65·5	122·5
23	Unmarr.	Blue ..	Gravesend, Kent	Carpenter	Town ...	226	78	80	70·5	36·5	69·8	142·5
23	Unmarr.	Blue ...	Bromley, Kent	Chemist...	Country ...	198	71	62	67·0	34·7	65·8	125·25
23	Unmarr.	Grey blue ...	Dumbarton...	Clerk	Town ...	188	76	75	69·5	37·1	69·2	135·25
23	Unmarr.	Grey ...	Worcestershire	Barrister	Town ...	222	71	73	68·5	35·1	66·8	154·0
23	Unmarr.	Blue grey ...	London, Surrey	Tea dealer	Suburb ...	218	78	79	70·6	36·0	67·5	143·0
23	Married	Brown grey	Wrexham, Denbigh ...	Law clerk	Suburb ...	172	69	62	65·0	34·7	65·3	127·0
23	Unmarr.	Blue ...	Oakham, Rutlandshire ...	Joiner	Suburb ...	228	80	75	70·7	35·7	68·2	137·5
23	Unmarr.	Brown ...	Dowderwell, Gloucester ...	Student	Town ...	280	112	108	70·25	38·0	69·7	161·5
23	Unmarr.	Grey ...	London	Tutor	Country ...	250	88	77	71·5	37·4	70·1	147·0
23	Unmarr.	Blue ...	Beddington, Surrey ...	Clerk	Cy. town ...	182	83	80	68·8	34·9	66·2	131·0
23	Unmarr.	Grey brown...	London	Bank clerk	Country ...	184	71	67	67·2	34·8	65·4	134·75
23	Unmarr.	Blue ...	Hull, Yorkshire	Clerk	Town ...	238	78	72	64·2	35·3	64·6	135·25
23	Unmarr.	Blue grey ...	Clapham, Surrey	Medical student	Suburb ...	306	79	75	69·5	38·0	69·7	169·25
23	Unmarr.	Grey blue ...	Woodford, Essex	Engineer	Country ...	318	102	97	72·3	36·4	69·5	172·5
23	Unmarr.	Blue ...	London	Sailor	?	184	89	78	66·25	36·4	63·9	145·0
23	Unmarr.	Brown ...	Colnbrook, Bucks ...	Teacher	Suburb ...	220	97	91	66·4	35·9	68·7	135·5
23	Married	Hazel brown	Portsmouth, Hants ..	Waiter	Town ...	206	86	78	70·0	34·0	66·2	142·0
23	Unmarr.	Brown ...	London	Accountant	Town ...	180	76	60	73·2	35·1	68·1	123·25
23	Unmarr.	Blue ...	Wandsworth, Surrey ...	Watchmaker	Suburb ...	160	70	68	64·0	?	62·4	117·0
23	Unmarr.	Grey brown	London	Medical student	Town ...	300	91	90	74·7	37·5	74·0	152·5
23	Unmarr.	Blue ...	London	Carpenter	Town ...	210	84	80	65·2	35·2	65·6	134·0
23	Unmarr.	Brown grey	Lambeth, Surrey	Labourer	Town ...	184	56	57	65·75	34·4	63·0	115·0
23	Unmarr.	Blue grey ...	Farringdon, Hants ...	Smith	Town ...	214	71	77	67·5	35·2	66·7	131·25
23	Unmarr.	Blue ...	Walworth, Surrey... ...	Carpenter	Suburb ...	230	89	80	70·1	33·9	66·5	131·0
23	Unmarr.	Blue ...	Redhill, Surrey	Clerk	Town ...	220	95	101	76·5	38·3	71·0	143·25
23	Unmarr.	Blue grey ...	London	Analytical chemist ...	Town ...	250	100	99	69·5	37·0	69·2	137·0
23	Unmarr.	Blue ...	Scotland	Clerk	Town ...	250	84	85	69·3	36·3	68·2	158·5
23	Unmarr.	Blue grey ...	North Devon	Architect	Country ...	214	68	69	69·0	34·8	66·8	145·0
23	Unmarr.	Grey blue ...	Warminster, Wiltshire ...	Footman	Town ...	200	85	83	68·2	36·0	66·5	137·5
23	Unmarr.	Grey ...	Swindon, Wiltshire ...	Clerk	Suburb ...	228	87	93	67·8	35·5	65·6	147·75
23	Unmarr.	Grey ...	Helston, Cornwall... ...	Tutor	Country ...	252	93	90	71·6	35·7	68·9	153·0
23	Unmarr.	Blue grey ...	Hants	Analytical chemist ...	Town ...	230	85	75	70·0	37·9	69·2	160·5
23	Unmarr.	Blue grey ...	London	— —	Town ...	237	69	61	70·9	36·4	69·2	136·0
23	Unmarr.	Blue grey ...	Bradford, Yorkshire ...	Merchant	Cy. town ...	248	92	82	71·8	36·1	69·3	155·5
23	Unmarr.	Hazel brown	Uppingham, Rutlandshire	Clerk	Town ...	238	89	89	67·0	34·8	66·4	153·0
23	Unmarr.	Grey ...	Devonshire...	Student	Suburb ...	298	95	88	70·8	36·1	67·2	146·0
23	Unmarr.	Light blue ...	London	Assistant warehouseman	Suburb ...	238	93	82	70·0	36·2	68·7	141·5
23	Unmarr.	Blue ...	London	Gentleman	Town ...	228	81	61	68·0	36·6	69·4	145·0
23	Unmarr.	Brown ...	Westminster, Middlesex ...	Printer's warehouseman	Suburb ...	190	78	74	65·0	35·4	64·4	148·0

Age last birth-day.	Married or un-married.	Eye Colour.	Birthplace.	Occupation.	Residence.	Breathg. Capacity. cubic inches.	Squeeze in lbs. Rt. hand.	Squeeze in lbs. Left hand.	Span. inches.	Height from Seat. inches.	Height. without shoes. inches.	Weight. lbs.
23	Unmarr.	Brown grey	Sheffield, Yorkshire	Clerk	Town	200	76	65	64·0	35·1	59·4	109·5
23	Unmarr.	Grey blue	Hants	Servant	Country	300	81	75	71·4	34·1	68·7	145·75
23	Unmarr.	Blue grey	London	Barrister	Town	260	87	78	74·5	38·3	71·2	157·25
23	Unmarr.	Grey blue	Brighton, Sussex	Provision merchant	Town	224	95	94	71·25	37·7	69·6	157·0
23	Unmarr.	Grey blue	Brentwood, Essex	Printer	Suburb	222	81	77	71·25	36·8	69·6	142·5
23	Unmarr.	Grey	Bromley, Kent	Chemist	Town	200	70	61	67·0	35·6	66·0	128·25
22	Unmarr.	Grey blue	Northampton	Engineer	Town	200	72	72	68·1	35·8	68·2	134·0
23	Unmarr.	Hazel	Berwick-on-Tweed	Letter carrier	Town	238	85	75	69·0	36·8	67·8	140·0
23	Unmarr.	Brown	London	Bank clerk	Town	218	103	79	69·0	37·2	68·2	138·0
23	Unmarr.	Blue	Clapham, Surrey	Theological student	Suburb	230	86	78	66·75	34·0	65·3	139·5
23	Unmarr.	Brown	Weybridge, Surrey	Surgeon	Town	307	87	88	72·5	37·1	70·3	154·0
23	Unmarr.	Grey brown	Petworth, Sussex	Evangelist	Town	198	94	83	74·2	35·6	70·0	164·5
23	Unmarr.	Hazel	Chelsea, Middlesex	Clerk	Suburb	185	74	72	68·5	33·8	63·9	120·5
23	Unmarr.	Blue	Liverpool, Lancashire	Shipowner	Suburb	180	74	74	67·0	35·5	67·5	151·0
23	Unmarr.	?	London	Stationer	Town	184	86	75	72·5	35·0	71·5	152·5
23	Unmarr.	Blue	Keighley, Yorkshire	Manufacturer	Country	178	85	72	64·7	34·8	64·7	130·5
23	Unmarr.	Brown grey	Edinburgh	Merchant	Town	220	95	81	69·5	34·0	68·2	153·25
23	Unmarr.	Grey blue	London	Solicitor	Town	278	98	95	70·5	36·2	68·0	158·0
23	Unmarr.	—	London	Curator (museum)	Town	210	85	80	69·25	36·3	67·0	143·0
23	Unmarr.	Blue	London	Clerk	Suburb	218	88	82	65·7	34·7	70·8	154·0
23	Unmarr.	Dark blue	London	Clerk	Town	290	95	94	70·5	36·8	69·3	138·75
23	Unmarr.	—	Gravesend, Kent	Clerk	Suburb	168	81	81	66·5	36·0	62·75	129·5
23	Unmarr.	Grey blue	Edinburgh	M.B.	Town	270	83	87	70·5	36·7	68·1	139·25
23	Unmarr.	Blue grey	Kensington, Middlesex	Civil Service	Suburb	180	80	80	68·5	35·0	67·0	157·0
23	Unmarr.	Blue	Melton, Yorkshire	Clerk	Suburb	130	45	65	65·0	33·6	62·0	131·5
23	Unmarr.	Brown	Manchester, Lancashire	Engineer	Suburb	180	85	84	67·5	34·9	65·2	124·0
23	Unmarr.	Brown	Camden Town, Middlesex	Clerk	Suburb	170	79	82	65·5	33·6	64·25	115·0
23	Unmarr.	Lt. blue grey	Enfield, Middlesex	Clerk	Suburb	184	89	85	67·5	34·75	67·0	140·0
23	Unmarr.	Dark blue	Sydenham, Kent	Clerk	Country	226	82	83	73·25	36·0	68·75	151·5
23	Unmarr.	Brown grey	Dublin	Physician	Town	286	80	81	74·5	36·8	72·5	152·0
23	Unmarr.	Dark blue	Dublin	Clerk	Suburb	219	82	82	67·0	33·5	64·0	119·0
23	Unmarr.	Brown	Hythe, Kent	Civil Service clerk	Suburb	184	85	90	68·0	36·7	68·1	139·5
23	Unmarr.	Grey blue	Edmonton, Middlesex	Lecturer	Country	244	73	74	68·0	35·5	67·2	141·0
23	Married	Blue grey	Tooting, Surrey	Grocer	Suburb	118	81	88	70·5	34·5	66·0	142·25
23	Unmrrr.	Lt. bwn. grey	Hastings, Sussex	Civil Service	Suburb	208	75	72	67·3	34·9	66·0	127·5
23	Unmarr.	Brown	Manchester	Joiner	Suburb	240	81	85	70·5	36·7	67·0	139·25
23	Unmarr.	Grey	Smethcote, Salop	Civil engineer	Country	196	90	83	76·75	35·7	71·8	145·5
23	Unmarr.	Blue	City	Clerk	Suburb	190	76	71	70·0	35·2	67·2	135·0
23	Unmarr.	Brown	Manchester, Lancashire	India rubber manufacturer	Town	216	96	78	70·0	36·8	68·3	130·0
23	Unmarr.	Blue	Aylesbury, Bucks	Draper's shopman	Country	196	84	87	64·3	34·3	64·4	124·5

Age last birth-day.	Married or un-married.	Eye Colour.	Birthplace.	Occupation.	Residence.	Breathg. Capacity. cubic inches.	Squeeze in lbs. Rt. hand.	Squeeze in lbs. Left hand.	Span. inches.	Height from Seat. inches.	Height without shoes. inches.	Weight. lbs.
23	Unmarr.	Grey blue ...	Wakefield, Yorkshire ...	Clerk	Town ...	218	99	93	72·0	36·3	71·3	154·25
23	Unmarr.	Brown grey...	Brighton, Sussex	Engineer	Country ...	220	100	99	71·2	39·2	71·6	145·25
23	Unmarr.	Blue	London...	Chemist	Suburb ...	208	87	96	68·3	36·0	67·7	149·25
23	Unmarr.	Grey blue ...	Reading, Berkshire	Chemist	Town ...	242	87	87	69·5	35·2	66·3	131·75
23	Unmarr.	Grey	Cambridge	Butler	Town ...	202	76	74	70·0	35·0	66·5	113·0
23	Unmarr.	Blue	Bucks	Civil engineer	Cy. town ...	270	80	86	68·5	36·8	70·0	147·25
23	Unmarr.	Grey blue ...	London...	Auctioneer	Town ...	290	95	91	75·0	36·9	72·3	164·0
23	Unmarr.	Grey	Mile End, Middlesex... ...	Librarian	Town ...	218	89	85	71·5	35·3	66·7	144·0
23	Unmarr.	Light brown	Norwich	Teacher	Suburb ...	208	81	81	63·9	35·4	62·5	120·75
23	Unmarr.	Brown ...	Pewsey, Wiltshire	Accountant	Town ...	172	68	71	68·0	36·8	69·0	131·5
23	Unmarr.	Hazel	Plymouth, Devonshire ...	H. M. dockyard	Town ...	208	74	71	69·5	36·0	67·4	132·5
23	Unmarr.	Brown	Limerick	Clerk	Town ...	198	70	65	66·5	36·0	66·0	126·4
23	Unmarr.	Brown grey...	St. Mary Cray, Kent ...	Teacher	Town ...	228	99	95	71·4	38·3	68·8	148·5
23	Unmarr.	Brown grey...	Bealings, Suffolk	Schoolmaster	Country ...	220	89	88	69·5	25·1	67·5	137·5
23	Unmarr.	Brown grey...	Durham	Clerk	Town ...	220	77	87	73·0	37·0	70·75	153·5
23	Unmarr.	Blue	Turnworth, Dorset	— —	Town ...	186	71	61	69·5	36·75	69·2	141·5
23	Married	Grey	London...	Clerk	Suburb ...	220	98	92	74·5	39·3	69·8	236·0
23	Married	Grey brown	London...	Clerk	Country ...	190	76	83	69·0	35·0	66·5	132·5
23	Unmarr.	Grey	London...	Surveyor	Country ...	220	85	83	72·0	36·4	68·3	149·0
23	Unmarr.	Grey brown...	Scarborough, Yorkshire ..	Chemist	Town ..	218	68	82	69·5	35·8	67·6	142·0
23	Unmarr.	Brown grey...	Portsmouth, Hants	Clerk	Suburb ...	229	79	74	72·5	35·2	69·5	132·0
23	Unmarr.	Grey	Liverpool, Lancashire ...	Chemist	Town ...	236	84	94	67·25	34·8	65·0	138·5
23	Unmarr.	Grey green ...	Huntingdon...	Clerk	Town ...	186	63	49	69·0	37·7	70·5	145·25
23	Unmarr.	Brown ...	Bury St. Edmunds, Suffolk...	Mechanical engineer	Town ...	188	74	77	69·9	36·0	68·4	138·5
24	Unmarr.	Brown grey...	Birmingham, Warwickshire	Letter carrier	Town ...	200	76	83	63·25	34·6	64·0	143·0
24	Unmarr.	Grey	Walsall, Staffordshire ...	Saddle-tree maker... ...	Suburb ...	164	72	66	67·8	34·3	65·3	133·5
24	Unmarr.	Grey	Wakefield, Yorkshire ...	Schoolmaster	Country ...	230	63	71	71·0	37·5	70·2	122·0
24	Unmarr.	Blue grey ...	London...	Scientific chemist	Suburb ...	246	80	72	69·5	34·8	66·7	136·0
24	Unmarr.	Grey blue ...	London...	Soldier	Town & cy.	234	78	95	74·9	37·2	72·0	150·0
24	Married	Grey brown...	Nottingham	Plasterer	Suburb ...	222	75	73	73·3	37·5	71·8	165·5
24	Unmarr.	Grey brown...	London...	Electrical engineer ...	Suburb ...	314	90	98	76·0	38·7	72·9	162·5
24	Unmarr.	Grey brown...	London...	Stock Exchange	Suburb ...	254	85	86	65·2	36·0	65·1	137·75
24	Unmarr.	Brown grey...	Hoxton, Middlesex	Printer	Town ...	242	78	70	70·25	37·1	68·4	138·0
24	Unmarr.	Blue grey ...	Newmarket, Cambridgeshire	Outfitter	Town ...	250	92	94	72·5	35·6	69·4	137·0
24	Unmarr.	Light blue ...	Portsmouth, Hants.	Clerk	Suburb ...	220	87	74	64·75	35·6	64·0	122·0
24	Unmarr.	Grey blue ...	Eltham, Kent...	Drapers' buyer	Town ...	164	67	66	64·8	35·2	65·6	116·5
24	Unmarr.	Blue grey ...	London...	Clerk	Town ...	268	86	80	69·7	37·5	70·7	204·75
24	Unmarr.	Brown ...	Winterton, Lincolnshire ...	Minister, Congregational	Suburb ...	254	90	96	68·25	37·1	67·6	154·25
24	Unmarr.	Blue	Brighton, Sussex	Builder	Town ...	196	88	84	68·5	36·0	67·2	123·0
24	Unmarr.	Grey blue ...	Wakefield, Yorkshire ...	Bar student...	Town ...	298	80	67	72·1	38·0	70·2	149·25

Age last birth-day.	Married or un-married.	Eye Colour.	Birthplace.	Occupation.	Residence.	Breathg. Capacity. cubic inches.	Squeeze in lbs. Rt. hand.	Squeeze in lbs. Left hand.	Span. inches.	Height from Seat. inches.	Height without shoes. inches.	Weight. lbs.
24	Unmarr.	Brown ...	London...	Mercantile	Town ...	274	100	97	73·5	37·4	69·7	159·0
24	Unmarr.	Blue	Windsor, Berkshire	Professor of music ...	Country ...	240	93	90	67·75	36·1	65·4	183·5
24	Unmarr.	Blue	Portwinkle, Cornwall ...	Cashier	Town ...	180	67	66	65·0	34·7	67·2	122·5
24	Unmarr.	Brown ...	London...	Architect...	Town ...	228	84	84	69·3	34·9	67·6	141·5
24	Unmarr.	Blue	Aberdare, Glamorganshire...	Assistant iron merchant	Town ...	186	82	81	72·0	36·0	68·0	139·0
24	Unmarr.	Brown grey...	Sydenham, Kent	Student	Suburb ...	250	99	97	73·0	36·7	72·0	153·75
24	Unmarr.	Brown ...	Middlesex	Engineer...	Suburb ...	302	92	95	72·8	38·1	71·6	170·75
24	Unmarr.	Brown grey...	Kensington, Middlesex ...	Clerk	Suburb ...	218	69	75	68·6	36·2	68·6	119·0
24	Unmarr	Brown ...	Manchester, Lancashire ...	Clerk	Suburb ...	204	64	70	64·0	35·4	64·3	126·5
24	Unmarr.	Grey	London...	Commercial clerk ...	Suburb ...	224	75	73	68·2	34·5	66·7	131·75
24	Unmarr.	Blue grey ...	London	Clerk	Suburb ...	250	96	97	73·0	36·0	69·5	150·5
24	Unmarr.	Brown ...	London	Electrician	Suburb ...	309	76	73	71·7	35·5	70·2	146·0
24	Unmarr.	Blue grey ...	Bolton, Lancashire	Electrician	Town ...	246	85	90	72·7	37·8	71·4	143·0
24	Unmarr.	Brown ...	Leeds, Yorkshire	Surgeon	Town ...	228	79	68	72·5	36·5	71·0	129·75
24	Unmarr.	Hazel ...	London	Civil engineer	Suburb ...	192	96	88	67·0	34·8	65·0	140·75
24	Unmarr.	Blue	London	Clerk	Country ...	340	101	101	74·0	38·0	72·6	186·75
24	Unmarr.	—	Devizes, Wiltshire	Draper	Cy. town ...	230	76	76	69·1	35·0	67·0	119·0
24	Unmarr.	Blue grey ...	London	Barrister...	Town ...	258	101	93	71·8	37·3	71·2	167·0
24	Unmarr.	Brown ...	Liverpool, Lancashire ...	Government clerk ...	Town ...	250	76	82	64·5	34·3	64·9	126·0
24	Unmarr.	Grey	Gainsborough, Lincolnshire	Clergyman	Town ...	208	108	104	65·9	37·4	67·8	145·25
24	Unmarr.	Blue	Sheffield, Yorkshire	Steel manufacturer ...	Town ...	204	60	61	70·5	36·7	65·8	130·5
24	Unmarr.	Brown grey...	Leeds, Yorkshire	Architect...	Country ...	228	64	63	65·0	35·0	65·6	131·0
24	Unmarr	Grey	Farnham, Surrey	Wine merchant	Town ...	224	88	95	72·5	36·9	70·2	176·0
24	Unmarr.	Grey	Chatteris, Cambridgeshire...	Professor of music ...	Town ...	280	79	72	72·8	36·3	69·5	140·25
24	Unmarr.	Brown ...	Boston, Lincolnshire ...	Solicitors' clerk	Town ...	236	81	70	70·5	36·2	69·2	151·75
24	Unmarr.	Blue	London	Shipping agent	Suburb ...	204	82	78	66·5	34·1	64·6	126·0
24	Unmarr.	Light brown	Mile End, Middlesex... ...	Schoolmaster	Country ...	266	74	70	72·4	37·7	71·4	152·5
24	Unmarr.	Grey blue ...	London	Clerk	Suburb ...	221	66	45	69·0	36·1	66·5	139·25
24	Unmarr.	Grey blue ...	Sydenham, Kent	Schoolmaster	Country ...	226	76	77	67·8	35·3	66·3	127·5
24	Unmarr.	Blue grey ...	Huddersfield, Yorkshire ...	Schoolmaster	Town ...	254	90	87	69·5	36·4	67·3	150·5
24	Unmarr.	Hazel ...	Tovil, Kent	Postal telegraph clerk ...	Suburb ...	200	73	79	65·25	35·2	64·4	133·5
24	Unmarr.	Brown ...	Peckham, Surrey	Missionary	Town ...	212	83	78	65·0	35·4	62·6	138·0
24	Unmarr.	Hazel ...	Paddington, Middlesex ...	Chemist	Town ...	226	74	70	71·3	35·7	68·6	139·0
24	Unmarr.	Grey	London	Goldsmith	Suburb ...	230	97	99	70·75	35·2	69·9	139·5
24	Unmarr.	Blue	Lincolnshire	Teacher...	Suburb ...	200	75	76	65·2	35·7	66·4	127·0
24	Unmarr.	Blue	Workington, Cumberland ...	Analyst...	Town ...	250	85	72	67·0	36·3	67·0	137·5
24	Unmarr.	Blue	Hingham, Norfolk	Clerk General Post Office	Town ...	280	87	100	74·5	38·4	72·4	172·5
24	Unmarr.	Blue	Elton, Lancashire	Clerk	Town ...	216	84	78	71·7	34·6	67·7	153·5
24	Unmarr.	Blue	Bristol, Gloucestershire ...	Builder	Town ...	230	106	101	69·6	36·1	68·6	164·0
24	Unmarr.	Blue	Gloucester	Clerk	Town ...	216	84	78	70·5	35·8	69·0	140·5

Age last birth-day.	Married or un-married.	Eye Colour.	Birthplace.	Occupation.	Residence.	Breathg. Capacity. cubic inches.	Squeeze in lbs.		Span. inches.	Height from Seat. inches.	Height without shoes. inches.	Weight. lbs.
							Rt. hand.	Left hand.				
24	Unmarr.	Blue grey ...	Croydon, Surrey	Law clerk	Town ...	290	90	91	75·8	37·9	74·1	170·0
24	Unmarr.	Hazel ...	Croydon, Surrey	Traveller	Suburb ...	186	68	64	67·1	34·8	65·6	126·5
24	Unmarr.	Grey blue ...	Hayden, Herts	Nil...	Town ...	180	82	71	68·5	35·3	68·4	150·0
24	Unmarr.	Grey	Clapham, Surrey	Draughtsman	Suburb ...	244	83	80	75·5	37·0	73·7	154·0
24	Unmarr.	Grey	London...	Commercial	Town &sub.	288	92	95	73·0	36·8	70·6	173·25
24	Unmarr.	Blue	Halifax, Yorkshire	Civil Service clerk ...	Town ..	216	69	61	68·3	24·4	68·6	133·25
24	Unmarr.	Brown grey...	Glasgow	Teacher	Country ...	280	96	90	72·25	36·2	69·7	168·25
24	Unmarr.	Grey	Buckingham...	Stone carver	Country ...	184	87	81	66·5	32·9	61·0	133·5
24	Unmarr.	Hazel brown	Bristol, Gloucestershire ...	Clerk	Town ...	178	76	74	67·3	34·3	65·0	137·5
24	Unmarr.	Brown grey...	Bristol, Gloucestershire ...	Engineer's assistant ...	Town ...	246	95	96	73·25	36·5	69·2	152·25
24	Unmarr.	Grey	London...	Clerk	Suburb ...	218	76	67	67·5	35·2	67·3	134·25
24	Unmarr.	Brown grey...	London...	Commercial clerk ...	Suburb ...	210	99	94	71·5	36·8	68·0	145·0
24	Unmarr.	Brown ...	London...	Clerk	Suburb ...	240	88	88	73·0	34·9	68·4	141·0
24	Unmarr.	Brown ...	London...	Surgeon	Town ...	266	89	81	67·3	35·4	64·3	144·25
24	Unmarr.	Blue grey ...	Warwickshire...	Engineer	Town ...	258	105	104	71·25	35·3	69·3	139·5
24	Unmarr.	Grey	London...	Minister	Country ...	276	84	88	72·0	35·9	69·5	143·5
24	Unmarr.	Brown grey...	Bro. Ferry, Scotland ...	Manufacturer	Country ...	200	75	84	69·25	35·7	66·2	139·5
24	Unmarr.	Grey	London...	Clerk	Town ...	212	79	74	68·4	34·9	66·2	126·0
24	Unmarr.	Grey	Ipswich, Suffolk	Merchant clerk... ...	Town ...	196	83	75	64·5	34·3	65·5	118·0
24	Unmarr.	Grey	London...	Clerk	Suburb ...	238	95	88	73·0	35·4	69·5	150·0
24	Unmarr.	Brown ...	London...	Clerk	Town ...	184	89	101	74·0	36·1	70·4	180·25
24	Unmarr.	Blue grey ...	London...	Engineer	Suburb ...	240	90	92	70·5	38·0	70·2	151·75
24	Unmarr.	Brown ...	Lambeth, Surrey	Hatter	Country ...	154	71	71	67·0	36·3	66·4	141·0
24	Unmarr.	Blue grey ...	Gainsborough, Lincolnshire	Clergyman	Country ...	216	104	106	65·0	36·3	67·5	147·0
24	Unmarr.	Grey blue ...	Cromer, Norfolk	Clergyman	Country ...	210	101	89	72·0	36·6	68·7	154·0
24	Unmarr.	Blue	Llandovery, Carmarthenshire	Teacher	Town ...	182	65	74	65·25	33·9	64·8	121·5
24	Unmarr.	Grey	St. Mary Cray, Kent... ...	Clerk	Country ...	210	92	93	70·7	36·4	68·8	145·0
24	Unmarr.	Brown ...	Doncaster, Yorkshire ...	Manufacturer	Suburb ...	232	84	85	70·5	37·2	68·5	149·5
24	Unmarr.	Blue	London...	Engineer...	Town ...	224	100	92	68·7	34·9	67·2	137·25
24	Unmarr.	Grey	Southampton	Examiner of Patents ...	Suburb ...	170	80	70	65·25	35·7	65·1	140·25
24	Unmarr.	Blue	London...	Artist	Town ..	266	76	77	73·0	38·7	72·2	141·5
24	Unmarr.	Blue	Hexham, Northumberland...	Law student	Town ...	198	83	81	70·5	34·2	66·7	122·0
24	Unmarr.	Grey	London...	Clerk	Town ...	208	93	77	68·25	37·0	68·4	158·5
24	Unmarr.	Grey	Huddersfield, Yorkshire ...	Insurance clerk	Town ..	280	73	78	73·0	36·8	71·2	138·0
24	Unmarr.	Brown ...	Reading, Berkshire	Woollen warehouseman	Suburb ...	282	87	86	72·5	38·2	73·1	157·5
24	Unmarr.	Blue grey ...	Leicestershire...	Teacher	Town ...	288	103	104	73·0	38·2	73·25	163·0
24	Unmarr.	Blue	Tranmere, Lancashire ...	Manufacturer	Country ...	244	91	90	70·0	36·5	68·4	154·5
24	Unmarr.	Grey blue ...	Manchester, Lancashire ...	Dentist	Suburb ...	210	94	79	67·0	33·9	65·7	128·0
24	Unmarr.	Brown ...	London...	Mercantile	Town ...	158	80	75	64·5	33·5	63·25	122·5
24	Unmarr.	Brown grey...	Renfrew, Scotland	Marine engineer ...	Sea ...	198	104	102	73·5	35·8	67·2	154·0

Age last birth-day.	Married or un-married.	Eye Colour.	Birthplace.	Occupation.	Residence.	Breathg. Capacity, cubic inches.	Squeeze in lbs. Rt. hand.	Squeeze in lbs. Left hand.	Span. inches.	Height from Seat. inches.	Height without shoes. inches.	Weight. lbs.
24	Unmarr.	Blue grey	London	Civil engineer	Town	220	95	94	72·5	35·75	68·75	154·0
24	Unmarr.	Light grey	Kensington, Middlesex	Clerk	?	164	74	75	66·5	34·8	67·0	131·0
24	Unmarr.	Brown grey	Wing, Buckinghamshire	Law student	Suburb	152	103	100	72·0	36·7	69·7	152·75
24	Unmarr.	Blue grey	London	Clerk	Suburb	206	90	77	69·5	38·0	70·6	206·75
24	Unmarr.	Brown	Mersham, Kent	Grocer	Country	140	77	72	68·5	34·3	65·3	124·5
24	Unmarr.	Blue	Penzance, Cornwall	Secretary	Town	236	100	98	68·3	36·3	68·3	145·0
24	Unmarr.	Blue	Maidstone, Kent	Clerk	Town	183	76	79	72·75	35·6	68·9	133·0
24	Unmarr.	Grey	Hemel Hempstead, Herts	Compositor	Country	184	78	79	67·9	35·0	66·1	123·5
24	Unmarr.	Blue grey	Bath, Somerset	Silversmith	Suburb	260	90	82	72·0	35·6	69·5	160·0
24	Unmarr.	Brown	City	Butterman	Town	170	78	83	67·0	33·2	63·2	143·75
24	Unmarr.	Grey	London	Draughtsman	Town	184	61	59	68·75	35·4	66·8	131·0
24	Unmarr.	Blue grey	Romsey, Hants	Medical Student	Town	222	98	83	75·5	37·1	71·6	151·5
24	Unmarr.	—	Birmingham, Warwickshire	Clerk	Town	224	82	72	71·2	36·4	68·2	130·5
24	Unmarr.	Brown	Montgomery	Architect	Country	175	94	99	70·9	35·1	68·8	145·75
24	Unmarr.	Dark brown	Devon	Saddler	Town	258	94	89	71·6	36·8	68·4	147·0
24	Unmarr.	Brown	Tooting, Surrey	Brewer	Country	248	87	87	73·25	37·0	69·3	167·0
24	Unmarr.	Brown	Southwick, Sussex	Grocer	Country	216	84	76	67·7	32·6	63·5	140·0
24	Unmarr.	Green grey	Walton, Northamptonshire	Clerk	Town	298	88	86	70·0	35·2	69·7	148·0
24	Unmarr.	Brown grey	Lincoln	Engineer	Town	246	90	84	68·8	35·8	67·9	142·0
24	Unmarr.	Brown	Enfield, Middlesex	Pawnbroker	Suburb	230	82	75	69·4	35·7	66·6	134·5
24	Unmarr.	Dark grey	Nettlebed, Oxfordshire	Manager of pottery	Country	224	75	78	70·6	33·4	65·8	128·0
24	Unmarr.	Brown grey	Portsmouth, Hants	Clerk	Suburb	196	78	79	66·0	33·3	63·3	120·0
24	Married	Brown	Stepney, Middlesex	Traveller	Suburb	228	83	81	70·7	35·3	68·0	142·75
24	Married	Brown	Lincoln	Gentleman	Country	264	41	67	71·2	35·9	68·9	142·5
24	Married	Dark grey	Colchester, Essex	Jeweller	Town	242	69	67	70·8	36·5	68·3	137·25
24	Married	Grey brown	Northfleet, Kent	Dairyman	Country	200	61	66	70·2	35·1	68·1	142·0
24	Married	Blue	Sunderland, Durham	Jeweller	Town	164	71	64	67·0	33·8	63·9	122·25
24	Married	Blue	Deptford, Kent	Policeman	Suburb	196	81	66	74·0	36·0	71·8	155·5
24	Married	Hazel	Chelsea, Middlesex	Messenger	Town	240	88	77	68·2	36·4	69·1	154·75
24	Married	Grey	Chelsea, Middlesex	Fishmonger	Town	188	79	70	66·5	35·4	65·5	137·5
24	Married	Hazel	Devon	Mariner	?	208	93	90	69·0	35·0	68·2	161·5
24	Married	Grey	Exning, Suffolk	Oilman	Town	220	100	87	67·0	34·7	64·6	155·75
24	Married	Hazel	London	Engraver on wood	Suburb	184	78	78	70·5	36·8	68·6	166·0
24	Married	Blue grey	Denham, Bucks	Hay salesman	Town	208	80	77	67·0	35·5	65·75	141·0
24	Married	Hazel brown	Market Rasen, Lincolnshire	Fish merchant	Town	208	92	92	69·5	36·2	68·0	142·5
24	Married	Blue	Walworth, Surrey	Sup. Refresh. Depart.	Town	266	92	80	75·4	36·8	73·2	147·25
24	Married	Blue grey	Farnham, Surrey	Stableman	Town	234	76	68	69·5	35·4	66·6	146·5
24	Married	Blue grey	London	Clerk	Suburb	266	98	93	73·75	36·6	69·2	144·5
24	Married	Blue	Walworth, Surrey	Sup. Refresh. Depart.	Town	246	96	90	75·5	36·9	73·0	147·75
24	Married	—	Bristol, Gloucestershire	Traveller	Town	174	81	86	72·5	36·7	69·6	146·0

Age last birth-day.	Married or un-married.	Eye Colour.	Birthplace.	Occupation.	Residence.	Breathg. Capacity. cubic inches.	Squeeze in lbs. Rt. hand.	Squeeze in lbs. Left hand.	Span. inches.	Height from Seat. inches.	Height without shoes. inches.	Weight. lbs.
24	Married	Dark brown	London	Clerk	Suburb	185	88	90	67·0	34·9	63·2	132·5
24	Married	Blue grey	London	Tradesman	Town	260	93	92	72·8	38·0	72·2	161·0
24	Married	Brown	South Shields, Durham	Magistrate's clerk	Town	240	83	79	68·0	37·1	67·6	139·0
24	Married	Brown	Sudbury, Suffolk	Miller	Town	158	73	71	67·8	35·5	67·1	144·75
25	Unmarr.	Brown	Lambeth, Surrey	Compositor	Town	206	88	85	75·5	38·5	73·5	177·5
25	Unmarr.	Hazel	Swansea, Glamorganshire	Mariner	?	328	85	80	75·75	38·6	74·2	169·25
25	Unmarr.	Brown	Plymouth, Devonshire	Printer and stationer	Town	244	89	88	69·8	37·7	68·7	158·0
25	Unmarr.	Blue grey	London	Barrister	Town	304	78	83	75·9	36·5	71·5	203·5
25	Unmarr.	Grey	London	Surgeon	Suburb	305	99	98	71·5	37·0	69·1	157·0
25	Unmarr.	Grey	Bucks	Policeman	Town	290	93	89	76·0	38·6	73·1	196·5
25	Unmarr.	Brown	East Peckham, Kent	Book-keeper	Suburb	—	83	66	67·5	34·6	66·3	117·75
25	Unmarr.	Hazel	Aberdeenshire	Clerk	Suburb	222	72	77	70·1	36·1	69·6	141·0
25	Unmarr.	Brown grey	Stanmore, Middlesex	Gentleman	Country	300	65	67	77·2	39·5	75·8	186·75
25	Unmarr.	Brown grey	Arbroath, Scotland	Wood turner	Town	178	78	75	69·25	35·8	65·8	127·5
25	Unmarr.	Hazel	Bedford	Lithographer	Town	236	83	77	64·6	35·3	64·7	135·5
25	Unmarr.	Hazel	Manchester, Lancashire	Schoolmaster	Suburb	192	70	56	67·75	36·2	68·1	141·75
25	Unmarr.	Grey	Barnstaple, Devonshire	Grate manufacturer	Cy. town	198	93	110	71·0	35·9	69·3	153·75
25	Unmarr.	Brown	Chelsea, Middlesex	Art master	Town	160	58	60	65·6	35·6	63·7	136·5
25	Unmarr.	Grey brown	Winchester, Hants	Gentleman	Country	230	85	83	74·0	36·1	68·1	151·0
25	Unmarr.	Blue	Yorkshire	Soap maker	Town	170	70	72	70·0	35·4	65·5	141·75
25	Unmarr.	Blue	Bourne End, Buckinghamshire	Clerk	Town	310	87	86	73·8	37·3	72·0	167·0
25	Unmarr.	Blue	Ockbrook, Derby	Tutor	Town	204	61	70	66·5	34·0	63·8	131·0
25	Unmarr.	Hazel	Wellingbro., Northamptonshire	Baker	Town	210	58	62	69·5	34·6	65·5	134·0
25	Unmarr.	Blue grey	Norfolk	Schoolmaster	Country	204	72	70	66·5	35·9	66·4	129·0
25	Unmarr.	Brown	Great Yarmouth, Norfolk	Solicitor	Town	192	84	83	70·3	35·3	66·5	155·0
25	Unmarr.	Light hazel	Maidstone, Kent	Solicitor	Town	296	81	83	71·3	35·5	69·4	160·25
25	Unmarr.	Grey	Perth	Joiner	Country	234	89	88	67·2	35·4	65·6	142·5
25	Unmarr.	Hazel	London	Schoolmaster	Suburb	120	76	64	69·0	35·5	66·8	123·5
25	Unmarr.	Blue grey	Charlton, Kent	Engineer	Country	188	80	77	74·75	36·4	67·5	170·0
25	Unmarr.	Brown	London	Shop assistant	Suburb	170	71	65	65·2	34·1	63·3	131·75
25	Unmarr.	Brown	Upton	Architect	Town	244	81	72	67·6	34·0	65·8	126·0
25	Unmarr.	Blue grey	London	Accountant	Suburb	204	79	71	66·25	35·9	66·4	128·5
25	Unmarr.	Grey blue	Dundee, Scotland	Art teacher	Town	254	80	67	69·0	36·5	67·6	140·25
25	Unmarr.	Dark grey	Kirky Overblow, Yorkshire	Tutor	Country	268	97	96	70·6	35·3	69·6	158·0
25	Unmarr.	Blue grey	Tichmarsh, Northamptonshire	Schoolmaster	Suburb	218	78	54	67·25	35·3	66·0	130·0
25	Unmarr.	Blue grey	Battle, Sussex	Stationer	Country	200	82	73	72·4	36·9	71·3	171·25
25	Unmarr.	Hazel brown	Brighton, Sussex	Stationer	Suburb	302	82	89	71·1	38·1	71·0	164·25
25	Unmarr.	Grey	Harrow, Middlesex	Tutor	Suburb	272	94	74	70·5	34·2	66·8	139·0
25	Unmarr.	Grey	Edinburgh	Butcher	Town	260	88	79	72·2	35·7	67·8	143·0
25	Unmarr.	Blue	London	Engineer	Suburb	240	98	95	70·25	37·0	67·9	141·75

Age last birth-day.	Married or un-married.	Eye Colour.	Birthplace.	Occupation.	Residence.	Breathg. Capacity. cubic inches.	Squeeze in lbs.		Span. inches.	Height from Seat. inches.	Height without shoes. inches.	Weight. lbs.
							Rt. hand.	Left hand.				
25	Unmarr.	Grey	London	Commercial	Town	260	97	95	71·1	36·0	70·2	147·25
25	Unmarr.	Grey blue	Paddington, Middlesex	Medical student	Town	218	95	95	72·6	34·9	68·0	170·0
25	Unmarr.	Brown	Plymouth, Devonshire	Mariner	?	210	93	90	69·2	35·1	66·3	159·5
25	Unmarr.	Hazel	Dublin	Surgeon	Town	248	91	70	71·0	37·0	69·3	199·0
25	Unmarr.	Dark grey	London	Insurance collector	Town	260	82	90	66·8	34·9	64·9	149·0
25	Unmarr.	Blue	Weymouth, Dorset	Tutor	Country	208	83	84	67·3	34·5	65·2	138·0
25	Unmarr.	Grey brown	Kenilworth, Warwickshire	Manufacturer	Town	188	72	78	66·0	34·1	64·5	115·0
25	Unmarr.	Hazel brown	Edinburgh	Cotton broker	Suburb	258	85	79	72·0	36·6	71·2	150·25
25	Unmarr.	Brown	London	Mercantile	Town	290	98	95	73·5	37·4	69·7	157·75
25	Unmarr.	Grey brown	Bilston, Staffordshire	Analyst	Town	218	68	69	66·5	34·1	65·8	129·5
25	Unmarr.	Blue	Sutton Bridge, Lincolnshire	Accountant	Country	196	74	77	72·4	35·0	68·9	138·5
25	Unmarr.	Grey blue	Uttoxeter, Staffordshire	Bank clerk	Town	206	66	74	65·8	35·3	65·8	129·25
25	Unmarr.	Brown	Coleraine, Ireland	Clerk	Country	164	67	47	68·2	35·2	66·7	136·75
25	Unmarr.	Grey	Sevenoaks, Kent	Accountant	Tn. &. cy.	160	77	71	71·75	35·8	69·1	138·0
25	Unmarr.	Brown	London	Tea dealer	Town	190	95	87	70·5	38·0	69·5	160·0
25	Unmarr.	Brown grey	Harborne, Warwickshire	Artist	Town	239	99	86	70·5	36·8	70·7	166·25
25	Unmarr.	Brown	Denbighshire	None	Suburb	226	84	86	67·0	36·5	66·7	130·0
25	Unmarr.	Blue	Burton on Trent, Staffordshire	— —	Town	180	73	72	66·5	34·7	64·2	119·75
25	Unmarr.	Grey blue	Primrose Hill, Middlesex	Engineer	Town	78	—	—	67·0	35·7	65·8	123·0
25	Unmarr.	Blue	Aberystwith, Cardiganshire	Chemist	Town	184	80	65	66·0	35·8	66·3	120·75
25	Unmarr.	Grey	London	Teacher	Suburb	200	91	85	69·5	35·5	66·7	137·25
25	Unmarr.	Blue	Bermondsey, Surrey	Salesman	Town	240	92	101	70·25	36·4	69·4	166·0
25	Unmarr.	Blue	Manchester, Lancashire	Clerk	Suburb	184	60	60	64·7	34·8	64·5	107·25
25	Unmarr.	Brown	Newton Longville, Bucks	Clergyman	Country	218	90	92	71·0	36·5	68·7	160·75
25	Unmarr.	Grey blue	Oxford	Clergyman	Town	288	101	109	71·25	37·7	71·2	168·5
25	Unmarr.	Hazel	Aberdeenshire	Lecturer	Town	220	67	59	68·6	36·0	68·3	137·5
25	Unmarr.	Brown grey	Hoddesdon, Hertfordshire	Clerk	Town	178	68	75	63·5	34·2	62·2	133·75
25	Unmarr.	Brown	Ruthin, Denbighshire	None	Suburb	260	75	76	66·5	36·0	67·0	130·0
25	Unmarr.	Grey blue	London	Tailor	Country	244	98	95	67·0	35·3	65·8	141·0
25	Unmarr.	Grey blue	London	Engineer	Town	240	96	103	72·0	35·0	68·0	166·0
25	Unmarr.	Blue grey	Derby	Lecturer on chemistry	Town	198	87	76	68·5	36·0	68·0	147·5
25	Unmarr.	Brown grey	London	Surgeon	Suburb	240	109	103	73·0	36·5	71·5	152·0
25	Unmarr.	Blue	E. Preston, Sussex	Estate agent	Country	260	73	74	72·2	36·5	69·0	140·0
25	Unmarr.	Grey	Camberwell, Surrey	Accountant	Town	170	90	89	72·25	35·4	69·1	149·5
25	Unmarr.	Brown grey	London	Engineer's draughtsman	Suburb	244	76	73	70·5	35·0	68·0	132·25
25	Unmarr.	Grey	Bath, Somerset	Solicitor	Suburb	220	87	88	67·75	36·7	67·1	149·0
25	Unmarr.	Grey	Bridgewater, Somerset	Servant	Town	170	67	81	68·75	34·7	66·1	141·5
25	Unmarr.	Hazel	Llandovery, Caermarthenshire	Schoolmaster	Country	200	90	88	72·0	37·3	69·2	162·0
25	Unmarr.	Brown grey	Oxfordshire	Solicitor	Suburb	250	106	98	75·4	34·6	71·4	151·0
25	Unmarr.	Grey blue	Jersey	Clerk	Town	180	73	66	68·3	33·7	66·7	123·5

Age last birth-day.	Married or un-married.	Eye Colour.	Birthplace.	Occupation.	Residence.	Breathg. Capacity. cubic inches.	Squeeze in lbs.		Span. inches.	Height from Seat. inches.	Height without shoes. inches.	Weight. lbs.
							Rt. hand.	Left hand.				
25	Unmarr.	—	London	Clerk	Town	192	59	54	64·6	34·9	64·1	123·5
25	Unmarr.	Grey	High Wycombe, Bucks	Ironmonger	Town	212	71	71	71·3	37·0	68·7	141·5
25	Unmarr.	Blue	Dover, Kent	Officer R.H.A.	Country	337	109	93	78·3	39·0	75·6	186·25
25	Unmarr.	Blue grey	Dover, Kent	Draper	Town	224	89	80	70·25	35·8	68·8	129·0
25	Unmarr.	Blue	Swansea, Glamorganshire	Timber importer	Town	222	73	70	68·6	33·9	63·6	140·0
25	Unmarr.	Light blue	Sunderland, Durham	Parson	Town	284	95	95	75·1	38·8	72·9	168·5
25	Unmarr.	Brown grey	Colchester, Essex	Clerk	Town	300	84	91	74·3	38·4	73·0	162·5
25	Unmarr.	Grey	London	Clerk	Town	284	84	80	72·7	36·7	69·8	143·5
25	Unmarr.	Grey	Northampton	Student	Country	220	78	84	65·8	35·1	66·3	137·0
25	Unmarr.	Brown	Lambeth, Surrey	Government clerk	Suburb	224	99	81	71·5	36·1	68·8	167·0
25	Unmarr.	Light grey	Marylebone, Middlesex	Printer	Town	152	71	72	69·25	36·0	67·9	134·0
25	Unmarr	Blue	Norton Bavant, Wiltshire	Schoolmaster	Town	194	83	96	66·6	36·1	66·8	140·75
25	Unmarr.	Blue	Croydon, Surrey	Mercantile clerk	Town	264	85	75	72·6	37·3	69·6	149·0
25	Unmarr.	Brown	Seacroft, Yorkshire	Clergyman	Suburb	240	89	87	69·5	35·2	66·5	148·5
25	Unmarr.	Grey blue	Reading, Berkshire	Surveyor	Suburb	248	89	85	74·0	36·4	70·0	153·75
25	Unmarr.	Brown	Wrexham, Denbighshire	Analyst	Suburb	224	65	62	67·5	34·2	65·3	123·0
25	Unmarr.	Grey brown	London	Clerk	Suburb	204	79	94	69·7	35·0	66·5	138·0
25	Unmarr.	Blue	Lanarkshire	Teacher	Town	290	92	82	71·5	38·1	70·9	177·0
25	Unmarr.	Blue	London	Merchant clerk	Town	272	87	98	72·1	39·1	72·1	149·25
25	Unmarr	Brown	Tunbridge, Kent	Solicitor	Country	248	77	75	73·2	36·8	70·3	141 0
25	Married	Light blue	Totnes, Devonshire	Schoolmaster	Suburb	276	88	75	73·75	37·1	71·0	138·5
25	Married	Brown	Shaftesbury, Dorsetshire	Furniture warehouseman	Suburb	244	97	84	68·5	36·6	68·5	145·5
25	Married	Grey	Teddington, Middlesex	Grocer	Suburb	180	86	82	70·0	35·5	65·7	138·0
25	Married	Brown	London	Manufacturer	Suburb	264	100	89	70·3	35·3	68·1	110·25
25	Married	Grey	Middlesex	Clerk	Town	240	89	83	73·8	37·4	71·5	135·25
25	Married	Brown	Essex	Cheesemonger	Town	192	74	84	70·5	34·6	67·8	127·5
25	Married	Grey	London	Carpenter	Country	—	76	83	65·8	33·0	62·2	118·25
25	Married	Blue	Lambeth, Surrey	Policeman	Town	220	78	77	68·5	37·4	69·5	155·25
25	Married	Blue	London	Electrical engineer	Town	302	88	93	73·2	38·4	72·9	158·0
25	Married	Blue grey	Ironbridge, Salop	Ironmonger	Suburb	320	100	100	74·0	37·8	71·2	188·5
25	Married	Brown grey	Harwich, Essex	Printer	Town	180	73	71	69·0	35·5	68·4	135·5
25	Married	Grey blue	Cambridge	Pastry cook	Town	260	70	77	70·3	36·1	66·8	134·25
25	Married	Blue grey	Richmond, Surrey	Electrician	Tn. & cy.	286	93	87	71·2	36·0	69·2	133·0
25	Married	Grey	Dundee	Telegraphist	Town	238	80	80	70·0	36·6	68·1	145·5
25	Married	Dark grey	Wallingford, Surrey	Chemist	Town	226	73	84	71·5	36·8	69·6	164·0
25	Married	Grey blue	Hampstead, Middlesex	Student	Suburb	234	96	95	76·0	38·5	72·3	166·5
25	Married	Brown	Rochester, Kent	Architect	Town	210	80	85	70·5	35·4	66·7	135·75
25	Married	Blue	Wigan, Lancashire	Civil engineer	Town	186	62	79	70·0	35·5	67·1	124·25
25	Married	Dark blue	London	Barrister	Town	298	102	96	77·9	36·6	71·3	152·75
25	Married	Grey brown	Clerkenwell, Middlesex	Warehouseman	Town	228	90	97	70·6	36·6	69·6	136·0

f. 7v

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"Co-relations and their Measurement, chiefly from Anthropometric Data." By Francis Galton, F.R.S. Received December 5, 1888.

"Co-relation or correlation of structure" is a phrase much used in biology, and not least in that branch of it which refers to heredity, and the idea is even more frequently present than the phrase; but I am not aware of any previous attempt to define it clearly, to trace its mode of action in detail, or to show how to measure its degree.

Two variable organs are said to be co-related when the variation of the one is accompanied on the average by more or less variation of the other, and in the same direction. Thus the length of the arm is said to be co-related with that of the leg, because a person with a long arm has usually a long leg, and conversely. If the co-relation be close, then a person with a very long arm would usually have a very long leg; if it be moderately close, then the length of his leg would usually be only long, not very long; and if there were no co-relation at all then the length of his leg would on the average be mediocre. It is easy to see that co-relation must be the consequence of the variations of the two organs being partly due to common causes. If they were wholly due to common causes, the co-relation would be perfect, as is approximately the case with the symmetrically disposed parts of the body. If they were in no respect due to common causes, the co-relation would be *nil*. Between these two extremes are an endless number of intermediate cases, and it will be shown how the closeness of co-relation in any particular case admits of being expressed by a simple number.

To avoid the possibility of misconception, it is well to point out that the subject in hand has nothing whatever to do with the average proportions between the various limbs, in different races, which have been often discussed from early times up to the present day, both by artists and by anthropologists. The fact that the average ratio between the stature and the cubit is as 100 to 37, or thereabouts, does not give the slightest information about the nearness with which they vary together. It would be an altogether erroneous inference to suppose their average proportion to be maintained so that when the cubit was, say, one-twentieth longer than the average cubit, the stature might be expected to be one-twentieth greater than the average stature, and conversely. Such a supposition is easily shown to be contradicted both by fact and theory.

The relation between the cubit and the stature will be shown to be such that for every inch, centimetre, or other unit of absolute length that the cubit deviates from the mean length of cubits, the stature will on the average deviate from the mean length of statures to the amount of 2·5 units, and in the same direction. Conversely, for each unit of deviation of stature, the average deviation of the cubit will be 0·26 unit. These relations are not numerically reciprocal, but the exactness of the co-relation becomes established when we have transmuted the inches or other measurement of the cubit and of the stature into units dependent on their respective scales of variability. We thus cause a long cubit and an equally long stature, as compared to the general run of cubits and statures, to be designated by an identical scale-value. The particular unit that I shall employ is the value of the probable error of any single measure in its own group. In that of the cubit, the probable error is 0·56 inch = 1·42 cm.; in the stature it is 1·75 inch = 4·44 cm. Therefore the measured lengths of the cubit in inches will be transmuted into terms of a new scale, in which each unit = 0·56 inch, and the measured lengths of the stature will be transmuted into terms of another new scale in which each unit is 1·75 inch. After this has been done, we shall find the deviation of the cubit as compared to the mean of the corresponding deviations of the stature, to be as 1 to 0·8. Conversely, the deviation of the stature as compared to the mean of the corresponding deviations of the cubit will also be as 1 to 0·8. Thus the existence of the co-relation is established, and its measure is found to be 0·8.

Now as to the evidence of all this. The data were obtained at my anthropometric laboratory at South Kensington. They are of 350 males of 21 years and upwards, but as a large proportion of them were students, and barely 21 years of age, they were not wholly full-grown; but neither that fact nor the small number of observations is prejudicial to the conclusions that will be reached. They were measured in various ways, partly for the purpose of this inquiry. It will be sufficient to give some of them as examples. The exact number of 350 is not preserved throughout, as injury to some limb or other reduced the available number by 1, 2, or 3 in different cases. After marshalling the measures of each limb in the order of their magnitudes, I noted the measures in each series that occupied respectively the positions of the first, second, and third quarterly divisions. Calling these measures in any one series, Q_1, M, and Q_3, I take M, which is the median or middlemost value, as that whence the deviations are to be measured, and $\frac{1}{2}\{Q_3-Q_1\} = Q$, as the probable error of any single measure in the series. This is practically the same as saying that one-half of the deviations fall within the distance of $\pm Q$ from the mean value, because the series run with fair symmetry. In this way I obtained the following values of M and Q, in which the second decimal must be taken as only roughly approximate. The M and Q of any particular series may be identified by a suffix, thus M_c, Q_c might stand for those of the cubit, and M_s, Q_s for those of the stature.

Table I.

	M.		Q.	
	Inch.	Centim.	Inch.	Centim.
Head length..........	7·62	19·35	0·19	0·48
Head breadth.........	6·00	15·24	0·18	0·46
Stature...............	67·20	170·69	1·75	4·44
Left middle finger	4·54	11·53	0·15	0·38
Left cubit............	18·05	45·70	0·56	1·42
Height of right knee....	20·50	52·00	0·80	2·03

NOTE.—The head length is its maximum length measured from the notch between and just below the eyebrows. The cubit is measured with the hand prone and without taking off the coat; it is the distance between the elbow of the bent left arm and the tip of the middle finger. The height of the knee is taken sitting when the knee is bent at right angles, less the measured thickness of the heel of the boot.

Tables were then constructed, each referring to a different pair of the above elements, like Tables II and III, which will suffice as examples of the whole of them. It will be understood that the Q value is a universal unit applicable to the most varied measurements, such as breathing capacity, strength, memory, keenness of eyesight, and enables them to be compared together on equal terms notwithstanding their intrinsic diversity. It does not only refer to measures of length, though partly for the sake of compactness, it is only those of length that will be here given as examples. It is unnecessary to extend the limits of Table II, as it includes every line and column in my MS. table that contains not less than twenty entries. None of the entries lying within the flanking lines and columns of Table II were used.

Table II.

Stature in inches.	Length of left cubit in inches, 348 adult males.								Total cases.
	Under 16·5.	16·5 and under 17·0.	17·0 and under 17·5.	17·5 and under 18·0.	18·0 and under 18·5.	18·5 and under 19·0.	19·0 and under 19·5.	19·5 and above.	
71 and above .	..	..	..	1	3	4	15	7	30
70...........	..	..	..	1	5	13	11	..	30
69...........	..	1	1	2	25	15	6	..	50
68...........	..	1	3	7	14	7	4	2	48
67...........	..	1	7	15	28	8	2	..	61
66...........	..	1	7	18	15	6	..	..	48
65...........	..	4	10	12	8	2	..	..	36
64...........	..	5	11	2	3	..	..	..	21
Below 64.....	9	12	10	3	1	..	..	..	34
Totals	9	25	49	61	102	55	38	9	348

The measures were made and recorded to the nearest tenth of an inch. The heading of 70 inches of stature includes all records

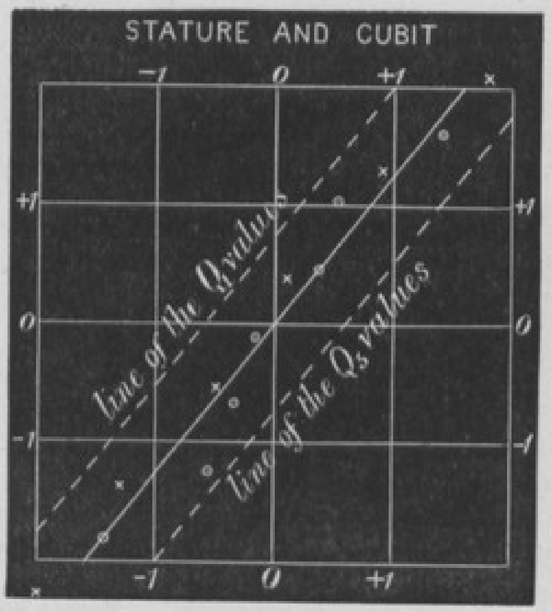

Table III.—Stature $M_s = 67{\cdot}2$ inches; $Q_s = 1{\cdot}75$ inch. Left Cubit $M_c = 18{\cdot}05$ inches; $Q_c = 0{\cdot}56$ inch.

No. of cases.	Stature.	Deviation from M_s reckoned in		Mean of corresponding left cubits.	Deviation from M_c reckoned in			Smoothed values multiplied by Q_c.	Added to M_c.
		Inches.	Units of Q_s.		Inches.	Units of Q_c. Observed.	Units of Q_c. Smoothed.		
	inches.			inches.					
30	70·0	+2·8	+1·60	18·8	+0·8	+1·42	+1·30	+0·73	18·8
50	69·0	+1·8	+1·03	18·3	+0·3	+0·53	+0·84	+0·47	18·5
38	68·0	+0·8	+0·46	18·2	+0·2	+0·36	+0·38	+0·21	18·3
61	67·0	−0·2	−0·11	18·1	+0·1	+0·18	−0·08	−0·04	18·0
48	66·0	−1·2	−0·69	17·8	−0·2	−0·36	−0·54	−0·30	17·8
36	65·0	−2·2	−1·25	17·7	−0·3	−0·53	−1·00	−0·56	17 5
21	64·0	−3·2	−1·83	17·2	−0·8	−1·46	−1·46	−0·80	17·2

No. of cases.	Left cubit.	Deviation from M_c reckoned in		Mean of corresponding statures.	Deviation from M_s reckoned in			Smoothed values multiplied by Q_s.	Added to M_s.
		Inches.	Units of Q_c.		Inches.	Units of Q_s. Observed.	Units of Q_s. Smoothed.		
	inches.			inches.					
38	19·25	+1·20	+2·14	70·3	+3·1	+1·8	+1·70	+3·0	70·2
55	18·75	+0·70	+1·25	68·7	+1·5	+0·9	+1·00	+1·8	69·0
102	18·25	+0 20	+0·36	67·4	+0·2	+0·1	+0·28	+0·5	67·7
61	17·75	−0·30	−0·53	66·3	−0·9	−0·5	−0·43	−0·8	66·4
49	17·25	−0·80	−1·42	65·0	−2·2	−1·3	−1·15	−2·0	65·2
25	16·75	−1·30	−2·31	63·7	−3·5	−2·0	−1·85	−3·2	64·0

f. 4v

between 69·5 and 70·4 inches; that of 69 includes all between 68·5 and 69·4, and so on.

The values derived from Table II, and from other similar tables, are entered in Table III, where they occupy all the columns up to the three last, the first of which is headed "smoothed." These smoothed values were obtained by plotting the observed values, after transmuting them as above described into their respective Q units, upon a diagram such as is shown in the figure. The deviations of the "subject" are measured parallel to the axis of y in the figure, and those of the mean of the corresponding values of the "relative" are measured parallel to the axis of x. When the stature is taken as the subject, the median positions of the corresponding cubits, which are given in the successive lines of Table III, are marked with small circles. When the cubit is the subject, the mean positions of the corresponding statures are marked with crosses. The firm line in the figure is drawn to represent the general run of the small circles and crosses. It is here seen to be a straight line, and it was similarly found to be straight in every other figure drawn from the different pairs of co-related variables that I have as yet tried. But the inclination of the line to the vertical differs considerably in different cases. In the present one the inclination is such that a deviation of 1 on the part of the subject, whether it be stature or cubit, is accompanied by a mean deviation on the part of the relative, whether it be cubit or stature, of 0·8. This decimal fraction is consequently the measure of the closeness of the co-relation. We easily retransmute it into inches. If the stature be taken as the subject, then Q_s is associated with $Q_c \times 0·8$; that is, a deviation of 1·75 inches in the one with $0·56 \times 0·8$ of the other. This is the same as 1 inch of stature being associated with a mean length of cubit equal to 0·26 inch. Conversely, if the cubit be taken as the subject, then Q_c is associated with $Q_s \times 0·8$; that is, a deviation of 0·56 inch in the one with $1·75 \times 0·8$ of the other. This is the same as 1 inch of cubit being associated with a mean length of 2·5 inches of stature. If centimetre be read for inch the same holds true.

Six other tables are now given in a summary form, to show how well calculation on the above principle agrees with observation.

Table IV.

No. of cases.	Length of head.	Mean of corresponding statures.	
		Observed.	Calculated.
32	7·90	68·5	68·1
41	7·80	67·2	67·8
46	7·70	67·6	67·5
52	7·60	66·7	67·2
58	7·50	66·8	66·8
34	7·40	66·0	66·5
26	7·30	66·7	66·2

No. of cases.	Height.	Mean of corresponding lengths of head.	
		Observed.	Calculated.
26	70·5	7·72	7·75
30	69·5	7·70	7·72
50	68·5	7·65	7·68
49	67·5	7·65	7·64
56	66·5	7·57	7·60
43	65·5	7·57	7·69
31	64·5	7·54	7·65

No. of cases.	Height.	Mean of corresponding lengths of left middle finger.	
		Observed.	Calculated.
30	70·5	4·71	4·74
50	69·5	4·55	4·68
37	68·5	4·57	4·62
62	67·5	4·58	4·56
48	66·5	4·50	4·50
37	65·5	4·47	4·44
20	64·5	4·33	4·38

No. of cases.	Length of left middle finger.	Mean of corresponding statures.	
		Observed.	Calculated.
23	4·80	70·2	69·4
49	4·70	68·1	68·5
62	4·60	68·0	67·7
63	4·50	67·3	66·9
57	4·40	66·0	66·1
35	4·30	65·7	65·3

No. of cases.	Left middle finger.	Mean of corresponding lengths of left cubit.	
		Observed.	Calculated.
23	4·80	18·97	18·80
50	4·70	18·55	18·49
62	4·60	18·24	18·18
62	4·50	18·00	17·87
57	4·40	17·72	17·55
34	4·30	17·27	17·24

No. of cases.	Length of left cubit.	Mean of corresponding length of left middle finger.	
		Observed.	Calculated.
29	19·00	4·76	4·75
32	18·70	4·64	4·69
48	18·40	4·60	4·62
70	18·10	4·56	4·55
37	17·80	4·49	4·48
31	17·50	4·40	4·41
28	17·20	4·37	4·34
24	16·90	4·32	4·28

Table IV—*continued.*

No. of cases.	Length of head.	Mean of corresponding breadths of head.		No. of cases.	Breadth of head.	Mean of corresponding lengths of head.	
		Observed.	Calculated.			Observed.	Calculated.
32	7·90	6·14	6·12	27	6·30	7·72	7·84
41	7·80	6·05	6·08	36	6·20	7·72	7·75
46	7·70	6·14	6·04	53	6·10	7·65	7·65
52	7·60	5·98	6·00	58	6·00	7·68	7·60
58	7·50	5·98	5·96	56	5·90	7·50	7·55
34	7·40	5·96	5·91	37	5·80	7·55	7·50
26	7·30	5·85	5·87	30	5·70	7·45	7·46

No. of cases.	Stature.	Mean of corresponding heights of knee.		No. of cases.	Height of knee.	Mean of corresponding statures.	
		Observed.	Calculated.			Observed.	Calculated.
30	70·0	21·7	21·7	23	22·2	70·5	70·6
50	69·0	21·1	21·3	32	21·7	69·8	69·6
38	68·0	20·7	20·9	50	21·2	68·7	68·6
61	67·0	20·5	20·5	68	20·7	67·3	67·7
49	66·0	20·2	20·1	74	20·2	66·2	66·7
36	65·0	19·7	19·7	41	19·7	65·5	65·7
				26	19·2	64·3	64·7

No. of cases.	Left cubit.	Mean of corresponding heights of knee.		No. of cases.	Height of knee.	Mean of corresponding left cubit.	
		Observed.	Calculated.			Observed.	Calculated.
29	19·0	21·5	21·6	23	22·25	18·98	18·97
32	18·7	21·4	21·2	30	21·75	18·68	18·70
48	18·4	20·8	20·9	52	21·25	18·38	18·44
70	17·1	20·7	20·6	69	20·75	18·15	18·17
37	17·8	20·4	20·2	70	20·25	17·75	17·90
31	17·5	20·0	19·9	41	19·75	17·55	17·63
28	17·2	19·8	19·6	27	19·25	17·02	17·36
23	16·9	19·3	19·2				

From Table IV the deductions given in Table V can be made; but they may be made directly from tables of the form of Table III, whence Table IV was itself derived.

When the deviations of the subject and those of the mean of the relatives are severally measured in units of their own Q, there is always a regression in the value of the latter. This is precisely

Table V.

Subject.	Relative.	In units of Q.		In units of ordinary measure.	
		r.	$\surd(1-r^2) = f$.	As 1 to	f.
Stature .... Cubit..........	Cubit .... Stature	0·8	0·60	0·26 2·5	0·45 1·4
Stature .... Head length....	Head length..... Stature	0·35	0·93	0·38 3·2	1·63 0·17
Stature .... Middle finger...	Middle finger.... Stature	0·7	0·72	0·06 8·2	0·10 1·26
Middle finger... Cubit..........	Cubit .... Middle finger....	0·85	0·61	3·13 0·21	0·34 0·09
Head length.... Head breadth...	Head breadth.... Head length.....	0·45	0·89	0·43 0·48	0·16 0·17
Stature .... Height of knee .	Height of knee .. Stature	0·9	0·44	0·41 1·20	0·35 0·77
Cubit.......... Height of knee .	Height of knee .. Cubit	0·8	0·60	1·14 0·56	0·64 0·45

analogous to what was observed in kinship, as I showed in my paper read before this Society on "Hereditary Stature" ('Roy. Soc. Proc.,' vol. 40, 1886, p. 42). The statures of kinsmen are co-related variables; thus, the stature of the father is correlated to that of the adult son, and the stature of the adult son to that of the father; the stature of the uncle to that of the adult nephew, and the stature of the adult nephew to that of the uncle, and so on; but the index of co-relation, which is what I there called "regression," is different in the different cases. In dealing with kinships there is usually no need to reduce the measures to units of Q, because the Q values are alike in all the kinsmen, being of the same value as that of the population at large. It however happened that the very first case that I analysed was different in this respect. It was the reciprocal relation between the statures of what I called the "mid-parent" and the son. The mid-parent is an ideal progenitor, whose stature is the average of that of the father on the one hand and of that of the mother on the other, after her stature had been transmuted into its male equivalent by the multiplication of the factor of 1·08. The Q of the mid-parental statures was found to be 1·2, that of the population dealt with was 1·7. Again, the mean deviation measured in inches of the statures of the sons was found to be two-thirds of the deviation of the mid-parents, while the mean deviation in inches of the mid-parent was one-third of the deviation of the sons. Here the regression, when calculated in Q units, is in the first case from $\frac{1}{1\cdot2}$ to $\frac{2}{3}\times1\cdot7 = 1$ to $0\cdot47$, and in the second case from $\frac{1}{1\cdot7}$ to $\frac{1}{3}\times\frac{1}{1\cdot2} = 1$ to $0\cdot44$, which is practically the same.

The *rationale* of all this will be found discussed in the paper on "Hereditary Stature," to which reference has already been made, and in the appendix to it by Mr. J. D. Hamilton Dickson. The entries in any table, such as Table II, may be looked upon as the values of the vertical ordinates to a surface of frequency, whose mathematical properties were discussed in the above-mentioned appendix, therefore I need not repeat them here. But there is always room for legitimate doubt whether conclusions based on the strict properties of the ideal law of error would be sufficiently correct to be serviceable in actual cases of co-relation between variables that conform only approximately to that law. It is therefore exceedingly desirable to put the theoretical conclusions to frequent test, as has been done with these anthropometric data. The result is that anthropologists may now have much less hesitation than before, in availing themselves of the properties of the law of frequency of error.

I have given in Table V a column headed $\sqrt{(1-r^2)} = f$. The meaning of f is explained in the paper on "Hereditary Stature." It is the Q value of the distribution of any system of x values, as x_1, x_2, x_3, &c., round the mean of all of them, which we may call X. The knowledge of f enables dotted lines to be drawn, as in the figure above, parallel to the line of M values, between which one half of the x observations, for each value of y, will be included. This value of f has much anthropological interest of its own, especially in connexion with M. Bertillon's system of anthropometric identification, to which I will not call attention now.

It is not necessary to extend the list of examples to show how to measure the degree in which one variable may be co-related with the combined effect of n other variables, whether these be themselves co-related or not. To do so, we begin by reducing each measure into others, each having the Q of its own system for a unit. We thus obtain a set of values that can be treated exactly in the same way as the measures of a single variable were treated in Tables II and onwards. Neither is it necessary to give examples of a method by which the degree may be measured, in which the variables in a series each member of which is the summed effect of n variables, may be modified by their partial co-relation. After transmuting the separate measures as above, and then summing them, we should find the probable error of any one of them to be $\sqrt{n}$ if the variables were perfectly independent, and n if they were rigidly and perfectly co-related. The observed value would be almost always somewhere intermediate between these extremes, and would give the information that is wanted.

To conclude, the prominent characteristics of any two co-related variables, so far at least as I have as yet tested them, are four in number. It is supposed that their respective measures have been first transmuted into others of which the unit is in each case equal to the probable error of a single measure in its own series. Let $y =$ the deviation of the subject, whichever of the two variables may be taken in that capacity; and let x_1, x_2, x_3, &c., be the corresponding deviations of the relative, and let the mean of these be X. Then we find: (1) that $y = r\mathrm{X}$ for all values of y; (2) that r is the same, whichever of the two variables is taken for the subject; (3) that r is always less than 1; (4) that r measures the closeness of co-relation.

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ADDRESS

DELIVERED AT THE

ANNIVERSARY MEETING

OF THE

ANTHROPOLOGICAL INSTITUTE

OF

GREAT BRITAIN AND IRELAND,

JANUARY 22ND, 1889.

BY

FRANCIS GALTON, F.R.S., President.

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ADDRESS *delivered at the* ANNIVERSARY MEETING *of the* ANTHROPOLOGICAL INSTITUTE *of* GREAT BRITAIN *and* IRELAND, *January 22nd*, 1889.

By FRANCIS GALTON, Esq., F.R.S., President.

IT would have been a pleasure to me in this address, given at the conclusion of my office as your President, to have cast a retrospect over the proceedings of our Institute during the four years that I have had the honour to hold it. But the subjects that have come before us are so varied that it seemed difficult to briefly summarize them in a manner that should not be too desultory.

On the whole, I thought it might be more useful if I kept to a branch of anthropometry with which many inquiries have made me familiar, and took the opportunity of urging certain views that seem to be worthy the attention of anthropologists.

Before entering upon these more solid topics, let me mention that the laboratory of which I spoke in my last address has been in work during the past year, and that about 1200 persons have been already measured at it in many ways, some more than once. I lay on the table a duplicate of one of the forms of application to be measured, and one of the filled-up schedules. It will be observed that I now have the impressions made in printers' ink of the two thumbs of each person who is measured, being desirous of investigating at leisure the possibilities of employing that method for the purpose of identification, not forgetting the success that attended Sir W. Herschel's use of it in India, but conscious at the same time of practical difficulties. There is no doubt that the imprints of the thumb or finger of different persons vary so much that a glance suffices to distinguish half a dozen varieties, while a minute investigation shows an extraordinary difference in small, though perfectly distinct, peculiarities. Neither is there any room for doubt that these peculiarities are persistent throughout life; nor, again that so satisfactory a method of raising a very strong presumption of identity would be valuable in many cases. It will suffice to quote the following. A newspaper was lately sent me from the distant British settlement of North Borneo, where, owing to the wide and rapid spread of information nowadays, attention had been drawn to an account of a lecture I gave on one of the Friday evenings last spring, at the Royal Institution. It was on "Personal Description and Identification," and a writer in the *British North Borneo Herald* commented upon the remarks there made on finger imprints. He spoke of the great difficulty of identifying coolies either by their photographs or measurements, and added that the question how this could best be done would probably become important in the early future of that country. I also am assured that the difficulty of identifying pensioners and annuitants has led to frequent fraud from personation, involving in the aggregate a very large sum of money annually, as there is good reason to believe. If finger imprints could be practically brought into use, such frauds would be extremely difficult. I am still unable to speak positively as to the easiest and best way of making them, but the plan adopted at the laboratory is as follows. A copper plate is smoothly covered with a very thin layer of printers' ink by a printers' roller, the plate being cleaned every day. Either the plate, or the roller, but preferably the roller, is lightly touched by the thumb, which is afterwards pressed on paper. As the layer of ink is thin, none of it penetrates into the delicate furrows of the skin, but the ridges only are inked, and these leave clear impressions. In this way a permanent mark is registered. A little turpentine cleans the fingers effectually afterwards. But for purposes of identification a simpler process is necessary, one by which a person suspected of personation could furnish an imprint for comparison with the registered mark without having recourse to the troublesome paraphernalia of the printer. Such a process may perhaps be afforded by slightly smoking a piece of smooth metal or glass over the candle, pressing the finger on it, and then making the imprint on a bit of gummed paper that is slightly damped. The impression is particularly distinct, and is sufficiently durable for the purpose. As for the gummed paper, luggage labels can be used; even the fringe to sheets of postage stamps is broad enough to include as much of the impression as is especially wanted—namely, where the whorl of ridges takes its origin.

I hope at some future time to recur to this subject.

Correlation.—The various measurements made at the laboratory have already afforded data for determining the general form of the relation that connects the measures of the different bodily parts of the same person. We know in a general way that a long arm or a long foot implies on the whole a tall stature—*ex pede Herculem;* and conversely that a tall stature implies a long foot. But the question is whether their reciprocal relation, or correlation as it is commonly called, admits of being precisely expressed. Correlation is a very wide subject indeed. It exists wherever the variations of two objects are in part due to common causes; but on this occasion I must only speak of those correlations that are of anthropological interest. The particular problem I first had in view was to ascertain the practical limitations of the ingenious method of anthropometric identification due to M. A. Bertillon, and now in habitual use in the criminal administration of France. As the lengths of the various limbs in the same person are to some degree related together, it was of interest to ascertain the extent to which they also admit of being treated as independent. The first results of the inquiry, which is not yet completed, have been to myself a grateful surprise. Not only did it turn out that the expression and the measure of correlation between any two variables are exceedingly simple and definite, but it became evident almost from the first that I had unconsciously explored the very same ground before. No sooner had I begun to tabulate the data than I saw that they ran in just the same form as those that referred to family likeness in stature, which were submitted to you two years ago. A very little reflection made it clear that family likeness was nothing more than a particular case of the wide subject of correlation, and that the whole of the reasoning already bestowed upon the special case of family likeness was equally applicable to correlation in its most general aspect.[1]

It may be recollected that family likeness in any given degree of kinship—say that between father and son—was expressed by the fact that any peculiarity, that is to say, any difference from mediocrity in the father appears in the son, reduced on the average to just one-third of its amount. Conversely, however paradoxical it might at first sight appear, any pecularity in a son appears in the father, also reduced on the average to one-third of its amount. The "regression," as I called it, from the stature of the known father to the average son, or from the known son to the average father, was from 1 to $\frac{1}{3}$; from the known brother to the unknown brother it was $\frac{2}{3}$; from uncle to nephew, or from nephew to uncle, it was $\frac{2}{9}$; and in kinship so distant as to have no sensible influence, it was from 1 to 0. Whether the peculiarity was large or small, these ratios remained unaltered. The reason of all this was thoroughly explained, and need not be repeated here. Now the relation of head-length to head-breadth, whose variations are on much the same scale, or speaking in technical language, whose probable errors are the same, is identical in character to the relation between kinsmen. There is regression in both cases, though its value differs. The lengths of head-lengths and head-breadths are akin to each other in the same sense as kinsmen are. So it is in the closer relation between the lengths of symmetrical limbs, left arm to right arm, left leg to right leg. The regression would be strictly reciprocal in these cases. When, however, we compare limbs whose variations take place on different scales, the differences of scale have

[1] "Proc. Roy. Soc.," 1886 p 42, and "Journ. Anthrop. Inst.," 1885, p. 252.

to be allowed for before the regression can assume a reciprocal form. The plan of making the requisite allowance is perfectly simple; it merely consists in dividing each result by the probable error of any one of the observations from which it was deduced. Unfortunately the method cannot be briefly explained except by using these technical terms. In some cases the scale of variation in the two correlated members is very different, and this divisor may be very large. Thus the length of the middle finger varies at so very different a rate from that of the stature that 1 inch of difference of middle finger length is associated on the average with 8·4 inches of stature. On the other hand, 10 inches of stature is associated on the average with 0·6 inch of middle finger length. There is no reciprocity in these numerals; yet, for all that, when the scale of their respective variations is taken into account by using the abovementioned divisor, the values become strictly reciprocal. I shall be better able to enter more fully into this subject later on, towards the close of this address.

Variety.—The principal topic of my further remarks will be the claims of Variety to more consideration from anthropologists than it usually receives. Anthropologists commonly narrow their inquiries to the purpose of ascertaining the mean values of different groups, while the variety of the individuals who constitute them is too often passed over with contented neglect. It seems to me a great loss of opportunity when, after observations have been laboriously collected and subsequently discussed in order to obtain mean values, the very little extra trouble has not been taken that would determine such other values as would go far to express the variety of the individuals in those groups. Much experience some years back, and much new experience during the past year, has proved to me the ease with which variety may be adequately expressed, and the high importance of taking it into account. Numerous problems that ought to be of especial interest to anthropologists, deal solely with variety.

There can be little doubt that most persons fail to have an adequate conception of the orderliness of variability, and think it useless to pay scientific attention to variety, as being, in their view, a subject wholly beyond the powers of definition. They forget that what is confessedly undefined in the individual may be definite in the group, and that uncertainty as regards the one is in no way incompatible with statistical assurance as regards the other. Almost everybody is familiar nowadays with the constancy of the Average in different samples of the same large group, but they do not often realise the way in which a similar statistical constancy permeates the whole of the relations between the various members of the group. The Mean or the Average is practically nothing more than the middlemost value in a marshaled series. A constancy analogous to that of the Mean characterises each value that occupies the other fractional positions, such as the 10th per cent., or the 20th per cent. of the total length of the marshaled series. The condition of constancy is not a peculiar attribute of the 50th per cent., or middlemost.

Greater interest is usually attached to individuals who occupy positions towards either of the ends of a marshaled series, than to those who stand about its middle. For example, an average man is morally and intellectually an uninteresting being. The class to which he belongs is bulky, and no doubt serves to keep the course of social life in action. It also affords, by its inertia, a regulator that, like the fly-wheel to the steam-engine, resists sudden and irregular changes. But the average man is of no direct help towards evolution, which appears to our dim vision to be the primary purpose, so to speak, of all living existence. Evolution is an unresting progression; the nature of the average individual is essentially unprogressive. His children tend to resemble him exactly, whereas the children of exceptional persons tend to regress to mediocrity. Consider the interest attached to variation in the moral and intellectual nature of man and the value of variability in those respects. For example, the average worth of the Hebrew race shows little that is worthy

f. 5r

of note, but that race has been of peculiar interest on account of the great varieties of character that it has produced. Its variability in ancient and modern times seems to have been extraordinarily great. It has been able to supply men, time after time, who have towered high above their fellows, and have left enduring marks on the history of the world.

Some thorough-going democrats may look with complacency on a mob of mediocrities, but to most other persons they are the reverse of attractive. The absence of elevated and heroic natures in any group of men is a heavy set-off against the freedom from a corresponding number of very degraded forms. The general standard of thought and morals in a mob of mediocrities must be mediocre, and, what is worse, contentedly so. The lack of living men to afford lofty examples, and to educate the virtue of reverence, must leave an irremediable blank. All men would in that case find themselves at nearly the same dead average level, each as meanly endowed as his neighbour.

These remarks apply with obvious modifications to variety in the physical faculties. Peculiar gifts, moreover, afford an especial justification for division of labour, each man doing that which he can do best.

The method I have myself usually adopted for expressing and dealing with the variety of the individuals in a group, so as to treat a whole population in a compendious way, has been already explained on more than one occasion. I should not have again alluded to it had I not had much occasion of late to test and develop it, also to devise an unpretentious little table of figures that I call a "table of normal distribution," which has been of singular assistance to myself. I trust it may be equally useful to other anthropologists. It is appended to these remarks, and I should like after a short necessary preface to say something about it. The table and its origin, and several uses to which it has been applied, will be found in a book by myself, that is on the point of publication, called "Natural Inheritance" (Macmillan and Co.). All the data to which I shall refer will be found in that book also, except such as concern correlation.

f. 5v

These accompanied a memoir read by me only a month ago before the Royal Society.[1]

The first step in the problem of expressing variety among the individual members of any sample, is to marshal their measures in order, into a class. We begin with the smallest measure and end with the greatest. The object of the next step is to free ourselves from the embarrassment due to the different numbers of individuals in different classes. This is effected by dividing the class, whatever its size may be, into 100 equal portions, calling the lines that divide the portions by the name of grades. The first of these portions will therefore lie between grades 0° and 1°, and the hundredth and last portion between grades 99° and 100°. We have next to find by interpolation the values that correspond to as many of these grades as we care to deal with. It is of no consequence whether or no the number in the class is evenly divisible by 100, because we can interpolate and get the values we want, all the same. This having been done, the value that corresponds to the 50th grade will be the middlemost. It is the equivalent for all ordinary purposes to the mean or average value; but as it may not be strictly the same, it is right to call it by a distinctive name,

[1] "Proc. Roy. Soc.," Dec. 20, 1888, vol. 45. "Correlations and their Measurement, chiefly from Anthropometric Data." The general result of the inquiry was that, when two variables that are severally conformable to the law of frequency of error, are correlated together, the conditions and measure of their closeness of correlation admits of being easily expressed. Let x_1, x_2, x_3, &c., be the deviations in inches, or other absolute measure of the several "relatives" of a large number of "subjects," each of whom has a deviation, y, and let X be the mean of the values of x_1, x_2, x_3, &c. Then (1) $y = r\text{X}$, whatever may be the value of y. (2) If the deviations are measured, not in inches or other absolute standard, but in units, each equal to the Q (that is, to the probable error) of their respective systems, then r will be the same, whichever of the two correlated variables is taken for the subject. In other words, the relation between them becomes reciprocal; it is strictly a correlation. (3) r is always less than 1. (4) r (which, in the memoir on hereditary stature, was called the ratio of regression) is a measure of the closeness of correlation. (5) The probable error, or Q, of the distribution of x_1, x_2, y_3, &c., about X, is the same for all values of y, and is equal to $\sqrt{(1-r^2)}$ when the conditions specified in (2) are observed.

It should be noted that the use of the Q unit enables the variations of the and none simpler or more convenient occurs than the letter M. So I will henceforth use M to denote the middlemost or median value, or, in other words, that which corresponds to the 50th (centesimal) grade.

The difference between the extreme ends of a marshaled series is no proper measure of the variety of the men who compose it. However few may be the objects in the series, it is always possible that a giant or a dwarf, so to speak, may be included among them. The presence of either would mislead as to the range of variety likely to be found in another equally numerous sample taken from the same group. The values in a marshaled series run with regularity only about its broad and middle part; they never do so in the parts near to either of its extremities. In a series that consists of a few hundreds of individuals, the regularity is usually found to begin at about grade 5°, and to continue up to about grade 95°. Therefore it is out of the middle part, between 5° and 95°, or better out of a still more central portion of it, that points should be selected between which the rate of its variety may be measured. Such points are conveniently found at the 25th and the 75th grades. Just as the grade 50° divides the class into two equal parts, so the grades 25° and 75° subdivide it into quarters, and the difference between those values affords an irreproachable basis for the unit of variety. The actual unit is taken as the half of the value of that difference, because the value at 25° tends to be just as much below that at 50°, as the value at 75° is above it. Therefore the average of these two values is a better measure than their sum. Briefly, if we distinguish the measure at 25° by the letter Q_1, and that at 75° by Q_3, then the unit of variety is $\frac{1}{2}(Q_3 - Q_1)$, and this unit we will henceforth call Q. It is practically, but not strictly, identical with the "probable error" of a single observation, and is a useful symbol, as con-

most diverse qualities to be compared with as much precision as those of the same quality. Thus, variations in lung-capacity which are measured in volume can be compared with those of strength measured by weight lifted, or of swiftness measured in time and distance. It places all variables on a common footing.

sisting of a single syllable and a single letter instead of the 5 syllables and the 13 letters that form the very misleading phrase of "probable error." As M measures the average, so Q measures the variety, and they are independent of one another. In strength, for example, the relation of Q to M in the particular group of adult males on which I worked was as 1 to 10; in the statures of the same group it was as 1 to 40; in breathing capacity as 1 to 9; in weight as 1 to 14.

The mean or average is an arithmetical muddle of all the values in the series; it presents to the imagination by no means so clean an idea as the middlemost value M. Therefore, although the peculiarities of an individual are commonly considered in the light of deviations from the mean or average value, I prefer to reckon them as deviations from M. Practically the two methods are identical, but I find the latter more convenient to work with, and believe it to be the better of the two in every other way.

The causes and the laws of deviation, or of variation, are identical with those of error, and the well-known law of frequency of error gives data whence the *relative* values of the deviations at the several grades may be calculated for any normal series. If we know the actual deviation at any one specified grade, then the *absolute* values of those at every other grade can be calculated; consequently the variety of the whole series is expressed by only two data, a grade and the corresponding deviation.

The small table of distribution, of which I spoke, gives the values at each grade when Q is equal to 1. In this case the value at 25° is − 1, and that at 75° is + 1. If we desire to determine the Q of any such series, the only required datum, as has been just laid, is the value of the deviation at some one known grade; then, by dividing that deviation by the tabular value, we obtain Q at once. Or, conversely, if we know the Q of the series, and wish to calculate the deviation at any given grade we multiply the tabular deviation by Q. Thus, in the stature of men, which varies in an approximately normal

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manner, the value of Q is about 1·7 inch, therefore to find the deviation in stature at any grade among adult males, we multiply the tabular value by 1·7 inch.

If we know the *measures* at any two specified grades of a normal series, we are easily able to calculate both Q and M, and can thence derive the measures at any other desired grades. I have long since pointed out the possibility of a traveller availing himself of this method of anthropological investigation; but, for the want of the annexed table of distribution, he would probably be puzzled in making the necessary calculation. With the aid of this table the calculation is most readily performed. Let us suppose that the traveller is among savages who use the bow, and that he desires to learn as much as he can about their strengths. He selects two bows; the one somewhat easy to draw, and the other somewhat difficult, and at his leisure, either before or after the experiment he ascertains exactly how many pounds weight is required to draw them severally to the full Then by exciting emulation and by the offer of small prizes, he induces a great many of the natives to try their strengths upon them. He notes how many make the attempt, and how many of them fail in either test. This is all the observation requisite, though common sense would suggest the use of three and not two bows, in order that the data from the third bow might correct or confirm the results derived from the other two. Let us work out a case, not an imaginary one, but derived from tables I have already published, and of which I will speak directly. Let the problem be as follows:—

30 per cent. of the men fail to exert a pulling strength of 68 pounds; 60 per cent. fail to pull 77 pounds. What is the Q and the M of the group?

Consider this 30 per cent. to be the exact equivalent of grade 30°, and the 60 per cent. of grade 60°. The reason why the percentage of failure, and the number of the grade are always to be taken as identical will be found in a footnote to the table, and I need not stop to speak of it. Now, the tabular value at grade 30° is $-0·78$; that at 60° is $+0·38$; the difference between them being 1·16. On the other hand, the difference between the two test values of 68 pounds and 77 pounds is 9 pounds. Therefore Q is equal to 9 pounds divided by 1·16; that is, to 7·8 pounds. M may be obtained by either of two ways, which will always give the same answer. We may subtract 0·38 × 7·8 pounds from 77 pounds, or we may add 0·78 × 7·8 pounds to 68 pounds. Each gives 74 pounds. Observation gave precisely these values both for Q and for M. The data were published in the Journal of this Institute in 1884 as a table of "percentiles," and were derived from measures made at the International Health Exhibition. The value of M is given directly in the table, but that of Q happens not to be given there; it may easily be found by interpolation between those that are.

That table of percentiles affords excellent material for experimental calculations on the principle of this test, and for estimating its trustworthiness in practice. It contains a variety of measures referring to eighteen different series, all corresponding to the same grades—namely, to 5°, 10°, 20°. and onwards for every tenth grade up to 90° and ending with 95°. The measures refer to stature, height sitting above seat of chair, span, weight, breathing capacity, strength of pull, strength of squeeze, swiftness of blow, keenness of eyesight, and in each case the values are given for adult males and adult females separately. I have since found ("Natural Inheritance," pages 56, 201), that when the deviations are all reduced in terms of their respective Q values, by dividing each of them by its Q, that the average value of all the deviations at each of the grades in the eighteen series closely corresponds to the normal series, though individually they differ more or less from it, some in one way, some in another. On the whole, the error of treating an unknown series as if it were a normal one can rarely be very large, always supposing that we do not meddle with grades lower than 5° or higher than 95°.

It will be of interest to put the comparison on record. It is as follows:—

Grades	5°	10°	20°	30°	40°	50°
Observed	− 2·44	− 1·87	− 1·24	− 0·77	− 0·40	0
Normal { − below 50° + above 50° }	2·44	1·90	1·25	0·78	0·38	0
Observed	+ 2·47	+ 1·92	+ 1·21	+ 0·75	+ 0·38	0
Grades	95°	90°	80°	70°	60°	50°

The "observed" are the mean values, made as above described, of the eighteen series; the "normal" are taken from the table of distribution given further on.

An ingenious traveller might obtain a great number of approximate but interesting data by the method just described, measuring various faculties of the natives, such as their delicacy of eyesight and hearing, their swiftness in running, their accuracy of aim with spear, arrow, boomerang, sling, gun, and so forth, laterally from the object aimed at, or else vertically; distance of throw, the stature, and much else. But he should certainly use three test objects, and not two only.

It should be remarked that, if the distribution of deviation proved to be constant throughout any large class of faculties, though the Q might differ in different sub-classes of it, then, even though the distribution of that faculty was very far indeed from being normal, an appropriate table of distribution could still be compiled in order to solve such problems as those mentioned above. I have as yet no accurate data to put this idea to a practical test.

There are three convenient stages of approximation in expressing the variety of the various measures in a series, each of which reaches considerably nearer to precision than the one before. The first is to give only Q and M; the second is to record the measures at the grades 10°, 25°, 50°, 75°, and 90°; the third is the more minute method, adopted in the tables of percentiles—viz., to give the measures at 5°, 10°, 20°, &c., 80°, 90°, and 95°. It may in some cases be found worth while to go further, say to 1° and 99°, or even also to 0°·1, and 99°·9. So much for the method of expressing variety.

The use of Q is by no means limited to the objects just named. It is a necessary datum wherever the law of frequency of error has to be applied, whose properties are applicable to a very large number of anthropological problems with more accuracy of result than might have been anticipated, as the series are only approximately normal. This has been practically shown by the agreement among themselves of several inquiries to which I will shortly allude. It is theoretically defensible by two considerations. The one is that the law of frequency supposes the amount of error or of deviation to be the same in symmetrically disposed grades on either side of 50°, their signs being alone different, *minus* on the one side of 50° and *plus* on the other. Now, in an observed series there may be, and often is, a want of symmetry, but if the deviate, say at 70°, is as much greater than the normal value as the deviate at 30° is less than the normal, then the effects of these two upon the final result will be much the same as if there had been exact symmetry at those points. The other consideration is that any nonconformity between the observed deviates and the theoretical ones mostly affects the extremities of the series, and consequently has but a small and perhaps insensible effect on the broad general conclusions. We need care little for any vagaries outside of the grades 5° and 95°, if the intervening portion gives fairly good results. As the latter forms nine-tenths of the whole series, the irregularities in the remaining tenth are of small relative importance.

One great use of Q or of any of its equivalents, as the mean error, &c., is to enable us to estimate the trustworthiness of our average results. We require to know both Q and the number of the observations, before it is possible to estimate the degree of dependence to be placed on M. If only one observation was accessible, then the degree of its trustworthiness (its "probable error") would of course be equal to Q; in other words, its error would be just as likely as not to exceed Q. If there were two, two hundred, two thousand, or any other number of observations, the probable errors of their respective values M would be reduced, but not in simple proportion. They would be equal to Q divided by the square roots of those numbers.

When we desire to ascertain the trustworthiness of the difference between the M values of two series, as between the mean statures of the professional and artisan class as derived from certain observations, the properties of the law of frequency of error must again be appealed to. Anthropologists are much engaged in studying such differences as these; but from their disregard of the simple datum Q or of some one of its equivalents, and from not being familiar with the way of employing them, there is usually a lamentable and quite unnecessary vagueness in the value that can be assigned to their results. This is especially the case in comparisons between the average dimensions of the skulls of various races, when the average values are alone given, and when they have been derived, as is often the case, from the measurement of only a few specimens. An almost solitary exception to this needless laxity in statistical treatment will be found in a brief but admirably-expressed memoir by Dr. Venn, the well-known author of the "Logic of Chance." It is upon Cambridge anthropometry, and was published in the last number of the Journal of this Institute. It deserves to be a model to those who are engaged in similar inquiries.

Another class of investigations in which a knowledge of Q is essential, was spoken of some time back—namely, into the questions of Correlation in the widest sense of the word. These problems have nothing to do with the relations of the M values, but are solely concerned with variations, that is with the deviations from M at the various grades. It is true that a knowledge of M is requisite. We have to subtract it from the measures in order to get at the deviations. But after this is done, M is put aside. It has no part in the work of the problem; it is only after the results have been arrived at without its use that it is again brought forward and added to them. Numerous properties of the law of frequency of error in which Q is the datum, were utilized in my inquiries into family likeness in stature, and in all cases they brought out consistent results. An excellent example of their consistency was seen in the results of the methods employed to determine the variety of individual statures in families of brothers. Four different properties of the law had to be applied to partly different samples of the same group in order to determine the value of the Q of stature in fraternities, and they respectively gave 1·07, 0·98, 1·10, and 1·10 inch, which, statistically speaking, are much alike. Certain properties of the law of frequency of error were also applied to family likeness in eye colour, with results that gave by calculation the total number of light-eyed children in families differently grouped according to their parentage and grandparentage, and according to three different sets of data. The resulting figures were 623, 601, and 614 respectively, the observed number being 629 (Proc. Roy. Soc., 1886, p. 415). Other properties of the same law have been applied by myself in the book already mentioned to determine the ratio of artistic to non-artistic children in families whose parentages were known to be either both artistic, one artistic and one not, or neither artistic. They gave the ratios of 64, 39, and 21 respectively, as against the observed values in 1507 children, of 60, 39, and 17.

Lastly, as regards the correlation of lengths of the different limbs. It has already been shown that the correlation connects the deviations, and has nothing to do with the mean or average values. Now, to express this relation truly, so that it shall be reciprocal, the scale of deviation of the correlated limbs, say, for example, of the cubit and of the stature of adult males, must be reduced to a common standard. We therefore reduce them severally to scales in each of which their own Q is the unit. The Q of the cubit is 0·56 inch, therefore we divide each of its deviations by 0·56. The Q of the stature is 1·75 inch, so we divide each of its deviations by 1·75. When this is done the correlation is perfect. The value of regression is found to be 0·8, whether the cubit be taken as the "subject" and the mean of the corresponding statures as the "relative," or *vice versâ*.

The value of the regression was ascertained for each of many pairs of the following elements, and a comparison made in each case between the correlated values as observed and those calculated from the ratio of regression. The coincidence was close throughout, quite as much so as the small number of cases under examination, 350 in all, could lead us to hope. The elements were nine in number,[1] viz., head length, breadth of head, length of right leg below the knee, of left cubit, of left middle finger, of the height sitting above the chair, of stature, of the differences between the two foregoing (which indicate the total

[1] The head length is here the maximum length measured from the notch below the brow. The cubit is measured with the hand prone, from the flexed elbow to the tip of the middle finger. The height of knee is taken from a stool, on which the foot rests with the knee flexed at right angles; from this the measured thickness of the heel of the boot is subtracted. All measures had to be made in the ordinary clothing. The M and Q values of these elements among adult males were found to be as follows: left cubit, 18·05 and 0·56; stature, 67·2 and 1·75; head length, 7·62 and 0·19; head breadth, 6·00 and 0·18; left middle finger, 4·54 and 0·15; height of right knee, 20·50 and 0·80; all the measures being in inches. The values of r in the following pairs of variables were found to be: head length and stature, 0·35; left middle finger and stature, 0·70; head breadth and head length, 0·45; height of knee and stature, 0·9; left cubit and height of right knee, 0·8. The comparison of the observed results with those calculated from the above data showed a very close agreement (Proc. Roy. Soc., Dec. 20, 1888). The measures were of 350 male adults, containing a large proportion of students barely above twenty-one years of age and several artizans, made at the laboratory at South Kensington, belonging to the author. The smallness of the number of measures, viz., 350, is of little importance, as the results run with fair smoothness. Neither does the fact of most of the persons measured being hardly full grown, and of others being of the generally short class of artizans, affect the main results. It somewhat diminishes the values of M, and very slightly increases that of Q, but it cannot be expected to have any considerable influence on the value of r.

length of the lower limbs), and of the span. Anthropologists seem to have little idea of the wide fields of inquiry open to them as soon as they are prepared to deal with the variety of individuals, and cease to narrow their view to the consideration of the average value of all of them.

Enough has now been said to justify the claims with which I started, and which take this final form. First, wherever it is likely to be of use, that, in those series of which the M is calculated, the measures at a certain number of selected grades should also be calculated and given, sufficient to enable the rest of the marshaled series to be found with adequate accuracy by interpolation. Secondly, that the value of Q or of one of its equivalents should always be given as well, and for two reasons. The one is, that M and Q suffice between them to give an approximate determination of the whole series, which is the more closely approximate as the series is more closely of the normal type; and, secondly, because Q or one of its equivalents is an essential datum before any application can be made of the law of frequency of error. The properties of this law are, as we have seen, largely available in anthropological inquiry. They enable us to define the trustworthiness of our results, and to deal with such interesting problems as those of correlation and family resemblance, which cannot be solved without their help.

Table of ordinates to the normal Curve of Distribution, in which the unit = the probable error, and the grades, which are the abscissæ, run from 0° to 100°.

Grades.	0	1	2	3	4	5	6	7	8	9
0	∞	−3·45	−3·05	−2·79	−2·60	−2·44	−2·31	−2·19	−2·08	−1·99
10	−1·90	−1·82	−1·74	−1·67	−1·60	−1·54	−1·47	−1·42	−1·36	−1·30
20	−1·25	−1·20	−1·15	−1·10	−1·05	−1·00	−0·95	−0·91	−0·86	−0·82
30	−0·78	−0·74	−0·69	−0·65	−0·61	−0·57	−0·53	−0·49	−0·45	−0·41
40	−0·38	−0·34	−0·30	−0·26	−0·22	−0·19	−0·15	−0·11	−0·07	−0·04
50	+0·00	+0·04	+0·07	+0·11	+0·15	+0·19	+0·22	+0·26	+0·30	+0·34
60	+0·38	+0·41	+0·45	+0·49	+0·53	+0·57	+0·61	+0·65	+0·69	+0·74
70	+0·78	+0·82	+0·86	+0·91	+0·95	+1·00	+1·05	+1·10	+1·15	+1·20
80	+1·25	+1·30	+1·36	+1·42	+1·47	+1·54	+1·60	+1·67	+1·74	+1·82
90	+1·99	+1·99	+2·08	+2·19	+2·31	+2·44	+2·60	+2·79	+3·05	+3·45

This table is an inverse rendering of the values derived by interpolation from the ordinary table of the probability integral, but its unit is changed from that of the modulus to that of the probable error, or what is almost exactly the same thing, to Q; and the (centesimal) grades are reckoned from 0° to 100°. In the usual way of reckoning, the 50th grade should have been reckoned as 0°, and the deviations should have run on the one side down to − 50°, and on the other up to + 50°.

Referring to what was said some way back, that if 30 per cent. of the natives whose strength was being tested fail to pull 60 pounds, then 60 pounds must be taken as the measure corresponding to the grade of 30°; the reason for this is as follows: The 30th grade separates the man who ranks 30th in a class of 100 men from his neighbour who ranks 31st. It does so for the same reason that grade 1° separates the man who ranks 1st from the man who ranks 2nd. Now, the 30th man failed in the test, and the 31st succeeded. Therefore the grade corresponding to bare success lies between them, and is the same as grade 30°.

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A new Instrument for Measuring the Rate of Movement of the various Limbs.

BY

FRANCIS GALTON, F.R.S.

Vice-President Anthropological Institute.

(WITH ZINCOGRAPH.)

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1890.

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A NEW INSTRUMENT *for measuring the* RATE *of* MOVEMENT *of the various* LIMBS.

By FRANCIS GALTON, F.R.S.,

Vice-President Anthropological Institute.

[WITH ZINCOGRAPH.]

DIFFICULTY has been found in making courses of experiment on the rates of muscular movement in different persons. This is partly due to the tedium of observing with a blackened cylinder and a vibrating tuning fork, or with a broken electrical current and a Hipp's chronograph, or other apparatus of the kind. More especially is it due to the violence and to the somewhat uncertain direction of the movements to be measured.

In the laboratory that I set up in 1884 in the International Health Exhibition, the instrument used for the purpose was a stout sliding bar, struck forward by the fist. As soon as it started, it released a fixed spring that had been deflected to one side, and which thenceforward vibrated across the bar. A pencil attached to the free end of the spring, left a sinuous trace on the bar, and the number of bends in the trace in any space was proportionate to the time taken by the bar to travel through that space. By using an appropriate scale the absolute mean velocity during any given period was easily read off.

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But it proved that few persons delivered their blow in a straight forward manner. They usually struck the deal bar to one side and often broke the apparatus, and when I replaced it with a bar of harder wood, they still broke it, and hurt themselves rather severely at the same time. Experience showed the necessity of eliminating this difficulty and danger. Whatever may be the violence or the direction of the blow, the recording apparatus should be safe, and the person tested should be unable to injure himself.

The method adopted in the present design is perhaps most simply explained by referring to the action of a spring measuring tape. When the end of one of these is pulled out and then let go, it springs sharply back, the tape running cleanly through a slit. Suppose for a moment that it runs back more quickly than the hand could follow it, then, if the end of the tape is retained in the hand that gives the blow, the tape will run through the slit at the exact rate at which the blow is given. It cannot go quicker, because the hand retards it; it will not go slower, because the spring urges it on. The hand need not be near to the tape; it may be connected with it by a long thread, and the action of the apparatus will remain unaltered. The instrument then would be quite out of reach of harm. In this way, a violent movement full of danger to most instruments is translated into a swift movement of a mere thread, running smoothly between eye holes in a straight line.

Having thus got a thread moving smoothly with the same velocity as the arm, the next question is how to measure that velocity. I do it by gravity. The thread during part of its course is arranged to travel vertically, and passes through a small inverted cone, to which it is fixed. The thread then passes loosely through a cylindrical bead of white ivory, whose bottom rests on the face of the cone. When the moving thread is suddenly arrested, the bead is tossed up to a height dependent on the velocity of the thread at the time when it was arrested. The momentary pause of the white bead, after it ceases to ascend and before it begins to descend, enables the height it has attained to be easily read off, upon an appropriate scale, which tells at how many feet per second the string was moving at the instant before it was checked.

The instrument that I show has worked well, but doubtless admits of much improvement in detail. It is exhibited in its present early stage for the benefit of criticism and suggestions.

The proportions of the instrument have been guided by the fact that the issuing thread must be at about the level of the shoulder, and that the scale must be opposite to the eye of the experimenter. It was also thought best to arrange the scale so

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as to show velocities between, about 5 feet and 30 feet per second. To do this, and at the same time to keep the scale of a convenient size, the velocity of the bead must be mechanically reduced to a fraction of that of the free end of the string. In my instrument I have reduced it to one-third. This being premised, the principle of the machine is here shown in diagrammatic form. In the actual machine there are some differences of detail, and an adjustment is added for readily bringing the bead to the zero position, when the machine is at rest. A piece of thin pianoforte wire is interpolated for the bead to run on, and the check is given by a small india-rubber ball on the string striking home against a fixed cork buffer. It is not of the least consequence that the check should be sharp; all that is necessary is that its motion should *begin* to be checked when the bead is at zero. Then the bead leaves the cone, and henceforward behaves as a free projectile.

We must satisfy ourselves that the spring can pull the thread more quickly than the arm can follow. This is easily done by seeing that the ball is tossed up considerably higher, when the string is allowed to run home unrestrained, than it does when it is held in the hand that delivers the blow.

I find considerable regularity in the readings, when the conditions under which the blow is delivered are similar, but a small alteration in those conditions may make a considerable alteration in the results. It is remarkable how greatly a movement of the wrist may increase the velocity of the hand. We see an effect of this kind in a thrown ball, which travels vastly quicker than the wrist of the hand that throws it. The question of the best measures to take, and the best conditions under which to take them, deserves careful consideration, and I should be grateful for suggestions. One good test position seems to be, to stand behind, and slightly pressing against a horizontal bar that lies lower than the elbow, to plant the feet in chalked spaces, the left foot parallel to the bar, and the right foot pointing to the front, then reaching forward as far as the bar conveniently permits, to seize the tightened string and to draw it back to the vertical post to which the bar is fixed, and from that position to deliver the blow.

For calculating the scale, let $v =$ the velocity of the cone in feet per second at the moment before it is checked at the zero point, and $s =$ the height in inches to which the bead will be tossed, then $s = v^2 \times 0{\cdot}186$ inches. By giving successive values to v the scale is easily calculated. As in my instrument v is only one-third of the velocity of the arm, we have to calculate for values of $v = \frac{10}{3}$ feet per second, $\frac{11}{3}$ feet per second, &c., in order to find the height to which the bead will be tossed, when the velocity of the arm is 10, 11, &c., feet per second, and these latter figures must be inscribed as the calculated heights. The actual velocity of a blow being taken at 20 feet per second, the difference on the scale between it and 21 feet per second, is then the difference between 8·28 inches and 9·13 inches, or nearly an inch, an ample and convenient interval. For calculating according to this scale, if $w =$ the velocity of the arm, $s = w^2 \times 0{\cdot}0207$.

I had hoped to have given more definite results in this paper but accidental delays in the completion of some carpenter's work have prevented me. Perhaps I may be allowed to add a foot-note before these notes are printed, if there be time and opportunity to do so.

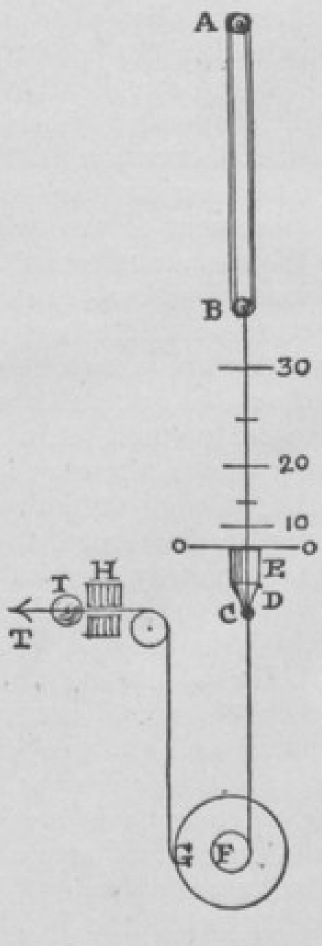
Description of the Figure.

AB. A stretched india-rubber band.

BC. Thin steel wire, upon which the ivory cylinder E runs loosely like a bead. The end of BC passes through an inverted cone D, into which it is fixed.

F, G. Are two grooved wheels fixed together, and turning freely on a common fixed axis. The diameter of F is one-third that of G. A thread passing from C is wrapped a few times round E, to which its other end is fixed. Another thread fixed to F is wrapped a few times round F, and is then carried, first vertically upwards, and afterwards horizontally, by passing over a grooved wheel. It then passes through a hole in a fixed buffer H. On the other side of H it passes through, and is attached to a small india-rubber ball I.

When the machine is at rest the tension of AB causes I to be pressed home against H. When T is drawn out, AB stretches further, D descends, and the cylinder E descends with it. On delivering a blow with the hand that holds the free end of T, C ascends up to the point at which



F. 4

the top of E is brought level with the zero line. There C stops, owing to I coming in contact with the buffers H. Consequently the ivory cylinder E is tossed up as a free projectile, and the graduation to which it ascends is noted. The number attached to that graduation shows the number of feet per second at which T was moving immediately before its motion was checked.

NOTE, OCTOBER 17.—The instrument has worked regularly at my laboratory after a little experience had suggested some minor amendments in detail. The chief of these was to greatly lengthen the elastic band, by passing it over a pulley at the top and bringing it thence downwards to the bottom of the frame. This greatly increased the uniformity of the strain and it makes the action very smooth.

The person experimented on stands with his back to a wall and strikes at the end of a long feather so placed that when the fist reaches the feather the india-rubber ball strikes the buffer. Care is taken that the wrist does not bend. I have not as yet worked up the results. The machine was made for me by Groves, 89, Bolsover Street, W. F. G.

[*Reprinted from the Journal of the Anthropological Institute, November,* 1890.]

Harrison and Sons, Printers in Ordinary to Her Majesty, St. Martin's Lane.

From the Proceedings of the Royal Society, Vol. 49.

METHOD OF INDEXING FINGER-MARKS.

BY

FRANCIS GALTON, F.R.S.

[*From the* PROCEEDINGS OF THE ROYAL SOCIETY, Vol. 49.]

"Method of indexing Finger-Marks." By FRANCIS GALTON, F.R.S. Received April 30, 1891.

Sufficient proof was adduced by me in a memoir read November 27, 1890, before the Royal Society ('Phil. Trans.,' B, 1891), of the extraordinary persistence of the papillary ridges on the inner surface of the hands throughout life. It was shown that the impression in ink upon paper of each finger tip, contained on the average from twenty-five to thirty distinct points of reference, every one of which, with the rarest exception, appeared to be absolutely persistent. Consequently that it was possible to affirm with practical certainty whether or no any two submitted impressions were made by the fingers of the same person.

In the present memoir I shall explain the way in which finger prints may be indexed and referred to after the fashion of a dictionary, and on the same general principle as that devised by A. Bertillon with respect to anthropometric measures, whose ingenious method is now in regular use on a very large scale in the criminal administration of France and elsewhere. I desire to show how vastly the practical efficiency of any such method as that of A. Bertillon admits of being increased by taking finger prints into account in the way about to be described.

It must not, however, be supposed that the use of indexing finger marks is limited to the above purpose, the power of doing so being equally needed for racial and hereditary inquiries. I do not dwell upon these applications now, simply because I am engaged in making them, and the results are not yet ready to be published. I ought, however, to mention that a great increase of experience has fully confirmed my earlier views, that finger marks are singularly appropriate subjects of anthropometric study owing to many distinct reasons. The impressions are easily to be made by anyone who has the proper appliances at hand. They are as durable as any other printed matter, and they occupy very little space. The patterns are usually sharp and clear, and their *minutiæ* are independent of age and growth. They are necessarily trustworthy, and no reluctance is shown in permitting them to be taken, which can be founded either upon personal vanity or upon an unwillingness to communicate undesirable family peculiarities.

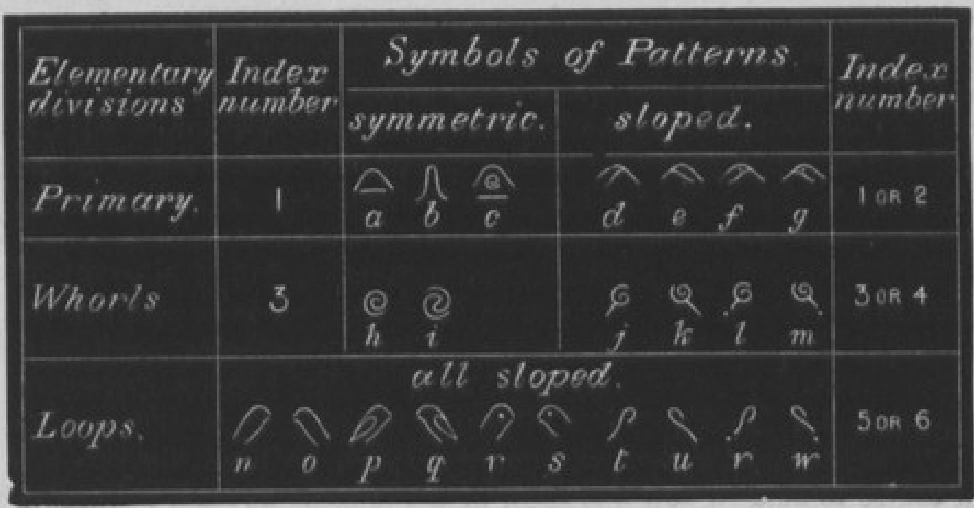
Without caring to dwell on many of my earlier failures to index the finger prints in a satisfactory way, my description shall be confined to that which has proved to be a success. It is based on a small variety of conspicuous differences of pattern in each of many digits, and not upon the numerous minute peculiarities of a single digit. My conclusions are principally based on a study of the impressions of all ten digits of 289 different persons, but the tables about to be given refer only to the first 100 on my list. These are sufficiently numerous to serve as a fair sample of what we might always expect to find, while they are not too cumbrous to print and to discuss in full detail.

I described in my previous memoir the way in which the impressions had been made that were then shown. A plate of copper was blackened with printer's ink, the ink being of a rather fluid character, and spread very thinly and evenly over its surface by a printer's roller. The thumb, which was then the subject of discussion, was pressed and slightly rolled on the inked plate, and afterwards on the paper. In the present collection of all ten digits, four operations were used in each case. First the four fingers of one hand were simultaneously printed from, and then its thumb in the way above described; afterwards, the other hand was treated in the same way.

Though I have spoken and shall speak only of impressions, it is not really necessary for the purpose of compiling an index to make any impression at all. The entries that are wanted for the index can be derived directly from the fingers themselves.

I rely, for the purpose of indexing, on the three elementary divisions of primaries, whorls, and loops. They are severally expressed by the numerals 1 and 2, 3 and 4, 5 and 6. The reason of this double numeration is that most of the patterns have a definite axis. Those that are formed by ridges which proceed from only one side of the finger, will necessarily lie in a sloping direction across its axis pointing to the one side or the other according to that from which the supply of ridges proceeds. The only patterns that are symmetrically disposed about a vertical axis are *b* and to a lesser degree *a*, *c*, *h*, and *i* in fig. 1. Usually, and, as we may say, normally, the slope of the axis of the pattern is (roughly) parallel to a line drawn from a tip of the forefinger to the base of the little finger. All normal slopes, as well as all the patterns that have no definite axis, are expressed by the odd numerals 1, 3, or 5. All abnormal slopes are expressed by the even numerals 2, 4, or 6. It cannot be too strongly insisted that the words right and left are ambiguous and should not be used here.

Fig. 1.



The forefingers are the most variable of all the digits in respect to their patterns, their slopes being almost as frequently abnormal as not (see Table II); the third fingers rank next; the little finger ranks last, as its pattern is a loop in nine cases out of ten. I, therefore, found it convenient not to index the fingers in their natural order, but in the way that is shown at the head of the column of figures on the left side of fig. 2. There, the sequence of the numerals that express the

Fig. 2.

patterns on the digits, is divided into two groups of three numerals and two groups of two numerals, as 355, 455, 55, 35. The first group 355 refers to the first, second, and third fingers of the left hand; the second group 455 to the first, second, and third fingers of the right hand; the third group 55 to the thumb and fourth finger of the left hand; the fourth group 35 to the thumb and fourth finger of the right hand. The index is arranged in the numerical sequence of these sets of numbers as shown in fig. 2 and in Table I.

Before translating the patterns into numerals, I find it an excellent plan to draw symbolic pictures of the several patterns in the order in which they appear in the impression, or in the fingers themselves, as the case may be, confining myself to the limited number of symbols shown in fig. 1, which have fairly well sufficed for my 289 sets or 2890 finger marks, as well as for many others. A little violence has of course to be used now and then, in fitting some unusual pattern to one of these symbols. But we are familiar with such processes in ordinary spelling, where the same letter does duty for different sounds, as *a* in the words *as*, *ask*, *ale*, and *all*. The merits of this process are many. It facilitates a leisurely revision of first determinations; it affords a pictorial record of the character of each pattern; it prevents mistakes between normal and abnormal slopes; it prevents confusion when changing the sequence of the entries from the order of the impressions to that used in the index; and, lastly, it affords considerable help to a yet further subdivision of the patterns. This may be inferred from the first two lines of fig. 2, which have the same index numbers, but whose pictured forms differ in respect to the two thumbs, and to the middle finger of the left hand.

I will now describe the symbols in detail, and show how such small difficulty as arises from rare transitional or border cases is minimised.

The primaries in their earliest and purest form are sufficiently expressed by the symbol *a*, fig. 1. From this elementary type all other sorts of patterns seem to be lineally descended. A fairly pure form of this type is seen in *b*; this is not infrequent in fingers, but I have not once met with it among some thousands of thumbs. A nascent whorl, still so immature as to count as a primary, is symbolised by *c*; similarly nascent loops, that should undoubtedly be counted as primaries, by *d* and *e*. When, however, the loop form is more pronounced and the pattern has been accepted as a primary only after reasonable hesitation as to whether it was not a loop, a dot is put inside the symbol, as in *f* and *g*, to serve as a warning. In this case, supposing another person to reckon the doubtful finger-mark as a loop and to refer and fail to find it under that head, he would make a second reference by treating it as a primary. A dot always means a possibly transitional case; thus *r* and *s* signify that they had been accepted as loops after some hesitation.

The whorls include circles, ellipses, and spirals, both simple and compound, whatever may be the direction or closeness of their twist. These are so apt to be confounded together unless the impression is from a *rolled* finger and is afterwards scrutinised and outlined (as explained in my previous memoir) that it seems best for the present purpose to group them all, with few exceptions, under the one symbol *h*. The exceptions are these. When two streams of ridges proceed from opposite sides of the finger and interlock, the symbol *i* is used, regardless of all other details. Again, when the whorl is crozier shaped, as in *j* and *k*, it is necessarily enclosed in a loop, but the loop is here ignored. If the crozier approaches very nearly and mistakably to either of the plain eyes *t*, *u*, it is dotted for a warning, as in *l* and *m*.

The loops in their simplest and common forms are shown by *n* and *o*. Frequently they have an internal offset which may be variously feathered or bent, short of being a whorl; all such cases are expressed by *p*, *q*. They have sometimes a conspicuous eye due to an internal curvature of the ridges upon themselves, or even to an eye in the central ridge; these are all expressed by *t* or *u*, in which the surrounding loop is left out in order to avoid multiplicity of lines. When the eye approaches nearly to a crozier as in *l*, *m*, the dotted symbols *r*, *w* are used.

In making a large and complete index, the symbols would, of course, be cast as movable types, and be printed with the letterpress. It will be seen from fig. 2 that there is space for 20 entries in one 8vo page.

I do not expect from my own reiterated experiences that there would be much trouble due to transitional cases, after a standard collection of doubtful forms had been establised so as to ensure that different persons should abide by a common rule. I find much uniformity in my own judgment.

I give an index of 100 cases; they are the first that occurred in my catalogue of impressions, which are pasted in two rows on each page, and are consequently numbered 1, 1′; 2, 2′, in order; but there are a few blanks, so the numbers in the index happen to run from 1 to 56′, with some omissions, and not from 1 to 50′.

These cases afford data for roughly measuring the increase in power of discrimination obtained by basing indexes on the patterns of 1, 2, 3, 6, and 10 digits respectively. It appears from Table III that when all 10 digits are used, the number of different patterns observed in the 100 cases was 83; therefore the average number of references required to pick out a single well-defined case from among these 100 would be equal to 100 divided by 83, that is, to about $1\frac{1}{4}$.

It will also be seen from Table III that, owing to the large effect of correlation, an index based on all the ten digits is not much superior

Table I.—Numerical

Three first fingers.		Thumb and fourth finger.		Book I.	Three first fingers.		Thumb and fourth finger.		Book I.
Left, 1, 2, 3.	Right, 1, 2, 3.	Left th., 4.	Right, th., 4.		Left, 1, 2, 3.	Right, 1, 2, 3.	Left, th., 4.	Right, th., 4.	
				page					page
111	111	15	15	52	215	115	55	55	48
”	”	”	”	20	”	255	55	55	20′
”	”	51	11	32	253	155	55	55	7′
”	”	55	35	37	255	655	35	35	51
”	151	51	51	46	333	155	55	35	14
115	113	55	55	39	”	333	35	33	2
”	115	15	15	55	”	”	55	33	31′
”	”	55	55	4	”	”	55	35	2′
”	155	15	55	34′	”	”	55	55	36
”	”	55	55	25′	”	353	33	33	45
151	151	54	51	33′	”	”	”	”	18
154	115	55	55	47	”	”	35	35	5′
155	113	55	55	12	”	”	53	33	53
”	115	55	55	20a	”	”	55	33	4′
”	116	35	53	1	”	433	33	33	14′
”	155	55	35	6	”	555	35	55	55′
”	”	55	55	35′	”	633	35	35	29
”	”	”	”	45′	”	”	”	”	13′
”	553	55	55	35	335	333	53	55	18′
”	555	35	35	23	”	653	55	55	30′
”	”	55	35	50′	353	333	35	35	38′
”	”	”	”	10	”	”	”	”	19′
”	”	55	35	54	”	353	15	55	6′
”	633	35	35	56′	”	653	35	35	17
”	655	55	35	44′	355	353	55	35	16
156	553	35	35	7	”	435	55	35	49

Index of 100 cases.

Three first fingers.		Thumb and fourth finger.		Book I.
Left, 1, 2, 3.	Right, 1, 2, 3.	Left, th., 4.	Right, th., 4.	
365	355	55	55	page 3′
415	555	35	55	21*a*
433	433	35	35	10′
453	355	55	55	32′
455	355	55	55	11
,,	,,	,,	,,	56
,,	455	35	35	41′
515	153	55	55	23′
,,	156	55	35	49′
553	153	15	15	37′
,,	333	55	35	13
,,	353	55	55	22′
,,	553	55	35	27′
,,	,,	,,	,,	16′
,,	,,	55	55	24
555	115	55	55	40
,,	151	55	35	27
,,	153	55	53	23
,,	253	35	35	26
,,	513	55	55	28
,,	553	55	55	39′
,,	555	55	55	15
,,	,,	,,	,,	41
,,	,,	,,	,,	17′

Three first fingers.		Thumb and fourth finger.		Book I.
Left, 1, 2, 3.	Right, 1, 2, 3.	Left, th., 4.	Right, th., 4.	
555	555	55	55	page 19
,,	,,	,,	,,	3
,,	,,	,,	,,	40′
565	155	55	35	22
633	655	35	35	5
635	653	55	55	29′
653	153	55	55	1′
,,	653	35	33	28′
655	155	55	35	36′
,,	,,	55	55	15′
,,	,,	,,	,,	12′
,,	335	55	55	21*a*′
,,	455	35	55	53′
,,	553	35	35	20*a*′
,,	555	35	65	47′
,,	,,	55	55	44
,,	,,	,,	,,	52′
,,	653	35	33	26′
,,	,,	35	55	21′
,,	655	55	35	25
,,	,,	55	55	51′
,,	,,	,,	,,	21
,,	,,	,,	,,	30
665	655	55	55	46′

f. 5r

in efficiency to one that is based on only six, namely, upon the first three fingers of both hands. In the 100 different sets there are 83 varieties of pattern in the one case and 65 in the other, which roughly accords with the relative efficiency of 5 to 4. When all the 289 cases are similarly treated, the relative efficiency comes out as 213 to 139, or roughly as 3 to 2. This is a little better but not much. It is, therefore, a fair question whether it is worth while to impress all the 10 digits. The chief advantage of doing so is to add to the volume of evidence, and to supply data which mutilation, or bad scars, or obliteration due to some exceptional cause might render of value. We also see from Table III that the three fingers of both hands are more than twice as efficient for the purposes of an index as those of one hand only; again, that three fingers are nearly twice as useful as two. I may mention that for my present inquiries into racial and hereditary patterns I am, for various reasons, dealing only with the three first fingers of the right hand, and slightly rolling the forefinger, so as to obtain a full impression of its pattern on the side of the thumb.

The greatest difficulty in constructing a uniformly efficient catalogue lies in the troublesome frequency of plain loops, so that even the method of picture writing fails to analyse satisfactorily the numerous 555, 555; 55, 55 cases. When searching through a large number of similarly indexed prints for a particular specimen, it is a very expeditious method to fix on any one well-marked characteristic of a minute kind, such as an island, or enclosure, or a couple of adjacent bifurcations, that may present itself in any one of the fingers, and in making the search to use a lens or lenses of low power, fixed at the end of an arm, and to confine the attention solely to looking for that one characteristic. The cards on which the finger marks have been made may then be passed successively under the lens with great rapidity. I fear that the method of counting ridges (as the number of ridges in the AH of my previous memoir) would be difficult to use by persons who were not experts. Anyhow, I have not yet been able to devise a plan for doing so that I can recommend.

f. 6r

Table II.—Analysis of the 100 Cases in Table I.

Forefinger of left hand.		
Pattern.	Distinguishing number of pattern.	Number of cases.
Primary, plain...................... " nascent loop, slope normal ...	1	26
" " " slope abnormal...	2	4
Whorl, plain .... " with tail, slope normal........	3	23
" " slope abnormal........	4	6
Loop, slope normal....................	5	21
" slope abnormal	6	20
Total cases............	..	100

Table III.—Further Analysis of the 100 Cases in Table I.

Number of times in which each pattern occurs.	Set of digits observed.							
	First 2 fingers of left hand.		First 3 fingers of left hand.		First 3 fingers of both hands.		All the digits of both hands.	
	Number of		Number of		Number of		Number of	
	Pat-terns.	Cases.	Pat-terns.	Cases.	Pat-terns.	Cases.	Pat-terns.	Cases.
1	5	5	13	13	49	49	71	71
2	4	8	5	10	6	12	10	20
3	—	—	1	3	4	12	1	3
4	1	4	1	4	4	16	—	—
5	—	—	2	10	1	5	—	—
6	1	6	1	6	1	6	1	6
—	—	—	—	—	—	—	—	—
10	1	10	—	—	—	—	—	—
11	—	—	—	—	—	—	—	—
12	—	—	1	12	—	—	—	—
13	—	—	1	13	—	—	—	—
14	—	—	1	14	—	—	—	—
15	—	—	1	15	—	—	—	—
16	2	32	—	—	—	—	—	—
17	1	17	—	—	—	—	—	—
18	1	18	—	—	—	—	—	—
Total cases	..	100	..	100	..	100	..	100
Number of different patterns	16	..	27	..	65	..	83	

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f. 1r

JOURNAL OF THE SOCIETY OF ARTS,

FRIDAY, NOVEMBER 28, 1890.

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ADVERTISEMENTS.

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Journal of the Society of Arts.

No. 1,984. Vol. XXXIX.

FRIDAY, NOVEMBER 28, 1890.

All communications for the Society should be addressed to the Secretary, John-street, Adelphi, London, W.C.

NOTICES.

CANTOR LECTURES.

Professor VIVIAN B. LEWES delivered the first lecture of his course on "Gaseous Illuminants," on Monday evening, 24th inst.

The lectures will be printed in the *Journal* during the Christmas recess.

UNION OF INSTITUTIONS.

The following Institution has been received into Union since the last announcement :—

Central Young Men's Christian Association, Exeter-hall, Strand, W.C.

Proceedings of the Society.

SECOND ORDINARY MEETING.

Wednesday, November 26, 1890; SIR WILLIAM S. SAVORY, Bart., F.R.S., in the chair.

The following candidates were proposed for election as members of the Society :—

Birkbeck, Henry, 34, Southampton-buildings, Chancery-lane, W.C.

Bostock, George Henry, Hatfield, Yorkshire.

Doubleday, William Bennett, 123, Tulse-hill, S.W.

Ebb-Smith, Joseph, Worcester-house, Walbrook, E.C.

Few, William Resbury, Oaklands, Southfields, Wandsworth, S.W.

Levey, George Collins, C.M.G., National Liberal Club, S.W.

Lucas, Charles Phipps, The Elms, Mottingham, Eltham, Kent.

Pope, Henry R., 34, New Bridge-street, E.C.

Roberts-Austen, Prof. William Chandler, C.B., F.R.S., Royal Mint, E.

Ruffer, Marc Armand, M.A., M.D., B.Sc., 27, Torrington-square, W.C.

Woollcombe, Robert Lloyd, LL.D., 14, Waterloo-road, Dublin.

The paper read was—

PHYSICAL TESTS IN COMPETITIVE EXAMINATIONS.

By FRANCIS GALTON, F.R.S.

In the autumn of last year, I brought the subject of the present paper before the Anthropological Section of the British Association. My views and proposals were favourably received, and, in the end, the Council of the Association were instructed to consider them at leisure, and if approved, to submit them to the authorities of the Army, the Navy, and the Indian Civil Service, and to the Civil Service Commissioners. This was done, and the replies were communicated by the Council to the Association at their recent meeting at Leeds.

It appears, from these replies, that the Civil Service Commissioners, moved thereto by the India-office, are now engaged in considering the practicability of the proposals. In the meantime the public are imperfectly informed of a matter that ought to interest many homes, whence candidates will shortly proceed to compete for Government appointments; while a few notes of alarm have been sounded by prominent newspapers, chiefly, as it seems to me, through misconception. I am glad, therefore, of the opportunity of making a fresh statement, with the emendations that have occurred to me, or have been suggested to others. I hope it may disarm some objectors, and more especially that it may encourage constructive criticism, which would be timely and, I presume, not unacceptable to the authorities, whose decision is still in the balance.

General Statement.—I will begin with a platitude that is also a truism, namely, that in selecting candidates to fill posts for which physical efficiency is desirable, it is proper that physical qualifications should be taken into account, supposing always that the existing system of study is not affected by doing so.

High physical powers are advantageous in certain active professions, or at least in some of their branches, and it is for these only that their recognition as subjects of examination is proposed. It is intended to be supplementary to the existing system and not to displace any portion of it. I neither praise nor dispraise that system, but leave it alone, including the medical pass examination, just as it is now, or as it may hereafter be modified; I am only concerned with faculties that are untouched by the present literary examinations, and which admit of being tested, as I shall show, without requiring special preparation on the part of the candidate that might distract his attention from his books or exhaust his energy and brain power.

Subject to these important reservations it is a mere truism to say that physical efficiency ought to be taken into account in selecting men to fill the particular posts above alluded to. My object to-night is to show that feasible and trustworthy tests exist, and to explain that if they be applied tentatively and on a small scale, with the avowed intention of reconsidering the whole matter after a few years experience, very considerable improvements of method are likely to follow.

Athletic Competition.—It will prevent one large class of objections from obtruding and distracting the attention if I first disclaim all intention of proposing athletic competition. A scheme in which that was proposed was brought forward in a report presented to the House of Lords on June 28, 1878, by a joint committee of the War-office and of the Civil Service Commissioners. It was discussed three times in the House, and objections were raised against it. It was said that the cost of preparation to the candidates would be considerable, and that the strain of athletic training would be more than the health of the already fully worked students could safely bear. So the recommendation of the committee was not adopted. The objections to athletic competitions appear to me both reasonable and forcible, and I have neither the wish nor the intention to advocate them. What is proposed is to test, not the most that a candidate could do after a severe course of training, but his natural capabilities.

Inspection.—The distinction I wish to draw is familiarly illustrated in the two methods of valuing horses. The athletic competition is that of the racecourse. The horses are trained for a long time at great cost till they reach the utmost efficiency that their natures permit, and then they are run against one another. The other method is in much more common use, as in buying horses at marts. The animals are looked over in their stalls, they are led out into the yard and put through their paces, and they are shrewdly valued after brief inspection. If the horse is young or out of sorts allowance is, of course, made for these temporary drawbacks to efficiency. The scheme of examination that I propose is of this latter kind, supplemented by simple physical tests, which serve as a backbone to the otherwise unchecked judgment of the examiner.

One of the proposals is, that the medical man who conducts the pass examination should in addition to his present duties, assign marks to each candidate according to his opinion of the physical efficiency of the candidate, after examining him. The practice is common of inspecting the candidates for adventurous services before making a final selection. It is certainly sometimes, and I believe always followed, in picking out the best men for such special work as arctic and other exploration. Indeed, it would be preposterous to neglect so obvious a precaution against gross mistakes.

I have often heard, or read, though I cannot now give good references, that when the practice of selling or buying slaves was practised by men of our race, with few qualms of conscience, the slaves were priced after a minute inspection. An experience of my own, of some forty-five years ago, while travelling in the Soudan, is to the point. An Egyptian, who possessed little besides a sword, had attached himself to the caravan with which I was travelling. He was on his way to join a slave-raiding expedition on the borders of Abyssinia, and he had, I found out, considerable experience in slave markets. I asked him many questions, from time to time, about the valuing of slaves, and, at last, begged him, as a favour, to price myself, just as if I was a light-coloured African; for I was curious to know my worth as an animal. He took evident pains, and I think was fairly honest, though with a bias towards flattery. Having regard to the then high state of the market, he estimated my worth on the spot, at a number of piastres that was about equal to £20.

I had the opportunity, a few months since, of seeing a collection of private memoranda that had been made by a gentleman who had occasion, from time to time, to select candidates for an important service. For reasons which I can appreciate, he begged me to give no clue whatever either to himself, to the nature of the service, not even as to nationality. I was amazed at the variety of his epithets (I will not say in what language), and at his skilful delineations of the characters of the candidates by a sentence or two, not drawn in coarse blacks and whites, but with delicate and humorous shade. I begged and prayed, in vain, to be allowed to take away with me a few anonymous extracts, and to publish them. If this could have been done, they might have given considerable impetus to the progress of the rare art of skilfully drawing characters. Here, then, I found a case in which selection was largely determined by inspection, not of physical qualities only, but of demeanour as well. Character, as a whole, does not concern us now, but a manly bearing and an air of general intelligence may do so to a certain degree, and these might well be taken into some account, as they are not touched by the literary examination.

Physical Tests.—I will next speak of the physical tests that are at present available, and afterwards of the way in which a system of marks may be founded upon them. So far as these particular tests are concerned, no latitude is left for uncertainty of judgment. They are definite measures made with standard instruments. The uncertainty is confined to the value of the deductions to be drawn from them. They are as follow:—

1. Stature.
2. A few other linear measures, sufficient to show whether the principal dimensions of the body are or are not well proportioned. These include the span of the arms, the height above the seat of the chair when sitting, and the length of neck.
3. Weight.
4. Strength of grasp.
5. Breathing capacity.
6. Quickness of response to a signal by sound.
7. Quickness of muscular action.
8. Simple tests of vision.
9. Ditto of hearing.

The length of time requisite to make a set of these measures is less than a quarter of an hour. I have caused many thousand sets to be taken that were more extensive than these, and which severally occupied little more than that time. Some were carried on at an anthropometric laboratory that I set up in the International Health Exhibition in 1884, and the rest at another that has been for two years in operation in the western galleries of the science collections at South Kensington. The working of the process is therefore thoroughly understood. So is its total cost, which including superintendence, registration, and bookwork, need not, under judicious management, exceed 6d. per head.

Instruments.—I exhibit a few of the instruments I am now using. They are mostly well known, but that for the measuring the quickness of response has been got into its present very convenient shape only about a year since,* and that for quickness of muscular action has been contrived still more lately.† It is almost needless to say that both of these qualities can be measured by more than one elaborate and troublesome method. For instance, by electrical action on a very light style, that lightly scratches the smoked surface of a revolving cylinder, whose rate of movement is recorded by another style attached to the end of a stiff spring, and which is maintained in vibration by an electrical impulse at the end of each excursion. This instrument is familiar to physiological students, and is most exact in its records; but it is far too troublesome for the purposes which we are now concerned. Hipp's chronograph is less troublesome, but it, too, is tedious. Something much readier to use and simpler to read off, is necessary if many persons are to be tested in rapid succession. The two instruments that I exhibit fulfil these requirements, and are at the same time more exact than is really needed.

The most difficult of the measures is that of the keenness of hearing. It is impossible, under the conditions of an examination, to obtain standard sounds, because the loudness of any note or noise is largely affected by the arrangements of the room in which the instrument may be set. The test to be employed must, I fear, be of a very ordinary kind, and of the same general character as the greatest distance at which a particular watch can be heard to tick, or a small electric bell to tingle.

I will not occupy time by explaining the process of performing the physical tests. They have been often described, and can be seen in operation at my laboratory by any one who chooses to go there, for it has hitherto been and still is freely open to the public. It is entered from the new Imperial Institute road, near to its end in Queen's Gate.

Marks.—The question is, what to do with

* For first account see "Journ. Anthrop. Inst.," xix. 1, p. 28; for a second account, "Report Brit. Assoc. for 1889," p. 784.

† "Journ. Anthrop. Inst.," xx. 2, p. 200

the measures after we have got them? How are they to be utilised as a basis for assigning a just allotment of marks? The real difficulty lies in these details, and not in accepting the general principle of the proposed examination. Later on I shall describe the safest guidance for drawing up a scheme of marks, but it is hardly available now. It would be attained after trying that tentative and provisional system which is asked for. There are three groups of faculties that must be dealt with differently.

First Group.—The more highly a man is gifted with the five following faculties, the better it would be for him in such posts as we are considering:—Absolute strength, quickness of response, swiftness of muscular action, keenness of vision, keenness of hearing. These faculties are also fairly independent of one another. It follows that we might with propriety mark the candidates according to their measured achievement. Again, it is a rough-and-ready practice to arbitrarily fix two limits in each case. Those who fall beneath the lowest limit are to obtain no marks at all. Those who exceed the highest are to obtain a certain maximum of them; for simplicity of illustration let us say ten. Then an achievement halfway between the limits counts as five, and others proportionately. This is by no means an ideally exact way, but it is good enough for our present purpose. In arbitrarily fixing the limits, we must be guided by some reasonable idea, and the one I should suggest is to take, either exactly or approximately, the lines that respectively cut off the lowest 5 per cent. and the highest 5 per cent. of men of the same age and social status as the candidates. I possess plenty of such statistical measures as these, which are very nearly, if not exactly, applicable to the candidates in view. It will be sufficient to give, merely as an example, a few figures from a table of values that I compiled from the measures taken at the International Health Exhibition of youths between the ages of 23 and 26. The upper and lower limits for strength of grasp are here 56 and 96 lbs. Five per cent. of the whole number stood below the first limit, and 5 per cent. above the second; but, according to the principal laid down, we arbitrarily refuse either to give negative marks for the very weak, or more than a certain maximum of marks to any man, however strong.

To reduce all this to as simple form as possible, let us suppose a maximum of ten marks; then we might change the limits a little, for the sake of obtaining round numbers, and allot no mark for a strength under 55 lbs., and one mark for each additional 5 lbs. above that limit, up to 105 lbs., as a maximum. If the maximum marks were to be other than ten, the rule would be modified accordingly, but the general principle would be the same. There is no difficulty of importance in this method of devising a system of marks. Neither would there be any difficulty in its practical application. The marks might be engraved on the scales of the various instruments, together with the correction to be applied for age in the case of absolute strength and of swiftness.

The maximum number to be allotted to each of the five faculties* I have mentioned would have to be arbitrarily determined, according to some common sense view of the whole case.

Second Group.—A second class of qualities has to be estimated relatively to one of the others, not independently or separately, like those just discussed. At least three faculties fall within this group, namely, strength and swiftness, considered from a fresh point of view, and breathing capacity. Here the examiner would probably work with printed tables, in each of which the measures of one faculty would be arranged along the top at the heads of successive columns, and those of the related faculty down the side at the beginnings of successive lines. The marks and the age correction would be read off in the square where the appropriate column and line crossed each other.

In the sense in which strength is now to be considered, a racer is stronger than a cart-horse. Though he cannot perform the same amount of work measured in foot-pounds, he is able to transport himself, and move his limbs the more easily of the two. His strength, relatively to his weight, is greater than that of the cart-horse. It is easy to express the value of this fraction. Strength is supposed always to be measured in the same units: say, in the number of pounds weight that the grasp can resist. Weight is also supposed to be always measured in the same units: say, in pounds

* I may here mention that the literature connected with "Reaction Time" is voluminous. It has been much experimented on, because it affords a powerful aid to psychological research. A remarkably clear and able compendium of what has been achieved by its measurement has just been published in a little book by Professor Jastrow, called "Time Relations of Mental Phenomena." (Hodges: New York, 1890.) A very useful bibliography of works of reference is contained in it.

also. Then the fraction we want has the units of strength for the numerator, and units of weight for the denominator, which is easily turned into an ordinary decimal. Then, just as in the first method, we find two limits of value. Those whose record falls beneath the lower limit receive no marks at all; those who rise above the upper, receive the maximum.

Swiftness should also be treated relatively to weight, though not as a fraction but as a a product. The units of swiftness have to be multiplied into the units of strength, to measure the momentum of a blow or of a rush.

Lung capacity has to be treated on parallel lines to strength. The human body is a locomotive worked by a chemical engine. It is stoked with food, and pumps in air to burn waste products; then it pumps out the air after it has been vitiated by the burnt products, doing this in alternate rythm. A respiratory apparatus, that is amply large enough for a small human body, may be altogether inadequate for a larger one. Therefore it is the lung capacity relatively to the size of the body that concerns us, and not the lung capacity in an absolute sense. We have to regard the fraction in which the numerator is the number, say, of cubic inches of lung capacity, and the denominator is the number, say, of pounds in the weight, or else some function of the number of inches in the stature. The marks can then be assigned as before.

Third Group.—As regards symmetry, I have little of my own to say that is worth saying. The normal proportions of the body are pretty well known in the different races, and it is presumable that a wide departure from them in any direction is prejudicial. We have here an instance of a third class of qualities, where the broad belt of average values would receive full marks, and deviations outside of it on either hand, would receive fewer, until at the limits roughly determined as before on the 5 per cent. principle, they became *nil*. There is at least one simple method of rapidly finding out the marks to be assigned in the cases that fall within this group, but I will not stop to describe it, as I cannot do so very briefly.

Stature is another instance of the same sort. It is largely dependent on race, but in each race there is a normal value. Moderate departures from the normal are not important, but wide ones are, whether in excess or deficiency. Some information about this is to be found in Gould's "Statistics of the American War," published by the Medical Department of the United States.

It will now, I hope, be understood that there is no important difficulty of principle in assigning fair marks to the results of physical tests, and that if the principle is agreed upon in the sense just explained, its practical application is simple.

These physical tests would afford guidance to the examiner in assigning the marks he has to give according to the result of his inspection. If his judgment appears to be contradicted by the tests, he would reconsider it; if corroborated by them, he would feel the more assurance in his view. The tests would afford a valuable safeguard to the correctness of the final opinion of the examiner.

All that have thus far been proposed could be carried into effect at once. I shall afterwards discuss the directions in which the experience of a few years might be expected to suggest improvements. In the meantime it will be well to answer objections that have been brought against the proposal generally.

First Objection.—The two objections that have been most frequently urged are merely blows in the air, struck wide of the scheme, as it will be easy to show. The first is that certain great commanders and strategists had a poor physique, and would have been excluded by the proposed tests from the profession in which they afterwards distinguished themselves; that Nelson, for example, would have been excluded. The reply is that the proposed plan does not peremptorily exclude anybody on the ground of poor physique. There exists already a very salutary medical pass examination that excludes youths who appear to be distinctly unfitted for active service; but the new and additional proposal merely asks that a candidate who is below par in bodily powers should be above par in mental powers as a counterpoise. There is a rather broad belt of barely distinguishable degrees of mediocrity of mind, through or near to which the line runs that divides success from failure in the ordinary competitive examinations. But among the candidates who fall within this belt we may expect to find just as great a variety of bodily powers as among any other group of candidates. It is upon these only that the proposal to give moderate marks for physical efficiency would have any effect of importance. It would raise some men of mediocre intellect, but of powerful frames, into the pleasant table-land of success, and it would depress an equal number of men of almost equal mediocrity of intellect but with puny bodies as well, into the gulf of failure. The State would gain by the exchange. As for the candidates who had a fair place in the literary class list, the total want of extra marks for bodily efficiency would be insufficient to exclude them. It would only make them lose a few places.

The discovery of Dr. Venn* is most important to us here. He found that the three classes of high honour men, poll men, and of those who were intermediate between the two, have on the whole almost exactly the same average physical faculties, and that the degree of individual variation amongst them is the same in both classes. He discussed with thoroughness and ingenuity two considerable batches of measurements that were made at Cambridge with some instruments I had set up there, both of which told the same tale independently. That men who are mediocre in intellect are not also mediocre in physique, but are just as variable as any others, has therefore been shown to be a trustworthy fact, and the absurdity of taking no account of their variability becomes conspicuous.

Second Objection.—The second objection is that these tests take no account at all of the important quality of energy, which includes pluck, strong will, and endurance. I fully grant it, but half a loaf is better than no bread. The proposed tests tell us something useful that was disregarded before. They do not profess to cover all the *lacunæ* left by a literary examination. There remain an abundance of important points of character, moral and other, that are wholly undealt with. We must be content with what we can get. It is not impossible that practicable tests of energy in some of its forms may yet be discovered. It must be associated with physiological signs that we have not yet had the wit to discover. The power of enduring fatigue evades measurement, owing to its being largely affected by practice and athletic training; and there occurs, as yet, no way of estimating these disturbing causes. Put one of two similar youths into high training, and he will climb mountains, run races, and so forth, without any disturbance to his health, which the other could not attempt without serious risk. It is greatly to be regretted that we can devise no test for natural energy. It is a characteristic of great men to have an unusually large share of it. Sometimes it shows itself in mental work, sometimes in bodily work; often in both combined. The work got through by great commanders, without injuring their health, is enormous. Brief hours of sleep, harrassing anxieties, multifarious objects of attention, climatic changes, are borne for months. Now and then occur the arduous preparations for a great battle, the fighting it, and afterwards a minute and lucid despatch has been written, perhaps in the late evening of the same day.

Third Objection.—Another objection concerns the untrustworthiness of the results of examination. There is no reason to suppose that physical tests would be more untrustworthy than literary ones; indeed, such experiments as have been made point the other way. I do not champion the system of examination, but as we have it, and as no one can devise a better way of selecting candidates, we ought to increase its value by making it less one-sided. Those who desire to have a definite notion of the variability of judgment in literary matters among examiners, should study the careful and ingenious memoir by Prof. F. Y. Edgeworth in the current number of the *Statistical Journal*.

Need of further data.—Before it is possible to devise as good a system of physical tests as we may reasonably hope at some time to obtain, we require observations in sufficient number and in a sufficiently exact form.

Worth of Physical Tests.—We have, for example, to compare the rank in physical efficiency that is assigned to youths by tests and inspection with the rank that they hold according to the consensus of their fellows and competitors in athletic sports. We should thereby ascertain more exactly than we can do now the relative importance that ought to be assigned to the various tests.

Promise in Youth.—Again, we have to compare the physique and promises of youth with their achievements in after-life. Now, we obviously cannot examine into this relation unless records are preserved and are easily accessible of the physical powers of youths. We require statistical data on a large scale before it is possible to deduce trustworthy conclusions; and it seems impossible to establish the needed system of records, except under the stimulus of such examinations as I have in view. If the present conditions continue unchanged, inquiries into the important question I have

* See *Nature*, 1888-90.

indicated will linger on indefinitely. It will remain in the hands of a few amateurs, whose energies are largely absorbed by the almost hopeless task of searching for materials. Apply the stimulus of examination, and not only will the required data begin to accumulate, but many intelligent schoolmasters and others will feel it a matter of business to discuss them. There ought to be no difficulty in finding out the physical antecedents of every man when he was a youth. It is easily to be done in respect to his class place in literary examinations, and the value of these in forecasting his future can be discussed; but there are absolutely no records worthy of trust, on a sufficient scale, in respect to his physical powers.

Vigour of Eminent Anglo-Indians.—I may here mention an observation that impressed me a good deal at the time. On the eve of the departure of the late Sir Bartle Frere on a high political mission to Africa, his numerous friends entertained him at a farewell banquet. I was present, and seized the opportunity of estimating, as dispassionately and as carefully as I could, the average physical appearance of the eminent Anglo-Indians, who were the preponderating element in the party. I was the more moved to do so because the first feeling was one of surprise at the difference between what they were and what I had expected. I was prepared to see a collection of bilious and worn-out men. Not a bit of it; they were remarkably hale and vigorous. They were above the average stature and breadth—there was an air of force and power about them. They were as fine a collection of human animals as I have ever seen. I am sure that such examiners as those I have in view, would have given high marks to most of them for physical efficiency.

Tolerance of Tropical Climates.—Our ignorance is crass in respect of not a few elementary questions of national importance, which cannot be answered until the habit of preserving physical records of youths shall have become more common than it is. One of these regards the indications that a youth will or will not be able to keep his health in a hot and feverish climate. No inquiry has ever been made into these with the thoroughness and precision that the question obviously deserves. There is great variability among men and animals of the same race in their power of enduring malaria and changes of climate. It has happened that I have been more than a third of a century closely associated with the doings of the Royal Geographical Society, and during that time I have often had occasion to remark the ease with which the constitutions of successful explorers have thrown off the effects of fever. They may have been attacked frequently and severely but the malady rarely takes so strong a hold, as to leave them permanent invalids. Still less does it kill them, as it speedily kills many others who were, to all appearances, equally vigorous and energetic. A careful investigation of numerous cases would probably show that physiological signs might be discerned during youth, either of a natural immunity from malarious fever, or of a disposition towards it. It is cruel and costly to tempt youths to the tropics who are less constitutionally capable than others of thriving there. If we could distinguish those who are fittest for life in hot countries, we should select them even though in other respects they may be somewhat wanting. The tropical possessions of England are become so large that it is a matter of national importance to investigate this question thoroughly. It may yet be possible to find varieties of our race who are capable of permanently establishing their families in those climates.

It would be easy to enlarge on the topic of our astounding and contented ignorance of very elementary questions in respect to the choice of the men who are most likely to succeed in special services, but enough has been said to show the need of stimulating a demand for the collection of trustworthy records. No stimulus would approach in efficiency to that of establishing physical tests in connection with certain of the competitive examinations. If this were done provisionally, as proposed, and on a small scale, many persons who are connected with education, or with the public services, would interest themselves warmly in the subject. It would become a matter of present importance to themselves, and what is now only an academic question would be raised to the rank of a practical one.

I trust now that the two points have been established that it has been the object of these remarks to prove. First, that it is possible at once to devise a system of physical tests and inspection that shall be of real utility. Secondly, that in addition to its present value it would afford a necessary first step towards a more satisfactory scheme.

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DISCUSSION.

The CHAIRMAN said that this paper would afford ample scope for discussion, as the subject was one on which wide difference of opinion might exist, not, indeed, on the principle itself, which was now conceded, for in many examinations physical tests were to a certain extent already used. The more difficult question to answer was the relative value to be attached to these tests, not only in comparison with literary work, but the relative value of the different tests themselves. To express it broadly, some might attach more importance to tests for mere strength, and some to those for physical health. He noticed that the author did not give so much prominence to personal and family history as some might be disposed to do. He took it Mr. Galton's motto would be *mens sana in corpore sano*, but there was a very broad and deep distinction between health and strength.

Admiral COLOMB said the subject of physical tests in competitive examinations was familiar now to every one in the army and navy, because it had been so much talked about, but until to-night he had only heard of it in connection with athletic competitions, which Mr. Galton proposed to discard altogether. So far as he understood the proposal, it would bring forward some useful men whose mental capacity was possibly a little below par, but who made up for the deficiency by their physical qualifications. Whether the Government would ever tentatively adopt such a system was a question, but one point struck him which might have some weight. In the old days a large number of officers were entered for both the army and the navy, and were then left to sink or swim as they could, and there was a sort of natural selection out of an immense body, the best men being retained for the services, and the others disappearing. That system had been abandoned, and by means of artificial tests the field from which officers were drawn was very much reduced. It therefore seemed desirable to make the examinations as complete as the knowledge of the time allowed, and he certainly thought that the tests described would help in selecting the best men and rejecting the worst. He should have thought a test of energy and possible capacity might be obtained from the pulse, the rapidity of the breathing and the effect of starts, and especially from the condition of the digestion. It was an old saying that a man "had no stomach for a fight," and he thought there was more in that than a mere trope. A man's want of courage might come from not having a good digestion. In the excessive fear experienced in dreams, the sensation felt on waking was always about the diaphragm, and when startled by a sudden apparition he had felt the same sensation himself in that part; so that probably digestion had a good deal to do with what was called courage and energy. One physical test not referred to was the cause of more rejection in the navy than almost any other, and that was the teeth. Men whose teeth decayed when young were almost always rejected for the navy, because it was said to show a bad constitution and to prevent good digestion.

Dr. DELEPINE said he had been obliged for some time o study this question, for the purpose of providing information in certain quarters, and he had become acquainted, probably, with all the statistics and methods proposed for testing the physical powers; but the result was that very few data existed which could be applied to candidates of the age of those who came up for examination, and those which did exist were often not sufficiently accurate to allow of any system being based upon them. The only data at present were those collected by Dr. Roberts some few years ago, in which the questions of age, occupation, and the correlation of measurements had been compared; and even in those tables a great many things were wanting, which Mr. Galton had either provided, or, he hoped, would provide before long. The great difficulty seemed to him to be the correlation of the measurements of height, weight, muscular power, and respiratory capacity. If they were paired two by two, as Mr. Galton suggested, certain important connections were apt to be overlooked; for instance, if a man of a certain height weighed a little more than he should —if his muscular power and breathing capacity were not taken at the same time, the excess of weight would be deemed rather a deficiency, but if it were found that he had unusual mnscular power, it was evident that the excess of weight might be due to a larger amount of muscle than the normal, and therefore the excess of weight would be a good feature. In the same way a man who measured more round the chest than he should, might be either too stout or more muscular than usual, or he might have a larger development of the lungs than normal. By taking all these four factors together all the required data were obtained. It would be impossible to obtain a satisfactory system of marking unless four or five measures were taken at the same time, and due correlation of the results must always have a chief place. There were other points which were not correlated, and which could be estimated on their own account; a man with good sight was certainly better than one with bad sight, and the same might be said of bad hearing. In awarding the marks also it should be remembered that the mean men of their race were generally found more serviceable than either very tall or very short men. The mean man, therefore, should have the maximum marks. It should also be remembered that there were two means, and sometimes more than two, in many races, which might be influenced by the occupation of the person himself or his ancestors.

Dr. BLACK (late Surgeon-Major) said he had had considerable experience in examining recruits, both officers and privates, and he thoroughly appreciated the great and growing importance of this subject. The acquisition of this knowledge would eventually be of great service to the public, and would enable parents and tutors to judge of the character of the candidates they produced either for the public or private service. Mr. Galton disclaimed any kind of athletic exercises in examination, and reduced his observations to the powers of the candidates—mental, muscular, and nervous. Observations as to some physical features, stature, &c., had been made for years and years, and there were piles of books at the Admiralty and War-office containing the records of centuries; but new branches of inquiry were now opened up which were still capable of much greater development. The power of will, for instance, could be exemplified by the stroke or blow of the fist, if the instrument were properly adjusted. Another point was the power of the voice, which was of great importance both to the private seaman as well as the officer. The captain or the colonel had to make his voice heard on parade, and the boatswain also had often to produce his voice in the most decisive and peremptory manner. Mr. Galton had an instrument for testing the strength of the arm, but it was equally important to test the strength of the leg. Nothing could be of greater importance to a commanding officer than to know the power of his regiment to march; but that had not hitherto been at all investigated by any instrumental method; it was judged of simply by the eye and other means; but it could scarcely be judged of by the appearance of the men. It was well known that the British private was not good at marching, though he might have a good physical appearance and be a good boxer; the Frenchman would beat him in marching, and the reason of that should be ascertained, for it had not yet been investigated by physical observation. Again, the power of balancing was a good test of the perfection of the whole system. Recruits were tested by standing on one leg; that power of balancing showed the perfection of the nervous system, both in the spinal cord and in the brain. It was also important to ascertain whether a man were left-handed or not; both feet and both hands should be of equal power, and both legs also. In fact the suggestions which might be made were almost innumerable, and he only threw these out as perhaps worth consideration.

Sir DOUGLAS GALTON, K.C.B., F.R.S., thought one of the great advantages which would result from adopting this system of examination would be that it would direct public attention more to the necessity for combining some system of physical training with education. At present, in the elementary schools, we were only developing one half of the children; and it was most essential, if we were to produce a race which should at all compare with the pictures on the walls —the Greeks, who were the most highly civilised race the world had ever seen—that the physical, as well as the mental qualities of the children should be developed; and their mental powers could not be developed unless their physical powers were developed also. On this account, it was of the highest importance that the Government should recognise the great labours of Mr. Francis Galton; and he trusted that before long some practical measures would be taken to adopt this system of marks in public examinations.

Mr. ROGERS pointed out that a great many civilians were now sent out to India of very different races, and that Bengalees, Parsees, and people from the upper provinces competed with Englishmen, Irishmen, and Scotchmen, and therefore he thought that the idea of putting them through a competitive examination in athletics might be relegated to the future. But after they had been through a literary course no one should be allowed to go to India unless he was able to ride on horseback, and showed a certain amount of physical energy.

Mr. GALTON, in reply, said one point made by the Chairman was that he had not said anything about heredity. He could only say that it was with great difficulty he refrained, but he thought it better not to encumber his paper with that important subject. He had been struck with the suggestion for testing the voice, and thought it would be very easy to devise an instrument for the purpose. The teeth would fall rather under the medical examination.

The CHAIRMAN, in proposing a vote of thanks to Mr. Galton, said there could be no nobler or more useful subject for consideration than the relation of the mental, moral, and physical qualities of man, and their influence on each other.

The vote of thanks was carried unanimously, and the proceedings terminated.

Miscellaneous.

SILK CULTURE IN NINGPO.

There are three kinds of silkworms in the district of Ningpo—the common, the horned, and the striped —all living on mulberry leaves. Consul Fowler states that there is also a worm which lives exclusively on the leaves of the oak. This comes from New Chwang. This worm is much larger and apparently harder to rear than the ordinary one. It is quite green in colour. The moth of this worm is of a deep yellow, and measures four inches across when the wings are spread out. On each wing are two or three transparent spots called eyes. The natural life of an ordinary silkworm, that is to say, from the time it is hatched to the death of the moth, is said to be from forty-five to fifty days, about five of which are spent in spinning, ten in a chrysalis state inside the cocoon, and the rest of the time in a caterpillar state. When first hatched the worm is black and just visible, and when it is ready to spin it becomes about two inches long. While in a caterpillar state it has, what are called in Ningpo, four sleeps, about five days apart and twenty-four to thirty hours duration each. During each of these sleeps the worm moults. The silkworm spins on a whisk of straw from eighteen to twenty-four inches high. The whisk is made by taking a handful of straw and tying it a little above the middle and spreading the two ends until it is able to stand upright. The worm is put on this, and it crawls about until it finds a convenient place, when it begins spinning by hanging silk to the straw and then winding itself round, and in a very short time the worm is hidden from sight. In about four days the cocoon is completed. After this the reeling takes place; that is, it must be done within a week after the cocoons are completed, otherwise the chrysalis, becoming the moth, will eat its way through the cocoon, after which it is useless for reeling. The process of reeling appears to be a very simple one. All utensils used consist of an earthen furnace, over which is put an iron pan and a bamboo wheel three feet in diameter, worked with a crank. This wheel has no felly, and the exterior ends of the spokes are connected with thread or cotton twine. The spokes are set in twos into the nave or hub of the wheels in such a way as to form the letter V, and therefore instead of the felly, the cord connection passes from spoke to spoke, forming a regular zigzag. The cocoons are placed in the pan of water, under which a slow fire is kept; a stick is then used to stir the cocoons until the desired number of fibres (or ends of the silk) have adhered to it, then these are removed, and passed through the hole of a cast attached to the furnace, and then fixed on to the wheel. The wheel is now set in motion, and the reeling actually begins. It is said that an expert can reel off about a pound and a half of silk in a day.

THE FOREIGN TRADE OF BULGARIA.

The *Journal de la Chambre de Commerce de Constantinople* says that the returns of the Bulgarian foreign trade are showing a decided increase. Cereals, chiefly corn and barley, form the largest part of the exports of the country. A portion of these cereals is absorbed by other parts of Turkey, by Roumania, and Austria-Hungary, which apply them either to the requirements of local consumption or for their export trade. England, however, is the principal market for Bulgarian cereals. For the export of its products Bulgaria employs several routes. By the Danube route the trade with Austria-Hungary and Roumania is carried on; the trade with over-sea countries is carried on *viâ* Varna and Black Sea; hitherto the Principality of Bulgaria has been obliged to use the port of Dedeaghatch, on the Ægean Sea; for the future it will be independent of this by the construction of the Yamboli-Bourgas Railway, terminating on the Black Sea. Apart from the extensive plains of Sophia and Philippopolis, but situated to the south of the Balkan chain, there are the large plateaux situated to the north of these mountains, between them and the Danube, which are the granaries of Bulgaria. After cereals, the exports of Bulgaria almost entirely consist of articles of general consumption—cheese, butter, eggs, and cattle, which are sold to the neighbouring people. There are, however, some articles of which it makes, if not a monopoly, at least a special feature, these are raw skins, carpets, and essence of roses. Bulgaria exports annually about 80,000 goat skins, 15,000 kid skins, and 180,000 lamb skins. The carpets of Berkovitza, woven by the Bulgarians with wool spun and dyed by themselves, enjoy in the East a considerable and well-deserved reputation. Very much appreciated in Austria and Servia, they are not yet, it is said, sufficiently sought after abroad, as they are not yet thoroughly known. The industry of making essence of roses belongs exclusively to the districts situated to the South of Bulgaria. The production of wine in Bulgaria but little exceeds, on an average, a value of £96,000, which gives a value of about 7d. a gallon. Notwithstanding the low price, it has hitherto supplied nothing, or next to nothing to the export trade. Among the imports into Bulgaria, cottons supplied almost exclusively by England take the first place. Then come metals in bars, and manufactured in the form of machinery and other articles. Almost all these articles come from England; Belgium supplying also a small quantity. Austria sends to Bulgaria almost all the ready-made clothing, *articles de luxe*, and furniture which are sold there. Sugar also comes, for the greater part, from Austria. Petroleum is supplied by Roumania, Russia, and the United States. Germany sends different articles in ordinary use, principally cotton tissues and hardware. The importance of the Bulgarian foreign trade is far from attaining all the development of which it is capable, says the *Journal de la Chambre de Commerce de Constantinople*, but there is progress year by year, and the recent opening of the Yamboli-Bourgas railway, which secures to the country a new direct output in the Black Sea, will certainly result in still further accentuating this progress.

Notes on Books.

THE THRESHOLD OF SCIENCE. By C. R. Alder Wright, D.S., F.R.S. London: Charles Griffin & Co., 1890.

This work is founded on the late James Wylde's "Magic of Science," published thirty years ago, and is briefly described on the title page as "a variety of simple and amusing experiments illustrating some of the chief physical and chemical properties of surrounding objects, and the effects upon them of light and heat." The advance in the popularity of science during the past thirty years is even greater than the progress of science itself; and it will, therefore, readily be perceived that there is not much room for the contents of this original work. Practically, indeed, this book may be considered as a new one, though here and there—as, for instance, in the chapter on photography—a somewhat undue amount of the old material has been suffered to remain. The experiments relate, for the most part, to chemistry or to light and heat. Some of them are of course familiar, but many are novel; and all are described in such a way as to be intelligible and interesting to the young student. The book should serve a useful purpose—by attracting young readers to the study of science, and inducing them to pursue it seriously. At the same time, it may serve to encourage those who have already commenced such serious study, by showing them the amusing and interesting side of their work.

TO SOUTH AFRICA AND BACK. By John Finch. London: Ward, Lock & Co.

The author, in a five months' journey to South Africa, visited Cape Colony, Natal, the Orange Free State, and the Transvaal, and in the narrative of his journey he gives special attention to the diamond and gold fields, relating anecdotes of the illicit diamond buyers and their tricks.

PUBLIC LIBRARIES: A History of the Movement and a Manual for the Organization and Management of Rate-supported Libraries. By Thomas Greenwood. Third edition, entirely re-written. London: Simpkin, Marshall, Hamilton, Kent & Co.

The first edition of this book was published in 1886, and was noticed in the *Journal* in that year. Since then much has been done in respect to the increase in the number of free libraries and in the production of labour-saving apparatus for their successful management. The author has brought the information up to date, and much enlarged his book.

DIE DECORATIVE KUNST-STICKEREI. I. Ausnäh Arbeit. Von Frieda Lipperheide. Berlin: 1890. 4to and folio.

This is a very elaborate work on art needlework, consisting of letterpress and illustrative plates. The various materials and instruments used in the practice of embroidery are described and illustrated; further on the different stitches are explained and specimens given. The atlas contains coloured lithographs of Spanish appliqué work of the 17th century, one exhibiting an escutcheon of arms. Italian appliqué of the 17th and 18th centuries and French appliqué of the 18th century, also Spanish appliqué on a netted ground of the 17th and 18th centuries.

MUSTERBLATTER FUR KUNSTLERISCHE HANDARBEITEN. Herausgegeben von Frieda Lipperheide. II. Sammlung (13–24 Blatt). Berlin: 1890.

This work, on a similar subject to the above, and by the same author, contains coloured plates of Oriental, Servian, Bulgarian, and Spanish patterns.

General Notes.

KEW REPORTS.—An index has now been issued to the Kew Reports from 1862 to 1882, the object of the index being to render the information contained in the Reports respecting economic and other plants more easy of reference. More detailed notes than those contained in the annual reports are now given in the Kew Bulletin, of which three volumes are already published, and the fourth is in course of publication.

MEETINGS OF THE SOCIETY.

ORDINARY MEETINGS.

DECEMBER 3.—JAMES DREDGE, "The Chicago Exhibition, 1893." The ATTORNEY-GENERAL, M.P., Chairman of Council, will preside.

DECEMBER 10.—F. BAILEY, "Electric Lighting Progress in London." SIR FREDERICK BRAMWELL, Bart., D.C.L., F.R.S., will preside.

DECEMBER 17.—GEORGE DAVISON, "Impressionism in Photography."

FOREIGN AND COLONIAL SECTION.

The meetings of this Section will take place on the following Tuesday afternoons, at Half-past Four o'clock:—

January 20; February 17; March 17; April 21; May 5, 26.

INDIAN SECTION.

The meetings of this Section will take place on the following Thursday afternoons, at Half-past Four o'clock:—

January 22; February 26; March 12; April 9, 30; May 28.

APPLIED ART SECTION.

The meetings of this Section will take place on the following Tuesday evenings, at Eight o'clock:—

January 27; February 10; March 10, 24; April 14; May 12.

CANTOR LECTURES.

The following Courses of Cantor Lectures will be delivered on Monday evenings at Eight o'clock:—

Prof. VIVIAN B. LEWES, "Gaseous Illuminants." Five lectures.

LECTURE II.—DECEMBER 1.—The composition of coal gas—Analysis of gas—The illuminants present in coal gas—Effect of class of coal, methods of manufacture, and diluents present, on the illuminating power of coal gas—The methods employed to enrich coal gas.

A. J. HIPKINS, F.S.A., "The Construction and Capabilities of Musical Instruments." Three lectures.

January 26; February 2, 9.

GISBERT KAPP, "The Electric Transmission of Power." Three lectures.

February 16, 23; March 2.

Prof. R. MELDOLA, F.R.S., "Photographic Chemistry." Three lectures.

March 9, 16, 23.

HUGH STANNUS, F.R.I.B.A., "The Decorative Treatment of Natural Foliage." Four lectures.

April 13, 20, 27; May 4.

JUVENILE LECTURES.

Two Lectures, suitable for a juvenile audience, will be delivered by E. B. POULTON, M.A., on "Mimicry in Animals," on Wednesday evenings, December 31, 1890, and January 7, 1891, at Seven o'clock.

MEETINGS FOR THE ENSUING WEEK.

MONDAY, DEC. 1... SOCIETY OF ARTS, John-street, Adelphi, W.C., 8 p.m. (Cantor Lectures.) Prof. Vivian B. Lewes, "Gaseous Illuminants." (Lecture II.)

Royal Institution, Albemarle-street, W. 5 p.m. General Monthly Meeting.

Engineers, Westminster Town-hall, S.W., 7½ p.m. Mr. J. J. F. Andrews, "Ship Caissons for Dock Basins and Dry Docks."

Microscopical, 20, Hanover-square, W., 8 p.m. Conversazione.

Chemical Industry (London Section), Burlington-house, W., 8 p.m. Mr. W. Webster, "The Electrical Treatment of Sewage."

Royal, Burlington-house, W., 8 p.m. Anniversary.

British Architects, 9, Conduit-street, W., 8 p.m. Mr. R. Phené Spiers, "Sassanian Architecture."

Medical, 11, Chandos-street, W., 8½ p.m.

Victoria Institute, 1A, Adelphi-terrace, W.C., 8 p.m. Prof. Hull, "Geological History of Egypt."

London Institution, Finsbury-circus, E.C., 5 p.m. Prof. W. H. Corfield, "The Houses we live in."

TUESDAY, DEC. 2...Civil Engineers, 25, Great George-street, S.W., 8 p.m.

Pathological, 20, Hanover-square, W., 8½ p.m.

Biblical Archæology, 9, Conduit-street, W., 8 p.m.

Zoological, 3, Hanover-square, W., 8½ p.m. 1. Mr. Richard Crawshay, "The Antelopes of Nyassa Land." 2. Prof. G. B. Howes (i.), "The Suspension of the Viscera in the Batoid *Hypnos subnigrum*;" (ii.) "Notes on the Pectoral Fin-skeleton of the Batoidea and of the Extinct Genus *Chlamydoselache*." 3. Mr. G. A. Boulenger, "The presence of Pterygoid Teeth in a Tailless Batrachian (*Pelobates cultripes*), with remarks on the localisation of Teeth on the Palate in Batrachians and Reptiles."

WEDNESDAY, DEC. 3...SOCIETY OF ARTS, John-street, Adelphi, W.C., 8 p.m. Mr. James Dredge, "The Chicago Exhibition, 1893."

Entomological, 11, Chandos-street, W., 7 p.m. 1. Mr. George T. Baker, "Notes on the Lepidoptera collected in Madeira by the late T. Vernon Wollaston." 2. Mr. Frederic Merrifield, "The conspicuous changes in the markings and colouring of Lepidoptera, caused by subjecting the pupæ to different temperature conditions." 3. Mr Hamilton H. Druce, "A Monograph of the Lycænoid genus *Hypochrysops*, with descriptions of new Species."

Archæological Association, 32, Sackville-street, W., 8 p.m.

Obstetrical, 20, Hanover-square, W., 8 p.m.

THURSDAY, DEC. 4... Antiquaries, Burlington-house, W., 8½ p.m.

Linnean, Burlington-house, W., 8 p.m. 1. Mr. H. N. Ridley, "The Genus of Orchid *Brownheadia*." 2. Mr. J. H. Lace, "The Botany of Kandahar." 3, 4. Mr. Thos. Kirk, "Botanical Visit to Auckland Isles."

Chemical, Burlington-house, 8 p.m. Dr. Branner, "The Volumetric Estimation of Tellurim."

London Institution, Finsbury-circus, E.C., 7 p.m. Mr. Armbruster, "Hector Berlioz" (illustrated).

Camera Club, Bedford-street, Strand, W.C., 8½ p.m. Mr. H. Sturmey, "Rollable Transparent Films."

Archæological Institution, Oxford-mansion, Oxford-street, W., 4 p.m.

FRIDAY, DEC. 5...National Association for the Promotion of Technical and Secondary Education (at the HOUSE OF THE SOCIETY OF ARTS), 5 p.m.

Teachers' Training and Registration Society (at the HOUSE OF THE SOCIETY OF ARTS) 8 p.m. Annual General Meeting.

Geologist's Association, University College, W.C., 8 p.m. 1. Messrs. C. Davies Sherborn and H. W. Burrows, "Report on the Microscopical Examination of some Samples of London Clay from the excavations for the widening of Cannon-street Railway Bridge, 1887." 2. Mr. Edwin Litchfield, "A short visit to Ingleton and to Filey Brigg (showing how a dangerous reef was converted into a perfect breakwater by an ancient race.)"

Philological, University College, W.C., 8 p.m.

Quekett Microscopical Club, 20, Hanover-square, W.C., 8 p.m.

SATURDAY, DEC. 6...Foremen Engineers and Draughtsmen, Cannon-street Hotel, E.C., 7½ p.m. Paper by Mr. W. Powrie.

F. 8r

Subject Matter Index of Advertisements.

*** *Where the number of the page is omitted the advertisement does not appear this week.*

JOURNAL OF THE SOCIETY OF ARTS,

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Journal of the Society of Arts.

No. 1,984. Vol. XXXIX.

FRIDAY, NOVEMBER 28, 1890.

All communications for the Society should be addressed to the Secretary, John-street, Adelphi, London, W.C.

NOTICES.

CANTOR LECTURES.

Professor VIVIAN B. LEWES delivered the first lecture of his course on "Gaseous Illuminants," on Monday evening, 24th inst.

The lectures will be printed in the *Journal* during the Christmas recess.

UNION OF INSTITUTIONS.

The following Institution has been received into Union since the last announcement:—

Central Young Men's Christian Association, Exeter-hall, Strand, W.C.

Proceedings of the Society.

SECOND ORDINARY MEETING.

Wednesday, November 26, 1890; SIR WILLIAM S. SAVORY, Bart., F.R.S., in the chair.

The following candidates were proposed for election as members of the Society:—

Birkbeck, Henry, 34, Southampton-buildings, Chancery-lane, W.C.

Bostock, George Henry, Hatfield, Yorkshire.

Doubleday, William Bennett, 123, Tulse-hill, S.W.

Ebb-Smith, Joseph, Worcester-house, Walbrook, E.C.

Few, William Resbury, Oaklands, Southfields, Wandsworth, S.W.

Levey, George Collins, C.M.G., National Liberal Club, S.W.

Lucas, Charles Phipps, The Elms, Mottingham, Eltham, Kent.

Pope, Henry R., 34, New Bridge-street, E.C.

Roberts-Austen, Prof. William Chandler, C.B., F.R.S., Royal Mint, E.

Ruffer, Marc Armand, M.A., M.D., B.Sc., 27, Torrington-square, W.C.

Woollcombe, Robert Lloyd, LL.D., 14, Waterloo-road, Dublin.

The paper read was—

PHYSICAL TESTS IN COMPETITIVE EXAMINATIONS.

By FRANCIS GALTON, F.R.S.

In the autumn of last year, I brought the subject of the present paper before the Anthropological Section of the British Association. My views and proposals were favourably received, and, in the end, the Council of the Association were instructed to consider them at leisure, and if approved, to submit them to the authorities of the Army, the Navy, and the Indian Civil Service, and to the Civil Service Commissioners. This was done, and the replies were communicated by the Council to the Association at their recent meeting at Leeds.

It appears, from these replies, that the Civil Service Commissioners, moved thereto by the India-office, are now engaged in considering the practicability of the proposals. In the meantime the public are imperfectly informed of a matter that ought to interest many homes, whence candidates will shortly proceed to compete for Government appointments; while a few notes of alarm have been sounded by prominent newspapers, chiefly, as it seems to me, through misconception. I am glad, therefore, of the opportunity of making a fresh statement, with the emendations that have occurred to me, or have been suggested to others. I hope it may disarm some objectors, and more especially that it may encourage constructive criticism, which would be timely and, I presume, not unacceptable to the authorities, whose decision is still in the balance.

General Statement.—I will begin with a platitude that is also a truism, namely, that in selecting candidates to fill posts for which physical efficiency is desirable, it is proper that physical qualifications should be taken into account, supposing always that the existing system of study is not affected by doing so.

High physical powers are advantageous in certain active professions, or at least in some of their branches, and it is for these only that their recognition as subjects of examination is proposed. It is intended to be supplementary to the existing system and not to displace any portion of it. I neither praise nor dispraise that system, but leave it alone, including the medical pass examination, just as it is now, or as it may hereafter be modified; I am only concerned with faculties that are untouched by the present literary examinations, and which admit of being tested, as I shall show, without requiring special preparation on the part of the candidate that might distract his attention from his books or exhaust his energy and brain power.

Subject to these important reservations it is a mere truism to say that physical efficiency ought to be taken into account in selecting men to fill the particular posts above alluded to. My object to-night is to show that feasible and trustworthy tests exist, and to explain that if they be applied tentatively and on a small scale, with the avowed intention of reconsidering the whole matter after a few years experience, very considerable improvements of method are likely to follow.

Athletic Competition.—It will prevent one large class of objections from obtruding and distracting the attention if I first disclaim all intention of proposing athletic competition. A scheme in which that was proposed was brought forward in a report presented to the House of Lords on June 28, 1878, by a joint committee of the War-office and of the Civil Service Commissioners. It was discussed three times in the House, and objections were raised against it. It was said that the cost of preparation to the candidates would be considerable, and that the strain of athletic training would be more than the health of the already fully worked students could safely bear. So the recommendation of the committee was not adopted. The objections to athletic competitions appear to me both reasonable and forcible, and I have neither the wish nor the intention to advocate them. What is proposed is to test, not the most that a candidate could do after a severe course of training, but his natural capabilities.

Inspection.—The distinction I wish to draw is familiarly illustrated in the two methods of valuing horses. The athletic competition is that of the racecourse. The horses are trained for a long time at great cost till they reach the utmost efficiency that their natures permit, and then they are run against one another. The other method is in much more common use, as in buying horses at marts. The animals are looked over in their stalls, they are led out into the yard and put through their paces, and they are shrewdly valued after brief inspection. If the horse is young or out of sorts allowance is, of course, made for these temporary drawbacks to efficiency. The scheme of examination that I propose is of this latter kind, supplemented by simple physical tests, which serve as a backbone to the otherwise unchecked judgment of the examiner.

One of the proposals is, that the medical man who conducts the pass examination should in addition to his present duties, assign marks to each candidate according to his opinion of the physical efficiency of the candidate, after examining him. The practice is common of inspecting the candidates for adventurous services before making a final selection. It is certainly sometimes, and I believe always followed, in picking out the best men for such special work as arctic and other exploration. Indeed, it would be preposterous to neglect so obvious a precaution against gross mistakes.

I have often heard, or read, though I cannot now give good references, that when the practice of selling or buying slaves was practised by men of our race, with few qualms of conscience, the slaves were priced after a minute inspection. An experience of my own, of some forty-five years ago, while travelling in the Soudan, is to the point. An Egyptian, who possessed little besides a sword, had attached himself to the caravan with which I was travelling. He was on his way to join a slave-raiding expedition on the borders of Abyssinia, and he had, I found out, considerable experience in slave markets. I asked him many questions, from time to time, about the valuing of slaves, and, at last, begged him, as a favour, to price myself, just as if I was a light-coloured African; for I was curious to know my worth as an animal. He took evident pains, and I think was fairly honest, though with a bias towards flattery. Having regard to the then high state of the market, he estimated my worth on the spot, at a number of piastres that was about equal to £20.

I had the opportunity, a few months since, of seeing a collection of private memoranda that had been made by a gentleman who had occasion, from time to time, to select candidates for an important service. For reasons which I can appreciate, he begged me to give no clue whatever either to himself, to the nature of the service, not even as to nationality. I was amazed at the variety of his epithets (I will not say in what language), and at his skilful delineations of the characters of the candidates by a sentence or two, not drawn in coarse blacks and whites, but with delicate and humorous shade. I begged and prayed, in vain, to be allowed to take away with me a few anonymous extracts, and to publish them. If this could have been done, they might have given considerable impetus to the progress of the rare art of skilfully drawing characters. Here, then, I found a case in which selection was largely determined by inspection, not of physical qualities only, but of demeanour as well. Character, as a whole, does not concern us now, but a manly bearing and an air of general intelligence may do so to a certain degree, and these might well be taken into some account, as they are not touched by the literary examination.

Physical Tests.—I will next speak of the physical tests that are at present available, and afterwards of the way in which a system of marks may be founded upon them. So far as these particular tests are concerned, no latitude is left for uncertainty of judgment. They are definite measures made with standard instruments. The uncertainty is confined to the value of the deductions to be drawn from them. They are as follow:—

1. Stature.
2. A few other linear measures, sufficient to show whether the principal dimensions of the body are or are not well proportioned. These include the span of the arms, the height above the seat of the chair when sitting, and the length of neck.
3. Weight.
4. Strength of grasp.
5. Breathing capacity.
6. Quickness of response to a signal by sound.
7. Quickness of muscular action.
8. Simple tests of vision.
9. Ditto of hearing.

The length of time requisite to make a set of these measures is less than a quarter of an hour. I have caused many thousand sets to be taken that were more extensive than these, and which severally occupied little more than that time. Some were carried on at an anthropometric laboratory that I set up in the International Health Exhibition in 1884, and the rest at another that has been for two years in operation in the western galleries of the science collections at South Kensington. The working of the process is therefore thoroughly understood. So is its total cost, which including superintendence, registration, and bookwork, need not, under judicious management, exceed 6d. per head.

Instruments.—I exhibit a few of the instruments I am now using. They are mostly well known, but that for the measuring the quickness of response has been got into its present very convenient shape only about a year since,* and that for quickness of muscular action has been contrived still more lately.† It is almost needless to say that both of these qualities can be measured by more than one elaborate and troublesome method. For instance, by electrical action on a very light style, that lightly scratches the smoked surface of a revolving cylinder, whose rate of movement is recorded by another style attached to the end of a stiff spring, and which is maintained in vibration by an electrical impulse at the end of each excursion. This instrument is familiar to physiological students, and is most exact in its records; but it is far too troublesome for the purposes which we are now concerned. Hipp's chronograph is less troublesome, but it, too, is tedious. Something much readier to use and simpler to read off, is necessary if many persons are to be tested in rapid succession. The two instruments that I exhibit fulfil these requirements, and are at the same time more exact than is really needed.

The most difficult of the measures is that of the keenness of hearing. It is impossible, under the conditions of an examination, to obtain standard sounds, because the loudness of any note or noise is largely affected by the arrangements of the room in which the instrument may be set. The test to be employed must, I fear, be of a very ordinary kind, and of the same general character as the greatest distance at which a particular watch can be heard to tick, or a small electric bell to tingle.

I will not occupy time by explaining the process of performing the physical tests. They have been often described, and can be seen in operation at my laboratory by any one who chooses to go there, for it has hitherto been and still is freely open to the public. It is entered from the new Imperial Institute road, near to its end in Queen's Gate.

Marks.—The question is, what to do with

* For first account see "Journ. Anthrop. Inst.," xix. 1, p. 28; for a second account, "Report Brit. Assoc. for 1889," p. 784.

† "Journ. Anthrop. Inst.," xx. 2, p. 200

the measures after we have got them? How are they to be utilised as a basis for assigning a just allotment of marks? The real difficulty lies in these details, and not in accepting the general principle of the proposed examination. Later on I shall describe the safest guidance for drawing up a scheme of marks, but it is hardly available now. It would be attained after trying that tentative and provisional system which is asked for. There are three groups of faculties that must be dealt with differently.

First Group.—The more highly a man is gifted with the five following faculties, the better it would be for him in such posts as we are considering:—Absolute strength, quickness of response, swiftness of muscular action, keenness of vision, keenness of hearing. These faculties are also fairly independent of one another. It follows that we might with propriety mark the candidates according to their measured achievement. Again, it is a rough-and-ready practice to arbitrarily fix two limits in each case. Those who fall beneath the lowest limit are to obtain no marks at all. Those who exceed the highest are to obtain a certain maximum of them; for simplicity of illustration let us say ten. Then an achievement halfway between the limits counts as five, and others proportionately. This is by no means an ideally exact way, but it is good enough for our present purpose. In arbitrarily fixing the limits, we must be guided by some reasonable idea, and the one I should suggest is to take, either exactly or approximately, the lines that respectively cut off the lowest 5 per cent. and the highest 5 per cent. of men of the same age and social status as the candidates. I possess plenty of such statistical measures as these, which are very nearly, if not exactly, applicable to the candidates in view. It will be sufficient to give, merely as an example, a few figures from a table of values that I compiled from the measures taken at the International Health Exhibition of youths between the ages of 23 and 26. The upper and lower limits for strength of grasp are here 56 and 96 lbs. Five per cent. of the whole number stood below the first limit, and 5 per cent. above the second; but, according to the principal laid down, we arbitrarily refuse either to give negative marks for the very weak, or more than a certain maximum of marks to any man, however strong.

To reduce all this to as simple form as possible, let us suppose a maximum of ten marks; then we might change the limits a little, for the sake of obtaining round numbers, and allot no mark for a strength under 55 lbs., and one mark for each additional 5 lbs. above that limit, up to 105 lbs., as a maximum. If the maximum marks were to be other than ten, the rule would be modified accordingly, but the general principle would be the same. There is no difficulty of importance in this method of devising a system of marks. Neither would there be any difficulty in its practical application. The marks might be engraved on the scales of the various instruments, together with the correction to be applied for age in the case of absolute strength and of swiftness.

The maximum number to be allotted to each of the five faculties* I have mentioned would have to be arbitrarily determined, according to some common sense view of the whole case.

Second Group.—A second class of qualities has to be estimated relatively to one of the others, not independently or separately, like those just discussed. At least three faculties fall within this group, namely, strength and swiftness, considered from a fresh point of view, and breathing capacity. Here the examiner would probably work with printed tables, in each of which the measures of one faculty would be arranged along the top at the heads of successive columns, and those of the related faculty down the side at the beginnings of successive lines. The marks and the age correction would be read off in the square where the appropriate column and line crossed each other.

In the sense in which strength is now to be considered, a racer is stronger than a cart-horse. Though he cannot perform the same amount of work measured in foot-pounds, he is able to transport himself, and move his limbs the more easily of the two. His strength, relatively to his weight, is greater than that of the cart-horse. It is easy to express the value of this fraction. Strength is supposed always to be measured in the same units: say, in the number of pounds weight that the grasp can resist. Weight is also supposed to be always measured in the same units: say, in pounds

* I may here mention that the literature connected with "Reaction Time" is voluminous. It has been much experimented on, because it affords a powerful aid to psychological research. A remarkably clear and able compendium of what has been achieved by its measurement has just been published in a little book by Professor Jastrow, called "Time Relations of Mental Phenomena." (Hodges: New York, 1890.) A very useful bibliography of works of reference is contained in it.

also. Then the fraction we want has the units of strength for the numerator, and units of weight for the denominator, which is easily turned into an ordinary decimal. Then, just as in the first method, we find two limits of value. Those whose record falls beneath the lower limit receive no marks at all; those who rise above the upper, receive the maximum.

Swiftness should also be treated relatively to weight, though not as a fraction but as a a product. The units of swiftness have to be multiplied into the units of strength, to measure the momentum of a blow or of a rush.

Lung capacity has to be treated on parallel lines to strength. The human body is a locomotive worked by a chemical engine. It is stoked with food, and pumps in air to burn waste products; then it pumps out the air after it has been vitiated by the burnt products, doing this in alternate rythm. A respiratory apparatus, that is amply large enough for a small human body, may be altogether inadequate for a larger one. Therefore it is the lung capacity relatively to the size of the body that concerns us, and not the lung capacity in an absolute sense. We have to regard the fraction in which the numerator is the number, say, of cubic inches of lung capacity, and the denominator is the number, say, of pounds in the weight, or else some function of the number of inches in the stature. The marks can then be assigned as before.

Third Group.—As regards symmetry, I have little of my own to say that is worth saying. The normal proportions of the body are pretty well known in the different races, and it is presumable that a wide departure from them in any direction is prejudicial. We have here an instance of a third class of qualities, where the broad belt of average values would receive full marks, and deviations outside of it on either hand, would receive fewer, until at the limits roughly determined as before on the 5 per cent. principle, they became *nil.* There is at least one simple method of rapidly finding out the marks to be assigned in the cases that fall within this group, but I will not stop to describe it, as I cannot do so very briefly.

Stature is another instance of the same sort. It is largely dependent on race, but in each race there is a normal value. Moderate departures from the normal are not important, but wide ones are, whether in excess or deficiency. Some information about this is to be found in Gould's "Statistics of the American War," published by the Medical Department of the United States.

It will now, I hope, be understood that there is no important difficulty of principle in assigning fair marks to the results of physical tests, and that if the principle is agreed upon in the sense just explained, its practical application is simple.

These physical tests would afford guidance to the examiner in assigning the marks he has to give according to the result of his inspection. If his judgment appears to be contradicted by the tests, he would reconsider it; if corroborated by them, he would feel the more assurance in his view. The tests would afford a valuable safeguard to the correctness of the final opinion of the examiner.

All that have thus far been proposed could be carried into effect at once. I shall afterwards discuss the directions in which the experience of a few years might be expected to suggest improvements. In the meantime it will be well to answer objections that have been brought against the proposal generally.

First Objection.—The two objections that have been most frequently urged are merely blows in the air, struck wide of the scheme, as it will be easy to show. The first is that certain great commanders and strategists had a poor physique, and would have been excluded by the proposed tests from the profession in which they afterwards distinguished themselves; that Nelson, for example, would have been excluded. The reply is that the proposed plan does not peremptorily exclude anybody on the ground of poor physique. There exists already a very salutary medical pass examination that excludes youths who appear to be distinctly unfitted for active service; but the new and additional proposal merely asks that a candidate who is below par in bodily powers should be above par in mental powers as a counterpoise. There is a rather broad belt of barely distinguishable degrees of mediocrity of mind, through or near to which the line runs that divides success from failure in the ordinary competitive examinations. But among the candidates who fall within this belt we may expect to find just as great a variety of bodily powers as among any other group of candidates. It is upon these only that the proposal to give moderate marks for physical efficiency would have any effect of importance. It would raise some men of mediocre intellect, but of powerful frames, into the pleasant table-land of success, and it would

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depress an equal number of men of almost equal mediocrity of intellect but with puny bodies as well, into the gulf of failure. The State would gain by the exchange. As for the candidates who had a fair place in the literary class list, the total want of extra marks for bodily efficiency would be insufficient to exclude them. It would only make them lose a few places.

The discovery of Dr. Venn* is most important to us here. He found that the three classes of high honour men, poll men, and of those who were intermediate between the two, have on the whole almost exactly the same average physical faculties, and that the degree of individual variation amongst them is the same in both classes. He discussed with thoroughness and ingenuity two considerable batches of measurements that were made at Cambridge with some instruments I had set up there, both of which told the same tale independently. That men who are mediocre in intellect are not also mediocre in physique, but are just as variable as any others, has therefore been shown to be a trustworthy fact, and the absurdity of taking no account of their variability becomes conspicuous.

Second Objection.—The second objection is that these tests take no account at all of the important quality of energy, which includes pluck, strong will, and endurance. I fully grant it, but half a loaf is better than no bread. The proposed tests tell us something useful that was disregarded before. They do not profess to cover all the *lacunæ* left by a literary examination. There remain an abundance of important points of character, moral and other, that are wholly undealt with. We must be content with what we can get. It is not impossible that practicable tests of energy in some of its forms may yet be discovered. It must be associated with physiological signs that we have not yet had the wit to discover. The power of enduring fatigue evades measurement, owing to its being largely affected by practice and athletic training; and there occurs, as yet, no way of estimating these disturbing causes. Put one of two similar youths into high training, and he will climb mountains, run races, and so forth, without any disturbance to his health, which the other could not attempt without serious risk. It is greatly to be regretted that we can devise no test for natural energy. It is a characteristic of great men to have an unusually large share of it. Sometimes it shows itself in mental work, sometimes in bodily work; often in both combined. The work got through by great commanders, without injuring their health, is enormous. Brief hours of sleep, harrassing anxieties, multifarious objects of attention, climatic changes, are borne for months. Now and then occur the arduous preparations for a great battle, the fighting it, and afterwards a minute and lucid despatch has been written, perhaps in the late evening of the same day.

Third Objection.—Another objection concerns the untrustworthiness of the results of examination. There is no reason to suppose that physical tests would be more untrustworthy than literary ones; indeed, such experiments as have been made point the other way. I do not champion the system of examination, but as we have it, and as no one can devise a better way of selecting candidates, we ought to increase its value by making it less one-sided. Those who desire to have a definite notion of the variability of judgment in literary matters among examiners, should study the careful and ingenious memoir by Prof. F. Y. Edgeworth in the current number of the *Statistical Journal*.

Need of further data.—Before it is possible to devise as good a system of physical tests as we may reasonably hope at some time to obtain, we require observations in sufficient number and in a sufficiently exact form.

Worth of Physical Tests.—We have, for example, to compare the rank in physical efficiency that is assigned to youths by tests and inspection with the rank that they hold according to the consensus of their fellows and competitors in athletic sports. We should thereby ascertain more exactly than we can do now the relative importance that ought to be assigned to the various tests.

Promise in Youth.—Again, we have to compare the physique and promises of youth with their achievements in after-life. Now, we obviously cannot examine into this relation unless records are preserved and are easily accessible of the physical powers of youths. We require statistical data on a large scale before it is possible to deduce trustworthy conclusions; and it seems impossible to establish the needed system of records, except under the stimulus of such examinations as I have in view. If the present conditions continue unchanged, inquiries into the important question I have

* See *Nature*, 1888–90.

indicated will linger on indefinitely. It will remain in the hands of a few amateurs, whose energies are largely absorbed by the almost hopeless task of searching for materials. Apply the stimulus of examination, and not only will the required data begin to accumulate, but many intelligent schoolmasters and others will feel it a matter of business to discuss them. There ought to be no difficulty in finding out the physical antecedents of every man when he was a youth. It is easily to be done in respect to his class place in literary examinations, and the value of these in forecasting his future can be discussed; but there are absolutely no records worthy of trust, on a sufficient scale, in respect to his physical powers.

Vigour of Eminent Anglo-Indians.—I may here mention an observation that impressed me a good deal at the time. On the eve of the departure of the late Sir Bartle Frere on a high political mission to Africa, his numerous friends entertained him at a farewell banquet. I was present, and seized the opportunity of estimating, as dispassionately and as carefully as I could, the average physical appearance of the eminent Anglo-Indians, who were the preponderating element in the party. I was the more moved to do so because the first feeling was one of surprise at the difference between what they were and what I had expected. I was prepared to see a collection of bilious and worn-out men. Not a bit of it; they were remarkably hale and vigorous. They were above the average stature and breadth—there was an air of force and power about them. They were as fine a collection of human animals as I have ever seen. I am sure that such examiners as those I have in view, would have given high marks to most of them for physical efficiency.

Tolerance of Tropical Climates.— Our ignorance is crass in respect of not a few elementary questions of national importance, which cannot be answered until the habit of preserving physical records of youths shall have become more common than it is. One of these regards the indications that a youth will or will not be able to keep his health in a hot and feverish climate. No inquiry has ever been made into these with the thoroughness and precision that the question obviously deserves. There is great variability among men and animals of the same race in their power of enduring malaria and changes of climate. It has happened that I have been more than a third of a century closely associated with the doings of the Royal Geographical Society, and during that time I have often had occasion to remark the ease with which the constitutions of successful explorers have thrown off the effects of fever. They may have been attacked frequently and severely but the malady rarely takes so strong a hold, as to leave them permanent invalids. Still less does it kill them, as it speedily kills many others who were, to all appearances, equally vigorous and energetic. A careful investigation of numerous cases would probably show that physiological signs might be discerned during youth, either of a natural immunity from malarious fever, or of a disposition towards it. It is cruel and costly to tempt youths to the tropics who are less constitutionally capable than others of thriving there. If we could distinguish those who are fittest for life in hot countries, we should select them even though in other respects they may be somewhat wanting. The tropical possessions of England are become so large that it is a matter of national importance to investigate this question thoroughly. It may yet be possible to find varieties of our race who are capable of permanently establishing their families in those climates.

It would be easy to enlarge on the topic of our astounding and contented ignorance of very elementary questions in respect to the choice of the men who are most likely to succeed in special services, but enough has been said to show the need of stimulating a demand for the collection of trustworthy records. No stimulus would approach in efficiency to that of establishing physical tests in connection with certain of the competitive examinations. If this were done provisionally, as proposed, and on a small scale, many persons who are connected with education, or with the public services, would interest themselves warmly in the subject. It would become a matter of present importance to themselves, and what is now only an academic question would be raised to the rank of a practical one.

I trust now that the two points have been established that it has been the object of these remarks to prove. First, that it is possible at once to devise a system of physical tests and inspection that shall be of real utility. Secondly, that in addition to its present value it would afford a necessary first step towards a more satisfactory scheme.

DISCUSSION.

The CHAIRMAN said that this paper would afford ample scope for discussion, as the subject was one on which wide difference of opinion might exist, not, indeed, on the principle itself, which was now conceded, for in many examinations physical tests were to a certain extent already used. The more difficult question to answer was the relative value to be attached to these tests, not only in comparison with literary work, but the relative value of the different tests themselves. To express it broadly, some might attach more importance to tests for mere strength, and some to those for physical health. He noticed that the author did not give so much prominence to personal and family history as some might be disposed to do. He took it Mr. Galton's motto would be *mens sana in corpore sano*, but there was a very broad and deep distinction between health and strength.

Admiral COLOMB said the subject of physical tests in competitive examinations was familiar now to every one in the army and navy, because it had been so much talked about, but until to-night he had only heard of it in connection with athletic competitions, which Mr. Galton proposed to discard altogether. So far as he understood the proposal, it would bring forward some useful men whose mental capacity was possibly a little below par, but who made up for the deficiency by their physical qualifications. Whether the Government would ever tentatively adopt such a system was a question, but one point struck him which might have some weight. In the old days a large number of officers were entered for both the army and the navy, and were then left to sink or swim as they could, and there was a sort of natural selection out of an immense body, the best men being retained for the services, and the others disappearing. That system had been abandoned, and by means of artificial tests the field from which officers were drawn was very much reduced. It therefore seemed desirable to make the examinations as complete as the knowledge of the time allowed, and he certainly thought that the tests described would help in selecting the best men and rejecting the worst. He should have thought a test of energy and possible capacity might be obtained from the pulse, the rapidity of the breathing and the effect of starts, and especially from the condition of the digestion. It was an old saying that a man "had no stomach for a fight," and he thought there was more in that than a mere trope. A man's want of courage might come from not having a good digestion. In the excessive fear experienced in dreams, the sensation felt on waking was always about the diaphragm, and when startled by a sudden apparition he had felt the same sensation himself in that part; so that probably digestion had a good deal to do with what was called courage and energy. One physical test not referred to was the cause of more rejection in the navy than almost any other, and that was the teeth. Men whose teeth decayed when young were almost always rejected for the navy, because it was said to show a bad constitution and to prevent good digestion.

Dr. DELEPINE said he had been obliged for some time o study this question, for the purpose of providing information in certain quarters, and he had become acquainted, probably, with all the statistics and methods proposed for testing the physical powers; but the result was that very few data existed which could be applied to candidates of the age of those who came up for examination, and those which did exist were often not sufficiently accurate to allow of any system being based upon them. The only data at present were those collected by Dr. Roberts some few years ago, in which the questions of age, occupation, and the correlation of measurements had been compared; and even in those tables a great many things were wanting, which Mr. Galton had either provided, or, he hoped, would provide before long. The great difficulty seemed to him to be the correlation of the measurements of height, weight, muscular power, and respiratory capacity. If they were paired two by two, as Mr. Galton suggested, certain important connections were apt to be overlooked; for instance, if a man of a certain height weighed a little more than he should —if his muscular power and breathing capacity were not taken at the same time, the excess of weight would be deemed rather a deficiency, but if it were found that he had unusual mnscular power, it was evident that the excess of weight might be due to a larger amount of muscle than the normal, and therefore the excess of weight would be a good feature. In the same way a man who measured more round the chest than he should, might be either too stout or more muscular than usual, or he might have a larger development of the lungs than normal. By taking all these four factors together all the required data were obtained. It would be impossible to obtain a satisfactory system of marking unless four or five measures were taken at the same time, and due correlation of the results must always have a chief place. There were other points which were not correlated, and which could be estimated on their own account; a man with good sight was certainly better than one with bad sight, and the same might be said of bad hearing. In awarding the marks also it should be remembered that the mean men of their race were generally found more serviceable than either very tall or very short men. The mean man, therefore, should have the maximum marks. It should also be remembered that there were two means, and sometimes more than two, in many races, which might be influenced by the occupation of the person himself or his ancestors.

Dr. BLACK (late Surgeon-Major) said he had had considerable experience in examining recruits, both officers and privates, and he thoroughly appreciated the great and growing importance of this subject. The acquisition of this knowledge would eventually be of great service to the public, and would enable parents and tutors to judge of the character of the candidates they produced either for the public or private service. Mr. Galton disclaimed any kind of athletic exercises in examination, and reduced his observations to the powers of the candidates—mental, muscular, and nervous. Observations as to some physical features, stature, &c., had been made for years and years, and there were piles of books at the Admiralty and War-office containing the records of centuries; but new branches of inquiry were now opened up which were still capable of much greater development. The power of will, for instance, could be exemplified by the stroke or blow of the fist, if the instrument were properly adjusted. Another point was the power of the voice, which was of great importance both to the private seaman as well as the officer. The captain or the colonel had to make his voice heard on parade, and the boatswain also had often to produce his voice in the most decisive and peremptory manner. Mr. Galton had an instrument for testing the strength of the arm, but it was equally important to test the strength of the leg. Nothing could be of greater importance to a commanding officer than to know the power of his regiment to march; but that had not hitherto been at all investigated by any instrumental method; it was judged of simply by the eye and other means; but it could scarcely be judged of by the appearance of the men. It was well known that the British private was not good at marching, though he might have a good physical appearance and be a good boxer; the Frenchman would beat him in marching, and the reason of that should be ascertained, for it had not yet been investigated by physical observation. Again, the power of balancing was a good test of the perfection of the whole system. Recruits were tested by standing on one leg; that power of balancing showed the perfection of the nervous system, both in the spinal cord and in the brain. It was also important to ascertain whether a man were left-handed or not; both feet and both hands should be of equal power, and both legs also. In fact the suggestions which might be made were almost innumerable, and he only threw these out as perhaps worth consideration.

Sir DOUGLAS GALTON, K.C.B., F.R.S., thought one of the great advantages which would result from adopting this system of examination would be that it would direct public attention more to the necessity for combining some system of physical training with education. At present, in the elementary schools, we were only developing one half of the children; and it was most essential, if we were to produce a race which should at all compare with the pictures on the walls —the Greeks, who were the most highly civilised race the world had ever seen—that the physical, as well as the mental qualities of the children should be developed; and their mental powers could not be developed unless their physical powers were developed also. On this account, it was of the highest importance that the Government should recognise the great labours of Mr. Francis Galton; and he trusted that before long some practical measures would be taken to adopt this system of marks in public examinations.

Mr. ROGERS pointed out that a great many civilians were now sent out to India of very different races, and that Bengalees, Parsees, and people from the upper provinces competed with Englishmen, Irishmen, and Scotchmen, and therefore he thought that the idea of putting them through a competitive examination in athletics might be relegated to the future. But after they had been through a literary course no one should be allowed to go to India unless he was able to ride on horseback, and showed a certain amount of physical energy.

Mr. GALTON, in reply, said one point made by the Chairman was that he had not said anything about heredity. He could only say that it was with great difficulty he refrained, but he thought it better not to encumber his paper with that important subject. He had been struck with the suggestion for testing the voice, and thought it would be very easy to devise an instrument for the purpose. The teeth would fall rather under the medical examination.

The CHAIRMAN, in proposing a vote of thanks to Mr. Galton, said there could be no nobler or more useful subject for consideration than the relation of the mental, moral, and physical qualities of man, and their influence on each other.

The vote of thanks was carried unanimously, and the proceedings terminated.

Miscellaneous.

SILK CULTURE IN NINGPO.

There are three kinds of silkworms in the district of Ningpo—the common, the horned, and the striped —all living on mulberry leaves. Consul Fowler states that there is also a worm which lives exclusively on the leaves of the oak. This comes from New Chwang. This worm is much larger and apparently harder to rear than the ordinary one. It is quite green in colour. The moth of this worm is of a deep yellow, and measures four inches across when the wings are spread out. On each wing are two or three transparent spots called eyes. The natural life of an ordinary silkworm, that is to say, from the time it is hatched to the death of the moth, is said to be from forty-five to fifty days, about five of which are spent in spinning, ten in a chrysalis state inside the cocoon, and the rest of the time in a caterpillar state. When first hatched the worm is black and just visible, and when it is ready to spin it becomes about two inches long. While in a caterpillar state it has, what are called in Ningpo, four sleeps, about five days apart and twenty-four to thirty hours duration each. During each of these sleeps the worm moults. The silkworm spins on a whisk of straw from eighteen to twenty-four inches high. The whisk is made by taking a handful of straw and tying it a little above the middle and spreading the two ends until it is able to stand upright. The worm is put on this, and it crawls about until it finds a convenient place, when it begins spinning by hanging silk to the straw and then winding itself round, and in a very short time the worm is hidden from sight. In about four days the cocoon is completed. After this the reeling takes place; that is, it must be done within a week after the cocoons are completed, otherwise the chrysalis, becoming the moth, will eat its way through the cocoon, after which it is useless for reeling. The process of reeling appears to be a very simple one. All utensils used consist of an earthen furnace, over which is put an iron pan and a bamboo wheel three feet in diameter, worked with a crank. This wheel has no felly, and the exterior ends of the spokes are connected with thread or cotton twine. The spokes are set in twos into the nave or hub of the wheels in such a way as to form the letter V, and therefore instead of the felly, the cord connection passes from spoke to spoke, forming a regular zigzag. The cocoons are placed in the pan of water, under which a slow fire is kept; a stick is then used to stir the cocoons until the desired number of fibres (or ends of the silk) have adhered to it, then these are removed, and passed through the hole of a cast attached to the furnace, and then fixed on to the wheel. The wheel is now set in motion, and the reeling actually begins. It is said that an expert can reel off about a pound and a half of silk in a day.

THE FOREIGN TRADE OF BULGARIA.

The *Journal de la Chambre de Commerce de Constantinople* says that the returns of the Bulgarian foreign trade are showing a decided increase. Cereals, chiefly corn and barley, form the largest part of the exports of the country. A portion of these cereals is absorbed by other parts of Turkey, by Roumania, and Austria-Hungary, which apply them either to the requirements of local consumption or for their export trade. England, however, is the principal market for Bulgarian cereals. For the export of its products Bulgaria employs several routes. By the Danube route the trade with Austria-Hungary and Roumania is carried on; the trade with over-sea countries is carried on *viâ* Varna and Black Sea; hitherto the Principality of Bulgaria has been obliged to use the port of Dedeaghatch, on the Ægean Sea; for the future it will be independent of this by the construction of the Yamboli-Bourgas Railway, terminating on the Black Sea. Apart from the extensive plains of Sophia and Philippopolis, but situated to the south of the Balkan chain, there are the large plateaux situated to the north of these mountains, between them and the Danube, which are the granaries of Bulgaria. After cereals, the exports of Bulgaria almost entirely consist of articles of general consumption—cheese, butter, eggs, and cattle, which are sold to the neighbouring people. There are, however, some articles of which it makes, if not a monopoly, at least a special feature, these are raw skins, carpets, and essence of roses. Bulgaria exports annually about 80,000 goat skins, 15,000 kid skins, and 180,000 lamb skins. The carpets of Berkovitza, woven by the Bulgarians with wool spun and dyed by themselves, enjoy in the East a considerable and well-deserved reputation. Very much appreciated in Austria and Servia, they are not yet, it is said, sufficiently sought after abroad, as they are not yet thoroughly known. The industry of making essence of roses belongs exclusively to the districts situated to the South of Bulgaria. The production of wine in Bulgaria but little exceeds, on an average, a value of £96,000, which gives a value of about 7d. a gallon. Notwithstanding the low price, it has hitherto supplied nothing, or next to nothing to the export trade. Among the imports into Bulgaria, cottons supplied almost exclusively by England take the first place. Then come metals in bars, and manufactured in the form of machinery and other articles. Almost all these articles come from England; Belgium supplying also a small quantity. Austria sends to Bulgaria almost all the ready-made clothing, *articles de luxe*, and furniture which are sold there. Sugar also comes, for the greater part, from Austria. Petroleum is supplied by Roumania, Russia, and the United States. Germany sends different articles in ordinary use, principally cotton tissues and hardware. The importance of the Bulgarian foreign trade is far from attaining all the development of which it is capable, says the *Journal de la Chambre de Commerce de Constantinople*, but there is progress year by year, and the recent opening of the Yamboli-Bourgas railway, which secures to the country a new direct output in the Black Sea, will certainly result in still further accentuating this progress.

Notes on Books.

THE THRESHOLD OF SCIENCE. By C. R. Alder Wright, D.S., F.R.S. London: Charles Griffin & Co., 1890.

This work is founded on the late James Wylde's "Magic of Science," published thirty years ago, and is briefly described on the title page as "a variety of simple and amusing experiments illustrating some of the chief physical and chemical properties of surrounding objects, and the effects upon them of light and heat." The advance in the popularity of science during the past thirty years is even greater than the progress of science itself; and it will, therefore, readily be perceived that there is not much room for the contents of this original work. Practically, indeed, this book may be considered as a new one, though here and there—as, for instance, in the chapter on photography—a somewhat undue amount of the old material has been suffered to remain. The experiments relate, for the most part, to chemistry or to light and heat. Some of them are of course familiar, but many are novel; and all are described in such a way as to be intelligible and interesting to the young student. The book should serve a useful purpose—by attracting young readers to the study of science, and inducing them to pursue it seriously. At the same time, it may serve to encourage those who have already commenced such serious study, by showing them the amusing and interesting side of their work.

TO SOUTH AFRICA AND BACK. By John Finch. London: Ward, Lock & Co.

The author, in a five months' journey to South Africa, visited Cape Colony, Natal, the Orange Free State, and the Transvaal, and in the narrative of his journey he gives special attention to the diamond and gold fields, relating anecdotes of the illicit diamond buyers and their tricks.

PUBLIC LIBRARIES: A History of the Movement and a Manual for the Organization and Management of Rate-supported Libraries. By Thomas Greenwood. Third edition, entirely re-written. London: Simpkin, Marshall, Hamilton, Kent & Co.

The first edition of this book was published in 1886, and was noticed in the *Journal* in that year. Since then much has been done in respect to the increase in the number of free libraries and in the production of labour-saving apparatus for their successful management. The author has brought the information up to date, and much enlarged his book.

DIE DECORATIVE KUNST-STICKEREI. I. Ausnäh Arbeit. Von Frieda Lipperheide. Berlin: 1890. 4to and folio.

This is a very elaborate work on art needlework, consisting of letterpress and illustrative plates. The various materials and instruments used in the practice of embroidery are described and illustrated; further on the different stitches are explained and specimens given. The atlas contains coloured lithographs of Spanish appliqué work of the 17th century, one exhibiting an escutcheon of arms. Italian appliqué of the 17th and 18th centuries and French appliqué of the 18th century, also Spanish appliqué on a netted ground of the 17th and 18th centuries.

MUSTERBLATTER FUR KUNSTLERISCHE HANDARBEITEN. Herausgegeben von Frieda Lipperheide. II. Sammlung (13–24 Blatt). Berlin: 1890.

This work, on a similar subject to the above, and by the same author, contains coloured plates of Oriental, Servian, Bulgarian, and Spanish patterns.

General Notes.

KEW REPORTS.—An index has now been issued to the Kew Reports from 1862 to 1882, the object of the index being to render the information contained in the Reports respecting economic and other plants more easy of reference. More detailed notes than those contained in the annual reports are now given in the Kew Bulletin, of which three volumes are already published, and the fourth is in course of publication.

MEETINGS OF THE SOCIETY.

ORDINARY MEETINGS.

DECEMBER 3.—JAMES DREDGE, "The Chicago Exhibition, 1893." The ATTORNEY-GENERAL, M.P., Chairman of Council, will preside.

DECEMBER 10.—F. BAILEY, "Electric Lighting Progress in London." SIR FREDERICK BRAMWELL, Bart., D.C.L., F.R.S., will preside.

DECEMBER 17.—GEORGE DAVISON, "Impressionism in Photography."

FOREIGN AND COLONIAL SECTION.

The meetings of this Section will take place on the following Tuesday afternoons, at Half-past Four o'clock:—

January 20; February 17; March 17; April 21; May 5, 26.

INDIAN SECTION.

The meetings of this Section will take place on the following Thursday afternoons, at Half-past Four o'clock:—

January 22; February 26; March 12; April 9, 30; May 28.

APPLIED ART SECTION.

The meetings of this Section will take place on the following Tuesday evenings, at Eight o'clock:—

January 27; February 10; March 10, 24; April 14; May 12.

CANTOR LECTURES.

The following Courses of Cantor Lectures will be delivered on Monday evenings at Eight o'clock :—

Prof. VIVIAN B. LEWES, "Gaseous Illuminants." Five lectures.

LECTURE II.—DECEMBER 1.—The composition of coal gas—Analysis of gas—The illuminants present in coal gas—Effect of class of coal, methods of manufacture, and diluents present, on the illuminating power of coal gas—The methods employed to enrich coal gas.

A. J. HIPKINS, F.S.A., "The Construction and Capabilities of Musical Instruments." Three lectures.

January 26; February 2, 9.

GISBERT KAPP, "The Electric Transmission of Power." Three lectures.

February 16, 23; March 2.

Prof. R. MELDOLA, F.R.S., "Photographic Chemistry." Three lectures.

March 9, 16, 23.

HUGH STANNUS, F.R.I.B.A., "The Decorative Treatment of Natural Foliage." Four lectures.

April 13, 20, 27; May 4.

JUVENILE LECTURES.

Two Lectures, suitable for a juvenile audience, will be delivered by E. B. POULTON, M.A., on "Mimicry in Animals," on Wednesday evenings, December 31, 1890, and January 7, 1891, at Seven o'clock.

MEETINGS FOR THE ENSUING WEEK.

MONDAY, DEC. 1 .. SOCIETY OF ARTS, John-street, Adelphi, W.C., 8 p.m. (Cantor Lectures.) Prof. Vivian B. Lewes, "Gaseous Illuminants." (Lecture II.)

Royal Institution, Albemarle-street, W. 5 p.m. General Monthly Meeting.

Engineers, Westminster Town-hall, S.W., 7½ p.m. Mr. J. J. F. Andrews, "Ship Caissons for Dock Basins and Dry Docks."

Microscopical, 20, Hanover-square, W., 8 p.m. Conversazione.

Chemical Industry (London Section), Burlington-house, W., 8 p.m. Mr. W. Webster, "The Electrical Treatment of Sewage."

Royal, Burlington-house, W., 8 p.m. Anniversary.

British Architects, 9, Conduit-street, W., 8 p.m. Mr. R. Phené Spiers, "Sassanian Architecture."

Medical, 11, Chandos-street, W., 8½ p.m.

Victoria Institute, 1A, Adelphi-terrace, W.C., 8 p.m. Prof. Hull, "Geological History of Egypt."

London Institution, Finsbury-circus, E.C., 5 p.m. Prof. W. H. Corfield, "The Houses we live in."

TUESDAY, DEC. 2...Civil Engineers, 25, Great George-street, S.W., 8 p.m.

Pathological, 20, Hanover-square, W., 8½ p.m.

Biblical Archæology, 9, Conduit-street, W., 8 p.m.

Zoological, 3, Hanover-square, W., 8½ p.m. 1. Mr. Richard Crawshay, "The Antelopes of Nyassa Land." 2. Prof. G. B. Howes (i.), "The Suspension of the Viscera in the Batoid *Hypnos subnigrum*;" (ii.) "Notes on the Pectoral Fin-skeleton of the Batoidea and of the Extinct Genus *Chlamydoselache*." 3. Mr. G. A. Boulenger, "The presence of Pterygoid Teeth in a Tailless Batrachian (*Pelobates cultripes*), with remarks on the localisation of Teeth on the Palate in Batrachians and Reptiles."

WEDNESDAY, DEC. 3 .SOCIETY OF ARTS, John-street, Adelphi, W.C., 8 p.m. Mr. James Dredge, "The Chicago Exhibition, 1893."

Entomological, 11, Chandos-street, W., 7 p.m. 1. Mr. George T. Baker, "Notes on the Lepidoptera collected in Madeira by the late T. Vernon Wollaston." 2. Mr. Frederic Merrifield, "The conspicuous changes in the markings and colouring of Lepidoptera, caused by subjecting the pupæ to different temperature conditions." 3. Mr Hamilton H. Druce, "A Monograph of the Lycænoid genus *Hypochrysops*, with descriptions of new Species."

Archæological Association, 32, Sackville-street, W., 8 p.m.

Obstetrical, 20, Hanover-square, W., 8 p.m.

THURSDAY, DEC. 4 ... Antiquaries, Burlington-house, W., 8½ p.m.

Linnean, Burlington-house, W., 8 p.m. 1. Mr. H. N. Ridley, "The Genus of Orchid *Brownheadia*." 2. Mr. J. H. Lace, "The Botany of Kandahar." 3, 4. Mr. Thos. Kirk, "Botanical Visit to Auckland Isles."

Chemical, Burlington-house, 8 p.m. Dr. Branner, "The Volumetric Estimation of Tellurim."

London Institution, Finsbury-circus, E.C., 7 p.m. Mr. Armbruster, "Hector Berlioz" (illustrated).

Camera Club, Bedford-street, Strand, W.C., 8½ p.m. Mr. H. Sturmey, "Rollable Transparent Films."

Archæological Institution, Oxford-mansion, Oxford-street, W., 4 p.m.

FRIDAY, DEC. 5...National Association for the Promotion of Technical and Secondary Education (at the HOUSE OF THE SOCIETY OF ARTS), 5 p.m.

Teachers' Training and Registration Society (at the HOUSE OF THE SOCIETY OF ARTS) 8 p.m. Annual General Meeting.

Geologist's Association, University College, W.C., 8 p.m. 1. Messrs. C. Davies Sherborn and H. W. Burrows, "Report on the Microscopical Examination of some Samples of London Clay from the excavations for the widening of Cannon-street Railway Bridge, 1887." 2. Mr. Edwin Litchfield, "A short visit to Ingleton and to Filey Brigg (showing how a dangerous reef was converted into a perfect breakwater by an ancient race.)"

Philological, University College, W.C., 8 p.m.

Quekett Microscopical Club, 20, Hanover-square, W.C., 8 p.m.

SATURDAY, DEC. 6...Foremen Engineers and Draughtsmen, Cannon-street Hotel, E.C., 7½ p.m. Paper by Mr. W. Powrie.

Subject Matter Index of Advertisements.

*** *Where the number of the page is omitted the advertisement does not appear this week.*

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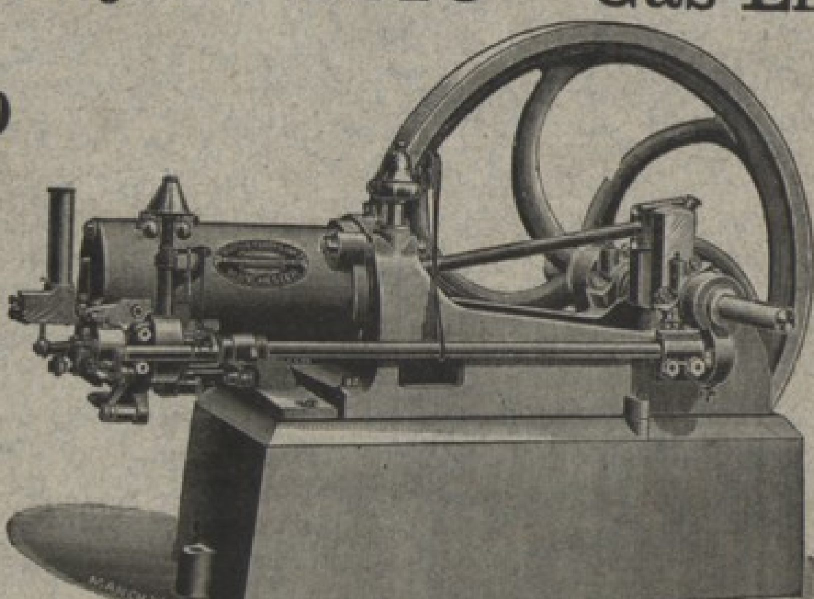

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PHILOSOPHICAL TRANSACTIONS.

I. *The Patterns in Thumb and Finger Marks.—On their arrangement into naturally distinct classes, the permanence of the papillary ridges that make them, and the resemblance of their classes to ordinary genera.*

By FRANCIS GALTON, *F.R.S.*

Received November 3,—Read November 27, 1890.

[PLATES 1, 2.]

I PROPOSE to describe some results of a recent inquiry into the patterns formed by the papillary ridges upon the bulbs of the thumbs and fingers of different persons. The points upon which I shall chiefly dwell are, the classification of the patterns, their permanence throughout life, and the apt confirmation they afford of certain views concerning the more important conditions by which the genera of plants and animals are determined.

My attention was drawn to the subject nearly three years ago, when preparing a lecture for the Royal Institution on "Personal Identification." (See either the 'Journal of the Royal Institution,' for Friday, May 25th, 1888, or 'Nature,' June 28th, 1888, in which the portion of the lecture with which we are now concerned is printed.)

I would refer to that lecture, as it contains numerous references to the existing literature on the subject, and because it formed the starting point from which the present inquiry proceeded. Two conclusions were strongly impressed on my mind at the time when I gave it :—

(1.) That although much had been asserted as to the permanence of these markings, and though I was then able, through the kindness of Sir W. J. HERSCHEL, to submit two instances in proof, the truth of the assertion had never been adequately investigated.

(2.) That the method of classifying the markings, which was originated by PURKINJE, in his 'Commentatio,' dated 1823 (a copy of this rare pamphlet is now in the library of the College of Surgeons), and subsequently adopted by other writers, with more or less variation, was not based on a sufficiently good foundation.

Since then I have steadily pursued the inquiry and found its interests to widen considerably as I proceeded. They led in many directions, and among others to the topic that will be the last discussed.

Data.

The data on which this memoir is based are :—

(1.) The impressions of the two thumbs of about 2500 persons made for me, at my Anthropometric Laboratory, together with several impressions of the fingers.

(2.) A small and unique collection of impressions put at my disposal by Sir W. J. HERSCHEL, of which one half were taken many years ago, and the other half were taken quite recently from the same persons. I will speak of these more at length when the time comes for using them.

As regards the first set :—

I chose the two thumbs rather than two adjacent fingers on the same hand, in order to obtain data respecting symmetry, on which however very little will be said here, and I chose a thumb of each hand, rather than a finger of each hand, because the thumb being greater than that of the finger the width of it affords a proportionately larger field for variety of pattern. However, all that will be said about thumb marks, applies with but little reservation to finger marks, but with much more reservation to those of the toe.

I have myself not studied the latter, but PURKINJE states that the patterns of the toes are always of that particular sort which I shall define later on, and call a loop.

Origin of the Ridges.

I do not attempt to discuss the origin of the papillary ridges, because my knowledge is entirely second hand, and it would be presumptuous in me to do so. It will be sufficient to say that KOLLMANN'S (A. KOLLMANN, 'Der Tastapparat der Hand.' Hamburg and Leipzig, LEOPOLD VOSS, 1883) dissections seem to prove (see his figs. 19, 20) :—

(1.) That each of the papillæ (which lie below the cuticle) has two heads, which I will symbolise by the fork in the printed capital letter Y.

(2.) That the duct of the sudorific glands in passing outwards between the papillæ, is bound up, as in a bundle, with the adjacent head of each of two neighbouring papillæ. So that if the sudorific duct is symbolised by the printed letter I, a section across the ridges might be symbolised by a row of the letters Y and I printed alternately, thus—YIYIYIY. Then the union of the I with the adjacent prongs of two Y's forms the foundation of a ridge, and the clefts between the heads of the Y's correspond to the furrows.

There is, I believe, no adequate explanation of the fact that the prominences through which the ducts issue, on the bulbs of the finger, and in some other parts, are strongly disposed to arrange themselves into continuous ridges, and not to form isolated craters. There is, however, abundant analogy in the animal kingdom of external ridges of various sorts running in a variety of spirals and whorls.

Obtaining Impressions.

The impressions in my collection were made by thinly inking a copper plate with printer's ink, by means of a printer's roller. The plate was about eight inches by twelve, and fixed to a solid block of wood. The thumb was rather lightly rolled on the inked plate, not simply pressed upon it, and then rolled on paper. Thus the impression it left was a cylindrical projection of the whole bulb of the thumb, extending nearly from one side round to the other (fig. 8), and including all the principal characteristics of the pattern, which a simple impression (see those in Plate 2) often does not. The thumbs were easily cleaned by dipping them into a dish of turpentine and wiping with a cloth. It is an essential condition for making clear impressions that the ink should not lie low down the sides of the ridges. The furrows should remain quite uninked. I had much difficulty at first in contriving a rough and ready method of obtaining good impressions, and do not say that the plan just described is the best. But it has acted well for a long time, and, therefore, it is hardly necessary for me to speak here of later experiments to improve it.

Reversal of Patterns.

Patterns of similar kinds lie on the two thumbs in opposite directions. They should never be read from right to left, but from outwards, inwards. Consequently, in order to make the pattern on the one thumb comparable with that on the other, it must be reversed. It is convenient to take a duplicate of the impressions upon tracing cloth, which shows the reversed pattern when it is viewed face downwards.

Origin of the Patterns.

The reason why the patterns appear on the bulbs of the thumb and finger is apparently to be found in the presence of the thumb nail, which disarranges the otherwise parallel course of the ridges in the way that is diagrammatically shown in fig. 1.

Here we see that the upper ridges near the tip of the thumb are thrown into bold arches, while the ridges that lie below the level of the nail run horizontally. There is, in consequence, a tendency to leave an interspace, which has somehow to be filled up with a scroll work of ridges, and this scroll work constitutes the patterns with which we are concerned.

In about one case in thirty, the interspace is avoided by an arrangement like that in a, figs. 7 and 9, but this is an unstable form, or it often shows signs of having been on the point of breaking into a different pattern, as will soon be explained more fully. I call these patterns "Primaries," because they are the fundamental arrangement from which all the vast varieties of other patterns are lineally descended, and in all of which the interspace of which we have spoken exists.

Fig. 1.

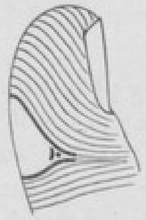


Fig. 2.

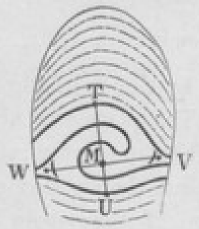


Fig. 3.

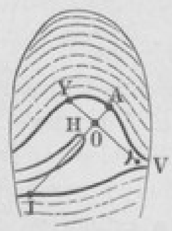
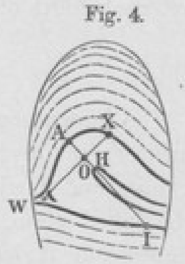


Fig. 4.



Points of Reference.

Wherever an interspace occurs, two ridges must have diverged in order to make room for it. There may be a divergence of the ridges on both sides of the interspace, as in fig. 2, or on one side only, as in figs. 3 and 4. Moreover, just in front of the place in the furrow, beyond which the parallel ridges begin to diverge, there are always one or more little cross lines, diagrammatically shown in all these figures, which cut off a small triangle.

The centres of these triangles form excellent spots or points of reference, though doubt may exist as to the exact position in which they should be placed. It is easy enough to determine their position approximately, and that is all we want.

Hereafter I shall always call these two points V and W. V being to the outside of the thumb, and W to the inside, that is to say, nearest to the rest of the hand. They are cardinal points in my classification, and are very useful in constituting the two ends of a base line (fig. 2) from which measurements may be made and bearings taken.

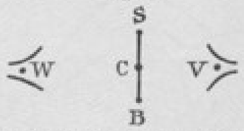
Reversals.

After the proper letters have been affixed to the points, it does not matter whether the pattern we are studying is direct or reversed. There is a curious variety in the way in which patterns are apt to be presented. Those on the right thumb are reversed forms of those met with on the left. The impression is the reversed form of the pattern itself. If made on a lithographic stone, it is re-reversed in the print. If made on transfer paper and thence put on the stone, it is re-re-reversed in the print. This is enough to show the confusion that will arise if the points V and W are not lettered, but it by no means exhausts the list of ordinary contingencies. As the letters V and W are unchanged in shape when they are reversed, they are convenient for the purpose to which they are here applied.

Basis of the Classification.

In one respect the divergent lines that bound the pattern admit, in the earlier part of their courses, of nine, and only nine, possible variations. Draw a line (figs. 5, 6) through what appears to be the most central part of the pattern (which we may call C), that shall be roughly parallel to the median line of the thumb, and shall cut

Fig. 5.



Print of left thumb, or appearance of face of right thumb.

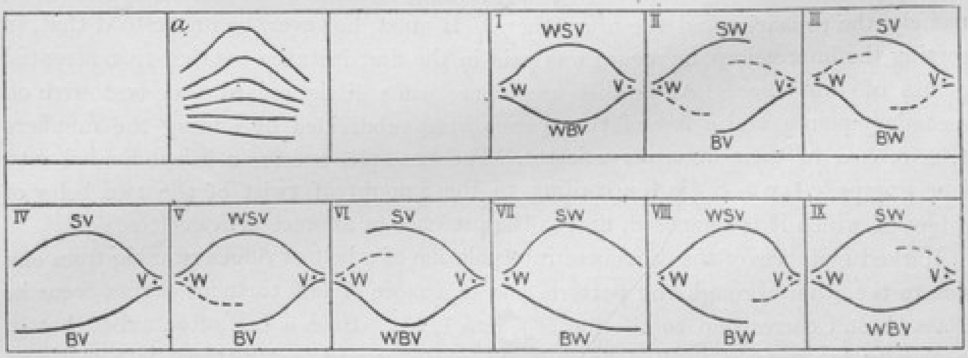
Fig. 6.

Print of right thumb, or appearance of face of left thumb.

the upper boundary of the pattern at S and the lower boundary at B. Consequently, S and B, whose positions are very roughly determined, may be taken to represent the summit and the base of the pattern. Now the ridge in which S is situated must, by construction, have come either from V. or from W, or from both. There are these three, and only these three, alternatives, SV, SW, WSV. Similarly, as regards the ridge on which B is situated, there are the three alternatives, BV, BW, WBV. As any one of the former events may be combined with any one of the latter, there are 3 × 3, or nine possible combinations. In the primaries neither V nor W exist, so they form a class by themselves, making a total of ten classes. The nine of which we have been speaking are as follows :—

I. WSV—WBV	IV. SV—BV	VII. SW—BW
II. SW—BV	V. WSV—BV	VIII. WSV—BW
III. SV—BW	VI. SV—WBV	IX. SW—WBV

Fig. 7.



These, as well as the primary, which is distinguished by the letter *a*, are drawn in the diagram, fig. 7.

Outlines of the Patterns.

A pattern is quickly analysed by following with a pencil the course of the two pair of divergent ridges from V and W respectively (fig. 8), or if one of these points

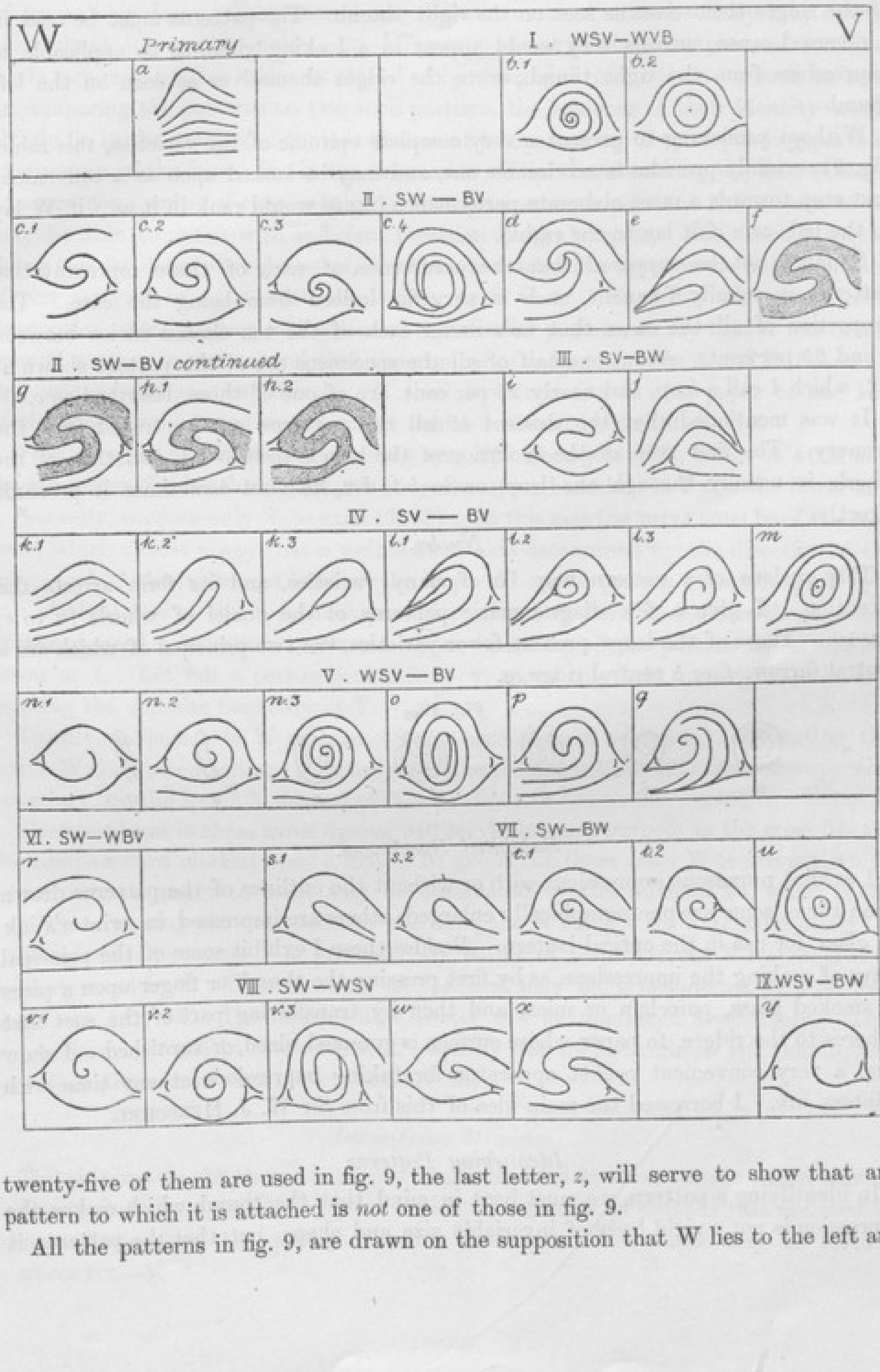
Fig. 8.

is absent, then following those that diverge from the other (see also figs. 2, 3, 4). As ridges are apt to bifurcate, to join with others, and also to end abruptly, it is necessary to follow a consistent course in such cases. In bifurcations the innermost branch should be followed. Whenever a ridge ends, the pencil should stop also, and recommence on a new ridge, selecting that which appears to continue the direction of the former one in the truest way. In case of doubt, the pencil should, as before, follow the innermost of the two lines between which the doubt lies. If opposite rules to these were imposed, the outline would be much less speedily analysed, and be by no means so simple when completed. The sudden transformation of a maze of ridges into an orderly pattern by this easy process is truly remarkable.

I outlined, where necessary, or otherwise examined, more than 1000 photographically enlarged impressions with much care, and found, on sorting them, that nearly all their patterns fell satisfactorily into one or other of the divisions in fig. 9, where twenty-five main divisions are arranged, according to the ten classes already described, namely, the primaries and the nine others. It must, however, be understood that, in sorting the impressions, no regard was paid in the first instance to other than essential points of difference. After this was done, some little regard was bestowed on secondary points, and a few of the species were subdivided by adding the numbers 1, 2, 3, &c., to their descriptive letter. For example, species *c* is subdivided into four groups, *c* 1, *c* 2, *c* 3, *c* 4, according to the amount of twist of the two belts of ridges of which it is composed, and to the presence or absence of a nucleus.

Marked instances of the occasional interpolation of a belt of ridges running from one side to the other through the pattern, and in a more or less tortuous course, occur in Class II. and correspond to the forms *f*, *g*, *h* 1, *h* 2. Such a belt often exists, but it is usually too narrow or ill defined to be worth regard. A pattern is sometimes composed altogether of such a tortuous belt, in which case it would rank along with the Primaries in Class *α*. As there are twenty-six letters of the alphabet, and only

Fig. 9.



twenty-five of them are used in fig. 9, the last letter, *z*, will serve to show that any pattern to which it is attached is *not* one of those in fig. 9.

All the patterns in fig. 9, are drawn on the supposition that W lies to the left and

V to the right. They are therefore those of impressions made by the left thumb, or of the ridges themselves as seen on the right thumb. The patterns must be read in a reversed sense, such as they would appear in a looking glass, to be applicable to impressions from the right thumb, or to the ridges themselves as seen on the left thumb.

Without professing to present a very complete epitome of the varieties, this table, (fig. 9) certainly provides a serviceable one, and may be looked upon as a convenient first step towards a more elaborate performance (fig. 8 would rank in it as *j* if W lay to the left, as *e* if it lay to the right).

It must not be supposed that the occurrence of each of these representative patterns is equally frequent, such is very far indeed from being the case. The proportion of all the cases that falls under each of the ten classes varies between 1 and 65 per cent. About one half of all the specimens are of the pattern shown at *k* 2, which I call a *loop*, and nearly 25 per cent. are of one of those described as *c*.

It was mentioned that the descent of all the patterns can be traced from the primary. The first step in the evolution of the loop is seen in *k* 1, and that of the whorls is usually through the loop, as in *l* 1, *l* 2, &c., but sometimes it proceeds directly.

Nuclei.

The nucleus of a pattern may be of many varieties, and its form arrests the attention. I give a few diagrammatic patterns of the nuclei of whorls (*a* to *e*, fig. 10. Those of the loops present fewer varieties, the two principal of which are a central furrow, *f*, or a central ridge, *g*.

Fig. 10.

Exhibited Specimens.

I exhibit numerous impressions with or without the outlines of the patterns drawn upon them, some are photographically enlarged, others are impressed in printer's ink on glass, for use in the optical lantern. Besides these I exhibit some of the principal ways of making the impressions, as by first pressing the thumb or finger upon a piece of smoked glass, porcelain or mica, and then by transferring part of the soot that adheres to the ridges, to paper whose surface is gummed, sized, or varnished. I show also, a very convenient pocket apparatus for taking impressions at any time with printers' ink. I borrowed the main idea of this from Sir W. J. HERSCHEL.

Identifying Patterns.

In identifying a pattern, we must bear in mind that the thumb which makes the impression is not a rigid body of invariable size and shape, but that the patterns it impresses at different times will vary. If those times are separated by long periods of growth or decay, the patterns may become much distorted. They may change their shape just as the pattern on different portions of the same piece of machine-made lace may become variously stretched by wear, or shrunk by wet, or even be torn. In comparing the patterns on two such portions, the evidence of their identity would chiefly lie in the number of threads that went to the making of corresponding parts of the pattern of the lace. So, in the impression of the thumb marks, the first point is to count the number of ridges that intervene between such points in the pattern as we may be able to define with sufficient precision for the purpose. The simplest way of doing this for descriptive purposes, would be to mark a few appropriate points on each of the patterns in fig. 9 with the letters, A, B, C, &c. Then it would give a clear description of the larger peculiarities of a particular pattern, to say that it was of the general pattern so and so, and that the number of ridges between A and B, C and D, &c., was so and so, respectively. I shall have occasion in this paper to use two methods of reference and measurement, which had best be described now.

First, suppose V and W both to exist (fig. 2). Then join VW, bisect it at M and draw a perpendicular through M, meeting the upper boundary in T and the lower in I.

Secondly, suppose only V to exist (fig. 3). In this case the curve must be of the loop form, which almost always has a well marked axis determined by the direction of the upper end of the innermost bend of the loop. There is usually quite enough length in a straight line of the uppermost portion of the inner bend to indicate the direction of the desired axis, which meets the upper boundary of the pattern at A, and the lower at I. Let fall a perpendicular from V on to this axis, cutting it at O, and meeting the opposite boundary at Y.

Thirdly, suppose only W to exist. Then proceed just as before, substituting the letter W for V, and calling the point where the prolongation of WO meets the opposite boundary, by the letter X instead of Y.

The cross lines in these three figures will serve the same purpose as the cross lines of the compass card marked upon a map. In two of the three cases W is present, which letter, since it suggests west, may be designated by the numeral 24, which is the number expressing the west point of the compass, N being 0 (or 32), and S being 16.

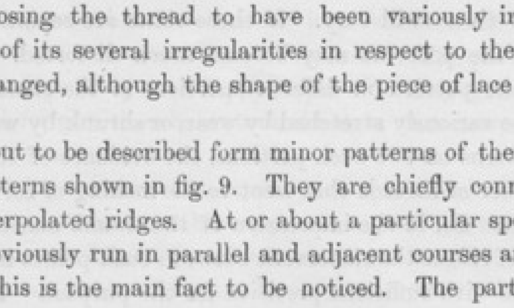
In the other two cases V is present. By parity of nomenclature V would always be designated by 8. Then T or A, as the case may be, is designated by 0 (or 32), and U or I by 16. The intermediate points will be numbered accordingly. This nomenclature, it will be observed, serves equally well for the right or left thumb, and for either direct or reversed impressions of them.

Identifying Minutiæ.

The patterns on the thumb have just been compared to those on lace which may be variously shrunk or stretched, but in which the number of threads that are used to form each detail of the pattern remains unaltered. The simile may be further extended by supposing the thread to have been variously irregular and divided. Then the position of its several irregularities in respect to the parts of the pattern would remain unchanged, although the shape of the piece of lace as a whole may have greatly altered.

The minutiæ about to be described form minor patterns of their own, quite distinct from the larger patterns shown in fig. 9. They are chiefly connected with the commencements of interpolated ridges. At or about a particular spot in the pattern two ridges that had previously run in parallel and adjacent courses are replaced by three ridges (fig. 11). This is the main fact to be noticed. The particular way in which

Fig. 11.



the two ridges seem to have been converted into three is by no means so important, because its appearance is often false and misleading. The conversion may have been affected either by a new ridge arising between two others which diverge so as to yield place to the intruder, as in fig. 11*a*, or else by one of the two ridges bifurcating, as in *b* and *c*.

But grave difficulties not unfrequently arise in distinguishing between these three cases. One impression may show one form, and another impression taken immediately afterwards from the same thumb may show another. The reason is that the ridges are not of a uniform height and that the head of the fork is often low, and fails to leave its mark on one of the prints, though it does so on the other. Thus neither of the two cases *b* and *c* admits of being certainly distinguished from *a*. This kind of difficulty is frequent where a ridge momentarily divides so as to enclose a small crater as in *d*. One lip of the crater may leave no mark, and the impression of *d* might have the appearance either of *e* or of *f*. Similarly as regards islands, as in *g*, *h*, and *i*.

These remarks are intended as a caution against placing too much trust in the specialities of these appearances, though usually the distinction between a fork and the beginning of a new ridge is clear enough.

Persistence of Patterns.

I submit the impressions in Plate 1 in justification of the conclusions to be drawn from them. They were all furnished to me by the kindness of Sir W. J. Herschel, who, when employed in the Indian Civil Service in Bengal many years ago, introduced in his district the practice of taking impressions from the first two fingers of the right hands of witnesses and others as a check against personation. I also possess photographs of some other impressions besides those in Plate 1, taken from other fingers of some of the same persons, and which tell a similar tale.

Each of the eight double sets in Plate 1 consists of two impressions of the same digit of the same person, taken at the beginning and end of a long interval of years. In six of the eight double sets the impressions are those of two different fingers of three different persons. In the remaining two the impressions are of one finger of two different persons. The entry 1 *r* means the forefinger of the right hand, 2 *r* its middle finger, 3 *r* its third finger. The cases 1 and 2 refer to a youth who was a child of seven and a half years old when the first impression was taken; this was nine years previous to the second impression. The remaining six cases refer to four men who were adults when the first impression was taken, and this occurred at a period varying between twenty-eight and thirty-one years before the second impression. The photographs of all these impressions are enlarged to twice their natural size for the greater convenience of reference, and every point suitable for comparison in each pair of impressions has been examined and noted in Plate 2. It is rare that the one impression presents quite the same portion of the pattern as its fellow. Also it occasionally happens that a portion of one impression is blotted or otherwise too imperfect to allow of fair comparison with the corresponding portion of the other. Subject to these necessary restrictions every fork, junction, crater, or island in each impression has been noted, and in every single case has been found to occur in both the members of the same pair, subject only to the reservations previously made, that is to say, what appears as a fork in a first impression sometimes appears as the independent interpolation of a new ridge in the second, or *vice versâ*. I have in these cases reckoned it as being of similar appearance in both, and have marked it with the same symbol in both of the skeleton charts, viz., by a fork or by a dot, selecting between the alternative symbols the one that appeared on the whole to be most suitable.

No. on the Plate.	Initials.	Digit.	Age at date of first impression.	Date of the first impression.	Date of the second impression.	No. of years interval.	No. of beginnings and ends of ridges.	No. of forks and junctions of ridges.	Total No. of points of comparison.
1	A.E.H.	1 *r*	7½	1881	1890	9	19	14	33
2	A.E.H.	3 *r*	7½	1881	1890	9	18	18	36
3	N.H.T.	1 *r*	adult	1862	1890	28	16	11	27
4	N.H.T.	2 *r*	adult	1862	1890	28	17	19	36
5	F.K.H.	1 *r*	adult	1862	1890	28	27	28	55
6	R.F.H.	2 *r*	adult	1859	1890	31	10	17	27
7	W.J.H.	thumb *r*	adult	1860	1890	30	18	32	50
8	W.J.H.	3 *r*	adult	1859	1890	31	15	17	32
Grand totals							140	156	296

I did my best to justly reckon the number of minutiæ in each impression that admitted of comparison, but found it difficult; perhaps it is impossible to be absolutely accurate.

Other persons may make estimates that differ slightly from mine, but mine are, I am sure, substantially correct and trustworthy for all practical purposes. I counted as separate points both of the ends of every island, however short the island might be, and both of the forks that enclosed every crater however minute.

The upshot of a careful step by step study is that I have found an absolute and most extraordinary coincidence between the details of each of the two impressions of the same finger and of the same person. There was, as the table shows, a grand total of no less than 296 (say, roundly, 300) points of comparison, and not a single one of them failed, though I had much trouble in deciphering the ridges, especially about the V-point in case 5. There was no one case found of a difference in the number of ridges between any two specified points. Never during the lapse of all these years did a new ridge arise, or an old one disappear. The pattern in all its minute details persisted unchanged, and, *à fortiori*, it remained unchanged in its general character.

[January 28. Since writing this memoir, I have had opportunities of making a considerably larger total of comparisons between other pairs of impressions, and I have thus far found one instance, and one instance only, of any fundamental disaccord. It was a ridge that had been partially cleft in a child, but when he had grown into a boy the cleft had disappeared.]

The comparison would, however, present discrepancies and be much less effectively carried on if it were performed by first registering the observed peculiarities of one pattern, next those of the other, and, lastly, comparing the two registers. Each would be likely to contain points in which the other was deficient, and not a few very characteristic features might be overlooked in both. For example it will be seen in the two impressions, No. 2 in the skeleton chart, that I have inserted an arrow head to draw attention to a small spot a little in front of it, which represents an isolated papilla. This spot would have been passed over as a mere accident of the ink, unworthy of record, had it not been that, in making the comparison from point to point, the same dot was observed in both impressions. It was then recognised to be of importance. It is pretty to notice how the small dot in the child has grown to a larger dot in the youth.

The lapse of about thirty years is seen in these eight examples to have introduced no fundamental difference in the patterns of four different adults, nor has the lapse of nine years, during the period of growth from childhood to youth, done so in a fifth person. The patterns often have become broadened and variously distorted, especially in case 6; but in respect to those characteristics, on which alone I have laid any stress, there has been no change whatever.

It appears that the ridges make their first appearance in the fourth month of fœtal life, and to be fully and finally developed in the sixth month, for they then seem to possess the same degree of complexity of structure that exists in the patterns of adults. Putting all together, we may fairly infer that, from birth to death, there is no change in the fundamental characteristics of the thumb and finger patterns, nor can there be any after death up to the time when the skin perishes through decomposition.

The popular idea that has hitherto been jumped at, without adequate evidence,* is now shown to be strictly correct on very good evidence and after careful inquiry. There appear to be no means of personal identification other than deep scars and tattoo marks, comparable in their permanence and certainty with those of the thumb and finger marks. All the dimensions of the limbs alter in the slow course of growth and decay. The colour of the hair, the quality and tint of the skin, the expression of the features, the gestures, the handwriting, even the eye colour, change after many years. There seems to be no persistence anywhere in the bodily structure, except in these minute and too much disregarded papillary ridges.

Scars.

The question remains to be considered as to how far the patterns may be affected by scars, or obliterated by rough usage. I find that, of the 2500 or more persons whose thumbs have been impressed at my small Anthropometric Laboratory at South Kensington, the patterns are rarely destroyed to any considerable degree. I have to search through hundreds of thumb marks to find an instance of even a small scar. Partial obliterations are more frequent, but here, though much is lost, a sufficiency remains; and if the thumb is rolled and not only pressed, more would be available. If the fingers had been rolled in making the impressions in Plate 2, there would have been perhaps twice as many points of comparison, for the areas they represent would have been twice, or nearly twice as great as they are now, and the number of points suitable for comparison would have been proportionately increased.

Analogy between the Classes and Ordinary Genera.

We have seen that the peculiarities which distinguish the classes of the patterns are fundamentally distinct. It might thence be inferred that the class of any given pattern would be clearly distinguishable. But this is not invariably the case. A characteristic, however fundamental in its character, may be so poorly developed in a particular case, as to be overlooked, or be barely, if at all, traceable.

* Subsequent inquiry confirmed the opinion expressed in my lecture at the Royal Institution, referred to above, that an often repeated assertion to the effect that impressions of the hand are used in Chinese prisons for purposes of identification, is erroneous. The impression of the finger in China, as elsewhere in the East, is sometimes affixed to documents merely as a ceremony of personal contact, much as the witnesses in an English court of law are required to hold and to kiss the Bible on which they are sworn.

A core as in *b*2, fig. 9, belongs to a WSV—WBV class, while a core that is enclosed in a loop, as *m*, belongs to a SV—BV, or one as *u*, to a SW—BW class. But the enveloping loop may be so narrow as to be overlooked. Nay, it may consist of but a single ridge, and that ridge may not make the complete circuit, but either stop by the way or form a junction with the outer ridge of the core. Transitional cases of this sort may and do occur, and they might conceivably occur frequently.

There are perhaps no two classes that might not be in some way connected by transitional cases, though it may often be difficult to imagine how. We are not justified in denying either the possibility or the frequency of any such transitional form on purely *à priori* grounds, but must appeal to observation, which assures us that they are rare.

In order to rightly understand the degree and the way in which any class of pattern is isolated, it is necessary to study a large number of specimens, consequently, as loops are so numerous, we cannot do better than to base the discussion upon them, and learn whether or no the individual variations of loops cluster around a central or typical form, or whether they are distributed in any other way. We must study the peculiarities of the loop separately and in detail, and the best detail for our purpose is the number of ridges in AH, where H is the point in the innermost bend of the loop at which it is cut by AI (see figs. 3 and 4).

The ridges in AH are easily counted because AH cuts them squarely, owing to the construction of the figure. I took a number of specimens of loops, in the order in which they came to hand, and had the number of ridges in AH counted in each loop. (I had also, myself, previously made more than one independent trial on a considerable scale, but the specimens had not been those of a strictly random selection, and I thought best not to use them).

The ridge at A was counted as 0, the next ridge as 1, and so on up to H. Whenever the line AH passed across the neck of a bifurcation, so that there was one ridge fewer on one side of the point of intersection than on the other, there would clearly be doubt whether to reckon it as 1 or as 2 ridges. A compromise had, therefore, to be made by counting it as $1\frac{1}{2}$. After the number of ridges in AH had been counted in each case, all residual fractions of $\frac{1}{2}$ were alternately treated as 0 and as 1. In a very few cases there was doubt whether to classify a pattern that approximated to *k* 1, as a loop, the number of whose ridges in AH was 0, or even 1, or whether to consider it as a Primary, *a*.

It is more convenient to work from the results when given in the form of the percentages, which will be found in Table I., and where the number of cases from which the percentages were made is entered at the top. It is quite unnecessary to work more closely than to the nearest integer. We see at a glance that the different numbers of ridges in AH do not occur with equal frequency, that 1 ridge is a rarity, and so are cases of ridges above 15 in number, but that the cases are frequent of 7, 8, and the neighbouring numbers of ridges. There is clearly a rude sort of order in their distribution, the cases being more numerous for median values, and tailing away into nothingness at the top and bottom of the column. A vast amount of statistical analogy assures us that the orderliness of the distribution would be increased if the cases observed had been much more numerous. Later on this inference will be corroborated. There is a sharp inferior limit to the numbers of ridges, because they cannot be less than 0, but independently of this, we notice the growing infrequency of small numbers as well as of large numbers of them. There is no strict limit to the latter, but the trend of the figures convinces us that say, 40 or more ridges in AH are practically impossible. Therefore, no individual number of ridges in AH can possibly depart very widely from the observed average numbers of ridges in AH; but the range of possible departures is not sharply limited, except at its lower margin. Their possibilities are not "rounded off," to use a common but very misleading expression that is often applied to the way in which genera are isolated. The range of the possible departures in the case of genera is not suddenly and sharply restricted, but the rarity of the stragglers from the average form rapidly increases with the degree of their departure, until no more of them are met with.

TABLE I.

No. of ridges in AH.	No. of cases reduced to per cents.		VY/OI.	No. of cases reduced to per cents.		AO/AH.	No. of cases reduced to per cents.	
	Left.	Right.		Left.	Right.		Left.	Right.
	171 cases.	166 cases.		149 cases.	140 cases.		176 cases.	163 cases.
1	..	1	0·3–0·4	2	3	0·1–0·2	1	2
2	1	2	0·5–0·6	11	8	0·3–0·4	3	7
3	3	2	0·7–0·8	14	9	0·5–0·6	3	11
4	5	2	0·9–1·0	18	21	0·7–0·8	9	9
5	5	3	1·1–1·2	23	16	0·9–1·0	15	22
6	18	4	1·3–1·4	7	24	1·1–1·2	13	15
7	14	8	1·5–1·6	10	8	1·3–1·4	12	12
8	16	8	1·7–1·8	6	3	1·5–1·6	14	11
9	10	11	1·9–2·0	6	5	1·7–1·8	10	8
10	8	9	2·1–2·2	1	1	1·9–2·0	5	1
11	10	14	above	2	2	2·1–2·2	..	..
12	8	11	..	..	..	2·3–2·4	6	1
13	2	10	..	..	..	2·5–2·6	4	..
14	..	7	..	..	..	2·7–2·8	3	..
15	..	6	..	..	..	2·9–3·0	1	..
above	..	2	..	..	..	above	1	1
	100	100		100	100		100	100

It is convenient to discuss these and similar cases in the way adopted in Tables II. and III. These show how far the distribution of the observed cases conforms to the well-known theoretical law of Frequency of Error. If they conform to it fairly well, we are justified in speaking of a central or typical number of ridges in AH, and of considering any other number of ridges as a departure from that typical and central value.

Table II.

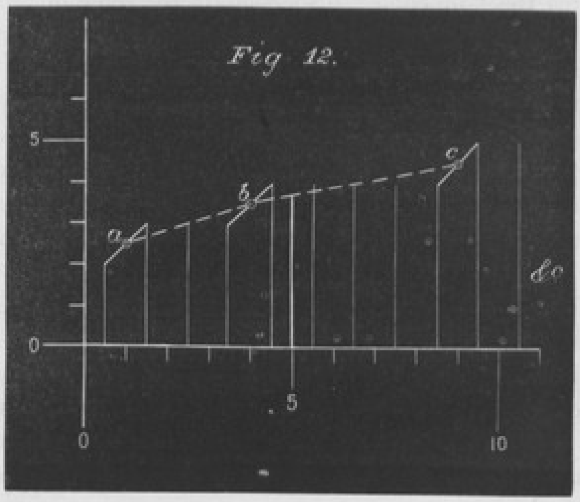
Abscissæ reckoned in centesimal parts of the interval between the limits of the scheme. 0 to 100.	Ordinates to the six schemes of Distribution, being the ordinates drawn from the base of each scheme at selected centesimal divisions of the base.											
	No. of ridges in AH.				Values of VY/OI.				Values of AO/AH.			
	Left.		Right.		Left.		Right.		Left.		Right.	
	Observed.	Calculated from M = 7·8 p.e. = 1·9	Observed.	Calculated from M = 10·4 p.e. = 2·3	Observed.	Calculated from M = 1·10 p.e. = 0·31	Observed.	Calculated from M = 1·15 p.e. = 0·25	Observed.	Calculated from M = 1·36 p.e. = 0·36	Observed.	Calculated from M = 1·08 p.e. = 0·30
5	3·8	3·2	3·8	4·8	0·49	0·35	0·54	0·54	0·58	0·48	0·36	0·32
10	4·8	4·2	5·5	6·0	0·59	0·51	0·64	0·67	0·74	0·68	0·50	0·48
20	5·8	5·4	7·3	7·5	0·78	0·71	0·85	0·84	0·96	0·91	0·66	0·67
25	6·1	5·9	7·9	8·1	0·83	0·79	0·91	0·90	1·00	1·00	0·79	0·75
30	6·4	6·3	8·5	8·6	0·89	0·86	0·99	0·95	1·04	1·08	0·87	0·82
40	7·1	7·4	9·5	9·5	1·00	0·98	1·05	1·05	1·21	1·22	0·98	0·93
50	7·8	7·8	10·5	10·4	1·10	1·10	1·15	1·15	1·37	1·36	1·04	1·05
60	8·4	8·2	11·3	11·3	1·18	1·22	1·29	1·25	1·48	1·50	1·18	1·17
70	9·3	9·3	12·1	12·2	1·32	1·34	1·33	1·35	1·66	1·64	1·31	1·28
75	9·9	9·7	12·5	12·7	1·46	1·41	1·41	1·40	1·73	1·72	1·39	1·35
80	11·0	10·2	13·0	13·3	1·53	1·49	1·45	1·46	1·90	2·81	1·48	1·43
90	11·5	11·4	14·3	14·8	1·73	1·69	1·77	1·63	2·23	2·04	1·69	1·62
95	12·2	12·2	15·0	16·0	1·80	1·85	2·00	1·76	2·48 about	2·24	1·81	1·78

Table III.

Abscissæ reckoned in centesimal parts of the interval between the limits of the curve. 0 to 100.	Ordinates to the six curves of distribution drawn from the axis of each curve at selected centesimal divisions of it. They are here reduced to a common measure, by dividing the observed deviations in each series by the probable error appropriate to the series and multiplying by 100. For the values of M, whence the deviations are measured, and for those of the corresponding probable error, see the headings to the columns in Table II.						Mean of the corresponding ordinates in the six curves after reduction to the common scale of p.e. = 100. 965 observations in all.	Ordinates to the normal curve of distribution, probable error = 100.
	No. of ridges in AH.		Values of VY/OI.		Values of AO/AH.			
	Left.	Right.	Left.	Right.	Left.	Right.		
5	−211	−291	−196	−244	−217	−230	−231	−244
10	−158	−213	−164	−204	−172	−183	−182	−190
20	−105	−135	−103	−120	−111	−130	−117	−125
25	− 84	−109	− 87	− 92	−100	− 87	− 93	−100
30	− 74	− 83	− 68	− 64	− 89	− 60	− 73	− 78
40	− 37	− 44	− 31	− 44	− 42	− 23	− 37	− 38
50	0	+ 4	0	0	0	0	+ 1	0
60	+ 31	+ 39	+ 23	+ 56	+ 33	+ 43	+ 38	+ 38
70	+ 79	+ 74	+ 68	+ 72	+ 83	+ 87	+ 77	+ 78
75	+116	+ 91	+116	+104	+103	+113	+107	+100
80	+168	+113	+138	+120	+150	+143	+139	+125
90	+200	+170	+203	+248	+242	+213	+213	+190
95	+231	+200	+225	+340	+311	+253	+260	+244

The method used here is one that I have often described, but I fear I must briefly describe it again because it is not generally understood, though it is already beginning to be used by anthropologists and others. The 100 cases (the percentages in Table I.) that refer, say, to the left thumb are entered upon a piece of paper ruled by 101 vertical lines, numbered from 0 to 100, which divide any horizontal line into 100 equal and horizontal spaces. It appears from the table that we may have to deal with various numbers of ridges from 0 up to 15, so there must be 16 horizontal lines at equal distances apart, and numbered from 0 to 15, enclosing 15 equal vertical spaces.

The table begins by telling us that out of the 100 cases there are 1 of two ridges, 3 of three ridges, 5 of four ridges, and so on. These values are entered on the ruled paper by erecting, (fig. 12,) one ordinate reaching to the second line in the middle of the



first space; three ordinates reaching to the third line and severally standing in the middle of each of the next three spaces (which, counted from the beginning, are the second, the third, and the fourth); five ordinates reaching to the fourth line and severally standing in the middle of each of the next five spaces; and so on, until all the 100 spaces have been utilized for the 100 tabular entries. Then a curve may be drawn with a free hand through the tops of the 100 ordinates, and the figure called a Scheme of Distribution is thereby produced. But there is an objection to free hand curves, in the temptation to draw them too smoothly. Therefore I do no more than unite with straight lines, as shown in fig. 12, the halfway points *a*, *b*, *c*, &c. between each successive step. The 100 ordinates have now served their purpose, and being in the way, had better be rubbed out (practically they are never drawn), leaving only the curve, the divisions between which the ordinates were or were supposed to be drawn, and the side scale.

New ordinates to the curve are now erected at the convenient divisions of the base given in the first columns of Tables II. and III. (see the broad white line corresponding to 5 in fig. 12). They are measured, and their lengths are recorded, and may at any future time be again mapped down in order to form a skeleton by which to reproduce the original scheme. The lengths of these interpolated ordinates are given in the column of Table II. headed "Observed." Being interpolations, they do not consist, except by chance, of an integral number of ridges. But fractional values are not meaningless; they have already been employed whenever AH cuts the neck of a fork.

The ordinate at the 25th division of the base, called Q_1, cuts off the lower quarter of the scheme; the ordinate at the 75th, called Q_3, cuts off the upper quarter. Half the difference between them, or $\frac{1}{2}(Q_3 - Q_1)$, is called the Quartile, and is expressed by Q. It measures the "probable" dispersion (in the sense of "probable error") of individual values from the value of $\frac{1}{2}(Q_3 + Q_1)$, which is called M′.

In a symmetrical curve M′ is identical with the ordinate at the 50th division, in other words, with the median value of all the ordinates in the series, and is called M. Further, in a symmetrical curve, the median M is identical with their arithmetic mean value. In the six different series contained in Table II., and in numerous analogous ones that I have worked out elsewhere, the values of M′ and of M are nearly identical. Whenever they differ, I have taken an intermediate value that is nearer to M than to M′. This correction has been always very trifling. The values of M and Q for each of the series with which we are concerned, are given at the heads of the second of each pair of columns in Table II.

The next step is to change from the Scheme to the Curve that bounds it; the ordinates are measured henceforth from the axis of the curve, up or down as the case may be. The axis is a line drawn parallel to the base of the scheme, which cuts the curve at the point where it was met by M; that is by the ordinate erected at the 50th division of the base.

The axis is divided into 100 divisions just like the base. The ordinates of a curve of this description, not founded on any observations, but wholly on the theoretical law of Frequency of Error, can be deduced from the well-known tables of the Probability Integral. They, and the curve itself, may be conveniently spoken of as "Normal."

The few ordinates of the normal curve with which we are concerned will be found in the last column of Table III. There the quartile (= probable error) is taken as 100 and not as 1, in order to avoid decimals in the tabular entries, which are restricted to three figures each.

When preparing to compare the ordinates of a curve drawn from observation with those of a normal curve, we must first multiply the ordinates of the normal curve, whose quartile (or probable error) = 100, by the value of the quartile of the observed curve. Or, conversely, if we wish to compare the ordinates of the normal curve whose quartile = 100, with those of the observed curve, we must first divide those of the observed curve by the value of their own quartile, and then multiply them by 100. The latter process has been adopted in Table III.

There is yet another useful step. Given the values of M and Q we may calculate the value of any ordinate in the scheme, by the help of the values of the normal ordinates to the curve given in the last column of Table III., and collate the calculated with the observed values. This has been done in Table II.

We will first consider the results shown in Table II. It is seen that the accordance between the calculated and observed number of ridges in AH, in the left and in the right thumbs severally, is respectably close. Considering the paucity of the observations, which are only 171 in the one case and 166 in the other, there is nothing in the results that contradicts the possibility of a much closer conformity when very many more observations are dealt with.

Precisely the same process has been gone through in respect to the values of the fractions of VY divided by OI (see fig. 3), which is practically the breadth of the loop divided by its length. The results are of a similar character to those yielded by the numbers of ridges in AH.

Again, I have tried the fraction of AO divided by AH, and still the results are found to be of the same kind.

Now turning to Table III. I there obtain a general average result from all of the three double sets, by an artifice. Each observed series of departures from the axis of the curves is reduced to what it would have been if the unit of the scale by which its departure had been measured, was equal to its own quartile multiplied by 100. In short, every one of the ordinates in each series was divided by the value of the quartile of that same series, and then multiplied by 100. Their average results are given in the last column but one, and the corresponding normal values in the last column. The orderly run of the figures is much closer now than it was in any one of the six separate series because they are derived from many more observations, namely, 965 of them.

We also see that though there is an obvious want of exact symmetry in the ordinates of the observed curve, their general accord with those of the normal curve is very fair. It is quite close enough to establish the general proposition that we are justified in relying upon the ideal conception of a typical form of loop, different for the two thumbs, from which individual loops differ. That the departure from the typical form is usually small, rarely rather greater, and very rarely indeed rather greater still.

It would be tedious to enumerate the many different trials that I have made for my own satisfaction, in order to assure myself that the variability of the several patterns was really of the quasi-normal kind just described. In my first trial I measured in various ways the dimensions of about 500 enlarged photographs of loops, and about as many of other patterns, and found that the measurements in each and every case formed a quasi-normal series. I do not care to submit these results, because they necessitate more explanation and analysis than the interest of the corrected results would, perhaps, justify, to eliminate from them the effect of variety of size of thumb, and some other uncertainties. Those measurements referred to some children, a few women, many youths, and a fair number of adults; and allowance has to be made for variability in stature in each of these classes.

The proportions of a typical loop are easily ascertained if we may assume that the most frequent values of its variable elements, taken separately, are the same as those that enter into the most frequent combination of the elements taken collectively. This would necessarily be true if the variability of each element separately, and that of the sum of them in combination, were all strictly normal, but as they are only quasi-normal the assumption must be tested. I have done so by making the comparisons shown in Table IV., which come out correctly to within the first decimal place.

TABLE IV.

	Left thumb.	Right thumb.
(*a*) Median of all the values of VY . . .	10·1	12·5
(*b*) Median of all the values of OI . . .	8·9	10·1
Value of *a*/*b*	1·11	1·24
Median of all the fractions VY/OI . .	1·10	1·15
(*c*) Median of all the values of AO . . .	4·6	4·6
(*d*) Median of all the values of AH . . .	3·3	4·4
Value of *c*/*d*	1·40	1·05
Median of all the fractions AO/AH . .	1·36	1·08

They show that it is practically the same thing whether we take the fraction, which is the median of all the fractions, or whether we take the fraction whose numerator is the median of all the numerators, and whose denominator is the median of all the denominators. I have used the medians here and throughout this inquiry instead of the arithmetic means, but an inference like the foregoing which is based on the medians, may be accepted without cavil as being equally true of the means.

This being premised, the proportions of the typical loop are to be taken as follows :—

	Left thumb.	Right thumb.
Length of OA in millimetres	4·6	4·6
,, OI ,,	8·9	10·1
,, OV ,,	7·6	8·3
,, OY ,,	3·1	4·2
,, AH ,,	3·3	4·4
Number of ridges in AH	7·3	9·9
Mean breadth of one of the ridge intervals in AH	0·46	0·45

As absolute measures, the above are too small for the average adult male and too large for the average adult female, but as proportions they are correct.

I do not see my way to discuss the primaries on the same general lines as the loops, because they possess no distinct points of reference. But their general appearance does not give the impression of clustering around a typical centre. They seem rather to suggest the idea of the head of a stream, that begins to diverge from the first.

As regards other patterns, I have made many measurements altogether, but the specimens of each sort were comparatively very few, except in *c* patterns. In all cases where I was able to form a well-founded opinion, the existence of a typical centre was indicated. It was not necessarily or usually the same in the two thumbs; indeed, there is a curious difference between their patterns, into which I do not propose to enter here.

There is reason to believe that the patterns are hereditary. I have no adequate amount of data whereby to test the truth of this belief by a direct inquiry, but rest the belief partly on analogy, but more especially on the ascertained existence of a considerable tendency to symmetry. When, for instance, there is a primary pattern on one thumb, there are not far from ten chances to one in favour of its been found on the other. Again, if there is a loop in one thumb, there is a strong chance that it will be found in the other thumb also. Similarly as regards each pair of corresponding fingers. Therefore the causes of the pattern must not be looked for in purely local influences. Part of the causes why it and not another pattern is present, are common to both sides of the body and may therefore be called constitutional, and be expected to be hereditary.

Accepting, then, the hypothesis that the patterns are to some extent hereditary, we possess in them an instructive instance of the effects of heredity under circumstances in which sexual selection has been neutral. The very existence of the patterns has been hitherto almost overlooked, because they are too small to attract attention, or thought too uninteresting to notice. Neither do they appear to be correlated with any desirable or repellent quality. It is true that the breadth of a ridge-interval may afford a direct indication of the delicacy or the reverse of the sense of touch, as measured by the just discernible distance between compass points, and some indirect indication of the sensibility generally. (I do not know that it is, but have planned experiments for testing the supposition.) Yet, even if so, the fact would have no bearing on the attractiveness or otherwise of any particular pattern, because the form of a pattern has nothing to do with the fineness or coarseness of the ridges that compose it. There has, therefore, been complete promiscuity of marriages, or, as it is now called, panmixia, in respect to these patterns. We might consequently have expected them to be hybridised. But that is most assuredly not the case; they refuse to blend. Their classes are as clearly separated as those of any of the genera of plants and animals, while we happen to know enough about their origin to understand that this must be the case, inasmuch as they are intrinsically different. Each of the patterns keeps as pure and distinct from the others as if they had been severally descended from a thorough-bred ancestry, each in respect to its own peculiar form.

As regards the influence of all other kinds of natural selection, we know that they co-operate in keeping races pure by their much more frequent destruction of the individuals who depart the more widely from the typical centre. But natural selection is wholly inoperative in respect to individual varieties of patterns, and unable to exercise the slightest check upon their vagaries. Yet, for all that, the different classes of patterns are isolated from one another, through the rarity of transitional cases, just as thoroughly, and just in the same way, as are the genera of plants and animals. There is no statistical difference between the form of the law of distribution of individual patterns about their respective typical centres, and that of the law by which, say, the Shrimps described in Mr. WELDON's recent memoir ('Roy. Soc. Proc.,' vol. 47, p. 445) are distributed about theirs. In both cases the distribution is in quasi-accordance with the theoretical law of Frequency of Error, and this form of distribution is caused in the case of the patterns entirely by internal conditions, and in no way by natural selection in the ordinary sense of that term.

It is impossible not to recognise the fact so clearly illustrated by these patterns in the thumbs, that natural selection has no monopoly of influence in forming genera, but that it could be wholly dispensed with, the internal conditions acting by themselves being amply sufficient to form them. When the internal conditions are in harmony with the external ones, as they appear to be in all long-established races, their joint effects will curb individual variability more tightly than either would do by itself. The normal *character* of the distribution about the typical centre will not be thereby interfered with. The probable divergence (= probable error) of an individual taken at random will be lessened, and that is all.

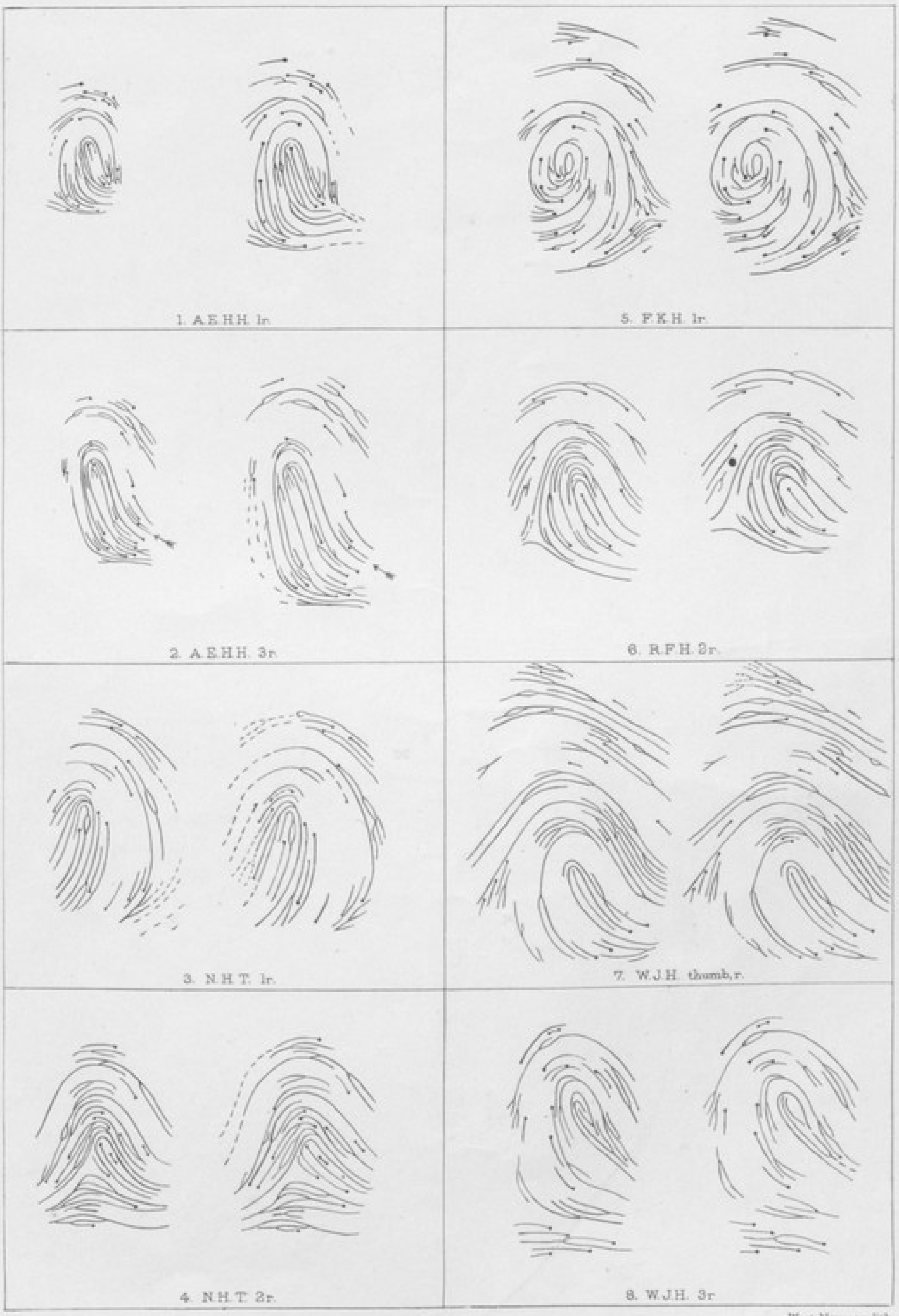
Not only is it impossible to substantiate a claim for natural selection that it is the sole agent in forming genera, but it seems, from the experience of artificial selection, that it is scarcely competent to do so by favouring mere *varieties*, in the sense in which I understand the term.

My contention is that it acts by favouring small *sports*. Mere varieties from a common typical centre blend freely in the offspring, and the offspring of every race whose *statistical* characters are constant, necessarily tend, as I have often shown, to revert towards their common typical centre. Sports do not blend freely; they are fresh typical centres or sub-species, which suddenly arise we do not yet know precisely through what uncommon concurrences of circumstance, and which observations show to be strongly transmissible by inheritance.

A mere variety can never afford a sticking point in the forward course of evolution, but each new sport implies a new condition of internal equilibrium, and does afford one. A change of type is effected, as I conceive, by a succession of sports or small changes of typical centre, each being in its turn favoured and established by natural selection to the exclusion of its competitors. The distinction between a mere variety and a sport is real and fundamental. I argued this point in a recent work ('Natural Inheritance,' Chapter III., MACMILLAN, 1889), but had then to draw my illustrations from non-physiological experiences. I could not at that time find an appropriate physiological one. The want is now excellently supplied by observations of the patterns made by the papillary ridges on the thumbs and fingers.

West, Newman, Photo. lith.

Plate, I. Eight cases in which the impression of a finger or thumb has been repeated after an interval of many years.



West, Newman, lith.

Plate II. Skeleton maps of the impressions in Plate I, showing the places where ridges begin, through bifurcation or independently.

PHILOSOPHICAL TRANSACTIONS

OF THE

ROYAL SOCIETY OF LONDON.

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[PLATES 1, 2.]

THE PATTERNS IN THUMB AND FINGER MARKS.

ON THEIR ARRANGEMENT INTO NATURALLY DISTINCT CLASSES, THE PERMANENCE OF THE PAPILLARY RIDGES THAT MAKE THEM, AND THE RESEMBLANCE OF THEIR CLASSES TO ORDINARY GENERA.

BY

FRANCIS GALTON, F.R.S.

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INDEX TO ACHIEVEMENTS

OF

NEAR KINSFOLK

OF SOME OF THE

FELLOWS OF THE ROYAL SOCIETY.

BY

FRANCIS GALTON, F.R.S.

PREFACE.

IT is now practically certain, from wide and exact observation, that the physical characters of all living beings, whether men, other animals, or plants, are subject approximately to the same hereditary laws. Also that mental qualities, such as ability and character, which are only partially measurable, follow the same laws as the physical and measurable ones.

The obvious result of this is that the experience gained in establishing improved breeds of domestic animals and plants is a safe guide to speculations on the theoretical possibility of establishing improved breeds of the human race.

It is not intended to enter here into such speculations, but to emphasise the undoubted fact that members of gifted families are, on the whole, appreciably more likely than the generality of their countrymen to produce gifted offspring.

No extensive collection exists of the biographies of Gifted Families, as distinguished from biographies of individuals; we are therefore without means of obtaining an idea of the distribution of ability in our very mixed race, incomparably more mixed than that of any domestic animal, where some conscious selection is always at work. We cannot tell, *a priori,* how far ability is sporadic at the present time, and how far it clusters in families. As a first attempt to supply the deficiency, both as to matter and form, I submit the present pamphlet, the result of inquiries made through a circular letter to all Fellows of the Royal

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Society, as to the "noteworthy" achievements of their near relatives. The standard of "noteworthiness" was defined as achievement in any occupation which was judged by the writer to be at least equal in dignity, among the fellows of the relative, to that of F.R.S. among men of science. It was the best standard I could think of; no one has as yet suggested a better, and notwithstanding its obvious faults it has served well. About half of the 454 fellows, or thereabouts, replied to my circular. Many of the replies were extremely interesting, while not a few were very jejune; still, I have collected enough material to be serviceable in many ways. I wrote a brief statistical article upon those I had received up to a certain date, in *Nature* on August 11. Evidence was there given that ability, as measured by achievement, tended in a marked degree to be a family characteristic. Besides the families so distinguished there were others reputed to have a high level of ability, whose members had nevertheless failed to achieve anything noteworthy; again, there were others in whom the ability was, in the language of horticulturists, a "sport"; it was shared by none of the collaterals or ancestry, but, presumably like all sports, may be highly capable of producing its like in descent.

The difficulty of estimating the ability of women, who have few opportunities of displaying it in a measurable way, was partly met by asking for the achievements of the brothers of the females, which are comparable on equal terms with those of the brothers of males.

Having collected and discussed my material, the question arose how best to present the results so as to bring out the fact that ability, as measured by achievement, is really clustered to a remarkable degree in certain families. Something more vivid was required than statistical figures; something in the nature of those Family Biographies above mentioned. It was, however, difficult to give them, because, although no stipulation whatever was made in the circular letter of inquiry that the replies should be treated as private documents, I found that a feeling existed that such restriction was implied. I could not disregard this view without risking the accusation of breach of trust. At length I thought of the course that has been adopted here. It is to take the replies as *guides* only, and rarely to quote from them, restricting the mention of achievements to those that have *already been published*; to extract the account of them, as a general rule, from publications where they appeared, and to give references as far as seemed reasonably desirable. The publications might be official or only local, but, as a matter of convenience, the references are in almost all cases either to the "Dictionary of National Biography" for deceased persons, and to the "Encyclopædia Britannica" or to "Who's Who" for living ones. A biography in either of the first two is in itself a mark of distinction; it is so, but in a much less degree, in "Who's Who." They all have the merit of giving detailed accounts of the achievements of the person in question, while the "Dict. N. Biog." gives full references to the memoirs and other sources whence the information in each article was derived.

The present pamphlet is styled an "Index," because it falls far short of being a collection of biographies and contains no account of failures. On the other hand, it does more than indicate families deserving of minute study, for it gives a fair idea of the quality of ability that dominates in each. This would be sufficient, if the collection were largely added to, to enable families to be sorted into different groups, according to their prevailing characteristic, each group being convenient for separate study. I could add other remarkable pedigrees from the same source, but these few will serve as a preliminary attempt to show the quality of material that exists, and a convenient form of treating it, which is the primary purpose of this small pamphlet.

The average number of kinsfolk in each degree should be borne in mind when reading the "Index." This was discussed by me in a paper in *Nature*, September 29. From that discussion I now conclude that the average numbers of near kinsmen who attain an age at which they would have achieved something noteworthy, if they possessed the necessary qualifications, would be roughly as follows:—grandfathers, 2 (1 *father's father* and 1 *mother's father*); father, 1; uncles, 2 (1 *father's brother* and 1 *mother's brother*); brothers, 1; first cousins, 4 (*see Table of Abbreviations*); making 10 altogether. Sons and nephews are rarely taken into account here, because they usually had not attained a sufficient age to enable them to do justice to their potentialities.

Persons who have earned a place, by virtue of their achievements, in the "Dict. N. Biog.," in the "Ency. Brit.," or even in "Who's Who," are so far rarer than one in ten, that the appearance of one of them within the inner degrees of kinship of Fellows of the Royal Society, would give a presumption of hereditary ability; but when, as in the families who are indexed here, an average of four of these noteworthy persons fall within those near degrees, the presumption grows into certainty.

The connection between achievement and ability is technically known as Correlation, though it be of a complex, entangled, and discontinuous kind. Still, it must be governed by the law that links every pair of systems of correlated variables. Let the members of one of the two systems be called "Subjects," and those of the other "Relatives"; then, although we can never guess beforehand what Relative will be associated with any particular Subject, we can tell something about the group of Relatives that will be associated with any considerable number of *similar* Subjects; namely, that the *average* of those Relatives will always be less exceptional than those Subjects. In other words, *very high* achievement will, on the average, be associated with only *high* ability; *high* achievement with *moderately high* ability; *average* with *average*; *low* with *moderately low*; *very low* with *low*. It is as yet impossible to say much more than this in respect to achievement and ability.

Arrangements are in progress for an inquiry into the Biographies of Modern Families, of every social grade, each of these families being distinguished, *as a whole*, for Ability, Character, or Physique. Chief among these is the following, as extracted from the *Times* of October 27 :—

London, October 26.—At their meeting this afternoon the Senate had before them, and on the recommendation of the Academic Council accepted, an offer from Mr. Francis Galton, F.R.S., to endow a Fellowship in the University for the promotion of the study of "National Eugenics," defined as "the study of the agencies under social control that may improve or impair the racial qualities of future generations either physically or mentally." The person appointed to this Fellowship will be required to devote the whole of his time to the study of the subject, and in particular to carry out investigations into the history of classes and families, and to deliver lectures and publish memoirs on the subject of his investigations. The endowment is sufficient to provide not only for the Fellowship, but also for the salary of an assistant, and for the general expenses of the contemplated work, which it is intended to place in one of the colleges or other institutions connected with the University. Full particulars of the post will be published shortly.

Many persons have expressed interest in the progress of inquiries of this character. I hope, therefore, that some may be disposed to assist actively in procuring and sending information. Blank forms to receive the entries will be sent on application.

It will be assumed that free use may be made of the information that is furnished, unless otherwise stated.

FRANCIS GALTON.

42 RUTLAND GATE, S.W.
October, 1904.

TABLE OF ABBREVIATIONS.

Males		Females	
Grandfather paternal	*fa fa*	Grandmother paternal	*fa me*
,, ,, maternal	*me fa*	,, ,, maternal	*me me*
Father	*fa*	Mother	*me*
Uncle paternal	*fa bro*	Aunt paternal	*fa si*
,, maternal	*me bro*	,, maternal	*me si*
Brother	*bro*	Sister	*si*
Son	*son*	Daughter	*da*
Nephew brother's side	*bro son*	Niece brother's side	*bro da*
,, sister's side	*si son*	,, sister's side	*si da*
Male first cousins—		Female first cousins—	
1 son of paternal uncle	*fa bro son*	1 dau. of paternal uncle	*fa bro da*
2 son of maternal uncle	*me bro son*	2 dau. of maternal uncle	*me bro da*
3 son of paternal aunt	*fa si son*	3 dau. of paternal aunt	*fa si da*
4 son of maternal aunt	*me si son*	4 dau. of maternal aunt	*me si da*

The kinships are reckoned from the person mentioned in the heading to the list, whom we may call P. Then *fa bro* means " P's *father's brother* is "; *me si son* means " P's *mother's sister's son* is."

INDEX TO ACHIEVEMENTS OF NEAR KINSFOLK OF SOME OF THE FELLOWS OF THE ROYAL SOCIETY.

Rt. Hon. Charles **Booth,** P.C., F.R.S. (*b,* 1840, economist and statistician; president of the R. Statistical Society, 1892–4. Originated and carried through a cooperative inquiry in minute detail into the houses and occupations of the inhabitants of London, which resulted in the volumes "Life and Labour of the People of London"; author of memoirs on allied subjects.—["Ency. Brit.," 26, 306; "Who's Who."]

fa fa, Thomas **Booth,** successful merchant and ship-owner at Liverpool.

fa bro, Henry **Booth** (1788–1869), railway projector, co-operated with Stephenson in applying steam to locomotion, published much relating to railways, and invented mechanical contrivances still in use on railways; secretary and then railway director.—["Dict. N. Biog.," 5, 382.]

fa bro, James **Booth,** C.B. (1796–1880), Parliamentary draughtsman; became permanent secretary to the Board of Trade.

me si son, Charles **Crompton,** fourth wrangler, Q.C., and for some years M.P. for the Leek Division of Staffordshire (*see* p. 17).

me si son, Henry **Crompton,** a leader in the Positivist Community; authority on Trades Union law, and author of "Industrial Conciliation" (*see* p. 17).

me si son, Sir Henry Enfield **Roscoe,** F.R.S., *q.v.*

Sir John Scott **Burdon-Sanderson,** Bart., *cr.* 1899, M.D., D.C.L., LL.D., D.Sc., F.R.S.; held a succession of important offices, beginning with Inspector med. dep. Privy Council, 1860–65; superintendent Brown Institution, 1871–78; professor of physiology University Coll., London, 1874–82; in Oxford, 1882–95; president Brit. Assoc., 1893; regius professor of medicine at Oxford, 1895–1904; served on three Royal Commissions; author of many physiological memoirs.—["Ency. Brit.," 26, 464; "Who's Who."]

fa fa, Sir Thomas **Burdon,** Kt., several times Mayor of Newcastle, knighted for his services in quelling a riot.

me fa, Sir James **Sanderson,** Bart., M.P., Lord Mayor of London; a successful merchant.

fa, Richard **Burdon-Sanderson,** fellow of Oriel College, Oxford; graduated first class and gained Newdigate prize; was secretary to Lord Chancellor Eldon.

bro, Richard **Burdon-Sanderson,** the first promoter of the "conciliation board" of coal owners and colliers at Newcastle-on-Tyne, and of the first reformatory in Northumberland.

si son, Rt. Hon. Richard Burdon **Haldane,** P.C., M.P., high honours at Edinburgh and three other Scotch universities. Author of "Life of Adam Smith" and of memoirs on education.—["Who's Who."]

si son, John Scott **Haldane,** M.D., F.R.S. (*b.* 1860), university lecturer on physiology at Oxford; joint editor and founder of *Journal of Hygiene.*—["Who's Who."]

si da, Elisabeth Sanderson **Haldane,** "Life of Professor Ferrier" and other works; promoter of education and of reforms in Scotland.

More distant kinsmen and connections.

fa me bro, John **Scott,** first Earl of **Eldon** (1751–1838), famous Lord Chancellor of England.—["Dict. N. Biog.," 51, 49.]

fa me bro, William **Scott,** first Baron **Stowell** (1745–1836), eminent maritime and international lawyer; judge of High Court of Admiralty, 1798–1828.—[" Dict. N. Biog.," 51, 108.]

wife's bro, **Farrer,** first Lord **Herschell,** Lord Chancellor of England.

Charles Robert **Darwin,** F.R.S. (1809–1882), the celebrated naturalist. The dates of his works are " Voyage of the *Beagle*," 1840; " Origin of Species," 1859; followed by a succession of eight important volumes ranging from 1862 to 1881, each of which confirmed and extended his theory of descent. Among the very numerous biographical memoirs it must suffice here to mention " Life and Letters," by Francis Darwin; and " Dict. N. Biog.," 14, 72.

fa fa, Erasmus **Darwin,** M.D., F.R.S. (1731–1802), physician, poet and philosopher. Author of " Botanic Garden," " Zoonomia," and other works, in which he maintained a view of evolution subsequently expounded by Lamarck.—[" Life," by Ch. Darwin, " Dict. N. Biog.," 14, 84.]

fa, Robert Waring **Darwin,** M.D., F.R.S. (1766–1848), sagacious and distinguished physician, described by his son, Charles R. Darwin, as " the wisest man I ever knew."—[" Life and Letters of Charles Darwin," 1, 10–20.]

fa bro, Charles **Darwin** (1758–1778), of extraordinary promise, gained first gold medal of Æsculapian Society for experimental research; died from a dissection wound, aged twenty; many obituary notices.—[" Life and Letters of Charles Darwin," 1, 7.]

bro, Erasmus **Darwin**; see Carlyle's inexact description, and the appreciations of him by his brother and others, in " Life and Letters of Charles Darwin," 1, 21–25.

fa, ½si son, Francis **Galton,** F.R.S. (*b.* 1822), traveller and biometrician; gold medal R. Geograph. Soc., 1853; Royal medal, 1886, and Darwin medal, 1902, of the Royal Society.—[" Ency. Brit.," 28, 578; " Who's Who."]

me fa, Josiah **Wedgwood,** F.R.S. (1730–1795), the

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famous founder of the pottery works.—["Dict. N. Biog.," 60, 140.]

me bro, Thomas **Wedgwood** (1771–1805), an experimenter in early life, and in one sense the first to create photography; a martyr to ill-health later. Sydney Smith knew "no man who appeared to have made such an impression on his friends," and his friends included many of the leading intellects of the day.—["Dict. N. Biog.," 60, 146.]

wife's fa fa (she was her husband's *fa bro dau*), Josiah **Wedgwood,** F.R.S.; see above.

wife's bro, Hensleigh **Wedgwood** (1803–1891), author of "Entomological Dictionary" and other works, partly mathematical.—["Dict. N. Biog.," 60, 140.]

wife's bro dau, Julia **Wedgwood,** essayist.

son, Francis **Darwin,** F.R.S. (*b.* 1848), botanist; biographer of his father; reader in botany at Cambridge, 1876–1903; foreign sec. Royal Society. Author of botanical works and memoirs.—["Who's Who."]

son, George **Darwin,** F.R.S. (*b.* 1845), second wrangler, 1868; Plumian professor of astronomy and experimental philosophy, Cambridge. Author of many papers in the *Philosophical Transactions* relating to tides, physical astronomy, and cognate subjects; president elect of British Association in 1905 at Cape Town.—["Who's Who."]

son, Horace **Darwin,** F.R.S. (*b.* 1851), engineer and mechanician; joint founder of the Cambridge Scientific Instrument Company and its proprietor, but now a limited company, of which he is chairman.—["Who's Who."]

son, Major Leonard **Darwin,** late R.E., second in the examination of his year for Woolwich; served on several scientific expeditions, including transit of Venus of 1874 and 1882; Staff Intelligence Dep. War Office, 1885–90; M.P. for Lichfield, 1892–95. Author of "Bimetallism," "Municipal Trade."—["Who's Who."]

Sir Victor A. Haden **Horsley,** F.R.S., M.D. (*b.* 1857), eminent surgeon and operator; professor-superintendent of Brown Institution, 1884–90; professor of pathology University College, 1893–96.

fa fa, William **Horsley,** Mus. Bac., Oxford (1774–1858), musical composer, especially of glees, and writer on musical topics.—[" Dict. N. Biog.," and Grove's " Dict. of Music."]

me fa, Charles Thomas **Haden,** a rising London physician, who initiated a treatment for gout, much noted at the time; *d.* young in 1823.—[Unpublished information.]

fa, John Callcott **Horsley,** R.A., distinguished painter.—[" Who's Who."]

fa bro, Charles Edward **Horsley** (1822–1876), musical composer of oratorios; best known in America. Author of " Text-book of Harmony."—[" Dict. N. Biog.," 27, 381, and Grove's " Dict. of Music."]

me bro, Sir F. Seymour **Haden** (*b.* 1818), surgeon; a well known sanitarian, especially in respect to the disposal of the dead, and artist in respect to etching; founder and president of the R. Society of Painter Etchers; Grand Prix, Paris, 1889 and 1900; many publications.—[" Who's Who."]

fa si son, Isambard **Brunel,** Chancellor to the Diocese of Ely; ecclesiastical barrister.

Ancestors in more remote degrees.

fa me fa, John Wall **Callcott** (1766–1821), composer, mainly of glees and catches; published " Musical Grammar," 1806.—[" Dict. N. Biog.," 8, 256, and Grove's " Dict. of Music."]

fa me fa bro, Sir Augustus Wall **Callcott,** R.A. (1779–1844), distinguished painter, mainly of landscapes; knighted, 1837.—[" Dict. N. Biog.," 8, 256.]

me fa fa, Thomas **Haden,** the principal doctor in Derby, and of great influence in the town; was three times mayor.—[Unpublished information.]

wife, née **Bramwell.**

wife's fa, Sir Frederick **Bramwell,** Bart., F.R.S. (1818–1903), eminent engineer; president British Association, 1888; of Institution of Civil Engineers, 1884–5; hon. sec. Royal Institution.—[" Who's Who."]

wife's fa bro, Lord **Bramwell** (1808–1892), Judge, 1856; Lord Justice, 1876–81; raised to peerage, 1882.—[" Dict. N. Biog.," Supp. 1, 256.]

me si son, Sir Joseph Dalton **Hooker,** G.C.S.I., F.R.S., and pres. R.S., 1872–77 (*b.* 1817), eminent botanist and traveller; director of the Royal Gardens, Kew, 1855–65; naturalist to H.M.S. *Erebus* in Antarctic expedition, 1839–43; botanical travels in the Himalaya, 1847–51; Morocco and Atlas in 1871; California and Rocky Mts., 1877; many botanical publications.—[" Ency. Brit.," 29, 324; " Who's Who."]

me fa, Dawson **Turner,** F.R.S. (1775–1858), see **Palgrave.**

fa, Sir William Jackson **Hooker,** F.R.S. (1785–1865), eminent botanist; director of Kew Gardens, which he greatly extended and threw open to the public, and where he founded the museum of economic botany; was regius professor of botany at Glasgow, 1820; knighted in 1847; many botanical publications.—[" Dict. N. Biog.," 27, 296.]

me si sons, the four brothers **Palgrave** (see **Palgrave**).

Sir Clements R. **Markham,** K.C.B., F.R.S. (*b.* 1830), president for many years of the R. Geograph. Society; served in Arctic expedition, 1850–51; travelled in Peru, 1852–4, bringing thence cinchona-bearing trees for cultivation in India; Geographer to the Abyssinian expedition; author and editor of numerous geographical works.—[" Ency. Brit.," 30, 544; " Who's Who."]

fa fa, William **Markham** (1760–1815), scholar; secretary to Warren Hastings in India.

fa bro son, Lieut.-General Sir Edwin **Markham,** R.E., K.C.B. (*b.* 1833), constant active service.—[" Who's Who."]

fa bro son, Admiral Sir Albert **Markham,** K.C.B. (*b.* 1841), commander of the *Alert* in Arctic Expedition, 1875–6; various high naval appointments, besides unprofessional work when unemployed on naval duties.—[" Who's Who."]

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me bro son, Rt. Hon. Sir Frederick **Milner,** Bart., P.C. (*b.* 1849), politician.—[" Who's Who."]

me si son, Rt. Hon. Francis **Foljambe,** P.C. (*b.* 1830), politician.—[" Who's Who."]

me si son, Rt. Hon. Sir Edwin **Egerton,** P.C., G.C.M.G. (*b.* 1841), Ambassador at Madrid, recently transferred to Rome.—[" Who's Who."]

More distant kinsmen.

fa fa fa, William **Markham,** P.C. (1719–1807), Archbishop of York; one of the best scholars of the day; headmaster of Westminster School, 1753–65; Dean of Christ Church; preceptor to the Royal Princes, 1771; Archbishop and Lord High Almoner, 1777.—[" Dict. N. Biog.," 36, 172.]

fa fa bro, Admiral John **Markham** (1761–1827), many services at sea; twice on Admiralty Board; M.P. for Portsmouth during seventeen years; proposed and carried appointment of Commission on dockyard abuses, 1806.—[" Dict. N. Biog.," 36, 171.]

fa fa bro, George **Markham** (1763–1823), Dean of York; scholar and numismatist.

Robert Harris Inglis **Palgrave,** F.R.S. (*b.* 1827), economist and statistician; editor of the " Economist," also of " Dictionary of Political Economy."—[" Who's Who."]

me fa, Dawson **Turner,** F.R.S. (1775–1858), botanist and antiquary.—[" Dict. N. Biog.," 57, 334.] His *fa bro,* Joseph **Turner,** was senior wrangler 1768.

fa, Sir Francis **Palgrave** (1788–1861) (son of Meyer **Cohen,** adopted the name Palgrave in 1823), historian; deputy keeper H.M. Records; assisted in their publication. Author of the " Rise and Progress of the English Commonwealth," 1832; " History of England and Normandy," 1851; and other works; greatly promoted study of mediæval history; knighted, 1832.—[" Dict. N. Biog.," 43, 107.]

me, Elizabeth, *née* Dawson **Turner,** greatly assisted her husband in his literary work.—[Unpublished information.]

me bro, Dawson William **Turner** (1815–1885), philanthropist and educational writer; Demy of Magdalen College, Oxford, D.C.L., 1862.

bro, Francis Turner **Palgrave** (1824–1897), poet and art critic; first class *lit. hum.*; prof. of poetry at Oxford; editor of "Golden Treasury"; author of many critical essays and other publications.—[" Dict. N. Biog.," Supp. 3, 242.]

bro, W. Gifford **Palgrave** (1826–1888), traveller and diplomatist; at twenty years of age gained first class *lit. hum.* at Oxford, and second class *math.*; became Roman Catholic, and travelled as Jesuit missionary in Syria and Arabia, assuming disguise for the purpose. Author of "A Year's Journey through Eastern and Central Arabia." Severed his connection with the Jesuits in 1865, and thenceforward served as English diplomatist in various distant countries.—[" Dict. N. Biog.," 43, 109.]

bro, Sir Reginald F. D. **Palgrave,** K.C.B. (1829–1904), Clerk of the House of Commons. Author of "Oliver Cromwell the Protector," &c.—[" Who's Who."]

me si son, Sir Joseph Dalton **Hooker,** F.R.S. (see separate genealogy above).

Sir Henry Enfield **Roscoe,** F.R.S., Ph.D., LL.D., D.C.L., professor of chemistry Owens College, Manchester, 1857–87; president Society of Chemical Industry, 1881; of Chemical Society, 1882; M.P. for S. division of Manchester, 1885–95; president of Brit. Assoc., 1887; Vice-Chancellor of the University of London, 1896–1902; knighted, 1884; author of many memoirs and works on chemistry.—[" Who's Who."]

fa fa, William **Roscoe** (1753–1831), historian, poet, and philanthropist; author of "Lives of Lorenzo de' Medici and of Leo X.," and of several volumes of verse; M.P. for Liverpool, 1806–7; promoter and first president of its Royal Institution.—[" Dict. N. Biog.," 49, 222.]

fa, Henry **Roscoe** (1800–1836), biographer, including life of his father.—[" Dict. N. Biog.," 49, 221.]

fa bro, Thomas **Roscoe** (1791–1871), miscellaneous writer and translator.—[" Dict. N. Biog.," 49, 222.]

fa bro, William Stanley **Roscoe,** poet.—[" Dict. N. Biog.," 49, 225.]

fa bro, Robert **Roscoe,** poet, "King Alfred."

me, Maria, *née* **Fletcher,** artist and authoress, "Life of Vittoria Colonna."

me si, Harriet **Fletcher,** authoress of "Tales for Children."

fa bro son, William Caldwell **Roscoe** (1822–59), poet and essayist.—["Dict. N. Biog.," 49, 225.]

fa si son, William Stanley **Jevons,** F.R.S. (1835–1882), economist and logician; professor of logic and political economy at Owens College, 1866–79; at University College, London, 1876–80; influential writer.—["Dict. N. Biog.," 29, 374.]

me si son, Rt. Hon. Charles **Booth,** P.C., F.R.S., *q.v.*

me si son, Charles **Crompton** (*see* p. 9).

me si son, Henry **Crompton** (*see* p. 9).

Lieut.-General Sir Richard **Strachey,** R.E. (retired 1875), G.C.S.I., F.R.S., LL.D. Camb. Sec. Govt. Central Provinces of India during mutiny, 1857–8; public-works Sec. to Govt. of India, 1862; legislative member of Gov. Gen.'s Council, 1869–70; Member of Council of India, 1875–89; acting financial member of Gov. Gen.'s Council, 1878; chairman of East Indian Rly. from 1889; chairman of Meteorol. Council from 1883; pres. R. Geograph. Soc., 1888–90; royal medal of Royal Society, 1897. *Publications:*—"Lectures on Geography"; "Finances and Public Works of India" (jointly with his brother, Sir John S.); various scientific memoirs.—["Ency. Brit.," 33, 1; "Who's Who."]

Noteworthy kinsfolk in near degrees, ascending and collateral.

fa fa, Sir Henry **Strachey** (1736–1810), private sec. to Lord Clive in India; joint under-sec. of state for the Home department, 1782; *cr.* baronet, 1801; F.S.A.—["Dict. N. Biog.," Supp. 3, 364.]

me fa, Lieut.-Gen. **Kirkpatrick** (1754–1812), orientalist; military sec. to Marquess Wellesley; Resident at Poona;

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translated Persian works, expert in Oriental tongues and in manners, customs and laws of India.—[" Dict. N. Biog.," 31, 222.]

fa, Edward **Strachey** (1774–1832), chief examiner of correspondence to the India House, the other two being Peacock and James Mill (secretaries' work, writing despatches, &c.).

fa bro, Sir Henry **Strachey,** Bart. (1772–1858), distinguished Indian civilian, described by James Mill (" Hist. Brit. India," 6, ch. 6) as " the most intelligent of the Company's servants."

fa bro, Richard **Strachey,** Resident at Lucknow and Gwalior.

me si, Isabella Barbara **Buller,** well known in her day as a centre of literary and political society.

bro, Sir John **Strachey,** G.C.S.I., eminent Indian states man; Lieut.-Governor of the N.W. Provinces; financial member of Gov. Gen.'s council; Member of Council of India. *Publications*:—" Finance and Public Works of India," 1882 (jointly with his brother, Sir Richard **S.**); " Hastings and the Rohilla War," 1892; " India," 1888, third ed., 1903.—[" Ency. Brit.," 33, 1; " Who's Who," 1904.]

bro, Colonel Henry **Strachey,** Thibetan explorer, gold medal of R. Geograph. Soc., 1852.

bro, Sir Edward **Strachey,** Bart. (*d.* 1904), author of " Hebrew Politics in the Time of Sargon and Sennacherib."

bro, George **Strachey** (1873–90), Chargé d'Affaires and Minister Resident at Dresden.

bro son, Sir Arthur **Strachey** (1858–1901) [son of Sir John **S.** and of Katherine, dau. of George **Batten**], Chief Justice Allahabad *æt.* 39, *d. æt.* 43.

bro son, John St. Loe **Strachey** (*b.* 1860) [son of Sir Edward **S.** and Mary, sister of John Addington **Symonds,** writer and critic], editor of the *Spectator*.—[" Who's Who."]

me si son, Charles **Buller** (1806–1848), distinguished politician, sent as secretary with Lord Durham to Canada, 1838, Chief Poor-law Commissioner.—[" Dict. N. Biog.," 7, 246.]

me si son, Sir Arthur **Buller,** judge of the Supreme Court, Calcutta.

Noteworthy kinsfolk in more remote degrees of ancestry.

fa fa bro, John **Strachey**, Archdeacon of Suffolk, Prebendary of Llandaff, preacher at the Rolls, LL.D. Camb., F.S.A.

fa fa fa fa, John **Strachey**, F.R.S. (1671–1743), geologist, said to have first suggested theory of stratification in his work "Observations on Different Strata of Earths and Minerals," 1727.—["Dict. N. Biog.," Supp. 3, 364.]

Wife, and her kinsfolk, ascending and collateral.

wife, Jane Maria, *née* **Grant,** 2nd wife, authoress of "Lay Texts," "Poets on Poets," "Memoirs of a Highland Lady," &c.—["Who's Who," 1904.]

wife's fa fa, Sir J. P. **Grant** (1774–1848), Chief Justice of Supreme Court of Calcutta.—["Dict. N. Biog.," 22, 398.]

wife's fa, Sir J. P. **Grant,** G.C.M.G., K.C.B. (1807–1893), Indian and Colonial Governor; Member of Council; Lieut.-Governor of Central Provinces of India; Lieut.-Governor of Bengal; Governor of Jamaica (1866–1873).—["Dict. N. Biog.," Supp. 3, 341.]

wife's me bro son, Sir Trevor Chichele **Plowden,** K.C.S.I., Resident at Kashmir, Hyderabad and Baghdad.

wife's me bro son, Sir Henry Meredith **Plowden,** Senior Judge of chief court, Punjab (1880–94).—["Who's Who," 1904.]

Descendants.

son, Giles Lytton **Strachey**, scholar Trin. Coll., Cambridge, Chancellor's medal for English verse.

son, Oliver **Strachey**, Eton scholarship.

son, James Beaumont **Strachey**, scholarship at St. Paul's School.

da, Joan Pernel **Strachey,** lecturer on old French at Royal Holloway College.

da, Marjorie Colvile **Strachey**, prize offered in 1904 by the British Ambassador in Paris to all undergraduates, male and female, of all colleges in Great Britain for examination in French; scholarship Royal Holloway College in 1904.

R. CLAY AND SONS, LTD., BREAD ST. HILL, E.C., AND BUNGAY, SUFFOLK.

FRANCIS GALTON.

LIST OF

HIS PRINCIPAL BOOKS AND MEMOIRS.

DATES OF AWARDS AND DEGREES,

OFFICES AND BIOGRAPHICAL EVENTS.

PRIVATELY PRINTED

1903.

PUBLICATIONS.

BOOKS.

Tropical South Africa (Murray), 1853; second edition (Ward Lock & Co., Minerva Press), 1889.

Art of Travel (Murray), 1855, and subsequent editions.

Vacation Tourists, Edited and containing two Memoirs by F. G. (Macmillan), 1860–3.

Meteorographica (Macmillan), 1863.

Hereditary Genius (Macmillan), 1869; second edition, 1892.

English Men of Science, their Nature and Nurture (Macmillan), 1874.

Human Faculty (Macmillan), 1883.

Record of Family Faculties (Macmillan), 1884; published in connection with an offer of Prizes.

Life History Album (Macmillan), 1884; second edition, 1903.

Natural Inheritance (Macmillan), 1889,

Finger Prints (Macmillan), 1893.

Blurred Finger Prints (Macmillan), 1893.

Finger Print Directory, 1895.

Consulting Editor of Biometrika, 1901– .

MEMOIRS.

TRAVEL.

Recent Expedition into the Interior of South-western Africa, "Geogr. Soc. Journ.," XXII., 1852, pp. 140–162.

The Exploration of Arid Countries, "Geogr. Soc. Proc.," II., 1858, pp. 60–77.

The Equipment of Exploring Expeditions now and Fifty Years Ago, "Brit. Assoc. Rep.," 1881, pp. 738–740.

GEOGRAPHY.

Stereoscopic Maps, taken from models of mountainous countries, "Geogr. Soc. Journ.," 1865, pp. 99–106.

Address to the Geographical Section of the British Association, "Brit. Assoc. Rep.," XLII., 1872 (Sect.), pp. 198–203.

Construction of Isochronic Passage-Charts, "Brit. Assoc. Rep.," 1881, pp. 740, 741; "Geogr. Soc. Proc.," 3, 1881, pp. 657, 658.

METEOROLOGY.

Meteorological Charts, "Phil. Mag.," XXII., 1861, pp. 34, 35.

A Development of the Theory of Cyclones (Anticlones), "Roy. Soc. Proc.," XII., 1862–63, pp. 385, 386.

Conversion of Wind-Charts into Passage-Charts, "Brit. Assoc. Rep.," XXXVI., 1866 (Sect.), pp. 17–20; "Phil. Mag.," XXXII., 1866, pp. 345-350.

Employment of Meteorological Statistics in determining the best course for a ship whose sailing qualities are known, "Roy. Soc. Proc.," XXI., 1873, pp. 263–274.

Barometric Predictions of Weather, "Brit. Assoc. Rep.," XL., 1870 (Sect.), pp. 31–33.

Determining the Heights and Distances of Clouds by their Reflections in a low Pool of Water, and in a Mercurial Horizon, "Brit. Assoc. Rep.," 1880, pp. 459–461.

Apparatus for the Rapid Verification of Thermometers; now in use at the Kew Observatory [1877], "Roy. Soc. Proc.," 26, 1878, pp. 84–89; "Phil. Mag.," 4, 1877, pp. 226–231.

OPTICAL, &c.

A Hand Heliostat, for the purpose of Flashing Sun Signals, from on board ship or on land, in Sunny Climates, "Brit. Assoc. Rep.," 1858 (Pt. 2), pp. 15–17; "Geogr. Soc. Proc.," IV., 1860, pp. 14–19.

Spectacles for Divers, and the Vision of Amphibious Animals, "Brit. Assoc. Rep.," XXXV., 1865 (Sect.), pp. 10, 11.

Intelligible Signals between Neighbouring Stars (or other inaccessible stations whose inhabitants had no common language), "Fortnightly Review," November, 1896.

On a New Principle for the Protection of Riflemen [based on the trajectory of the spherical bullets then in use], "United Service Journ.," IV., 1861, pp. 393–396.

PHOTOGRAPHICAL.

Composite Portraits, made by combining those of many different persons into a single resultant figure [1878], "Anthropol. Instit. Journ.," 8, 1879, pp. 132–142; "Nature," 18, 1878, pp. 97–100; "Revue Scientif.," 15, 1879, pp. 33–38.

Inquiry into the Physiognomy of Phthisis by the Method of Composite Portraiture (in connection with Dr. Mahomed), "Guy's Hospital Reports," Vol. XXV.

Analytical Photography, "Nature," 62, 1900, p. 320; "Photogr. Soc. Journ." (New Series), 25, 1900–1901, pp. 135–138.

A Rapid - View Instrument for Momentary Attitudes, "Nature," 26, 1882, pp. 249–251.

Conventional Representation of the Horse in Motion, "Nature," 26, 1882, pp. 228, 229.

Photographic Record of Pedigree Stock, "Brit. Assoc. Rep.," 1898, pp. 597–603 (wrongly indexed as p. 567).

Photographic Measurement of Horses and other Animals, "Nature," 57, 1898, pp. 230–232.

STATISTICAL.

On an Error in the usual method of obtaining Meteorological Statistics of the Ocean, "Brit. Assoc. Rep.," XXXVI., 1866 (Sect.), pp. 16, 17.

Statistics by Intercomparison, with Remarks on the Law of Frequency of Error, "Phil. Mag.," 49, 1875, pp. 33–46.

The Geometric Mean in Vital and Social Statistics, "Roy. Soc. Proc.," 29, 1879, pp. 365–367.

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Anthropometric Per-Centiles, "Nature," 31, 1885, pp. 223–225.

A New Step in Statistical Science, "Nature," 51, 1895, p. 319.

Dice for Statistical Experiments, "Nature," 42, 1890, pp. 13, 14.

Correlations and their Measurement chiefly from Anthropometric Data, [1888], "Roy. Soc. Proc.," XLV., 1889, pp. 135–145.

The Median Estimate, "Brit. Assoc. Rep." (Dover), 1899, pp. 638–640.

A Geometric Determination of the Median Value of a System of Normal Variants, from two of its Centiles, "Nature," 61, 1900, pp. 102–104.

Results derived from the Natality Table of Kőrösi, by employing the Method of Contours, or Isogens, "Roy. Soc. Proc.," LV., 1894, pp. 18–23.

First and Second Prizes, "Biometrika," I., 1901–1902, pp. 385–390.

A Plausible Paradox in Chances, "Nature," 49, 1894, pp. 365, 366.

Terms of Imprisonment (Distribution of Sentences), "Nature," 52, 1895, pp. 174–176.

HEREDITY.

Experiments in Pangenesis, by breeding from rabbits of a pure variety, into whose circulation blood taken from other varieties had previously been largely transfused, "Roy. Soc. Proc.," XIX., 1871, pp. 393–410; "Ann. Mag. Nat. Hist.," VII., 1871, pp. 372–388.

The History of Twins, as a Criterion of the Relative Powers of Nature and Nurture [1875], "Anthropol. Instit. Journ.," 5, 1876, pp. 391–406.

Short Notes on Heredity, &c., in Twins [1875], "Anthropol. Instit. Journ.," 5, 1876, pp. 324–329.

Family Likeness in Stature, with an Appendix by J. D. Hamilton Dickson, "Roy. Soc. Proc.," XL., 1886, pp. 42–73.

Family Likeness in Eye-Colour, "Roy. Soc. Proc.," XL., 1886, pp. 402–416.

Hereditary Colour in Horses, "Nature," 56, 1897, pp. 598, 599.

The American Trotting-Horse, "Nature," 28, 1883, p. 29.

An Examination into the Registered Speeds of American Trotting-Horses, with Remarks on their Value as Hereditary Data, "Roy. Soc. Proc.," LXII., 1898, pp. 310–315; "Nature," 57, 1898, pp. 333, 334.

Pedigree Stock Records, "Brit. Assoc. Rep." (Dover), 1899, pp. 424–430.

Pedigree Moth-Breeding as a means of Verifying certain Important Constants in the General Theory of Heredity, "Trans. Entomol. Soc. London," 1887, pp. 19–28.

Three Generations of Lunatic Cats, "Spectator," April 11, 1896.

HEREDITY, THEORIES OF.

On Blood Relationship, "Roy. Soc. Proc.," XX., 1872, pp. 394–402.

A Theory of Heredity [1875], "Anthropol. Instit. Journ.," 5, 1876, pp. 329–348; "Revue Scientif.," 10, 1876, pp. 198–205.

Typical Laws of Heredity [1877], "Roy. Instit. Proc.," 8, 1879, pp. 282–301; "Nature," 15, 1877, pp. 492–495, 512–514, 532, 533; "Revue Scientif.," 13, 1877, pp. 385–394.

Discontinuity in Evolution, Mind, 3 (new series), 1894, No. 11.

The Average Contribution of each several Ancestor to the Total Heritage of the Offspring, "Roy. Soc. Proc.," LXI., 1897, pp. 401–413.

A New Law of Heredity, "Nature," 56, 1897, pp. 235–237.

Rate of Racial Change that accompanies Different Degrees of Severity in Selection, "Nature," 55, 1897, pp. 605, 606.

Retrograde Selection, "Gardeners' Chronicle," May 15, 1897.

The Possible Improvement of the Human Breed under the Existing Conditions of Law and Sentiment (Huxley Lecture of the Anthropological Institute), "Nature," 64, 1901, pp. 659–665; "Smithsonian Institution Report," 1901, pp. 523–538.

Arithmetic Notation of Kinship, "Nature," 28, 1883, p. 435.

A Diagram of Heredity, "Nature," 57, 1898, p. 293.

Pedigrees, "Nature," 67, 1903, pp. 586, 587.

f. 4v

Address to the Department of Anthropology of the Brit. Assoc., Plymouth, 1877; [On the Study of Types (or Groups) of Men], "Brit. Assoc. Rep.," 1877 (Sect.), pp. 94–100; "Nature," 16, 1877, pp. 344–347; "Revue Scientif.," 13, 1877, pp. 494–498.

Address to the Anthropological Section of the British Association, Aberdeen, 1885; [On Inheritance and Regression], "Brit. Assoc. Rep.," 1885, pp 1206–1214; "Anthropol. Instit. Journ.," 15, 1886, pp. 246–263.

On the Probability of the Extinction of Families [1874], "Anthropol. Instit. Journ.," 4, 1875, pp. 138–144 [in association with Rev. H. W. Watson].

ANTHROPOMETRY.

Whistles for Determining the Upper Limits of Audible Sound in Different Persons. South Kensington Conferences, Volume on "Chemistry, Biology, &c.," 1876, p. 61.

Hydrogen Whistles, "Nature," 27, 1883, pp. 491, 492; "Nature," 28, 1883, p. 54.

Apparatus for Testing the Delicacy of the Muscular and other Senses in Different Persons [1882], "Anthropol. Instit. Journ.," 12, 1883, pp. 469–475.

Anthropometric Laboratory at the International Health Exhibition, "Anthropol. Instit. Journ.," 14, 1885, pp. 205–221.

Notes on Permanent Colour Types in Mosaics [1886], "Anthropol. Instit. Journ.," 16, 1887, pp. 145–147.

Recent Designs for Anthropometric Instruments, [1886], "Anthropol. Instit. Journ.," 16, 1887, pp. 2–9.

A New Instrument for Measuring the Rate of Movement of the Various Limbs, [1890], "Anthropol. Instit. Journ.," 20, 1891, pp. 200–204.

Table of Observations [of 400 persons] [1884], "Anthropol. Instit. Journ.," 18, 1889, pp. 420–430.

Retrospect of Work Done at my Anthropometric Laboratory at South Kensington [1891], "Anthropol. Instit. Journ.," 21, 1892, pp. 32–35.

Proposal to apply for Anthropological Statistics from Schools [1873], "Anthropol. Instit. Journ.," 3, 1874, pp. 308–311.

f. 5r

Height and Weight of Boys, aged 14, in Town and Country Public Schools [1875], "Anthropol. Instit. Journ.," 5, 1876, pp. 174–180.

Notes on the Marlborough School Statistics [1874], "Anthropol. Instit. Journ.," 4, 1875, pp. 130–135.

Head Growth in Students at the University of Cambridge [1888], "Anthropol. Instit. Journ.," 18, 1889, pp. 155, 156; "Nature," 38, 1888, p. 15; "Nature," 41, 1890, p. 454.

Advisability of Assigning Marks for Bodily Efficiency in the Examination of Candidates for the Public Services, "Brit. Assoc. Rep.," 1889, p. 471.

Physical Tests in Competitive Examinations, "Soc. of Arts Journ.," 39, 1890, pp. 19–25.

Notes on Australian Marriage Systems [1888], "Anthropol. Instit. Journ.," 18, 1899, pp. 70–72.

PSYCHOMETRY.

Remarks on Replies by Teachers to Questions respecting Mental Fatigue [1888], "Anthropol. Instit. Journ.," 18, 1889, pp. 157–168.

Generic Images [1879], "Roy. Instit. Proc.," 9, 1882, pp. 161–170; "Revue Scientif.," 17, 1879, pp. 221–225.

Visions of Sane Persons [1881], "Roy. Instit. Proc.," 9, 1882, pp. 644–655.

Visualised Numerals (Preliminary Memoir), "Nature," 21, 1880, pp. 252–256, 323, 494, 495.

Visualised Numerals [1880], "Anthropol. Instit. Journ.," 10, 1881, pp. 85–97.

Psychometric Experiments [1879], "Brain," 2, 1880, pp. 149–162.

The Just Perceptible Difference [Descriptive Portraiture], [January 27, 1893], "Proc. Roy. Instit.," 14, 1896, pp. 13–26.

Freewill, Observations and Inferences, "Mind," XXXV., 1884, p. 406.

Thoughts Without Words, "Nature," 36, 1887, pp. 28, 29, 100, 101.

Arithmetic by Smell, "Psychological Review," January, 1894.

A Curious Idiosyncrasy [Faintness at Sight of an Injured Finger Nail], "Nature," 54, 1896, p. 76.

Relative Sensitivity of Men and Women, "Nature," 50, 1894, pp. 40, 41.

f. 5v

FINGER PRINTS.

Personal Identification and Description, [1888], "Roy. Instit. Proc.," XII., 1889, pp. 346–360; "Nature," 38, 1888, pp. 173–177, 201–203.

Patterns in Thumb and Finger Marks; their arrangement into naturally distinct classes, the permanence of the Papillary Ridges that make them, and the resemblance of their classes to ordinary genera, "Phil. Trans.," B, 182, 1891, pp. 1–23; (Abstract), "Roy. Soc. Proc.," XLVIII., 1891, pp. 455–457.

Methods of Indexing Finger Marks, "Roy. Soc. Proc.," XLIX., 1891, pp. 540–548.

Finger Print Evidence, "Nature," 66, 1902, p. 606.

Identification, "Nature," 48, 1893, p. 222.

Enlarged Finger Prints, "Photographic Work," February 10th, 1893.

Prints of Scars, "Nature," 53, 1896, p. 295.

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Principal AWARDS and DEGREES.

Gold Medal, Royal Geographical Society, 1853.
Silver Medal, French Geographical Society, 1854.
Elected Athenæum Club under Rule II, 1855.
Fellow, Royal Society, 1856.
Gold Medal, Royal Society, 1886.
Officier de l'Instruction Publique, France, 1891.
D.C.L., Oxford, 1894.
Sc.D. (Hon.), Cambridge, 1895.
Huxley Medal, Anthropological Institute, 1901.
Elected Hon. Fellow Trinity College, Cambridge, 1902.
Darwin Medal, Royal Society, 1902.

Corresp. Member of the Geograph. Societies of Berlin and Vienna, and of Anthrop. Soc. of Rome; Hon. Member of Geograph. Soc. of Italy, also of the Inst. Internat. de Statistique.

Principal OFFICES.

Many years on Council of the Royal Geograph. Soc.; eight years on Council of the Royal Society. Hon. Secretary British Association, 1863–68. President of the Anthrop. Inst., 1885–88. Member of the Meteorological Council from its first establishment until 1901. Chairman of Committee of Management of the Kew Observatory of the Royal Society for many years, until its incorporation, in 1901, with the National Physical Laboratory.

BIOGRAPHICAL EVENTS.

Born Feb. 16, 1822, seventh and youngest child of S. T. Galton of Duddeston and Claverdon in Warwickshire, and of Violetta, *dau.* of Erasmus Galton, F.R.S., physician, philosopher and poet. Schools—Boulogne, *æt.* 8–10; Kenilworth, *æt.* 11–13; King Edward's, Birmingham, *æt.* 14–16. Resident house-pupil at Birmingham Hospital, *æt.* 16–17; medical student at King's College, *æt.* 17. Made in 1840 a short journey to the East and then entered Cambridge University, where after two years his health gave way and compelled abstinence from mental work. He took an ordinary degree in 1844 and resumed medical studies at St. George's Hospital. On the death of his father in 1845, he renounced the intention of qualifying as a physician; he travelled in 1846 through Egypt to Khartum and the White Nile, then rarely reached by tourists. In 1847–9 he hunted in Warwickshire and shot in Scotland. In 1850–2 he made an expedition to S.W. Africa, with Mr. Charles Andersson as a companion, and explored Damara and Ovampo lands. For the results of this journey the gold medal of the Royal Geographical Society was awarded him and he was elected to its Council, on which he served for many years with rare intermissions. He lectured on the Arts of Camp Life at Aldershot when the camp was first established there. He was appointed a member of the Committee of Management of the Kew Observatory of the Royal Society, and took an active part in the improvement and verification of instruments used by travellers. He remained a member of that Committee, and latterly its Chairman, until 1901, when the Kew Observatory was merged into the newly established National Physical Laboratory. He became Honorary Secretary of the British Association in 1863 but was incapacitated from mental work by a second break down of health in 1867, and he resigned the office. In 1863 he constructed and published the earliest maps of contemporary weather in Europe, derived from meteorological observations collected by himself, on which occasion he discovered the existence of Anticyclones, a previously unknown but necessary link in the theory of atmospheric circulation. He was nominated in 1868 as one of the original members of the Meteorological Committee appointed by Government, which was afterwards developed into the Meteorological Council with revised duties and an enlarged grant. He was a member of these successive boards of management until his retirement in 1901. He took an active part in the Council of the Anthropological Institute, of which he became President. About 1865 he began to investigate the inheritance of Mental Faculty by new statistical methods, for which he was subsequently awarded the gold medal of the Royal Society. He obtained through an offer of prizes a large collection of Family Records which were essential to his investigations in the absence of more copious and trustworthy material. Their contents have been frequently utilised for statistical purposes up to a recent date. He established two Anthropometric Laboratories; the first in the Loan Exhibition of Scientific Instruments at South Kensington, the second in rooms granted him for the purpose in the galleries assigned to the collection of Scientific Instruments, and which was maintained by him during several years. These were forerunners of analogous institutions in schools and colleges, especially in America. The results obtained at the laboratories enabled him to prove that the mathematical theory of mechanical correlation was applicable to biometry, and of great service to theoretical deductions from it, and that heredity and relationship were in the statistical sense, particular forms of correlation.

His laboratory also afforded opportunity for collecting Finger-prints and of testing their value as a means of identification. He proved (1) that the papillary ridges which impressed characteristic patterns, were permanent throughout life, even in their minutest details; (2) that sets of prints of the ten fingers admitted of being so classified that the duplicate of any set might be discovered in a large collection almost as easily as a name can be found in an alphabetical dictionary. The practice was adopted of asking each comer whether he had been at the laboratory before, and if so, of finding his first card by means of his finger-prints alone; thus the method of classification was tested and improved. By these means a sure foundation was laid for the system now adopted throughout India, England and elsewhere, for the detection and identification of criminals by the method of finger prints, also for its future development as a trustworthy signature by illiterates and for the more effectual registration of men of coloured and half-civilised races.

Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, January 27, 1893.

David Edward Hughes, Esq. F.R.S. Vice-President, in the Chair.

Francis Galton, Esq. F.R.S. *M.R.I.*

The Just-Perceptible Difference.

We seem to ourselves to belong to two worlds, which are governed by entirely different laws; the world of feeling and the world of matter—the psychical and the physical—whose mutual relations are the subject of the science of Psycho-physics, in which the just-perceptible difference plays a large part.

It will be explained in the first of the two principal divisions of this lecture that the study of just-perceptible differences leads us not only up to, but beyond, the frontier of the mysterious region of mental operations which are not vivid enough to rise above the threshold of consciousness. It will there be shown how important a part is commonly played by the imagination in producing faint sensations, and how its power on those occasions admits of actual measurement.

The last part of the lecture will deal with the limits of the power of optical discrimination, as shown by the smallest number of adjacent dots that suffice to give the appearance of a continuous line, and the feasibility will be explained of transmitting very beautiful outline drawings of a minute size, and larger and rougher plans, maps, and designs of all kinds, by means of telegraphy.

Material objects are measurable by external standards, about which it is sufficient to say that when we speak of a pound, a yard, or an hour, we use terms whose meanings are defined and understood in the same sense by all physicists. The feelings, on the other hand, cannot be measured by external standards, so we are driven to use internal ones, and to adopt a scale of sensation formed by units of just-perceptible differences, rising in the arithmetical order of 1, 2, 3, &c., and by their side a scale of measurements of the stimuli that provoked them. The attempts of those who first experimentalised in Psycho-physics were mainly directed to ascertain the relation between the increase of stimulus and the corresponding increment of sensation.

Their net result has been to confirm, within moderate limits, the trustworthiness of Weber's law, namely, that each successive increment of sensation is caused by the same *percentage* increment of the previous stimulus.

The rate at which a stimulus must be increased in order to give a just-perceptible increment of sensation, has been taken at the average of 1 per cent. for light, 6 per cent. for muscular effort, 33 per cent. for sound and warmth; also 33 per cent. for pressure upon most parts of the body, and as high as 16 per cent. upon the finger tips. But these values must not be trusted too far; they cease to be exact towards the two ends of the scale.

A mechanical arrangement clearly illustrates the consequences of Weber's law. It includes an axle to which is fixed a wheel, a part of a logarithmic spiral, and an index hand. This portion of the machine is carefully balanced, so that it will remain steady in any position in which it is set, while a small force is sufficient to cause it to turn; behind all is a card with equal graduations upon it, over which the index travels. A string, with a scale pan at one end and a counterpoise at the other, is wrapped round the wheel. A string fastened to the axle passes over the logarithmic arm, and a ball is fastened to its free end. The varying weights put in the scale pan will now represent varying amounts of stimulus, and the graduations to which the index points, represent the corresponding variations of sensation.

I exhibit a diagrammatic model of the apparatus, much too rough to give exact indications, but still sufficient for rough explanatory purposes.

Owing to the obvious properties of a spiral, the more the axle to which it is fixed is rotated in the direction of its concave side, the further does the point at which the string is hanging travel away from the axis, and the leverage exerted by the weight of the ball will increase. Whatever be the weight in the scale pan, there is within the working range of the apparatus some position of the beam at which that weight will be counterbalanced by the ball. The property of the logarithmic spiral is that equal degrees of rotation correspond to equal percentage increments of leverage. Hence, when percentage increments of weight are successively placed in the scale pan, the index attached to the beam will successively travel over equal divisions of the scale, in accordance with Weber's formula.

The progressive increase in the effective length of the logarithmic arm is small at first, but is seen soon to augment rapidly, and then to become extravagant. We thus gain a vivid insight through this piece of mechanism into the enormous increase of stimulus, when it is already large, that is required to produce a fresh increment of sensation, and how soon the time must arrive when the organ of sense, like the machine, will break down under the strain rather than admit of being goaded farther.

The result of all this is, that although the senses may perceive very small stimuli, and can endure very large ones without suffering damage, the number of units in the scale of sensation is comparatively small. The hugest increase of good fortune will not make a man who was already well off, many degrees happier than before; the utmost torture that can be applied to him will not give much greater pain than he has already sometimes suffered. The experience of a life that we call uneventful usually includes a large share of the utmost possible range of human pleasures and human pains. Thus the physiological law which is expressed by Weber's formula is a great leveller, by preventing the diversities of fortune from creating by any means so great a diversity in human happiness.

The least-perceptible difference varies considerably in different persons, delicacy of perception being a usual criterion of superiority of nature. The sense of pain is curiously blunt in idiots. It varies also in the same person with his health, and extraordinarily so in hysteria and hypnotism, at which times sensitivity is sometimes almost absent, and at other times exceptionally acute. It is somewhat affected by drugs. Thus Dr. Lauder Brunton writes concerning strychnine, that when taken in small doses for a long time, the impressions are felt more keenly and are of longer duration. The sense of touch is rendered more acute; the field of vision is increased, distant objects are more distinct, and the sense of hearing is sharpened. (*Pharmacology*, 1885, p. 888.)

Other drugs or intoxicants may yet be discovered and legitimately used to heighten the sensitivity, or indeed any other faculty during a brief period, in order to perform that which could not otherwise be performed at all, at the cheap price of a subsequent period of fatigue.

Measure of the Imagination.—The first perceptible sensation is seldom due to a solitary stimulus. Internal causes of stimulation are in continual activity, whose effects are usually too faint to be perceived by themselves, but they may combine with minute external stimuli, and so produce a sensation which neither of them could have done singly. I desire now to draw attention to another concurring cause which has hitherto been unduly overlooked, or only partially allowed for under the titles of Expectation and Attention. I mean the Imagination, believing that it should be frankly recognised as a frequent factor in the production of a just-perceptible sensation. Let us reflect for a moment on the frequency with which the imagination produces effects that actually overpass the threshold of consciousness, and give rise to what is indistinguishable from, and mistaken for, a real sensation. Every one has observed instances of it in his own person and in those of others. Illustrations are almost needless; I may, however, mention one as a reminder; it was current in my boyhood, and the incident probably took place not many yards from where I now stand. Sir Humphry Davy had recently discovered the metal potassium, and showed specimens of it to the greedy gaze of a philosophical friend as it lay immersed in a dish of alcohol to shield it from the air, explaining its chemical claim to be considered a metal. All the known metals at that time were of such high specific gravity that weight was commonly considered to be a peculiar characteristic of metals; potassium, however, is lighter than water. The philosopher not being aware of this, but convinced as to its metallic nature by the reasoning of Sir Humphry, fished a piece out of the alcohol, and, weighing it awhile between his finger and thumb, said seriously, as in further confirmation, "How heavy it is!"

In childhood the imagination is peculiarly vivid, and notoriously leads to mistakes, but the discipline of after life is steadily directed to checking its vagaries and to establishing a clear distinction between fancy and fact. Nevertheless, the force of the imagination may endure with extraordinary power and even be cherished by persons of poetic temperament, on which point the experiences of our two latest Poets-Laureate, Wordsworth and Tennyson, are extremely instructive. Wordsworth's famous "Ode to Immortality" contains three lines which long puzzled his readers. They occur after his grand description of the glorious imagery of childhood, and the "perpetual benediction" of its memories, when he suddenly breaks off into—

"Not for these I raise
The song of thanks and praise,
But for those obstinate questionings
Of sense and outward things,
Fallings from us, vanishings," &c.

Why, it was asked, should any sane person be "obstinately" disposed to question the testimony of his senses, and be peculiarly thankful that he had the power to do so? What was meant by the "fallings off and vanishings," for which he raises his "song of thanks and praise"? The explanation is now to be found in a note by Wordsworth himself, prefixed to the ode in Knight's edition. Wordsworth there writes, "I was often unable to think of external things as having external existence, and I communed with all I saw as something not apart from, but inherent in, my own immaterial nature. Many times while going to school have I grasped at a wall or tree to recall myself from this abyss of idealism to the reality. At that time I was afraid of such processes. In later times I have deplored, as we all have reason to do, a subjugation of an opposite character, and have rejoiced over the remembrances, as is expressed in the lines 'Obstinate questionings,' &c." * He then gives those I have just quoted.

It is a remarkable coincidence that a closely similar idea is found in the verses of the successor of Wordsworth, namely, the great poet whose recent loss is mourned by all English-speaking nations, and that a closely similar explanation exists with respect to them. For in Lord Tennyson's "Holy Grail" the aged Sir Percivale, then a monk, recounts to a brother monk the following words of King Arthur:—

"Let visions of the night or of the day
Come, as they will; and many a time they come
Until this earth he walks on seems not earth,
This light that strikes his eyeball is not light,
The air that smites his forehead is not air,
But vision," &c.

* Knight's edition of Wordsworth, vol. iv. p. 47.

Sir Percivale concludes just as Wordsworth's admirers formerly had done: "I knew not all he meant."

Now, in the *Nineteenth Century* of the present month Mr. Knowles, in his article entitled "Aspects of Tennyson," mentions a conversational incident curiously parallel to Wordsworth's own remarks about himself:—"He [Tennyson] said to me one day, 'Sometimes as I sit alone in this great room I get carried away, out of sense and body, and rapt into mere existence, till the accidental touch or movement of one of my own fingers is like a great shock and blow, and brings the body back with a terrible start.'"

Considering how often the imagination is sufficiently intense to mimic a real sensation, a vastly greater number of cases must exist in which it excites the physiological centres in too feeble a degree for their response to reach to the level of consciousness. So that if the imagination has been anyhow set into motion, it shall, as a rule, originate what may be termed *incomplete* sensations, and whenever one of these concurs with a real sensation of the same kind, it would swell its volume.

This supposition admits of being submitted to experiment by comparing the amount of stimulus required to produce a just-perceptable sensation, under the two conditions of the imagination being either excited or passive.

Several conditions have to be observed in designing suitable experiments. The imagined sensation and the real sensation must be of the same quality; an expected scream and an actual groan could not reinforce one another. Again, the place where the image is localised in the theatre of the imagination must be the same as it is in the real sensation. This condition requires to be more carefully regarded in respect to the visual imagination than to that of the other senses, because the theatre of the visual imagination is described by most persons, though not by all, as internal, whereas the theatre of actual vision is external. The important part played by points of reference in visual illusions is to be explained by the aid they afford in compelling the imaginary figures to externalise themselves, superimposing them on fragments of a reality. Then the visualisation and the actual vision fuse together in some parts, and supplement each other elsewhere.

The theatre of audition is by no means so purely external as that of sight. Certain persuasive tones of voice sink deeply, as it were, into the mind, and even simulate our own original sentiments. The power of localising external sounds, which is almost absent in those who are deaf with one ear, is very imperfect generally, otherwise the illusions of the ventriloquist would be impossible. There was an account in the newspapers a few weeks ago of an Austrian lady of rank who purchased a parrot at a high price, as being able to repeat the Paternoster in seven different languages. She took the bird home, but it was mute. At last it was discovered that the apparent performances of the parrot had been due to the ventriloquism of the dealer. An analogous trick upon the sight could not be performed by a conjuror. Thus he could never make his audience believe that the floor of the room was the ceiling.

As regards the other senses, the theatre of the imagination coincides fairly well with that of the sensations. It is so with taste and smell, also with touch, in so far that an imagined impression or pain is always located in some particular part of the body, then if it be localised in the same place as a real pain it must coalesce with it.

Finally, it is of high importance to success in experiments on Imagination that the object and its associated imagery should be so habitually connected that a critical attitude of the mind shall not easily separate them. Suppose an apparatus arranged to associate the waxing and waning of a light with the rising and falling of a sound, holding means in reserve for privately modifying the illumination at the will of the experimenter, in order that the waxing and waning may be lessened, abolished, or even reversed. It is quite possible that a person who had no idea of the purport of the experiment might be deceived, and be led by his imagination to declare that the light still waxed and waned in unison with the sound after its ups and downs had been reduced to zero. But if the subject of the experiment suspected its object, he would be thrown into a critical mood; his mind would stiffen itself, as it were, and he would be difficult to deceive.

Having made these preliminary remarks, I will mention one only of some experiments I have made and am making from time to time, to measure the force of my own imagination. It happens that although most persons train themselves from childhood upwards to distinguish imagination from fact, there is at least one instance in which we do the exact reverse, namely, in respect to the auditory presentation of the words that are perused by the eye. It would be otherwise impossible to realise the sonorous flow of the passages, whether in prose or poetry, that are read only with the eyes. We all of us value and cultivate this form of auditory imagination, and it commonly grows into a well-developed faculty. I infer that when we are listening to the words of a reader while our eyes are simultaneously perusing a copy of the book from which he is reading, that the effects of the auditory imagination concur with the actual sound, and produce a stronger impression than the latter alone would be able to make.

I have very frequently experimented on myself with success, with the view of analysing this concurrent impression into its constituents, being aided thereto by two helpful conditions, the one is a degree of deafness which prevents me when sitting on a seat in the middle rows from following memoirs that are read in tones suitable to the audience at large; and the other is the accident of belonging to societies in which unrevised copies of the memoirs that are about to be read, usually in a monotonous voice, are obtainable, in order to be perused simultaneously by the eye. Now it sometimes happens that

f. 4r

portions of these papers, however valuable they may be in themselves, do not interest me, in which case it has been a never-flagging source of diversion to compare my capabilities of following the reader when I am using my eyes, and when I am not. The result depends somewhat on the quality of the voice; if it be a familiar tone I can imagine what is coming much more accurately than otherwise. It depends much on the phraseology, familiar words being vividly represented. Something also depends on the mood at the time, for imagination is powerfully affected by all forms of emotion. The result is that I frequently find myself in a position in which I hear every word distinctly so long as they accord with those I am perusing, but whenever a word is changed, although the change is perceived, the new word is not recognised. Then, should I raise my eyes from the copy, nothing whatever of the reading can be understood, the overtones by which words are distinguished being too faint to be heard. As a rule, I estimate that I have to approach the reader by about a quarter of the previous distance, before I can distinguish his words by the ear alone. Accepting this rough estimate for the purposes of present calculation, it follows that the potency of my hearing alone is to that of my hearing *plus* imagination as the loudness of the same overtones heard at 3 and at 4 units of distance respectively; that is as about 3^2 to 4^2, or as 9 to 16. Consequently the potency of my auditory imagination is to that of a just-perceptible sound as $16 - 9$ to 16, or as 7 units to 16. So the effect of the imagination in this case reaches nearly half-way to the level of consciousness. If it were a little more than twice as strong it would be able by itself to produce an effect indistinguishable from a real sound.

Two copies of the same newspaper afford easily accessible materials for making this experiment, a few words having been altered here and there in the copy to be read from.

I will conclude this portion of my remarks by suggesting that some of my audience should repeat these experiments on themselves. If they do so, I should be grateful if they would communicate to me their results.

Optical Continuity.—Keenness of sight is measured by the angular distance apart of two dots when they can only just be distinguished as two, and do not become confused together. It is usually reckoned that the normal eye is just able or just unable to distinguish points that lie one minute of a degree asunder. Now, one minute of a degree is the angle subtended by two points, separated by the 300th part of an inch, when they are viewed at the ordinary reading distance of one foot from the eye. If, then, a row of fine dots touching one another, each as small as a bead of one 300th part of an inch in diameter, be arranged on the page of a book, they would appear to the ordinary reader to be an almost invisibly fine and continuous line. If the dots be replaced by short cross strokes, the line would look broader, but its apparent continuity would not be affected. It is impossible to draw any line that shall commend itself to the eye as possessing more regularity than the image of a succession of dots or cross strokes, 300 to the inch, when viewed at the distance of a foot. Every design, however delicate, that can be drawn with a line of uniform thickness by the best machine or the most consummate artist, admits of being mimicked by the coarsest chain, when it is viewed at such a distance that the angular length of each of its links shall not exceed one minute of a degree. One of the apparently smoothest outlines in nature is that of the horizon of the sea during ordinary weather, although it is formed by waves. The slopes of *débris* down the sides of distant mountains appear to sweep in beautifully smooth curves, but on reaching those mountains and climbing up the *débris*, the path may be exceedingly rough.

The members of an audience sit at such various distances from the lecture table and screen that it is not possible to illustrate as well as is desirable the stages through which a row of dots appears to run into a continuous line, as the angular distance between the dots is lessened. I have, however, hung up chains and rows of beads of various degrees of coarseness. Some of these will appear as pure lines to all the audience; others, whose coarseness of structure is obvious to those who sit nearest, will seem to be pure lines when viewed from the farthest seats.

Although 300 dots to the inch are required to give the idea of perfect continuity at the distance of one foot, it will shortly be seen that a much smaller number suffices to suggest it.

The cyclostyle, which is an instrument used for multiple writing, makes about 140 dots to the inch. The style has a minute spur-wheel or roller, instead of a point; the writing is made on stencil paper, whose surface is covered with a brittle glaze. This is perforated by the teeth of the spur-wheel wherever they press against it. The half perforated sheet is then laid on writing paper, and an inked roller is worked over the glaze. The ink passes through the perforations and soaks through them on to the paper below; consequently the impression consists entirely of short and irregular cross bars or dots.

I exhibit on the screen a circular letter summoning a committee, that was written by the cyclostyle. The writing seems beautifully regular when the circular is photographically reduced; when it is enlarged, the discontinuity of the strokes becomes conspicuous. Thus, I have enlarged the word *the* six times; the dots can then be easily seen and counted. There are 42 of them in the long stroke of the letter *h*.

The appearance of the work done by the cyclostyle would be greatly improved if a fault in its mechanism could be removed, which causes it to run with very unequal freedom in different directions. It leaves an ugly, jagged mark wherever the direction of a line changes suddenly.

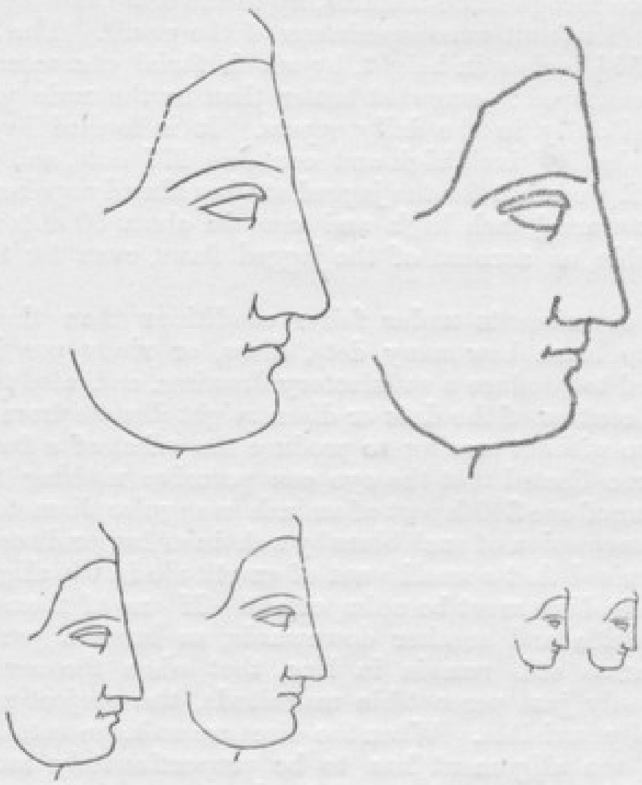
A much coarser representation of continuous lines is given by embroidery and tapestry, and coarser still by those obsolete school samplers which our ancestresses worked in their girlhood, with an average of about sixteen stitched dots to each letter. Perhaps the coarsest lettering, or rather figuring, that is ever practically employed is used in perforating the books of railway coupons so familiar to travellers. Ten or eleven holes are used for each figure.

A good test of the degree of approximation with which a cyclostyle making 140 perforations to the inch is able to simulate continuous lines, is to use it for drawing outline portraits. I asked the clerk who wrote the circular just exhibited to draw me a few profiles of different sizes, ranging from the smallest scale on which the cyclostyle could produce recognisable features, up to the scale at which it acted fairly well. I submit some specimens of the result. The largest is a portrait of $1\frac{1}{2}$ inches in height, by which facial characteristics are fairly well conveyed; somewhat better than by the rude prints that appear occasionally in the daily papers. It is formed by 366 dots. A medium size is $\frac{3}{4}$ inch high and contains 177 dots, and would be tolerable if it were not for the jagged strokes already spoken of. The smallest sizes are $\frac{1}{3}$ inch high and contain about 90 dots; they are barely passable, on account of the jagged flaws, even for the rudest portraiture.

I made experiments under fairer conditions than those of the cyclostyle, to learn how many dots, discs, or rings per inch were really needed to produce a satisfactory drawing, and also to discover how far the centres of the dots or discs might deviate from a strictly smooth curve without ceasing to produce the effect of a flowing line. It must be recollected that the eye can perceive nothing finer than a minute blur of one 300th part of an inch in angular diameter. If we represent a succession of such blurs by a chain of larger discs, it will be easily recognised that a small want of exactitude in the alignments of the successive discs must be unimportant. If one of them is pushed upwards a trifle and another downwards, so large a part of their respective areas still remain in line, that when the several discs become of only just perceptible magnitude, the projecting portion will be wholly invisible. When the discs are so large as to be plainly perceptible, the alignment has to be proportionately more exact. After a few trials it seemed that if the *bearing* of the centre of each disc from that of its predecessor which touched it, was correctly given to the nearest of the 16 principal points of the compass, N., NNE., NE., &c., it was fairly sufficient. Consequently a simple record of the successive bearings of each of a series of small equidistant steps is enough to define a curve.

The briefest way of writing down these bearings is to assign a separate letter of the alphabet to each of them, *a* for north (the top of the paper counting as north), *b* for north-north-east, *c* for north-east, and so on in order up to *p*. This makes *e* represent east, *i* south, and *m* west.

To test the efficiency of the plan, I enlarged one of the cylostyle profiles, and making a small protractor with a piece of tracing paper, rapidly laid down a series of equidistant points on the above principle, noting at the same time the bearing of each from its predecessor. I thereby obtained a formula for the profile, consisting of 271 letters. Then I but aside the drawing, and set to work to reproduce it solely from the formula. I exhibit the result; it is fairly successful. Emboldened by this first trial, I made a more ambitious attempt, by dealing with the profile of a Greek girl copied from a gem. I was very desirous of learning how far the pure outline of the original admitted of being mimicked in this rough way.



The result is here; a ring has been painted round each dot in order to make its position clearly seen, without obliterating it. The reproduction has been photographically reduced to various different sizes. That which contains only fifty dots to the inch, which is consequently six times as coarse as the theoretical 300 to an inch, is a very creditable production. Many persons to whom this portrait has been shown, failed to notice the difference between it and an ordinary woodcut. The medium size, and much more the smallest size, would deceive anybody who viewed them at the distance of one foot. The protractor used in making them was a square card with a piece cut out of its middle, over which transparent tracing paper was pasted. A small hole of about $\frac{1}{8}$ of an inch in diameter was punched out of the centre of the tracing paper; sixteen minute holes just large enough to allow the entry of the sharp point of a hard lead-pencil were perforated through the tracing paper in a circle round the centre of the hole at a radius of $\frac{1}{4}$ inch. They corresponded to the sixteen principal points of the compass, and had their appropriate letters written by their sides. The outline to be formulated was fixed to a drawing-board, with a T rule laid across it as a guide to the eye in keeping the protractor always parallel to itself. The centre of the small hole was then brought over the beginning of the outline, and a dot was made with the pencil through the perforation nearest to the further course of the outline, and this became the next point of departure. While moving the protractor from the old point to the new one it was stopped on the way, in order that the letter for the bearing might be written through the central hole. These were afterwards copied on a separate piece of paper.

A clear distinction must be made between the proposed plan and that of recording the angle made by each step from the *preceding one*. In the latter case, any error of bearing would falsify the direction of all that followed, like a bend in a wire.

The difficulties of dealing with detached portions of the drawing, such as the eye, were easily surmounted by employing two of the spare letters, R and S, to indicate brackets, and other spare letters to indicate points of reference. The bearings included between an R and an S were taken to signify directive dots, not to be inked in. The points of reference indicated by other letters are those to which the previous bearing leads, and from which the next bearing departs. Here is the formula whence the *eye* was drawn. It includes a very small part of the profile of the brow, and the directive dots leading thence to the eye.

The letters should be read from the left to the right, across the vertical lines. They are broken into groups of five, merely for avoiding confusion and for the convenience of after reference.

The part of the Profile that includes U

&c. iiiilU jiihi &c. &c.

The Eye.

URkkk	kklll	mSVap	ponmn	mmlmm
mlmlm	llmZZ	VnTnn	mnmmm	mmmlm
mmnZZ	Tjjjj	jjkkc	chmmn	mnnn
onooZ				

Letters used as Symbols.

R....S=(....). Z=end.
U, V, T are points of reference.

By succeeding in so severe a test case as this Greek outline, it may be justly inferred that rougher designs can be easily dealt with in the same way.

At first sight it may seem to be a silly waste of time and trouble to translate a drawing into a formula, and then, working backwards, to retranslate the formula into a reproduction of the original drawing, but further reflection shows that the process may be of much practical utility. Let us bear two facts in mind, the one is that a very large quantity of telegraphic information is daily published in the papers, anticipating the post by many days or weeks. The other is that pictorial illustrations of current events, of a rude kind, but acceptable to the reader, appear from time to time in the daily papers. We may be sure that the quantity of telegraphic intelligence will steadily increase, and that the art of newspaper illustration will improve and be more resorted to. Important local events frequently occur in far-off regions, of which no description can give an exact idea without the help of pictorial illustration; some catastrophe, or site of a battle, or an exploration, or it may be some design or even some portrait. There is therefore reason to expect a demand for such drawings as these by telegraph, if their expense does not render it impracticable to have them. Let us then go into details of expense, on the basis of the present tariff from America to this country, of one shilling per word, 5 figures counting as one word, cypher letters not being sent at a corresponding rate. It requires two figures to perform each of the operations described above, which were performed by a single letter. So a formula for 5 dots would require 10 figures, which is the telegraphic equivalent of 2 words; therefore the cost for every 5 dots telegraphed from the United States would be 2 shillings, or 2*l.* for every 100 dots or other indications.

In the Greek outline there is a total of 400 indications, including those for directive dots, and for points of reference. The transmission of these to us from the United States would cost 8*l.* I exhibit a map of England made with 248 dots, as a specimen of the amount of work in plans, which could be effected at the cost of 5*l.* It is easy to arrange counters into various patterns or parts of patterns, learning thereby the real power of the process. The expense of pictorial telegraphs to foreign countries would be large in itself, but not large relatively to the present great expenditure by newspapers on telegraphic information, so the process might be expected to be employed whenever it was of obvious utility.

The risk is small of errors of importance arising from mistakes in telegraphy. I inquired into the experience of the Meteorological Office, whose numerous weather telegrams are wholly conveyed by numerical signals. Of the 20,625 figures that were telegraphed this year to the office from continental stations, only 49 seem to have been erroneous, that is two and a third per thousand. At this rate the 800 figures needed to telegraph the Greek profile would have been liable to two mistakes. A mistake in a figure would have exactly the same effect on the outline as a rent in the paper on which a similar outline had been drawn, which had not been pasted together again with perfect precision. The dislocation thereby occasioned would never exceed the thickness of the outline.

The command of 100 figures from 0 to 99, instead of only 26 letters, puts 74 fresh signals at our disposal, which would enable us to use all the 32 points of the compass, instead of 16, and to deal with long lines and curves. I cannot enter into this now, nor into the control of the general accuracy of the picture by means of the distances between the points of triangles each formed by any three points of reference. Neither need I speak of better forms of protractor. There is one on the table by which the ghost of a compass card is thrown on the drawing. It is made of a doubly refracting image of Iceland spar, which throws the so-called "extraordinary" image of the compass card on to the ordinary image of the drawing, and is easy to manipulate. All that I wish now to explain is that this peculiar application of the law of the just-perceptible difference to optical continuity gives us a new power that has practical bearings.

Postscript.—A promising method for practical purposes that I have tried, is to use "sectional" paper; that is, paper ruled into very small squares, or else coarse cloth, and either to make the drawing upon it, or else to lay transparent sectional paper or muslin over the drawing. Dots are to be made at distances not exceeding three spaces apart, along the course of the outline, at those intersections of the ruled lines (or threads) that best accord with the outline. Each dot in succession is to be considered as the *central point*, numbered **44** in the following

11	21	31	41	51	61	71
12	22	32	42	52	62	72
13	23	33	43	53	63	73
14	24	34	**44**	54	64	74
15	25	35	45	55	65	75
16	26	36	46	56	66	76
17	27	37	47	57	67	77

schedule, and the couplet of figures corresponding to the portion of the next dot is to be written with a fine-pointed pencil in the interval between the two dots. These are subsequently copied, and make the formula. By employing 4 for zero, the signs $+$ and $-$ are avoided; 3 standing for -1, 2 for -2, and 1 for -3. The first figure in each couplet defines its horizontal coordinate from zero; the second figure, its vertical one. Thus any one of 49 different points are indicated, corresponding to steps from zero of 0, ± 1, ± 2, and ± 3

intervals, in either direction, horizontal or vertical. Half an hour's practice suffices to learn the numbers. The figures 0, 8, and 9 do not enter into any of the couplets in the schedule, the remaining 51 couplets in the complete series of 100 (ranging from 00 to 99), contain 21 cases in which 0, 8, or 9 forms the first figure only; 21 cases in which one of them forms the second figure only; and 9 cases in which both of the figures are formed by one or other of them. These latter are especially distinctive. This method has five merits—medium, short, or very short steps can be taken according to the character of the lineation at any point; there is no trouble about orientation; the bearings are defined without a protractor, the work can be easily revised, and the correctness of the records may be checked by comparing the sums of the successive small co-ordinates leading to a point of reference, with their total value as read off directly.

A method of signalling is also in use for military purposes, in which positions are fixed by co-ordinates, afterwards to be connected by lines.

[F. G.]

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[*From the* Obituary Notices *of the* Proceedings of the Royal Society, Vol. 60.]

Dr. John Rae, LL.D. (Edin.), a traveller in Arctic America, of extraordinary energy and endurance, a keen observer of Nature, and the discoverer of the fate of the Franklin expedition, was born in Orkney in 1813, died in London in 1893, and is buried in the cathedral of St. Magnus at Kirkwall, where a statue is erected to his memory.

He qualified as a surgeon in Edinburgh, and as such he accompanied one of the ships of the Hudson's Bay Company, whose service he joined, and then for ten years he resided at Moose Factory. (1) His first journey of pure exploration was a boat voyage along the coast of Hudson's Bay to Repulse Bay, where he wintered, and, in the following year he surveyed a coast line of 700 miles, connecting the surveys of Ross in Boothia with those of Parry at Fury and Heckla Strait. (2) Next he joined the expedition of Sir J. Richardson in 1848 in search for Sir J. Franklin, during which the whole coast was explored that lay between the mouths of the Mackenzie and the Coppermine Rivers. (3) In 1851, at the request of Government, he explored and mapped, with the slenderest outfit, 700 miles of the south coast of Wollaston Land and Victoria Land, still in search of Sir J. Franklin, for which achievement he received the gold medal of the Geographical Society. Its result was greatly to narrow the range of possibilities as to the locality of the missing expedition. (4) He took charge of a boat expedition, proved the insular character of King William's Land, and came at last upon relics of Franklin's party and received verbal information from the Eskimo that gave the first definite information as to their fate. The disaster occurred at the mouth of the Back River, a little more than 200 miles in a direct line from the place where he heard of it. For this achievement he received the promised grant of £10,000 from Government. He did not visit the spot himself, but his information as to the site and the completeness of the disaster, was soon abundantly confirmed. After this he made some further travel of interest, though by no means of the importance of the above, surveying a route for a telegraph line across Iceland and in North America.

This bald statement of itineraries will give but a poor idea, except to Arctic travellers, of the severity of the work accomplished. To supply the deficiency, the following quotation is given from the address of Sir R. Murchison when presenting the Gold Medal to Dr. Rae; his remarks chiefly referring to the journeys numbered above as (1) and (3).

"With a boldness never surpassed, he (Dr. Rae) determined on wintering on the proverbially desolate shores of Repulse Bay, where, or in the immediate neighbourhood, one expedition of two ships had previously wholly perished, and two others were all but lost. There he maintained his party on deer shot principally by himself, and spent ten months of an Arctic winter in a hut of stones, the locality not even yielding drift timber. With no other fuel than a kind of hay made of the *Andromeda tetragona*, he preserved his men in health, and thus enabled them to execute their arduous surveying journeys of upwards of 1,000 miles round Committee Bay (the southern portion of Boothia Gulf) in the spring. Next season he brought his party back to the Hudson Bay posts in better working condition than when he set out, and with but a small diminution of the few bags of provisions he had taken with him.

"On his last journeys, in which he travelled more than 3,000 miles in snow-shoes, Dr. Rae has shown equal judgment and perseverance. Dreading, from his former experience, that the sea might be frozen, he determined on a spring journey over the ice, and performed a most extraordinary one. His last starting place at Fort Confidence on the

Great Bear Lake, being at a distance of more than 150 miles from the coast by the route he was compelled to take, he could not, as in the parties of our naval expeditions, travel on the ice with capacious sledges, and was, therefore, obliged to restrict his provisions and baggage to the smallest possible weight. With a pound of fat daily for fuel, and without the possibility of carrying a tent, he set out accompanied by two men only, and trusting solely for shelter to snow houses he taught his men to build, accomplished a distance of 1,060 miles in 39 days, or 27 miles per day including stoppages, and this without the aid of advanced depôts, and dragging a sledge himself great part of the way. The spring journey, and that which followed in the summer in boats, during which 1,700 miles were traversed in 80 days, have proved the continuity of Wollaston and Victoria lands along a distance of nearly 1,100 miles, and have shown that they are separated by a strait from N. Somerset and Boothia, through which the flood tide sets from the north. In this way Dr. Rae has performed most essential service, even in reference to the search after Franklin, by limiting the channels of outlet between the continent of America and the Arctic Islands."

It is easy to understand that Dr. Rae's views as to the equipment of expeditions in Arctic travel would differ in many respects, rightly or wrongly, from those who advocated the costly naval expeditions then in vogue. He could point to instances of his own superior success, and to the disasters that befel the survivors of the Franklin expedition, as they toiled homewards with a miscellaneous collection of heavy articles. Putting forward his views, as he did with point and insistence, his remarks were, as a rule, somewhat unwelcome to the naval authorities.

In early middle life Dr. Rae was remarkable for manly beauty in form and feature, combined with a temper that was quick and somewhat fiery. In a book on Ethnology, where each of the human races was represented by a single specimen, it was noticed that an old photograph of Dr. Rae had been utilised to represent the Caucasian type.

Dr. Rae's house contained an interesting series of specimens illustrating the fauna and flora of arctic America and the domestic methods of the Eskimo, which he delighted to show and to explain, for he was a most courteous host, well aided by his wife. As a narrator he was delightful, being always lucid while full and circumstantial. His memoirs and speeches were stamped throughout with those characteristics.

His interest in the regions where he gained his fame remained unabated to the last. He died, regretted by many friends, in his eightieth year.

F. G.

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Les empreintes digitales.

Rapport présenté par M. FRANCIS GALTON, F. R. S., *ancien président de l'Institut anthropologique de Londres.*

Les empreintes digitales rendent déjà de grands services, en Angleterre et aux Indes, pour les recherches de police. On s'en sert aussi en France. La grande probabilité de voir s'étendre leur usage, lorsque la valeur remarquable du système sera plus généralement connue, montre que le moment est arrivé de faire des investigations dans les bureaux de police criminelle des différents pays, pour s'assurer de la nature des divers détails qui permettront un emploi général de ce système.

On trouvera dans le dernier livre que j'ai publié *(Fingerprint Directories)*, chez Macmillan, Londres, 1895) des détails qui, avec ceux contenus dans un volume antérieur *(Finger Prints* 1892), contiennent tout ce que j'avais à dire jusqu'à cette époque. Je ne fais par conséquent les remarques suivantes, qu'en renvoyant quiconque désire approfondir la question, aux deux volumes illustrés dont je viens de parler.

Certaines de mes assertions peuvent aujourd'hui être considérées comme suffisamment établies pour servir de base aux progrès à venir. Elles ont été démontrées dans mes livres et par des expériences publiques dans mon laboratoire (aujourd'hui fermé); elles ont été discutées par des critiques indépendants et acceptées sans réserve. Ce serait par conséquent perdre un temps précieux que de récapituler ici les différents arguments et observations qui justifient ces déclarations, à savoir :

1. L'art de prendre des impressions claires des doigts, avec l'encre d'imprimerie, s'apprend vite; les geôliers de toutes les prisons anglaises, par exemple, prennent aujourd'hui d'excellentes impressions. Ceux qui étudient cette question devront cependant assister à l'opération, avant d'essayer de la pratiquer. Cela ne demande qu'un outillage d'imprimerie très simple.

2. Les patrons formés par le sillonage papillaire dont les bouts des doigts sont garnis, restent les mêmes pendant toute la vie. Cette constance remarquable dans leur apparence, s'applique non seulement à leur forme générale, mais aux nombreux détails particuliers à chaque arrangement individuel tel que les « bifurcations », « îles » et « enclos » dont il existe en moyenne 30 dans le patron de chaque doigt et qui ne changent jamais.

3. Les coupures et les cicatrices ne détruisent pas la lisibilité des patrons, excepté dans les cas extrêmes; d'un autre côté leur présence aide à l'identification.

4. Par la méthode de classification adoptée dans ma collection expérimentale, composée de plus de 2500 séries d'empreintes digitales, il est facile de retrouver n'importe quel spécimen particulier. Toutes les fois que les impressions des dix doigts d'une personne furent soumises à notre examen, il fut facile, soit à mon adjoint, soit à moi-même, de reconnaître si une autre impression des mêmes mains, prise à une époque antérieure, existait dans la collection. Ceci peut se faire, soit en se rapportant aux cartes sur lesquelles on avait pris les impressions, soit en se servant du catalogue dans lequel on a placé par ordre alphabétique les « titres », au moyen desquels on peut distinguer les séries d'impressions.

5. Les « titres » mentionnés ci-dessus s'obtiennent en classant le patron de chacun des doigts, pris séparément, dans l'une des quatre classes fondamentales ; A, R, U et W et, de temps en temps aussi, en comptant les sillons (voir 8) et en se servant des suffixes descriptives (voir 8).

6. La fréquence relative avec laquelle les patrons tombent dans les différentes classes est telle que sur chacun des 13 patrons d'index nous trouvons en moyenne 2 spécimens de A, 3 cas de R, 4 cas de U et 4 de W. Les proportions ne sont pas les mêmes pour les autres doigts.

7. A (signifiant en anglais « Arches »). Ici les sillons papillaires traversent le doigt en lignes droites au niveau et auprès de la dernière articulation. De là, en se rapprochant des extrémités du doigt, elles deviennent de plus en plus arquées. Toute cette disposition constitue un système continu dans lequel aucun sillon ne revient sur lui-même.

R et U sont des divisions du grand groupe L (en anglais « loops »). Ici la disposition des sillons, vers l'articulation et vers le bout du doigt, est la même qu'en A, mais ils forment deux systèmes différents entre lesquels s'intercale le troisième système L. Ce troisième système L consiste en sillons se repliant sur eux-mêmes ; ils se courbent une fois, mais une fois seulement, ne formant jamais un cercle complet. Au point où les deux premiers systèmes divergent pour entourer le groupe L, il se trouve toujours un endroit ayant une certaine ressemblance avec le delta formé par les alluvions déposées par une rivière à son entrée dans les eaux tranquilles d'un lac et on le désigne sous ce nom. Le groupe L se distingue par conséquent par un delta, le groupe A n'en possédant pas. L'ouverture de L doit être tournée vers l'un des côtés du doigt ; si elle est tournée vers le côté radial ou du pouce, on l'appelle R, mais si elle est tournée vers le côté ulnaire, ou du petit doigt, on l'appelle U.

W (en anglais « Whorls »). Cette classe est très variée, caractérisée communément par la présence de deux deltas, et par conséquent quelques-uns des sillons qui en font partie forment un cercle complet. C'est une classe qu'il est difficile de subdiviser à cause des formes remarquablement distinctes qu'elle comprend et que l'on ne peut pas isoler facilement. Du reste, une multitude d'autres formes intermédiaires les relient entre elles par des gradations imperceptibles.

8. Les classes R et U peuvent se subdiviser facilement en comptant le nombre de sillons que traverserait une ligne imaginaire tirée du delta au noyau. Les sillons peuvent facilement et exactement se compter avec une installation optique appropriée. Mon aide et moi, après avoir pratiqué ensemble jusqu'à ce que nous fussions d'accord sur les termini précis (bien définis dans mon livre), différions rarement de plus d'un ou deux sillons dans une longue série d'expériences, et quand nous différions, on pouvait en outre généralement prévoir la nature de la différence. Le nombre des sillons, entre les termini de l'index, varie de 1 à plus de 20 et la fréquence relative de chaque

nombre de sillons entre 3 et 16 est approximativement la même. Les sections R et U peuvent par conséquent se subdiviser considérablement en comptant les sillons. On a réussi aussi à subdiviser la classe W en comptant du delta radial au noyau ou, s'il y a deux noyaux dans le patron, jusqu'au plus rapproché.

9. La dernière chose qu'il nous reste à mentionner est le système de suffixes qui offrent des indications utiles pour les particularités du patron.

Ce long préambule est nécessaire pour expliquer les détails de ma proposition : qu'il soit fait des recherches dans les administrations de police des différentes nations pour déterminer la nomenclature la plus convenable et les autres détails relatifs aux empreintes digitales pour les services internationaux, c'est-à-dire pour communiquer, par lettre ou télégraphe, et en termes généralement intelligibles, le signalement par les empreintes digitales, des personnes soupçonnées. Les points qui demandent principalement une solution semblent être les quatre suivants :

a. Quelle est la meilleure nomenclature à adopter pour décrire les divers cas ambigus qui se trouvent entre A et L, A et W, L et W, et les quelques rares cas qui se trouvent entièrement indéterminés ?

La variété de ces patrons ambigus n'est pas du tout trop grande pour pouvoir être classifiée au moyen de suffixes ajoutés aux A, R, U et W, selon le cas. Mon propre système de suffixes pourrait être revu et amélioré. Il serait surtout à désirer qu'on fit de nouveaux efforts pour déterminer exactement les variétés bien marquées de W, et pour subdiviser A.

b. De quels doigts doit-on prendre des empreintes quand tous les doigts ne sont pas marqués ? Il n'est pas facile de répondre à cette question. Il est cependant évident que lorsqu'on ne prend qu'un doigt, ce doigt doit être toujours le même, appelons-le *m*. Lorsqu'on en prendra deux, l'un de ces doigts devra être *m* et l'on pourra appeler l'autre *n*. Quand on prendra trois doigts, le premier devra être *m*, le second *n*, et ainsi de suite. Voir aussi le paragraphe suivant.

c. Dans quel ordre doit-on lire et écrire les empreintes pour former le titre sous lequel la série est classifiée ou cataloguée. Il serait bon (si l'on négligeait d'autres considérations importantes) que les doigts qui sont le plus universellement adoptés fussent choisis en premier lieu (voir *b*). Je ne suis point du tout satisfait de l'ordre que j'ai adopté jusqu'à présent, et je préférerais maintenant lire les empreintes dans l'ordre naturel des impressions, en commençant par celle du petit doigt de la main gauche et en terminant par le petit doigt de la main droite, mais cette méthode présente quelques inconvénients indépendamment de ceux mentionnés ci-dessus. Peut-être, pourrait-on arriver à un compromis dans le choix de la meilleure méthode tel que : 1. Main droite, — du pouce au petit doigt ; 2. Main gauche, — du petit doigt au pouce.

d. Quelle notation est la plus commode pour les titres ? Les lettres A, R, U, W ne sont pas très claires pour les diverses combinaisons dans les séries de 10 lettres. Elles sont aussi un peu ennuyeuses à écrire avec la clarté nécessaire et je préfère aujourd'hui me servir de traits fermes et simples, comme ceux qui sont employés en sténographie et qui ont quelque ressemblance avec les patrons qu'ils représentent. Ce sont : un accent circonflexe ^, un accent aigu ´, un accent grave `, et un petit cercle °. Dans cette notation, la classe L est divisée d'une nouvelle manière et les classifications actuelles des systèmes R et U devront être arrangées de nouveau, R et U ayant des pentes opposées aux deux mains. Dans la nouvelle méthode, L est divisé en deux classes selon la pente du patron, comme on le voit dans l'impression. La nouvelle méthode est aujourd'hui en usage dans le bureau anglais et semble donner de bons résultats. Les raisons qui parlent en faveur de ces changements, ou contre eux, ont été mentionnées en détail dans mon livre.

Nous espérons que, quoique ces observations puissent paraître difficiles à saisir au premier abord, pour les personnes qui n'ont jamais étudié les empreintes digitales, elles seront assez intelligibles pour ceux qui s'adonnent sérieusement à l'étude de cette importante branche de l'anthropométrie criminelle pour en étendre les effets et arriver à établir une uniformité internationale dans les moyens.

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From the PROCEEDINGS OF THE ROYAL SOCIETY, VOL. 61.

THE AVERAGE CONTRIBUTION OF EACH SEVERAL ANCESTOR TO THE TOTAL HERITAGE OF THE OFFSPRING.

BY

FRANCIS GALTON, D.C.L., Sc.D., F.R.S.

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[From the PROCEEDINGS OF THE ROYAL SOCIETY, Vol. 61.]

"The average Contribution of each several Ancestor to the total Heritage of the Offspring." By FRANCIS GALTON, D.C.L., Sc.D., F.R.S. Received and Read June 3, 1897.

In the following memoir the truth will be verified in a particular instance, of a statistical law of heredity that appears to be universally applicable to bisexual descent. I stated it briefly and with hesitation in my book 'Natural Inheritance' (Macmillan, 1889; page 134), because it was then unsupported by sufficient evidence. Its existence was originally suggested by general considerations, and it might, as will be shown, have been inferred from them with considerable assurance. Consequently, as it is now found to hold good in a special case, there are strong grounds for believing it to be a general law of heredity.

I have had great difficulty in obtaining a sufficient amount of suitable evidence for the purpose of verification A somewhat extensive series of experiments with moths were carried on, in order to supply it, but they unfortunately failed, partly owing to the diminishing fertility of successive broods and partly to the large disturbing effects of differences in food and environment on different broods and in different places and years. No statistical results of any consistence or value could be obtained from them. Latterly, while engaged in planning another extensive experiment with small, fast-breeding mammals, I became acquainted with the existence of a long series of records, preserved by Sir Everett Millais, of the colours during many successive generations of a large pedigree stock of Basset hounds, that he originated some twenty years ago, having purchased ninety-three of them on the Continent, for the purpose. These records afford the foundation upon which this memoir rests.

The law to be verified may seem at first sight too artificial to be true, but a closer examination shows that prejudice arising from the cursory impression is unfounded. This subject will be alluded to again, in the meantime the law shall be stated. It is that the two parents contribute between them on the average one-half, or (0·5) of the total heritage of the offspring; the four grandparents, one-quarter, or $(0\cdot5)^2$; the eight great-grandparents, one-eighth, or $(0\cdot5)^3$, and so on. Thus the sum of the ancestral contributions is expressed by the series $\{(0\cdot5)+(0\cdot5)^2+(0\cdot5)^3, \&c.\}$, which, being equal to 1, accounts for the whole heritage.

The same statement may be put into a different form, in which a parent, grandparent, &c., is spoken of without reference to sex, by saying that each parent contributes on an average one-quarter, or $(0\cdot5)^2$, each grandparent one-sixteenth, or $(0\cdot5)^4$, and so on, and that generally the occupier of each ancestral place in the *n*th degree, whatever be the value of n, contributes $(0\cdot5)^{2n}$ of the heritage.

In interbred stock there are always fewer, and usually far fewer, different individuals among the ancestry than ancestral places for them to fill. A pedigree stock descended from a single couple, m generations back, will have 2^m ancestral places of the *m*th order, but only two individuals to fill them; therefore if $m = 10$ there are 1024 such places; if $m = 20$ there are more than a million. Whenever the same individual occupies many places he will be separately rated for each of them.

The neglect of individual prepotencies is justified in a law that avowedly relates to average results; they must of course be taken into account when applying the general law to individual cases. No difficulty arises in dealing with characters that are limited by sex, when their equivalents in the opposite sex are known, for instance in the statures of men and women.

The law may be applied *either* to total values or to deviations, as will be gathered from the following equation. Let M be the mean value from which all deviations are reckoned, and let D_1, D_2, &c., be the means of all the deviations, including their signs, of the ancestors in the 1st, 2nd, &c., degrees respectively; then

$$\tfrac{1}{2}(M+D_1)+\tfrac{1}{4}(M+D_2)+\&c. = M+(\tfrac{1}{2}D_1+\tfrac{1}{4}D_2+\&c.)$$

It should noted that nothing in this statistical law contradicts the generally accepted view that the chief, if not the sole, line of descent runs from germ to germ and not from person to person. The person may be accepted on the whole as a fair representative of the germ, and, being so, the statistical laws which apply to the persons would apply to the germs also, though with less precision in individual cases. Now this law is strictly consonant with the observed binary subdivisions of the germ cells, and the concomitant extrusion and loss of one-half of the several contributions from each of the two parents to the germ-cell of the offspring. The apparent artificiality of the law ceases on these grounds to afford cause for doubt; its close agreement with physiological phenomena ought to give a prejudice in *favour* of its truth rather than the contrary.

Again, a wide though limited range of observation assures us that the occupier of each ancestral place *may* contribute something of his own personal peculiarity, apart from all others, to the heritage of the offspring. Therefore there is such a thing as an average contribution appropriate to each ancestral place, which admits of statistical valuation, however minute it may be. It is also well known that the more remote stages of ancestry contribute considerably less than the nearer ones. Further, it is reasonable to believe that the contributions of parents to children are in the same proportion as those of the grandparents to the parents, of the great-grandparents to the grandparents, and so on; in short, that their total amount is to be expressed by the sum of the terms in an infinite geometric series diminishing to zero. Lastly, it is an essential condition that their total amount should be equal to 1, in order to account for the whole of the heritage. All these conditions are fulfilled by the series of $\frac{1}{2}+\frac{1}{2}^2+\frac{1}{2}^3+$ &c., and by no other. These and the foregoing considerations were referred to when saying that the law might be inferred with considerable assurance *à priori*; consequently, being found true in the particular case about to be stated, there is good reason to accept the law in a general sense.

The Bassets are dwarf blood-hounds, of two, and only two, recognised varieties of colour. Excluding, as I have done, a solitary exception of black and tan, they are either white, with large blotches ranging between red and yellow, or they may in addition be marked with more or less black. In the former case they are technically known and registered as "lemon and white," in the latter case as "tricolour." Tricolour is, in fact, the introduction of melanism, so I shall treat the colours simply as being "tricolour" or "non-tricolour;" more briefly, as T. or N. I am assured that transitional cases between T. and N. are very rare, and that experts would hardly ever disagree about the class to which any particular hound should be assigned. A stud-book is published from time to time containing the pedigrees, dates of birth, and the names of the breeders of these valuable animals. The one I have used bears the title 'The Basset Hound Club Rules and Stud-Book,' compiled by Everett Millais, 1874–1896. It contains the names of nearly 1000 hounds, to which Sir Everett Millais has very obligingly, at my request, appended their colours so far as they have been registered, which during later years has almost invariably been done. The upshot is that I have had the good fortune to discuss a total of 817 hounds of known colour, all descended from parents of known colour. In 567 out of these 817, the colours of all four grandparents were also known. These two sets are summarised in Table I and discussed in Table V, and they afford the data for Tables II, III, and IV. In 188 of the above cases the colours of all the eight great-grandparents were known as well; this third set is discussed in Table VI.

Partly owing to inequality in the numbers of the tricolours and non-tricolours, and partly owing to a selective mating in favour of the former, the different possible combinations of T. and N. ancestry are by no means equally common. The effect of this is conspicuous in Table I, where the entries are huddled together in some parts and absent in others. Still, though the data are not distributed as evenly as could be wished, they will serve our purpose if we are justified in grouping them without regard to sex; or, more generally, if we treat the 2^n components of each several A_n, whatever be the value of n, as equally efficient contributors.

Our first inquiry then must be, "Is or is not one sex so markedly prepotent over the other, in transmitting colour, that a disregard of sex would introduce statistical error?" In answering this, we should bear in mind a common experience, that statistical questions relating to sex are very difficult to deal with. Large and unknown disturbing causes appear commonly to exist, that make data which are seemingly homogeneous, very heterogeneous in reality. Some of these are undoubtedly present here, especially such as may be due to individual prepotencies combined with close interbreeding. For although this pedigree stock originated in as many as ninety-three different hounds, presumably more or less distant relations to one another, some of them proved of so much greater value than the rest that very close interbreeding has subsequently been resorted to in numerous instances. In order to show the danger of trusting blindly to averages of sex, even when the numbers are large, I have compared the results derived from different sets of data, namely from those contained in the last two columns of Table I, where they are distinguished by the letters A and B, and have treated them both separately and together in Table II. They will be seen to disagree widely, concurring only in showing that the dam is prepotent over the sire in transmitting colour. According to the A data, their relative efficacy in this respect is as 58 to 51, say 114 to 100; according to the B data, it is as 47 to 32, say 147 to 100. Taking all the data together, it is as 54 to 45, say 120 to 100, or as 6 to 5.

It does not seem to me that this ratio of efficacy of 6 to 5 is sufficient to overbear the statistical advantages of grouping the sexes as if they were equally efficient, the error in one case being more or less balanced by an opposite error in the other. It is true that the reciprocal forms of mating are by no means equally numerous, the prevailing tendency to use tricolours as sires being conspicuous. Still, as will be found later, on the application of a general test, the error feared is too insignificant to be observed. Should, however, a much larger collection of these data be obtained hereafter, minutiæ ought to be taken into account which may now be disregarded, and the neglect of female prepotency would cease to be justified.

The law to be verified supposes all the ancestors to be known, or to be known for so many generations back that the effects of the unknown residue are too small for consideration. The amount of the residual effect, beyond any given generation, is easily determined by the fact that in the series $\frac{1}{2}+\frac{1}{4}+\frac{1}{8}$, &c., each term is equal to the sum of all its successors. Now in the two sets of cases to be dealt with the larger refers to only two generations, therefore as the effect of the second generation is $\frac{1}{4}$, that of the unknown residue is $\frac{1}{4}$ also. The smaller set refers to three generations, leaving an unknown residual effect of $\frac{1}{8}$. These large residues cannot be ignored, amounting, as they do, to 25 and 12·5 per cent. respectively. We have, therefore, to determine fixed and reasonable rules by which they should be apportioned.

The requisite data for doing this are given in Table III, which shows that 79 per cent. of the parents of tricolour hounds are tricolour also, and that 56 per cent. of the parents of non-tricolour hounds are tricolour. It is not to be supposed that the trustworthiness of these results reaches to 1 per cent., but they are the best available data, so I adopt them.

It will be convenient to use the following nomenclature in calculation:—

a_0 stands for a single member of the offspring.

a_1 for a single parent; a_2 for a single grandparent, and so on, the suffix denoting the number of the generation. A parallel nomenclature, using capital letters, is:—

A_0 stands for all the offspring of the same ancestry.

A_1 for the two parents; A_2 for all the four grandparents, and so on. Consequently A_n contains 2^n individuals, each of the form a_n, and A_n contributes $(0{\cdot}5)^n$ to the heritage of each a_0; while each a_n contributes $(0{\cdot}5)^{2n}$ to it.

In the upper part of Table IV the ratios are entered of the average contributions of T. supplied by *known* ancestors. Nothing further need be said about these, except that they are styled coefficients because they must be multiplied into the total number of offspring, in order to calculate the number of them that will, on their separate and independent accounts, be probably tricolours.

We have next to explain how the coefficients for the *unknown* ancestry have been calculated, namely, those which are entered in the lower part of Table IV. Suppose all the four grandparents, A_2, to be tricolour, then only 0·79 of A_3 will be tricolour also, $(0{\cdot}79)^2$ of A_4, and so on. These several orders of ancestry will respectively contribute an average of tricolour to each a_0 of the amounts of $(0{\cdot}5)^3\times(0{\cdot}79)$, $(0{\cdot}5)^4\times(0{\cdot}79)^2$, &c. Consequently the sum of their tricolour contributions is

$$(0{\cdot}5)^3\times(0{\cdot}79)\,\{1+(0{\cdot}5)\times(0{\cdot}79)+(0{\cdot}5)^2\times(0{\cdot}79)^2+\&c.\},$$

which equals 0·1632. The average tricolour contribution from the ancestry of *each* of the four tricolour grandparents must be reckoned as the quarter of this, namely, 0·0408.

By a similar process, the average tricolour contribution from the ancestry of *each* non-tricolour grandparent is found to be 0·0243.

When the furthest known generation is that of the great-grandparents, the formula differs from the foregoing only by substituting $(0{\cdot}5)^4\times(0{\cdot}79)$ for $(0{\cdot}5)^3\times(0{\cdot}79)$. This makes the average tricolour contribution from the ancestry of the whole eight tricolour great-grandparents equal to 0·08160, and that from the ancestry of *each* of them to be one-eighth of this, or 0·0102.

In a similar way the tricolour contribution from the ancestry of *each* non-tricolour great-grandparent is found to be 0·0061.

The following example shows how the coefficients in Table IV were utilised in calculating the general coefficients entered in Table V.

2 Parents, T_1 (personal)	0·5000
3 Grandparents, T_2 (personal)	0·1875
1 Grandparent, N_2 (personal)	—
3 Grandparents, T_2 (ancestral)......	0·1224
1 Grandparent, N_2 (ancestral)	0·0243
Total tricolour contribution ..	0·8342

The coefficient 0·83 will consequently be found under the appropriate head in Table V, where the total number of offspring ("all cases") is recorded as 119. By multiplying these together, viz., $0{\cdot}83\times119$, the "calculated" number of 99 is obtained. It will be seen that the observed number was 101, a difference of only 2 per cent.

The extraordinarily close coincidence throughout the two tables,

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V and VI, between calculation and observation, proves that the law is correct in the present instance, and that the principle by which the unknown ancestry was apportioned, is practically exact also. It is not so strictly exact as it might have been, because the whole of the available knowledge has not been utilised. The 0·79 applied to A_4, &c., requires some small correction according to the known colours of the *offspring* of A_3. If they had been all tricolour the 0·79 would have to be increased; if all non-tricolour, it would have to be diminished. Having insufficient data to check a theoretical emendation, I note its omission, but shall not discuss the matter further.

It will be easily understood from these remarks how *collateral* data are to be brought into calculation, for if the collaterals were more tricolour than the average of hounds, the 0·79 would have to be somewhat increased (but not beyond the limiting value of 1·00); if less tricolour than the average, the 0·79 would have to be diminished. The knowledge of collaterals would be superfluous, if that of the direct ancestry were complete, but this important prolongation of the present subject must not be considered further on this occasion.

There are three stages in Tables V and VI at which comparisons may be made between calculated results and observed facts.

(1) *The Grand Totals.*—In Table V the sum of all the calculated values amounts to 391; that of all the observed ones to 387, which are closely alike. In Table VI they amount to 180 and 181 respectively, which is a still closer resemblance. Consequently the calculations are practically exact *on the whole*, and the error occasioned by neglect of sex, &c., is insignificant.

(2) *The Subordinate Pairs of Totals.*—These are entered at the sides of the tables, and are nine in number, namely, 236, 239; 149, 139; 6, 9; 53, 56; 52, 56: 9, 9; 8, 6; 49, 46; 9, 8. The coincidences are striking, in comparison with such results as statisticians have usually to be contented with; the second pair, 149, 139, is the least good, and will be considered in the next paragraph.

(3) *Individual Pairs of Entries.*—There are 32 of these; here also calculation compares excellently well with observation, excepting in the line that furnishes the "subordinate totals" of 149, 139, where the "all cases" of 37, 158, 60 yield the tricolour contingents of 20, 79, 36. Dividing each tricolour by the corresponding "all cases," we obtain what may be called "Coefficients from Observation," to compare with the calculated coefficients. They are as follows:—

		Diff.		Diff.	
Coefficients from observation	54	(−4)	50	(+10)	60
" " calculation	66	(−8)	58	(−7)	51

The great irregularity of the entries in the upper line shows that the observed values cannot be accepted as true representa-

f. 5v

tives of the normal condition. I have not unravelled the causes of this error, and it is not urgent to do so, since its ill effects are swamped by the large number of successes elsewhere.

In order to satisfy myself that the correspondence between calculated and observed values was a sharp test of the correctness of the coefficients, I made many experiments by altering them slightly, and recalculating. In every case there was a notable diminution in the accuracy of the results. The test that the theory has successfully undergone appeared on that account, to be even more searching and severe than I had anticipated.

It is hardly necessary to insist on the importance of possessing a correct law of heredity. Vast sums of money are spent in rearing pedigree stock of the most varied kinds, as horses, cattle, sheep, pigs, dogs, and other animals, besides flowers and fruits. The current views of breeders and horticulturists on heredity are contradictory in important respects, and therefore *must* be more or less erroneous. Certainly no popular view at all resembles that which is justified by the present memoir. A correct law of heredity would also be of service in discussing actuarial problems relating to hereditary longevity and disease, and it might throw light on many questions connected with the theory of evolution.

Table I.—Pedigrees of Parental and Grand Parental Colours (Tricolour (T) or Non-Tricolour (N)).

The Sex of the Ancestors is taken into Account, but not that of the Offspring.

Sire.	Dam.	Offspring.	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*i*	*j*	*k*	*l*	*m*	*n*	*o*	*p*	Totals. A.	Others of which all G. P.'s are not known. B.
Sire's sire			T	T	T	T	T	T	T	T	N	N	N	N	N	N	N	N		
Sire's dam			T	T	N	N	T	T	N	N	T	T	N	N	T	T	N	N		
Dam's sire			T	T	T	T	N	N	N	N	T	T	T	T	N	N	N	N		
Dam's dam			T	N	T	N	T	N	T	N	T	N	T	N	T	N	T	N		
Tric.	Tric. ..	Tric.	106	38	47	12	12	7	2	4	3	..	3	4	..	..	..	..	239	87
		Non-T.	13	4	12	2	2	1	1	..	..	..	..	3	..	..	..	..	38	20
Tric.	Non-T.	Tric.	..	..	16	3	..	..	1	..	3	..	..	..	1	1	..	..	25	15
		Non-T.	..	..	6	1	..	..	3	..	2	..	..	4	1	1	..	..	18	17
Non-T. ..	Tric. ..	Tric.	20	45	4	20	11	4	4	..	..	2	..	3	1	..	..	..	114	30
		Non-T.	17	50	16	9	5	..	1	1	..	8	.	1	1	..	..	..	109	64
Non-T. ..	Non-T.	Tric.	..	..	..	2	..	..	..	..	..	5	..	1	..	1	..	..	9	11
		Non-T.	..	..	..	8	..	..	..	4	..	3	..	..	..	.	..	..	15	6
Totals			156	138	101	57	30	12	12	9	8	18	3	16	4	3	..	..	567	250

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Table II.—Offspring of one parent Tricolour (T) and of one Non-tricolour (N). The sex of the parents is not regarded.

From Table I.		Observed.			Per cents.	
		Tricolour.	Non-tricolour.		Tricolour.	Non-tricolour.
Sire T, dam N	A	114	109	223	51	49
	B	30	64	94	32	68
	Sum	144	173	317	45	55
Dam T, sire N	A	25	18	43	58	42
	B	15	17	32	47	53
	Sum	40	35	75	54	46

Table III.—Distribution of T and N colour in Parents, when the Offspring are T and N respectively.

From Table I.		No. of T offspring.			Parents* of T offspring.	
Sires.	Dams.	A.	B.	Total.	T.	N.
T	T	239	87	326	652	0
T	N	25	15	40	40	40
N	T	114	30	144	144	144
N	N	9	11	20	0	40
Totals				530	836	224
Per cent. of parents*				50	79	21

From Table I.		No. of N offspring.			Parents* of N offspring.	
Sires.	Dams.	A.	B.	Total.	T.	N.
T	T	38	20	58	116	0
T	N	18	17	35	35	35
N	T	109	64	173	173	173
N	N	15	6	21	0	42
Totals				287	324	250
Per cent. of Parents*				50	56	44

* More properly "Parental Places"; the number of these, though not that of the individual parents, being always double the number of any group of offspring.

Table IV.—Tricolour coefficients.

	Ancestry known up to and inclusive of	
	Grandparents.	Great-grandparents.
Personal allowance of T for each		
Tricolour parent	0·2500	0·2500
„ grandparent	0·0625	0·0625
„ great-grandparent	—	0·0156
(No allowance for Non-tricolours.)	—	—
Ancestral allowance of T for each		
Tricolour grandparent	0·0408	—
Non-tricolour „	0·0243	—
Tricolour great-grandparent	—	0·0102
Non-tricolour „ „	—	0·0061

Table V.—Calculation and Observation Compared.

The pedigrees are utilised up to the second ascending generation. Sex not taken into account.

No. of tricolours in parents.		Number of tricolours in grand-parents.				Total tricolour offspring.	
		4	3	2	1	Calculated.	Observed.
		a	*bcei*	*dfgjkm*	*hlno*		
2	All cases	119	119	28	11		
	Coefficient	0·91	0·83	0·76	0·68		
	Tricolour calc'd.	*108*	*99*	*21*	*8*	*236*	
	„ observed	106	101	24	8		239
1	All cases	37	158	60	6		
	Coefficient	0·66	0·58	0·51	0·43		
	Tricolour calc'd.	*24*	*92*	*30*	*3*	*149*	
	„ observed	20	79	36	4		139
0	All cases	..	..	18	6		
	Coefficient	..	..	0·26	0·18		
	Tricolour calc'd.	..	..	*5*	*1*	*6*	
	„ observed	..	..	7	2		9
				Grand totals		*391*	387

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Table VI.—Calculation and Observation Compared.

The pedigrees are utilised up to the third ascending generation. Sex not taken into account.

No. of tricolours in parents.	No. of tricolours in grand-parents.		Number of tricolours in great-grandparents: 8	7	6	5	4	Total tricolour offspring: Calculated.	Observed.
2	4	All cases	2	25	14	16			
		Coefficient	0·96	0·94	0·92	0·90			
		Tricolours calc'd.	*2*	*24*	*13*	*14*	..	*53*	
		„ observed	2	25	14	15	..		56
	3	All cases	..	18	21	16	6		
		Coefficient	..	0·87	0·85	0·83	0·81		
		Tricolours calc'd.	..	*16*	*18*	*13*	*5*	*52*	
		„ observed	..	17	19	14	6		56
	2	All cases	..	3	2	3	3		
		Coefficient	..	0·81	0·79	0·77	0·75		
		Tricolours calc'd.	..	*2*	*3*	*2*	*2*	*9*	
		„ observed	..	2	2	3	2		9
1	4	All cases	..	2	1	9			
		Coefficient	..	0·69	0·67	0·65			
		Tricolours calc'd.	..	*1*	*1*	*6*	..	*8*	
		„ observed	..	1	..	5	..		6
	3	All cases	1	28	14	31	9		
		Coefficient	0·64	0·62	0·60	0·58	0·56		
		Tricolours calc'd.	*1*	*17*	*8*	*18*	*5*	*49*	
		„ observed.	1	16	12	8	9		46
	2	All cases	..	..	4	13			
		Coefficient	..	..	0·54	0·52			
		Tricolours calc'd.	..	..	*2*	*7*	..	*9*	
		„ observed	..	..	1	7	..		8
						Grand totals ..		*180*	181

The summed data derived from Table IV, form the coefficients entered in Tables V and VI. These are multiplied into the corresponding number of "all cases," and the result gives the "calculated" number of tricolour hounds among them.

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The entries of "all cases" and of "tricolours observed" in Table V are deduced from Table I, by combining the appropriate columns. The letters at the top show which columns are combined.

Seven other observed cases, disposed in three groups, are scattered beyond the limits of Table VI; two of these seven cases are tricolour.

Harrison and Sons, Printers in Ordinary to Her Majesty St. Martin's Lane.

Erasmus Galton

f. 10

From FRANCIS GALTON,
42, Rutland Gate, London.

From the PROCEEDINGS OF THE ROYAL SOCIETY, VOL. 61.

Duplicate

THE AVERAGE CONTRIBUTION OF EACH SEVERAL ANCESTOR TO THE TOTAL HERITAGE OF THE OFFSPRING.

BY

FRANCIS GALTON, D.C.L., Sc.D., F.R.S.

f. 11r

[From the PROCEEDINGS OF THE ROYAL SOCIETY, Vol. 61.]

"The average Contribution of each several Ancestor to the total Heritage of the Offspring." By FRANCIS GALTON, D.C.L., Sc.D., F.R.S. Received and Read June 3, 1897.

In the following memoir the truth will be verified in a particular instance, of a statistical law of heredity that appears to be universally applicable to bisexual descent. I stated it briefly and with hesitation in my book 'Natural Inheritance' (Macmillan, 1889; page 134), because it was then unsupported by sufficient evidence. Its existence was originally suggested by general considerations, and it might, as will be shown, have been inferred from them with considerable assurance. Consequently, as it is now found to hold good in a special case, there are strong grounds for believing it to be a general law of heredity.

I have had great difficulty in obtaining a sufficient amount of suitable evidence for the purpose of verification A somewhat extensive series of experiments with moths were carried on, in order to supply it, but they unfortunately failed, partly owing to the diminishing fertility of successive broods and partly to the large disturbing effects of differences in food and environment on different

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broods and in different places and years. No statistical results of any consistence or value could be obtained from them. Latterly, while engaged in planning another extensive experiment with small, fast-breeding mammals, I became acquainted with the existence of a long series of records, preserved by Sir Everett Millais, of the colours during many successive generations of a large pedigree stock of Basset hounds, that he originated some twenty years ago, having purchased ninety-three of them on the Continent, for the purpose. These records afford the foundation upon which this memoir rests.

The law to be verified may seem at first sight too artificial to be true, but a closer examination shows that prejudice arising from the cursory impression is unfounded. This subject will be alluded to again, in the meantime the law shall be stated. It is that the two parents contribute between them on the average one-half, or (0·5) of the total heritage of the offspring; the four grand-parents, one-quarter, or $(0{\cdot}5)^2$; the eight great-grandparents, one-eighth, or $(0{\cdot}5)^3$, and so on. Thus the sum of the ancestral contributions is expressed by the series $\{(0{\cdot}5)+(0{\cdot}5)^2+(0{\cdot}5)^3, \text{ \&c.}\}$, which, being equal to 1, accounts for the whole heritage.

The same statement may be put into a different form, in which a parent, grandparent, &c., is spoken of without reference to sex, by saying that each parent contributes on an average one-quarter, or $(0{\cdot}5)^2$, each grandparent one-sixteenth, or $(0{\cdot}5)^4$, and so on, and that generally the occupier of each ancestral place in the *n*th degree, whatever be the value of n, contributes $(0{\cdot}5)^{2n}$ of the heritage.

In interbred stock there are always fewer, and usually far fewer, different individuals among the ancestry than ancestral places for them to fill. A pedigree stock descended from a single couple, m generations back, will have 2^m ancestral places of the *m*th order, but only two individuals to fill them; therefore if $m = 10$ there are 1024 such places; if $m = 20$ there are more than a million. Whenever the same individual occupies many places he will be separately rated for each of them.

The neglect of individual prepotencies is justified in a law that avowedly relates to average results; they must of course be taken into account when applying the general law to individual cases. No difficulty arises in dealing with characters that are limited by sex, when their equivalents in the opposite sex are known, for instance in the statures of men and women.

The law may be applied *either* to total values or to deviations, as will be gathered from the following equation. Let M be the mean value from which all deviations are reckoned, and let D_1, D_2, &c., be the means of all the deviations, including their signs, of the ancestors in the 1st, 2nd, &c., degrees respectively; then

$$\tfrac{1}{2}(M+D_1)+\tfrac{1}{4}(M+D_2)+\text{\&c.} = M+(\tfrac{1}{2}D_1+\tfrac{1}{4}D_2+\text{\&c.})$$

It should noted that nothing in this statistical law contradicts the generally accepted view that the chief, if not the sole, line of descent runs from germ to germ and not from person to person. The person may be accepted on the whole as a fair representative of the germ, and, being so, the statistical laws which apply to the persons would apply to the germs also, though with less precision in individual cases. Now this law is strictly consonant with the observed binary subdivisions of the germ cells, and the concomitant extrusion and loss of one-half of the several contributions from each of the two parents to the germ-cell of the offspring. The apparent artificiality of the law ceases on these grounds to afford cause for doubt; its close agreement with physiological phenomena ought to give a prejudice in *favour* of its truth rather than the contrary.

Again, a wide though limited range of observation assures us that the occupier of each ancestral place *may* contribute something of his own personal peculiarity, apart from all others, to the heritage of the offspring. Therefore there is such a thing as an average contribution appropriate to each ancestral place, which admits of statistical valuation, however minute it may be. It is also well known that the more remote stages of ancestry contribute considerably less than the nearer ones. Further, it is reasonable to believe that the contributions of parents to children are in the same proportion as those of the grandparents to the parents, of the great-grandparents to the grandparents, and so on; in short, that their total amount is to be expressed by the sum of the terms in an infinite geometric series diminishing to zero. Lastly, it is an essential condition that their total amount should be equal to 1, in order to account for the whole of the heritage. All these conditions are fulfilled by the series of $\frac{1}{2}+\frac{1}{2}^2+\frac{1}{2}^3+$ &c., and by no other. These and the foregoing considerations were referred to when saying that the law might be inferred with considerable assurance *à priori*; consequently, being found true in the particular case about to be stated, there is good reason to accept the law in a general sense.

The Bassets are dwarf blood-hounds, of two, and only two, recognised varieties of colour. Excluding, as I have done, a solitary exception of black and tan, they are either white, with large blotches ranging between red and yellow, or they may in addition be marked with more or less black. In the former case they are technically known and registered as "lemon and white," in the latter case as "tricolour." Tricolour is, in fact, the introduction of melanism, so I shall treat the colours simply as being "tricolour" or "non-tricolour;" more briefly, as T. or N. I am assured that transitional cases between T. and N. are very rare, and that experts would hardly ever disagree about the class to which any particular hound should be assigned. A stud-book is published from time to time

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containing the pedigrees, dates of birth, and the names of the breeders of these valuable animals. The one I have used bears the title 'The Basset Hound Club Rules and Stud-Book,' compiled by Everett Millais, 1874–1896. It contains the names of nearly 1000 hounds, to which Sir Everett Millais has very obligingly, at my request, appended their colours so far as they have been registered, which during later years has almost invariably been done. The upshot is that I have had the good fortune to discuss a total of 817 hounds of known colour, all descended from parents of known colour. In 567 out of these 817, the colours of all four grandparents were also known. These two sets are summarised in Table I and discussed in Table V, and they afford the data for Tables II, III, and IV. In 188 of the above cases the colours of all the eight great-grandparents were known as well; this third set is discussed in Table VI.

Partly owing to inequality in the numbers of the tricolours and non-tricolours, and partly owing to a selective mating in favour of the former, the different possible combinations of T. and N. ancestry are by no means equally common. The effect of this is conspicuous in Table I, where the entries are huddled together in some parts and absent in others. Still, though the data are not distributed as evenly as could be wished, they will serve our purpose if we are justified in grouping them without regard to sex; or, more generally, if we treat the 2^n components of each several A_n, whatever be the value of n, as equally efficient contributors.

Our first inquiry then must be, "Is or is not one sex so markedly prepotent over the other, in transmitting colour, that a disregard of sex would introduce statistical error?" In answering this, we should bear in mind a common experience, that statistical questions relating to sex are very difficult to deal with. Large and unknown disturbing causes appear commonly to exist, that make data which are seemingly homogeneous, very heterogeneous in reality. Some of these are undoubtedly present here, especially such as may be due to individual prepotencies combined with close interbreeding. For although this pedigree stock originated in as many as ninety-three different hounds, presumably more or less distant relations to one another, some of them proved of so much greater value than the rest that very close interbreeding has subsequently been resorted to in numerous instances. In order to show the danger of trusting blindly to averages of sex, even when the numbers are large, I have compared the results derived from different sets of data, namely from those contained in the last two columns of Table I, where they are distinguished by the letters A and B, and have treated them both separately and together in Table II. They will be seen to disagree widely, concurring only in showing that the dam is prepotent over the sire in transmitting colour. According to the A data, their relative efficacy in this respect is as 58 to 51, say 114 to 100; according to the B data, it is as 47 to 32, say 147 to 100. Taking all the data together, it is as 54 to 45, say 120 to 100, or as 6 to 5.

It does not seem to me that this ratio of efficacy of 6 to 5 is sufficient to overbear the statistical advantages of grouping the sexes as if they were equally efficient, the error in one case being more or less balanced by an opposite error in the other. It is true that the reciprocal forms of mating are by no means equally numerous, the prevailing tendency to use tricolours as sires being conspicuous. Still, as will be found later, on the application of a general test, the error feared is too insignificant to be observed. Should, however, a much larger collection of these data be obtained hereafter, minutiæ ought to be taken into account which may now be disregarded, and the neglect of female prepotency would cease to be justified.

The law to be verified supposes all the ancestors to be known, or to be known for so many generations back that the effects of the unknown residue are too small for consideration. The amount of the residual effect, beyond any given generation, is easily determined by the fact that in the series $\frac{1}{2}+\frac{1}{4}+\frac{1}{8}$, &c., each term is equal to the sum of all its successors. Now in the two sets of cases to be dealt with the larger refers to only two generations, therefore as the effect of the second generation is $\frac{1}{4}$, that of the unknown residue is $\frac{1}{4}$ also. The smaller set refers to three generations, leaving an unknown residual effect of $\frac{1}{8}$. These large residues cannot be ignored, amounting, as they do, to 25 and 12·5 per cent. respectively. We have, therefore, to determine fixed and reasonable rules by which they should be apportioned.

The requisite data for doing this are given in Table III, which shows that 79 per cent. of the parents of tricolour hounds are tricolour also, and that 56 per cent. of the parents of non-tricolour hounds are tricolour. It is not to be supposed that the trustworthiness of these results reaches to 1 per cent., but they are the best available data, so I adopt them.

It will be convenient to use the following nomenclature in calculation:—

a_0 stands for a single member of the offspring.

a_1 for a single parent; a_2 for a single grandparent, and so on, the suffix denoting the number of the generation. A parallel nomenclature, using capital letters, is:—

A_0 stands for all the offspring of the same ancestry.

A_1 for the two parents; A_2 for all the four grandparents, and so on. Consequently A_n contains 2^n individuals, each of the form a_n, and A_n contributes $(0{\cdot}5)^n$ to the heritage of each a_0; while each a_n contributes $(0{\cdot}5)^{2n}$ to it.

In the upper part of Table IV the ratios are entered of the average contributions of T. supplied by *known* ancestors. Nothing further need be said about these, except that they are styled coefficients because they must be multiplied into the total number of offspring, in order to calculate the number of them that will, on their separate and independent accounts, be probably tricolours.

We have next to explain how the coefficients for the *unknown* ancestry have been calculated, namely, those which are entered in the lower part of Table IV. Suppose all the four grandparents, A_2, to be tricolour, then only 0·79 of A_3 will be tricolour also, $(0{\cdot}79)^2$ of A_4, and so on. These several orders of ancestry will respectively contribute an average of tricolour to each a_0 of the amounts of $(0{\cdot}5)^3 \times (0{\cdot}79)$, $(0{\cdot}5)^4 \times (0{\cdot}79)^2$, &c. Consequently the sum of their tricolour contributions is

$$(0{\cdot}5)^3 \times (0{\cdot}79)\,\{1 + (0{\cdot}5) \times (0{\cdot}79) + (0{\cdot}5)^2 \times (0{\cdot}79)^2 + \&c.\},$$

which equals 0·1632. The average tricolour contribution from the ancestry of *each* of the four tricolour grandparents must be reckoned as the quarter of this, namely, 0·0408.

By a similar process, the average tricolour contribution from the ancestry of *each* non-tricolour grandparent is found to be 0·0243.

When the furthest known generation is that of the great-grandparents, the formula differs from the foregoing only by substituting $(0{\cdot}5)^4 \times (0{\cdot}79)$ for $(0{\cdot}5)^3 \times (0{\cdot}79)$. This makes the average tricolour contribution from the ancestry of the whole eight tricolour great-grandparents equal to 0·08160, and that from the ancestry of *each* of them to be one-eighth of this, or 0·0102.

In a similar way the tricolour contribution from the ancestry of *each* non-tricolour great-grandparent is found to be 0·0061.

The following example shows how the coefficients in Table IV were utilised in calculating the general coefficients entered in Table V.

2 Parents, T_1 (personal)	0·5000
3 Grandparents, T_2 (personal)	0·1875
1 Grandparent, N_2 (personal)	—
3 Grandparents, T_2 (ancestral).	0·1224
1 Grandparent, N_2 (ancestral)	0·0243
Total tricolour contribution ..	0·8342

The coefficient 0·83 will consequently be found under the appropriate head in Table V, where the total number of offspring ("all cases") is recorded as 119. By multiplying these together, viz., $0{\cdot}83 \times 119$, the "calculated" number of 99 is obtained. It will be seen that the observed number was 101, a difference of only 2 per cent.

The extraordinarily close coincidence throughout the two tables, V and VI, between calculation and observation, proves that the law is correct in the present instance, and that the principle by which the unknown ancestry was apportioned, is practically exact also. It is not so strictly exact as it might have been, because the whole of the available knowledge has not been utilised. The 0·79 applied to A_4, &c., requires some small correction according to the known colours of the *offspring* of A_3. If they had been all tricolour the 0·79 would have to be increased; if all non-tricolour, it would have to be diminished. Having insufficient data to check a theoretical emendation, I note its omission, but shall not discuss the matter further.

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Coefficients from observation	54	(−4)	50	(+10)	60
" " calculation	66	(−8)	58	(−7)	51

The great irregularity of the entries in the upper line shows that the observed values cannot be accepted as true representatives of the normal condition. I have not unravelled the causes of this error, and it is not urgent to do so, since its ill effects are swamped by the large number of successes elsewhere.

In order to satisfy myself that the correspondence between calculated and observed values was a sharp test of the correctness of the coefficients, I made many experiments by altering them slightly, and recalculating. In every case there was a notable diminution in the accuracy of the results. The test that the theory has successfully undergone appeared on that account, to be even more searching and severe than I had anticipated.

It is hardly necessary to insist on the importance of possessing a correct law of heredity. Vast sums of money are spent in rearing pedigree stock of the most varied kinds, as horses, cattle, sheep, pigs, dogs, and other animals, besides flowers and fruits. The current views of breeders and horticulturists on heredity are contradictory in important respects, and therefore *must* be more or less erroneous. Certainly no popular view at all resembles that which is justified by the present memoir. A correct law of heredity would also be of service in discussing actuarial problems relating to hereditary longevity and disease, and it might throw light on many questions connected with the theory of evolution.

Table I.—Pedigrees of Parental and Grand Parental Colours (Tricolour (T) or Non-Tricolour (N)).

The Sex of the Ancestors is taken into Account, but not that of the Offspring.

																			Totals.	Others of which all G. P.'s are not known.
Sire's sire / Sire's dam			T T	T T	T N	T N	T T	T T	T N	T N	N T	N T	N N	N N	N T	N T	N N	N N		
Dam's sire / Dam's dam			T T	T N	T T	T N	N T	N N	N T	N N	T T	T N	T T	T N	N T	N N	N T	N N		
Sire.	Dam.	Offspring.	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*i*	*j*	*k*	*l*	*m*	*n*	*o*	*p*	A.	B.
Tric.	Tric. ..	Tric.	106	38	47	12	12	7	2	4	3	..	3	4	..	..	..	..	239	87
		Non-T.	13	4	12	2	2	1	1	..	..	..	..	3	..	..	..	..	38	20
Tric.	Non-T.	Tric.	..	..	16	3	..	..	1	..	3	..	..	..	1	1	..	..	25	15
		Non-T.	..	..	6	1	..	..	3	..	2	..	..	4	1	1	..	..	18	17
Non-T. ..	Tric. ..	Tric.	20	45	4	20	11	4	4	..	..	2	..	3	1	..	..	..	114	30
		Non-T.	17	50	16	9	5	..	1	1	..	8	.	1	1	..	..	..	109	64
Non-T. ..	Non-T.	Tric.	..	..	..	2	..	..	..	..	..	5	..	1	..	1	..	..	9	11
		Non-T.	..	..	..	8	..	..	..	4	..	3	..	..	..	.	..	..	15	6
Totals			156	138	101	57	30	12	12	9	8	18	3	16	4	3	..	..	567	250

Table II.—Offspring of one parent Tricolour (T) and of one Non-tricolour (N). The sex of the parents is not regarded.

From Table I.		Observed.			Per cents.	
		Tricolour.	Non-tricolour.		Tricolour.	Non-tricolour.
Sire T, dam N	A	114	109	223	51	49
	B	30	64	94	32	68
	Sum	144	173	317	45	55
Dam T, sire N	A	25	18	43	58	42
	B	15	17	32	47	53
	Sum	40	35	75	54	46

Table III.—Distribution of T and N colour in Parents, when the Offspring are T and N respectively.

From Table I.		No. of T offspring.			Parents* of T offspring.	
Sires.	Dams.	A.	B.	Total.	T.	N.
T	T	239	87	326	652	0
T	N	25	15	40	40	40
N	T	114	30	144	144	144
N	N	9	11	20	0	40
Totals				530	836	224
Per cent. of parents*				50	79	21

From Table I.		No. of N offspring.			Parents* of N offspring.	
Sires.	Dams.	A.	B.	Total.	T.	N.
T	T	38	20	58	116	0
T	N	18	17	35	35	35
N	T	109	64	173	173	173
N	N	15	6	21	0	42
Totals				287	324	250
Per cent. of Parents*				50	56	44

* More properly "Parental Places"; the number of these, though not that of the individual parents, being always double the number of any group of offspring.

Table IV.—Tricolour coefficients.

	Ancestry known up to and inclusive of	
	Grandparents.	Great-grandparents.
Personal allowance of T for each		
Tricolour parent	0·2500	0·2500
„ grandparent	0·0625	0·0625
„ great-grandparent	—	0·0156
(No allowance for Non-tricolours.)	—	—
Ancestral allowance of T for each		
Tricolour grandparent	0·0408	—
Non-tricolour „	0·0243	—
Tricolour great-grandparent	—	0·0102
Non-tricolour „ „	—	0·0061

Table V.—Calculation and Observation Compared.

The pedigrees are utilised up to the second ascending generation. Sex not taken into account.

No. of tricolours in parents.		Number of tricolours in grand-parents.				Total tricolour offspring.	
		4	3	2	1	Calculated.	Observed.
		a	*bcei*	*dfgjkm*	*hlno*		
2	All cases	119	119	28	11		
	Coefficient......	0·91	0 83	0·76	0·68		
	Tricolour calc'd.	*108*	*99*	*21*	*8*	*236*	
	„ observed	106	101	24	8		239
1	All cases	37	158	60	6		
	Coefficient......	0·66	0·58	0·51	0·43		
	Tricolour calc'd.	*24*	*92*	*30*	*3*	*149*	
	„ observed	20	79	36	4		139
0	All cases	..	..	18	6		
	Coefficient......	..	..	0·26	0·18		
	Tricolour calc'd.	..	..	*5*	*1*	*6*	
	„ observed	..	..	7	2		9
					Grand totals	*391*	387

Table VI.—Calculation and Observation Compared.

The pedigrees are utilised up to the third ascending generation. Sex not taken into account.

No. of tricolours in parents.	No. of tricolours in grand-parents.		Number of tricolours in great-grandparents: 8	7	6	5	4	Total tricolour offspring: Calcu-lated.	Observed.
2	4	All cases	2	25	14	16			
		Coefficient	0·96	0·94	0·92	0·90			
		Tricolours calc'd.	*2*	*24*	*13*	*14*	..	*53*	
		,, observed	2	25	14	15	..		56
	3	All cases	..	18	21	16	6		
		Coefficient	..	0·87	0·85	0·83	0·81		
		Tricolours calc'd.	..	*16*	*18*	*13*	*5*	*52*	
		,, observed	..	17	19	14	6		56
	2	All cases	..	3	2	3	3		
		Coefficient	..	0·81	0·79	0·77	0·75		
		Tricolours calc'd.	..	*2*	*3*	*2*	*2*	*9*	
		,, observed	..	2	2	3	2		9
1	4	All cases	..	2	1	9			
		Coefficient	..	0·69	0·67	0·65			
		Tricolours calc'd.	..	*1*	*1*	*6*	..	*8*	
		,, observed	..	1	..	5	..		6
	3	All cases	1	28	14	31	9		
		Coefficient	0·64	0·62	0·60	0·58	0·56		
		Tricolours calc'd.	*1*	*17*	*8*	*18*	*5*	*49*	
		,, observed.	1	16	12	8	9		46
	2	All cases	..	..	4	13			
		Coefficient	..	..	0·54	0·52			
		Tricolours calc'd.	..	..	*2*	*7*	..	*9*	
		,, observed	..	..	1	7	..		8
							Grand totals ..	*180*	181

The summed data derived from Table IV, form the coefficients entered in Tables V and VI. These are multiplied into the corresponding number of "all cases," and the result gives the "calculated" number of tricolour hounds among them.

The entries of "all cases" and of "tricolours observed" in Table V are deduced from Table I, by combining the appropriate columns. The letters at the top show which columns are combined.

Seven other observed cases, disposed in three groups, are scattered beyond the limits of Table VI; two of these seven cases are tricolour.

HARRISON AND SONS, Printers in Ordinary to Her Majesty St. Martin's Lane.

DECEMBER, 1900.

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Gratis to Members.

The Photographic Journal

INCLUDING THE TRANSACTIONS OF THE ROYAL PHOTOGRAPHIC SOCIETY.

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EDITED BY SIR W. DE W. ABNEY, K.C.B., D.C.L., F.R.S., F.R.A.S.

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HARRISON & SONS, 59, PALL MALL, S.W.

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THE PHOTOGRAPHIC JOURNAL

(THE JOURNAL OF THE ROYAL PHOTOGRAPHIC SOCIETY).

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Communications regarding advertisements should be addressed to the Advertisement Department, 150, Holborn, London, E.C.

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THE PHOTOGRAPHIC JOURNAL,

INCLUDING THE JOURNAL AND TRANSACTIONS OF THE ROYAL PHOTOGRAPHIC SOCIETY OF GREAT BRITAIN.

NEW SERIES. VOL. XXV., No. 4. DECEMBER, 1900.

Readers of papers, etc., before the Society are informed that the Society expects first publication thereof in THE PHOTOGRAPHIC JOURNAL, *unless special exception be made by the Council.*

NOTICES.

ARRANGEMENTS FOR MEETINGS, ETC., TO BE HELD AT 66, RUSSELL SQUARE, AT 8 P.M.

JANUARY 8TH.—*Ordinary Meeting.*

Professor C. H. BOTHAMLEY will read a paper upon "Some new methods of intensification and reduction."

JANUARY 22ND.—*Technical Meeting.*

Mr. W. EDWIN TINDALL, R.B.A., will read a paper entitled "Imitative *versus* Creative." (A comparison.)

FEBRUARY 5TH.—*Lantern Meeting.*

Mr. ERNEST MARRIAGE will give an illustrated lecture on "Romanesque Architecture."

FEBRUARY 12TH.—*Annual General Meeting.*

FEBRUARY 26TH.—*Technical Meeting.*

Mr. WM. WEBSTER will read a paper entitled "Notes from five years' work with X rays."

MARCH 5TH.—*Lantern Meeting.*

Mr. W. B. FERGUSON, Q.C., M.A., will give an illustrated lecture on "Rome."

MARCH 12TH.—*Ordinary Meeting.*

Dr. HARTING will read a paper entitled "Recent rapid lenses."

MARCH 19TH.—*Photo-Mechanical Meeting.*

Mr. CHAS. B. HOWDILL, A.R.I.B.A., will read a paper upon "Photographing stained glass by the three-colour process."

MARCH 26TH.—*Technical Meeting.*

Mr. J. H. AGAR BAUGH will read a paper on "Some improvements in optical projection."

APRIL 2ND.—*Lantern Meeting.*

Mr. CHARLES REID will give an illustrated lecture entitled "Animals and birds in their native haunts."

Members of the Society who may wish to read papers or bring forward objects of interest at the ordinary, photo-mechanical and technical meetings during the ensuing session will greatly oblige by informing the Honorary Secretary as soon as possible of their intention, so that he may make the necessary arrangements.

The Honorary Secretary will be glad at any time to make arrangements for immediately bringing before the Society any subject of urgent importance.

SUBSCRIPTIONS.

The subscription for 1900 became due on January 1st. Cheques should be drawn in favour of the Royal Photographic Society, and should be addressed to the Honorary Treasurer at 66, Russell Square, London, W.C.

The attention of Members whose subscriptions are in arrear is particularly called to Article 18, *which reads as follows :—*

"Members whose subscriptions are twelve months in arrear shall not be entitled to attend or take part in the meetings of the Society, nor to receive the Society's printed papers, nor to vote. Any Member whose subscriptions are two years in arrear shall be deemed to have forfeited his claim to membership, and his name shall be suspended on the notice board unless the Council give instructions to the contrary. His name may be removed from the Register by order of the Council, but he shall, nevertheless, continue liable to pay the arrears of subscription due at the time of his name being so removed."

TO MEMBERS.

A circular has been prepared giving a short account of the history and aims of the Society, of the advantages accruing to Members of it as well as particulars of the subscription, etc. To it is attached a membership proposal form. The Honorary Secretary will be pleased to forward copies to any Members who wish to bring the Society before the notice of their friends. Applications should be addressed to him at No. 66, Russell Square, London, W.C.

ELECTION OF OFFICERS AND COUNCIL AND EXHIBITION JUDGES.

Abstract from the Articles of Association.

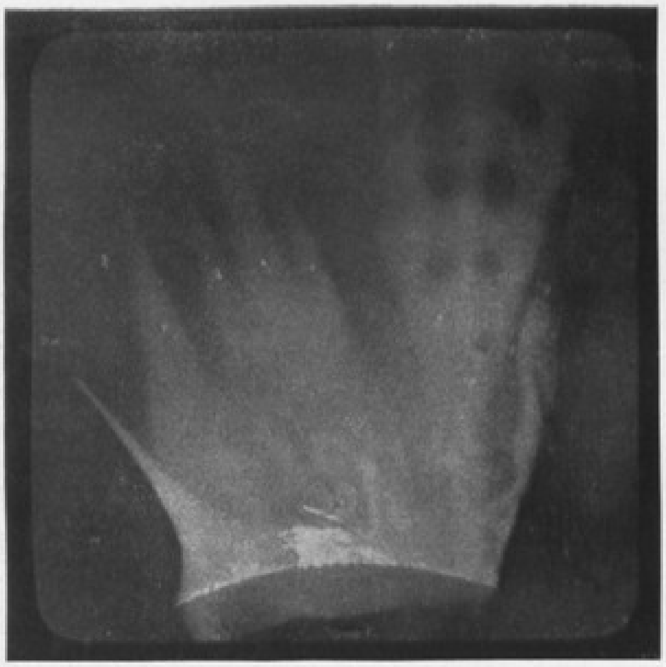
ART. 21. The Officers of the Society shall be a President, four Vice-Presidents, a Treasurer, a Secretary, a Librarian, a Solicitor, an Editor of the Journal, and an Assistant Secretary. All the Officers except the Assistant Secretary shall be elected from the Members, and all offices shall be honorary except that of the Assistant Secretary.

ART. 36. The election of the Officers and Council, other than the Officers appointed by the Council, shall be by a ballot of all those Members of the Society who are entitled to vote. Except the Assistant Secretary the Officers and other Members of Council shall retire annually, and shall be eligible for re-election. Every Member of the Society who is entitled to vote shall have the right to nominate one Member as President, four Members as Vice-Presidents, one Member as Treasurer, and twenty Members as Ordinary Members of Council. . . . No Members shall be eligible for nomination to the Council unless he shall have paid his subscription for the previous year.

ART. 37. All nominations must be sent to the Secretary not less than twenty-five days before the Annual General Meeting. . . . .

The election of Exhibition Judges will take place simultaneously with that of the Officers and Council.

A form of nomination is sent to each Member with this issue of the Journal. It may be returned at any time before, but not later than, January 18th, and should



CORONAL STRUCTURE VISIBLE IN THE PHOTOGRAPHS OF THE ECLIPSE OF 12TH DECEMBER, 1871, SHOWING DARK MARKINGS. (See p. 129.)

(*Photography in the eclipse of 28th May*, 1900. *By E. Walter Maunder, F.R.A.S.*)

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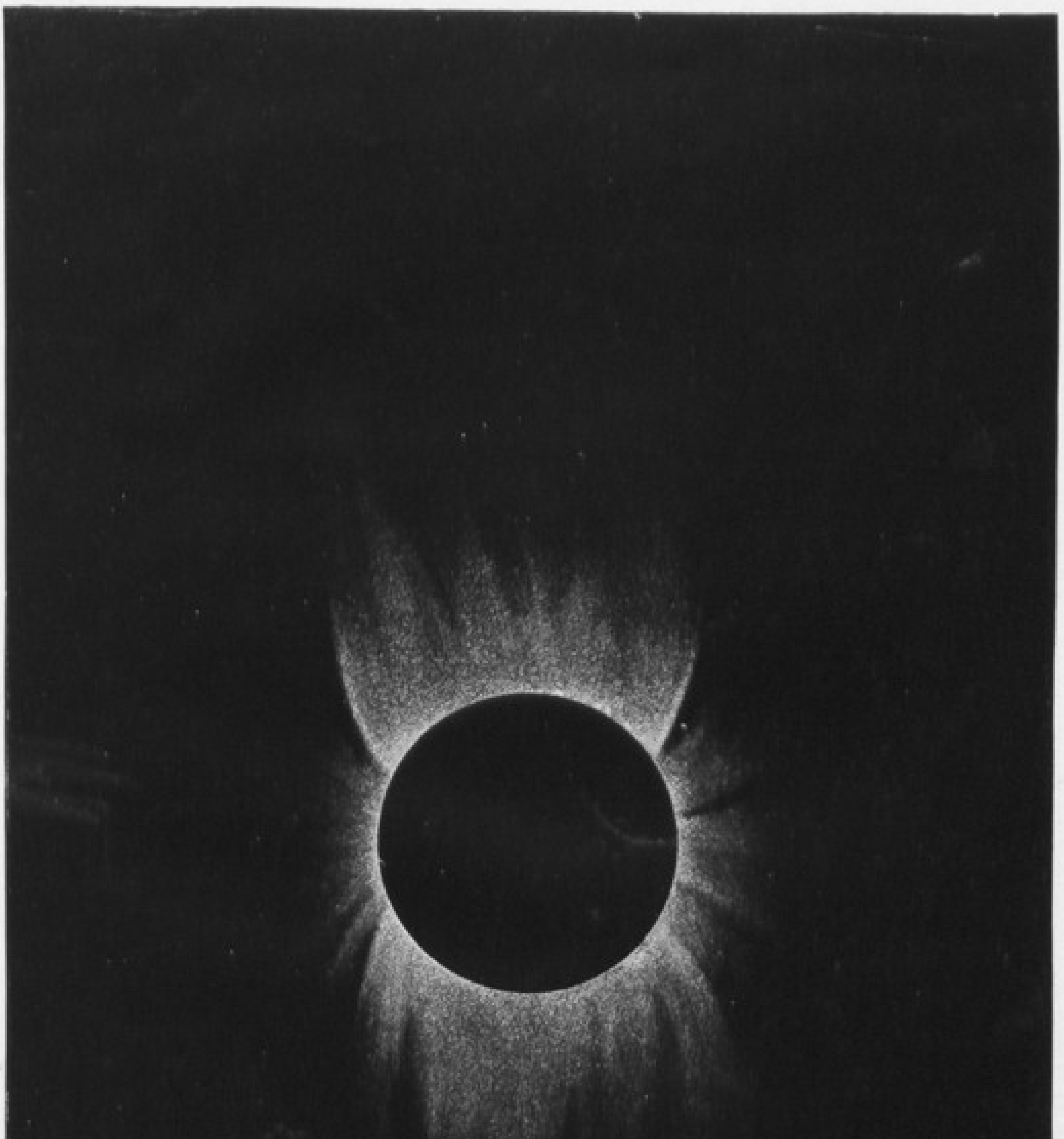

CORONA OF 28TH MAY, 1900.

DRAWN BY MR. W. H. WESLEY, FROM PHOTOGRAPHS TAKEN BY MR. WALTER MAUNDER AT ALGIERS. (See p. 129.)

Reproduced by permission of the proprietors of "Knowledge."

Photography in the eclipse of 28th May, 1900. By E. Walter Maunder, F.R.A.S.

be addressed to the Secretary, Royal Photographic Society, No. 66, Russell Square, London, W.C.

A list of the retiring Officers and Members of Council, with the number of their attendances at Council and Committee Meetings, is given on the last page of this number of the Journal.

THE SOCIETY'S ROOMS.

The library and dark rooms at No. 66, Russell Square, W.C., are open daily from 10 a.m. to 4 p.m.; on Wednesdays from 10 a.m. to 8 p.m.; and on Saturdays, from 10 a.m. to 1 p.m. On meeting nights the house is open until 10 p.m.

TRANSACTIONS.

ORDINARY MEETING.

Held at No. 66, Russell Square, on Tuesday, November 13th, 1900. Mr. Thomas R. Dallmeyer, F.R.A.S., President, in the Chair.

(*Concluded from p. 99.*)

The following paper was read by the author:—

PHOTOGRAPHY IN THE ECLIPSE OF MAY 28TH, 1900.

By E. Walter Maunder, F.R.A.S.

When I last had the honour of addressing the Royal Photographic Society, a year and a half ago, I took for my subject "Exposures in Coronal Photography." My purpose was to ascertain what guide the photographs which had been taken in the eclipse of January 22nd, 1898, in India, gave us as to the best exposures to be employed in coronal photography. The question, as I formulated it, was this, "Given a total eclipse of the sun, the sky clear, the sun high, and all conditions favourable, what exposure or exposures should be given in order to secure a complete record of the different details of the corona?"

The conclusion to which I came was this, "Given the conditions which I have assumed throughout, then for *f*/15, any exposure from one-sixtieth of a second up to half a second may be expected to give us a valuable result." And I added, "It is to be much desired that exposures from one-twentieth to one-fifth of a second should be much more frequently tried than they have been as yet." And again, "I hope I have made my point quite clear, namely, that very short exposures—say from one-sixtieth to one-fifth of a second—are greatly needed. Such bring out to best effect the prominences and the important details of the lower corona."

I also alluded to the success which had attended some very long exposures which Mrs. Maunder had given in the 1898 eclipse. These were equivalent to exposures of 125 seconds with *f*/15, and had recorded the faint outer streamers of the Corona to a much greater distance than they had ever been photographed before, or than indeed it had been believed that they could possibly be photographed. I therefore announced my intention of attempting on the next occasion exposures much longer than we had given in India. My principal aim in the paper was to urge that what I may call medium exposures, that is to say, exposures for *f*/15 of from three to ten or sixteen seconds, the exposures which have indeed been most commonly given, are just those which are, in truth, the least adapted to the object upon which they are given, and the least satisfactory in their results. The exposures which were really required were either very much shorter or very much longer.

The reason for this conclusion is a very simple one. The brightness of the corona diminishes very rapidly with increase of distance from the sun; apparently the diminution is, roughly speaking, as the square of the distance. It follows therefore that the lower regions of the corona are immensely brighter than the outer, and a point is soon reached when the over-exposure of the brightest parts of the corona is so pronounced as practically to burn out all detail. And this may, and indeed does, take place before we have reached such exposures as are necessary to bring up the characteristic forms of the extreme outer portions of the corona. Short exposures therefore may be expected to give us details of the structure of the brightest part of the corona, but without any hint of the outer streamers. Long exposures may give us these streamers, but the details of the inner corona will be entirely lost. Medium exposures, as we may for distinction call those which have been most frequently given, have been open to the disadvantage that they over-expose the brighter details and fail to render any account of the faint long rays.

You will permit me, I feel sure, to express a little personal gratification that the conclusions which I laid before you in April twelvemonth, and which I published very shortly after in *The Indian Eclipse*, 1898—the official report of the British Astronomical Association—were received with very general approval by astronomers the world over. The official instructions for the eclipse of last May, issued by the United States Naval Observatory, were avowedly based upon them. The astronomers of the Yerkes Observatory, in the suggestions which they published in the *Astrophysical Journal* adopted them with full acknowledgment; and I think it may be said that English astronomers in general in the late eclipse acted upon them. I think the result has been that there has been a great increase in the proportion of beautiful and serviceable photographs of the inner corona. Especially was my concluding suggestion taken up—that "the corona can be perfectly well photographed by the great majority of cameras by a snap-shot. There is no necessity, therefore, if our f/a does not exceed 15 or 20 and the sun's image is not more than an inch, for equatorial stand or driving-clock. The camera should be pointed directly to the eclipse and firmly fixed."

On the present occasion I propose to give a brief outline of the different experiments in photography that were made during the eclipse of May last. When I put my paper together I felt sorry that I had not postponed it to a considerably later date, since we may reasonably expect that three or four months hence several reports will have appeared which would have afforded additional materials for my purpose.

The track of the eclipse of May last passed over Mexico, the United States of America, the Atlantic, then touched Portugal and passed over Spain; then it passed into Algeria, over the city of Algiers, and finally the shadow died away in Tripoli and on the border of Egypt. It so happened—a most wonderful circumstance for a total solar eclipse—that all along the track, I believe at every single station where astronomers had taken their places, the weather was exceedingly fine. It had been so in 1898, in India, but there the astronomers may be said to have been massed together all the stations lying within the borders of a single country; but in the last eclipse astronomers were distributed over three continents, and at each station they were favoured with fine weather.

The first slide that I have to show you is a view of the town of Algiers, taken from the hill just above it. Our own station at Algiers was on the roof of the hotel at which we were staying, so we had not far to go to set up our instruments. My cameras, which are seen in the next photograph, were all small. One of the lenses was that which we had taken to India in 1898, and with which Mrs. Maunder secured the long rays; it is a Dallmeyer Stigmatic of 1½ inches aperture, and I had a second lens of exactly the same aperture and focal length so as to take a second image of the corona on the same scale and at the same time. A second pair of cameras had R.R. lenses of about 3 inches full aperture and 18 inches focus, but they were stopped down to about 1½ inches; and a fifth camera had a photographic object glass of 4 inches aperture and 34 inches focus. In each case the photograph was taken in the primary focus. The first pair of cameras was equatorially mounted and clock-driven, the

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second was also equatorially mounted, but was driven by hand, my wife guiding on the planet Mercury; our other instrument, the 4-inch telescope, was rigidly fixed.

(Slides from photographs taken by the fixed telescope were then shown.)

Another party, of amateur astronomers belonging to the British Astronomical Association, was on the other side of the Atlantic, in North Carolina. The Rev. J. M. Bacon, the head of the party, used a fixed camera, and two of his photographs—the exposures of which were one-fifth of a second and one second respectively—form the subjects of two of my slides, in which the great beauty of the aigrettes round the two poles of the sun is well seen.

A third station was occupied by the Astronomer Royal, at Ovar, in Portugal. His apparatus was quite different, and of course very much more powerful, than the little cameras which I took out, or which Mr. Bacon had. The ground glass of his camera was 15 inches square; he used a 9-inch photographic lens, given to the Royal Observatory, Greenwich, by Sir Henry Thompson, in conjunction with a negative enlarger which gave an image of 4 inches for the sun or moon, the diameter of the sun in the primary focus of the lens being 1 inch, and the negative combination enlarging it four times. The light of the corona was reflected into the camera by a cœlostat. The Astronomer Royal's longest exposure was 12 seconds, which would correspond to about 1·2 second with $f/15$, and the photograph shows not only a very considerable extension of the corona but also a great deal of detail. He also took several photographs with much shorter exposures, one in particular, taken on a lantern plate with $2\frac{1}{2}$ seconds, showing the principal group of prominence and the lowest section of the corona very finely.*

The American astronomers used much more powerful instruments even than the Astronomer Royal. For the most part they preferred to take their photographs in the primary focus of their instruments, and for that purpose they had gigantic telescopes, in one case over 130 feet in length; in other instances, 40 feet, or 62 feet. Prints from some of the American negatives have reached us, and some of these show the same group of prominences that is seen in the Astronomer Royal's photograph, and just the beginning of the corona in that neighbourhood. The manipulation of these giant instruments differed; in some cases the great telescope pointed up to the eclipse; in that case, instead of the camera being moved bodily to follow the sun, the plate in the dark room was made to follow it; in other cases a cœlostat or heliostat was employed to reflect the light into the object glass.

The ablest photographer amongst all the astronomers who observed this eclipse was Professor Barnard, whose name is now a familiar word on both sides of the Atlantic, and whose skill as a photographer is of the very highest order. Indeed, before he became an astronomer he was a professional photographer. His instrument was of some 62 feet focal length. I have a slide from his photograph of the same prominences already shown in the slide from one of the Astronomer Royal's negatives, and it brings out some very beautiful detail. To ordinary sight the prominence appeared rather as a solid mass of flame, but in this photograph it is broken up into a great number of fine filaments rushing upwards with a screw-like motion. Another broader and flatter prominence near shows the same filamentous structure. Both prominences changed their shape very markedly in the time between the exposure of Professor Barnard's plate in North Carolina and the time of the exposure of the Astronomer Royal's plate in Portugal; and a still further change took place before the eclipse had reached us in Algiers, an interval of two hours and a half from the first photograph to the last.

Previous to the eclipse of May, 1900, the only successful way of giving in one picture a perfect view of the whole of the corona has been to take a number of photographs with very different exposures and afterwards combine them to make one complete picture, for the reason that I pointed out before—the regions near the sun

* Throughout this paper, as in my former one, I have spoken of exposures on the assumption that a "Rapid" plate is used. The first of the above-mentioned photographs by the Astronomer Royal was taken on an "Empress" plate; this would reduce its equivalent exposure to about 0·6 second, whilst the lantern plate would correspond to about 0·02 second.

are so immensely brighter than the regions farther out, that any exposure which will bring out the faint extensions invariably burns out the lower corona. Professor Burckhalter, an American astronomer, has for some time been trying to get over this difficulty. He was not in the least content to adopt what seemed to him but a makeshift way of securing a record of the whole phenomenon, and he hit upon the device of making a hole in the centre of the plate and running a pivot through; on the end of a pivot he had a screen so graduated that whilst it allowed practically the whole of the rays to pass from the outside of the corona, those close to the sun were very nearly cut off. This fan or screen was kept revolving very rapidly, by means of a little electric motor, during the whole of the time of exposure, with the result that the corona close to the limb had only $\frac{1}{200}$th part of the exposure of the corona far out. I am able to show you one of the results of his experiments, and you will see how very much more detail he has got in the lower corona than there is in any of the plates I have hitherto exhibited. The maximum exposure given to this plate was eight seconds, and you may compare the photograph with the next, which received the same exposure all over the field, the fan not being used. There can be no doubt that Professor Burckhalter's experiment has been, on the whole, a most gratifying success. He had tried the experiment in 1898, but was only partly successful then; the experience gained on that occasion, however, guided him in the exposures he made this year.

Another photograph, taken with the fan, enables the streamers of the corona to be traced to a very considerable distance out on both sides, whilst the inner corona still shows itself; it also shows the "combing out" of the corona in the west wing, which was very characteristic of this eclipse; and, but only to some extent, the delicate polar plumes, the failure to delineate well these plumes being the great drawback of this scheme.

Another, taken with the fan and a very short exposure, just shows the prominences and chromosphere—the same magnificent group of prominences shown already in the photographs of the Astronomer Royal and Professor Barnard.

At the late eclipse, therefore, unusual attention was paid to very short exposures —exposures which bring out the prominences and only just the faintest part of the corona. But besides these a large number of plates was exposed in order to try and get the long rays, and exposures relatively very long indeed were given. Thus one photograph was taken by one of the Royal Observatory party, Mr. Davidson, at Ovar, in Portugal; a Unar lens was used, with a ratio of focus to aperture of about 1 to 5, and an exposure of 3 seconds, equivalent to 30 seconds with *f*/15. On the original a ray can be traced nearly up to Mercury.

There were many experiments made at different stations under practically the same conditions as in Portugal, and with practically the same result. In no case was any one of the long rays traced to anything like the extent as in India in 1898; about half that distance was the record, and it seemed to be about the same at every station. It appears to me, therefore, that there was not on this occasion the possibility of photographing these long rays farther than the distance I have mentioned—about three diameters instead of about six diameters—because some of the exposures were considerably greater than those we gave in India. For instance, our longest exposure in India was 20 seconds, and although our longest exposure with the same lens and plates in Algiers was 48 seconds, our longest extension is only about one-half what it was in India. There are two or three reasons to be assigned for this difference. One is that the sky illumination was much greater in Algiers than in India—necessarily so, because the eclipse was so much shorter. Professor Turner, of Oxford, made a photographic estimation of the total light from the corona and the sky round it during the May eclipse, and found it was about ten times the full moon. A determination made on the same lines in India, by some members of the British Astronomical Association, gave the result as seven times the full moon; and, of course, the more glare there is in the sky the less chance there is of picking up photographically the very faint extensions upon that bright background. Again, I do not think any observer whatever followed these extensions by his unassisted sight much farther than Mercury during the late eclipse, if so far; whilst one or two observers in India said that they were able to trace by eye the extensions as far as or farther than our photographs showed them. So that it may very well have been that the extensions this year were not present to the same distance as they were in India.

Though our long exposures in this eclipse were not successful in bringing up the extensions farther than, or, as I have already pointed out, nearly as far as, in India, I do not think they were thrown away, because they brought up what was to me an unexpected feature—it was not an entirely new feature—in the corona, that is to say, the dark rays in it (Plate I). In the eclipse of 1871 two photographs were taken, the one at Dodabetta and the other at Baikul, which are reproduced in Mr. Ranyard's great eclipse volume of the *Memoirs of the Royal Astronomical Society*. Both of them show some curious markings: a little black spot surrounded by three concentric curves; these markings occur in the photographs taken at quite different stations, and there can be no question but that they represent a real object photographed in both cases on the background of the bright corona. Mr. Ranyard believed that it was a comet seen as a black object against the bright background of the corona. It had, of course, always been known that the corona ran into strange forms, and that every now and then its brightness was broken by a dark wedge-shaped marking or rift that went down nearly or quite to the limb of the moon, and very naturally these rifts were supposed to be merely interspaces between bright portions of the corona. In this case, however, I do not think the markings can be so regarded, because they interrupt so markedly and in so singular a manner the regular formation of the corona; and remembering that the corona must have, necessarily, on the whole, a spherical form, that is to say, it must exist in three dimensions, we can scarcely suppose that there were a number of circular tunnels through the corona, all of which were turned end-on towards the earth at the time that the photograph was taken.

The next slide (Plate II) which I have to show is of a drawing which was made by Mr. Wesley from eight photographs which we took during the last eclipse. On looking at a positive which I had taken from one of my smallest negatives, some considerable time after the eclipse, I was struck by seeing a black line upon it which extended entirely clear of the corona and was seen right out on the background of the sky, and looking further, I found that there was not only one but several of these. Mr. Wesley, to whom I gave the photographs for examination, found some seven of them, of the reality of the existence of which he was pretty sure. What is peculiar about them, and what makes me think that the dark objects are not mere interspaces between bright portions of the corona, is this: that not only is this black line sharp on its inner edge where it comes into contact with one of the very brightest lines of the corona, and where it is therefore in strong contrast with it, but the outer edge of the same black line, seen only against the sky, is just as sharp as the inner one; and further, the black line can be traced farther on the sky than one can trace the actual bright wing of the corona.

Further, another of these black markings is closed at the end—the corona is seen beyond it; it is not a mere interspace between two bright masses like some of the dark triangular rifts, but the corona is seen beyond it. It only extends about half a diameter from the limb, and the corona can be traced farther out still. It seemed to me, therefore, that the prolonged exposure that we had given to our plates had not been without its reward; because unless we had exposed for such a length of time as was sufficient to bring up the general solar glare round the corona, I do not think we could possibly have had those dark rays so clearly seen.

It must not be forgotten that beside the definite corona, there is around the sun in an eclipse a large amount of glare, which is not due to illumination of our atmosphere. We can see that that is the case, because in an eclipse the dark body of the moon is darker than the sky is for an immense distance from the sun; and yet we know perfectly well that in an eclipse the moon is really very strongly illuminated by earth-shine. It is more fully illuminated by earth-shine, even than when we see it as "the old moon in the new moon's arms," and that we know is quite an appreciable light. But yet in an eclipse it is the blackest object in the sky, and it is seen so in the photographs. Professor Turner, in the 1893 eclipse, measured the density of the deposits on the photographs of the corona, and found that a very great way out—indeed, right to the end of the plate—the deposit was greater over the sky than it was on the surface of the moon.

We made one experiment in 1898, in India, which was successful as far as it went, and which encouraged us to try it again on a very much larger scale during this late eclipse, although we did not on this occasion gain much more than experience. It was an attempt to photograph the corona after the total eclipse was over. The photograph now on the screen was taken by Mr. E. C. Willis at Elche, in Spain, some 30 seconds after the total eclipse was over. We tried exposing plates during the partial phase, both before totality and afterwards, and succeeded in getting the corona further from totality than we did in India, which was only 40 seconds, but we did not make sufficient progress to justify our showing you anything that we ourselves did. But an experiment was made by another observer, which, I think, was of considerable importance. Mr. Nevil Maskelyne went out to America with the object of taking a kinematograph view of the eclipse. He was very unfortunate, because, having put his instrument in the charge of a shipping agent in very good time, he found, when he reached New York, that it had not been put on board, and he therefore had to devise a makeshift arrangement; this, however, acted very fairly, and gave a much better idea of the eclipse than I had imagined could be secured by that means. But that is not the point to which I wish to draw attention. Mr. Maskelyne ran the film through the camera for a very considerable time after the sunlight had come back, and he gave me a copy of that film; this I have carefully examined, and I find that fully two minutes after the sunlight has come back, the corona can be seen perfectly clearly, and it is extremely pretty to see how it gradually gets fainter and fainter as the sunlight grows, until the corona dies out. So that from the kinematograph film we are able to trace pretty well the limit to which the corona can be traced in sunlight under that particular exposure.

So much for photographs of the corona itself. Of course, photography is of use in total eclipses not merely for actually photographing the corona, but for photographing its spectrum. An immense number of photographs of the spectrum of the eclipse were obtained at different stations; but the only station which I had the good fortune to visit where such work was carried on was that of Mr. Evershed, about 20 miles south of Algiers. Mr. Evershed was anxious to get on the southern edge of the shadow track, because there the moon just hides the sun for a moment and no more; and by that means he hoped to get the spectrum of the lowest regions of the sun's atmosphere much prolonged. He did not care about the total phase so much as getting a prolongation of the contact, if one may so describe it, so he fixed himself in a hut in a somewhat out-of-the-way place some 20 miles south of Algiers. His instrument was a novel one; it was an ordinary reflecting telescope with a couple of big prisms put before it, and is shown in a photograph of his station, another slide being a photograph taken by Mr. Evershed of what we now call the flash spectrum. He was disappointed in one thing—he never got any totality at all. He had gone a little too far south and was just outside the actual shadow track, and the sun was never perfectly obscured, so that there are bright lines running through the spectrum, due to what are called "Baily's beads," but the bright lines due to the lowest portions of the solar atmosphere are extremely clear. The focus of his instrument is remarkably sharp from end to end; as he was using a reflector, of course all the rays came to the same focus, so that he had not the trouble—which is frequently very great—of getting the two ends of the spectrum into focus at the same time. I show you an enlargement of one of Mr. Evershed's photographs taken in Algiers, and also one taken by him in India near the end of totality; the latter shows the H and K lines of calcium, the hydrogen lines, and, very faintly, a line curved in the opposite direction to these and belonging to the corona itself.

A very ingenious suggestion has been made by Mr. Shackleton for photographing the corona, when the sun is uneclipsed, by means of this coronium line—a line in the yellowish green, or where the green is beginning to take a yellowish tinge. His idea was that by using a sensitive plate of limited range as to colour, and a couple of coloured screens, he might practically cut down the sensitiveness of the plate to the immediate neighbourhood of this particular line, and that, as this line is due to a gas which is distributed in the corona itself, we should in that way have a chance of getting a photograph of the corona even when the sun was not eclipsed. I do not know whether the experiment was actually tried during the late eclipse, but I have no doubt that in due course of time a very high amount of success will be attained in that direction. But that will not, in my own view, be really a solution of the problem of photographing the corona in full sunshine; because, after all, this gas—coronium—is not traced with the spectroscope all over the corona by any means, and it does not follow that its distribution would at all conform to the visible corona as we see it during totality. All the same, such a scheme, if successfully carried out, would be an advance of very great value.

My next slides illustrate a very ingenious apparatus which Professor Todd, like Professors Barnard and Burckhalter, an American astronomer, has set on foot in order to secure a large number of photographs of different characters without any danger of the manipulation being upset by nervousness on the part of the observer. He devised this instrument something like thirteen or fourteen years ago and carried it to one eclipse after another; but as often as he went to an eclipse, the station to which he went—no matter what the weather prospects were—was always favoured with clouds. This time he went to Tripoli by himself. There was a very unsubstantial legend to the effect that he had tried to take up his position with other astronomers, but that they threatened to lynch him if he came with them, because he had been so long known as the stormy petrel of eclipses. At Tripoli, however, on this occasion, he had a perfectly fine day, and I do not know that he altogether regrets his many disappointments now because in the interim he has worked out his instrument so much more perfectly than his first designs. The whole instrument is controlled by what looks like the inside of a musical box, which it closely resembles in principle. He has a brass barrel in which he sticks as many brass studs as he feels inclined; he says he can arrange for something like 170,000 combinations, but that number is not necessary in an eclipse. The barrel revolves once in three minutes, and he can set any stud correct to $\frac{1}{20}$ of a second. The studs work electric contacts, and they operate upon automatic cameras, changing the plates and making the exposures; some of the cameras are adapted to ordinary photography, and some are attached to polariscopes, some to spectroscopes, and so on. His mountings, which are simple and ingenious, are iron pipes, and are very rigid when set up and also very portable. The clock is a very ingenious glycerine clock; he has his instrument very much overweighted, so that its tendency is to fall heavily in one direction, and it tends to push down a piston into a cylinder filled with glycerine. There are four little valves by which he can regulate the outflow of the glycerine and so bring the speed of the instrument to any required pace. By having his instrument heavily overbalanced on one side he increases its stability—there is no possibility of shaking it.

Professor Todd has also his own method of getting over the difficulty which Professor Burckhalter overcame by means of the revolving fan. Instead of having a hole through the plate and a revolving fan carried on a spindle, his telescope has a number of concentric rings fixed just before the plate, and a little electro magnet brings first one and then another into position. Full aperture is given at first, and then down comes one ring, and then another and another, until the different parts of the corona are shut off and only the outside is left; then the reverse action takes place—up goes the last ring that fell, then the next, and so on to the last, so that the inner corona only gets a small fraction of the exposure that is given to the other parts. Professor Todd's arrangement for giving a great many exposures on the same telescope are equally ingenious and efficient.

I think you will see that astronomers have not been idle or slow, during this late eclipse, in bringing your beautiful science further into their service. Next May there will be another eclipse of great duration, but unfortunately not nearly so near home as that which we had during last May. It will be total in Sumatra, where it will have a duration of over six minutes, and in Mauritius and New Guinea there will be three and a half minutes' duration. We cannot expect that any large number of observers will take part in the observation, but in all probability every one of these devices which I have shown will come into play again under the much more favourable circumstances, so far as duration is concerned, of an eclipse of from three to six minutes instead of one minute; and I am sure you will wish us even better success.

The PRESIDENT remarked that although the class of photography referred to by Mr. Maunder was chiefly carried on by astronomers with large instruments, it would be interesting to know what could be done with small cameras and without special apparatus. He asked whether in using a small lens Mr. Maunder had attempted to photograph the corona with polarised light—through a Nicol's prism, for instance. There had been shown in the Society's Exhibition some beautiful examples of cloud effects, which he thought could not have been obtained by any other means than polarised light. The President also inquired whether there would be a sufficient degree of difference of illumination between the centre and the edge of the corona if the sector had been made to revolve outside the lens, and a stop had been used behind the lens. That principle had been adopted in several instances for the purpose of obtaining perfect equality over a considerable angle on an ordinary plate, to avoid the falling off of light due to obliquity upon the plate itself.

Major-General J. WATERHOUSE, I.S.C., referred to his experiences at Dodabetta in the eclipse of 1871, when there were no gelatine dry plates, and he had a very anxious time with wet collodion. A question having lately been raised as to whether in view of the greater delicacy of detail obtained on the wet collodion negatives, taken at Dodabetta and Baikul in 1871, it might not be desirable to go back to the old process for corona photographs; he had examined some negatives taken by both methods, and could not see so marked a difference between them as he had expected. In 1871 the exposure with a wet collodion plate of the photograph giving the greatest extension and detail, was fifteen seconds, using the full aperture of a Dallmeyer R.R. lens of 33 inches equivalent focus; as much, and more, had lately been given with very sensitive gelatine plates. The lowest exposure in 1871 was five seconds. Among the collection of eclipse photographs in possession of the Royal Astronomical Society, he saw a gelatine negative of the corona by Captain Hills, R.E., which was very beautiful, being perfectly clear and possessing a great deal of detail; the photographs of nebulæ from Greenwich, exhibited at the recent exhibition in the New Gallery, also showed what very fine detail could be obtained on gelatine plates. Mr. Maunder's note as to the attempt to photograph the corona by the light of coronium was very interesting, and it was desirable that experiments should be continued in that direction. He (the speaker) had more than once attempted to photograph traces of the corona on stained plates, but unsuccessfully, although on one plate faint corona-like appearances were noticed.

Mr. W. E. DEBENHAM asked whether multiple-coated plates had been used for photography of the character under consideration. Objects of extremely different luminosities were rendered by them much more satisfactorily than by ordinary plates; and it was to be presumed that they would be advantageous towards securing a rendering of the bright light of the corona near the sun, and also of the much fainter light of the streamers.

Mr. CHAPMAN JONES inquired what developer was employed by Mr. Maunder. From a photographic point of view the photographs which had been shown appeared to be rather hard, and he thought that softer results and a greater range of detail might have been secured by the use of a developer such as metol.

Mr. H. SNOWDEN WARD asked whether any material results had accrued from the suggestion that useful work might be done with ordinary cameras by "every-day" photographers. I those results were numerous and not useful, could Mr. Maunder offer any further suggestions whereby more useful results could be obtained in future? And further, was there any possibility of the successful use of pinhole cameras?

A MEMBER asked whether any attempt had been made to adapt trichromatic photography to eclipse work?

Mr. J. J. VEZEY alluded to the great ingenuity which had been displayed by astronomers in the successful adaptation of photography to the peculiar and difficult circumstances of their researches.

The PRESIDENT asked Mr. Maunder whether he could offer any suggestions for the guidance of amateurs in the photography of the Leonids?

Mr. MAUNDER, replying, said: With regard to the President's first question, one or two observers in the late eclipse, Professor Turner and Mr. Newall in particular, did photograph the corona by polarised light; their object, however, was not to get a picture of the corona, but to ascertain the amount of polarisation in it. Major-General Waterhouse referred to the 1871 eclipse, in which he took at Dodabetta that superb photograph of which I showed a slide from the drawing in Mr. Ranyard's celebrated book. There is no doubt, I think, that up to the present time that particular eclipse of 1871 "holds the belt" for beautiful detail of the lower corona; this was partly due to the extreme adaptability of wet collodion for that sort of work, and partly to the fact that the exposures were relatively far less than astronomers have given in more recent total eclipses. I think that when astronomers first had gelatine plates put into their hands they went a little astray in their use of them in total eclipses. The natural result of having a plate that one could expose as long as one liked, and that was extremely sensitive, was that we exposed as long as we could. That course was very successful when we were photographing a nebula, which was a very faint object, and from which one always got something fresh the farther the exposure was pushed; but I do not think it paid altogether in eclipse photography, and I think there was distinctly a time in eclipses when we failed to get the best out of the corona because on the whole we over-exposed, and yet failed to give those extreme exposures which were necessary in order to give the faint streamers. There is another point: I think there is considerable evidence that the amount of detail in the corona itself changes from one to eclipse to another, and I think a corona of the time of sunspot minimum is apt to be rather uninteresting as to detail; whilst the corona of 1871, being a maximum corona, was full of detail. Mr. Debenham asked whether multiple-coated plates were used. I used some myself, and a good many other photographers used them; and if I have the chance next year I shall use them for the purpose of trying to get detail in the lower corona at the same time as a considerable amount of extension. But I did not use them for that purpose this time, because I had no instrument of any great focal length, and I knew that it was impossible to get any appreciable amount of detail—the small size of the image precluded that, the sun on my plates being one twelfth of an inch in diameter.

Mr. DEBENHAM said he meant gradation, not detail in point of fineness.

Mr. MAUNDER: Quite so. If I had been using a larger scale I should have used triple-coated plates for the purpose of getting detail, and exposed accordingly; but I used them simply for my very long exposures, in order to keep back the glare from the lower corona. If I were using the same proportion of aperture to focal length with a big caméra, I should take a Sandell plate and expose say one-tenth of a second; but I actually exposed for 48 seconds, because I wanted if possible to get up the extreme long rays. The Sandell plate gave the advantage that I was not swamped by the bright corona that I got, but I could not expect to get any detail in it with an exposure of so great a length. To put it roughly, the brightness from the brightest part of the corona to the very faintest is something like 1 to 10,000, and necessarily one must regulate one's exposure by what one wants to get. I do not think triple-coated plates have yet been fairly tried for the purpose of getting proper gradation in an ordinary picture of the corona. Mr. Chapman Jones referred to the hardness of some of the slides. That is no evidence of hardness in the originals from which they were prepared. They were kindly supplied me from many sources, and in that sense they are a "scratch" collection. For the actual eclipse negatives I used a mixture of metol and hydroquinone.

Mr. Chapman Jones: Would you not have done better without the hydroquinone?

Mr. Maunder: I prefer the combination. Mr. Snowden Ward asked if pinhole cameras had been used. No, they have not been used for the corona during totality, so far as I know, and I do not think the corona is bright enough to permit the use of a pinhole camera in so short an eclipse as that of 1900, certainly not where it is so short as we had it in Algiers. They might possibly be used in such an eclipse as we are going to have in Sumatra next year, but in order to get an image of any size you would have to give a very great length to the camera, and that would mean that the light would be very small. The law of a pinhole camera shows that the diminution of light (and in consequence the increase of exposure required) will vary in direct proportion to the increase in the length of the camera, since the length for best definition increases as the square of the diameter of the pinhole. I tried a camera of about 15½ inches with a pinhole, and I reckoned that in order to get even the inner corona you would want five or six minutes with a fast plate. I have taken the sun with a pinhole camera very successfully several times. With regard to the question as to whether ordinary cameras are of any use for total eclipses, a great number of amateurs took photographs, but usually with too short focal length. If you have a focal length of 9 inches, the sun is only one-twelfth of an inch in diameter, and you cannot get very much out of an image that size. I think that was the great drawback to many of the experiments made during this last eclipse. A professional photographer, who was not an astronomer, took a very fine series of photographs in 1898 with a Dallmeyer R.R. lens, but he removed the front lens, and so got a focal length of about 32 inches. I do not think a shorter focal length than about 30 inches should be used for a stationary camera. On that scale an ordinary camera rigidly fixed is very good for the work. I do not see why trichromatic photography should not be successful in a total eclipse, and I should be very glad if somebody would try it; the result could not fail to be interesting, and might be valuable and important.

With regard to the Leonids, if you have no driving-clock or equatorial, I should say the best way would be to turn the camera up pretty nearly to the zenith. The best point to which to turn it depends upon the time of night, and whether the Leonid radiant has just risen or is already fairly high, but I should fix the camera rigidly pointing nearly upwards, but inclined somewhat to the east, and expose for a quarter of an hour at a time. A careful record should be made of the time of opening and closing the camera, and also of the time when any extremely bright meteor crossed the part of the sky which it commands. I think that is all the advice I can give, and —I wish you much success.

A vote of thanks was passed to Mr. Maunder.

TECHNICAL MEETING.

Held at No. 66, Russell Square, W.C., on Tuesday, November 27th, 1900. Mr. J. J. Vezey, F.R.M.S., in the Chair.

The Minutes of the last Meeting were read and confirmed.

The following paper was read by the author :—

ANALYTICAL PHOTOGRAPHY.

By FRANCIS GALTON, F.R.S.

So far back as the year 1881, I submitted to this Society my method of composite portraiture, which, as many of you are aware, consists in throwing the images of different pictures successively upon the same screen, giving to each a proportionate fraction of the total length of exposure required to produce an ordinary photograph; the result being that what is common to all the pictures has been adequately exposed and is retained in the resulting photograph, and what is individual to each of them has been too under exposed to leave any image at all, and consequently disappears. At that time the idea of a converse process occurred to me:—a composite shows what is common to all, the converse process should show what was individual to each. I thought of this from time to time, but could not see my way to effect it. Last summer however, on returning from abroad, I made a few experiments which proved that the process was feasible, and I produced preliminary results, but not sufficiently good for exhibition. I then wrote a short letter published in *Nature* on the 2nd August last, describing the outlines of the process and stating that I felt it very difficult to proceed further without special apparatus, such as might perhaps exist in some physical laboratories but which I could not instal in my own house. However, within a week after the appearance of my letter—such is the zeal of the editors of newspapers—the editor of *Photography* had not only mastered and practised my process, but had actually written and published a very effective article upon it, illustrated by two of his own pictures. The article appeared in the issue of *Photography* for August 9th, 1900, copies of which are passed round for you to look at.

I had no sooner posted my letter to *Nature* than—as I believe, very commonly happens to persons who are working at new things—it flashed upon me that I had "thrown up the sponge," as it were, too soon. I saw that a small apparatus could be planned that would do what was wanted, and in fact such an apparatus is now on the table. Without going into details at this moment, you will see that there are bright lights at three of the four corners of the instrument; opposite to each of them is a photographic transparency, and at the fourth corner is a ground glass screen upon which the combined images of the three transparencies are thrown, and where they can be seen all together or singly or in any other desired combination. The construction of the lighting portion of the apparatus is by my own hand and consequently rather ramshackle, but it acts. The method that first occurred to me of combining the three images did not prove successful, at all events in the form in which it was carried out; and then Mr. Dallmeyer, your President, suggested the plan that has been used here. I was away from England during the autumn, and the construction of the instrument was thereby delayed, but when it was made its lenses and their adjustments acted perfectly, which is rarely the case with new forms of optical apparatus. The only difficulty then lay in procuring full illumination, a matter which occupied me so long that the photographic results I have been able to prepare for this meeting are far inferior to the optical ones that the instrument can produce. Though I cannot show you much I shall be able to exhibit the process fully.

Let the two pictures whose differences have to be isolated be called *A* and *B*. The process requires that a faint transparent positive *pos. a*, and a faint transparent negative *neg. a*, should be made of each of them. I place *pos. a* in one corner of the apparatus, and *neg. a* in another corner, and in the third corner I place *pos. b*. These three images are seen in superposition on the ground glass screen which occupies the fourth corner. There are means for adjusting the transparencies independently of one another, and this is done to them in turns until they are exactly fitted and form as good a composite as may be. Then *pos. a* and *neg. a* will antagonise one another, all the features of *a* practically disappear, and those two images produce in combination a uniformly grey ground, upon which *pos. b* is projected; so the result of compositing the three images is to produce a darkened representation of the faint *pos. b*. Now shut off the light from *pos. a*; what remains is a composite of *neg. a* and *pos. b*, which,

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as has just been seen, has this property, that when added to *pos. a* it will transform *pos. a* into a darkened *pos. b*. Each step of the process can be watched and repeated by turning the various lights off and on. I call the composite of *neg. a* and *pos. b* the "transformer" of *a* into a darkened *b*. It is obvious that this transformer exhibits the various excesses of the darkened *pos. b* over *pos. a*, in other words it exhibits the differences between them, as expressed in the form of darkened *pos. b*, *minus pos. a*. Similarly the composite of *neg. b* and *pos. a* exhibits those same differences in the form of darkened *pos. a*, *minus pos. b*, the relations of the two transformers are thus those of a positive and its negative. This, in brief, is an account of the principle of analytical portraiture, in which some points require, and will receive further explanation. In the meantime a few of the purposes may be named to which the process admits of being applied. It must be clearly understood that it records differences and nothing else, so that if the portraits *a* and *b* were the same except that one of them had, say, a wafer stuck on it, then the result of the perfect process would be to produce a uniformly grey surface with only the wafer upon it. Speaking generally, the results of analytical portraiture are neither pleasing as pictures nor intelligible to an untrained eye; on the other hand, they give materials for an exact study of differences in such important cases as the following. Changes of expression, in which the process shows exactly what are the additions and what are the subtractions to be made from *pos. a*, which expresses the features in repose, to convert it into a darkened *pos. b*, which expresses the same features under the new expression. Alterations due to growth, as in flowers or foliage or even in animals. Those that are due to decay or wear. Differences between copies of the same original. Differences between the features of different races, in which the features characteristic of each race have been severally defined by composites of many individuals. Similarly as regards family likenesses and the individual differences of each member of a family from the family as a whole.

An illustration will make the character of the result more easily understood. Suppose we require the differences between the composites *a* and *b* representing respectively the characteristic features of two different races. Lay a sheet of perfectly transparent paper over *a* and paint upon this paper with transparent shades, using as few as possible, until a darkened likeness of *b* is produced by the combination of what is on the transparency with what lies beneath it. The transparent paper on being removed, will exhibit exactly what is given by the photographic process of analytical portraiture.

Light and Dark Tones.—I must now enter more closely into certain topics which have thus far been lightly touched. The first of them regards light and dark pictures. Suppose a scale of nine tones, ascending by equal steps from 0 as pure white, to 8 as perfect black. I have here a box of te-totums which I made for conveniently isolating any one of these tones; their originally white faces are painted with black sectors of the several angular widths suitable for the purpose; that is, in increasing stages of 45°. The medium tone is where the angular width of the sector is 180°, that is where the te-totum, or whirling disc, is painted black on the whole of one side of its diameter. I call those pictures faint that are painted in tones up to and including the medium value which ranks as four on the scale, and those pictures will be called dark that are painted in tones both deeper than and including the same medium value. I exhibit three sketches of the same portrait to show the differences of effect under these conditions, and how very little the mere question of more or less likeness is affected by them. All the tones from 0 to 8 were used in painting the first picture. Then a grey mixture that matched the medium grey was made in one corner of a palette, and pure white was squeezed out upon another corner. The artist by using mixtures of this grey and white, and nothing else, made the second picture as a copy of the first. It is evident that its resemblance is not affected by the limitation of range in the tones. The third picture was made on the same principle as the second, except that black and medium grey were employed instead of white and medium grey, and here again the resemblance to the original is perfect. It follows that the value of the analytical process is not much affected by the fact that it is unable to transform, in other words that it cannot produce a transformer, or in still other words that it cannot isolate the differences between *any* two portraits, but only those between a light half-toned copy of the one and a dark half-toned copy of the other. It should be remarked that although the light-toned *a* and the dark-toned *b* severally contain one-half of the complete scale of tones, yet the transformer of the light-toned *a* into the dark-toned *b* contains the complete scale. For, going back to the illustration of the transparent paper, if a grey spot in the faint *a*, which corresponds to a black spot in the original full-toned picture A, has to become the representative of a white spot in the full toned B, it must appear as a grey spot in the darkened *b*, because the lightest spot in a dark half-toned picture is a medium grey. Consequently nothing is painted on the transparent paper over that spot; it is simply left transparent; that is, white. Again if a white spot in the faint *a* has to be transformed into a black spot in the darkened *b*, pure black must be put on the transparent paper at the place above the white spot in *a*.

Scales of tone ;—real, perceived, and *actinic.*—I daresay many of you who have thought of these subjects will say that what is true with the real scale of tones is not wholly true with the scale of perceived or sense-tones. The middle tone as estimated by the eye is not the real mid-tone; and again, the actinic scale is different both to the real and to the sense-scale, but for all that the differences are inappreciable so far as resemblance is concerned, in evidence of which I submit two pictures in mosaic squares of the same flower, one of which is painted in tones numbered according to the real scale (as given by the te-totums), the other in corresponding values of sense-tones, so I am content to ignore differences between the three scales and to argue solely from the real scale.

Conversion of full tones into faint tones.—The conversion of an ordinary picture into a faint one may be effected in many ways. The ideally perfect one would be that of an engraving or lithograph that contained a full scale of tones when printed in black ink on white paper, but which was printed in grey ink on white paper to form a faint picture, or in black ink on grey paper to form a dark one. The simplest plan, however, is to use a short exposure and to stop development as soon as the darkest part of the picture becomes of the half-tone that is desired.

Obliteration of a positive by its negative.—In the perfect positive and negative, if the positive is put over the negative it antagonises it, and if they are both faint the result approximates to a uniform grey as I will now show.

[Slides shown.]

But negatives and positives do not wholly obliterate one another. They do so to all intents and purposes when the tones are not very far from the middle of the scale; an extreme white is not nearly obliterated by its negative.

I will now show the transformation of a St. George's Cross into a St. Andrew's, first by a diagram, in which the various toned mosaic squares of which the crosses are severally composed, are represented by numerals corresponding to their several scale values, and then by their actual photographic transformation.

[Slides shown.]

(As it would take too much space to reprint the diagrams, they are replaced by the lines below, which represent the transformation of one band of four mosaic squares of different tones into another band in which the squares are differently arranged. FG.)

1	A (original full-toned portrait)	6	4	2	2
2	B (,, ,, ,,)	4	6	2	6
3	*pos. a* (faint half-toned)	3	2	1	1
4	*neg. a* (,, ,,)	1	2	3	3
5	*pos. b* (,, ,,)	2	3	1	3
6	darkened *pos. b* (4 added to each entry)	6	7	5	7
7	*pos. a* + *neg. a* (uniform grey)	4	4	4	4
8	*neg. a* + *pos. b* (the transformer)	3	5	4	6
9	*pos. a* + *neg. a* + *pos. b* (same as 6)	6	7	5	7

One of my very earliest successes was to turn an F into a G. I have here all the stages of it; positive G and negative G; positive F and negative F; then I got the transformer, and that clapped on to the original F turns it into a G, and a very good G, too.

Now let me show you some others, but I have greatly to apologise. I have been compelled to prepare these slides at the last moment, and I really have not proper ones to show. The only faint photographs I possessed were some old composites; they were convenient for the purpose, because they had all been reduced to the same scale, but by a piece of strange ill luck, the transformer has disappeared and I cannot find it, so I can only show you the other stages.

(Mr. Galton next proceeded to explain his apparatus and to exhibit it at work; also an instrument which he had used last summer, for accurately adjusting photographs and registering them by means of crossed wires and needle-pricks.)

A vote of thanks was passed to Mr. Galton.

LANTERN MEETING.

Held at No. 66, Russell Square, W.C., on Tuesday, December 4th, 1900.
Mr. J. J. Vezey, F.R.M.S., in the Chair.

Lieut.-Colonel J. Gale gave an exhibition of

LANTERN SLIDES, PASTORAL AND SUNDRY.

Colonel Gale, in the course of some introductory remarks, said:—

The views I shall put before you this evening, by means of the lantern, are all of a homely character, such as may be seen any day on a ramble in the country, if only you go at the right time and in the right direction. They are the results of opportunities seized in order to break away from sterner duties from which relief must sometimes be found, or something may happen—something give way.

> "If thou art worn and hard beset
> With sorrows that thou would'st forget;
> If thou wouldst read a lesson that will keep,
> Thy heart from fainting and thy soul from sleep,
> Go to the hills and woods—no tears
> Dim the sweet face that Nature wears."

And I know of no pursuit like out-door photography for effectually driving dull care away and making one forget the sorrows that the poet refers to.

None of the views to be shown this evening are taken with the hand camera, or by the so-called instantaneous process; and yet it will be seen that figures, human or other, appear in most of them and play an important part in their composition.

I will begin by showing you a group of rustics, taken in my early photographic days. I took the negative in 1860. In those days the smockfrock was the garment of the farm labourer, and a very picturesque dress it was. These smocks were sometimes beautifully worked in braid and were often handed down from father to son. Nowadays you may search the country through and will probably not find such a thing, although in remote districts, even recently, I have met with one or two peasants of the old type, clad in the typical smock that has seen many washings, with gaiters of an antique pattern on their legs, and finished off at the top with what must have been, at one time, a splendid tall beaver hat.

This, and other photographs of customs and landmarks which have now disappeared, caused Colonel Gale to call attention to the value and importance of such subjects as historical records, and to urge photographers to lose no opportunity of securing negatives thereof. Many of the views, including studies of sheep and cattle, were taken on the northern slopes of the South Downs, and among the other localities represented were Strand-on-the-Green and other parts of the Thames, the river Arun, Appledore (North Devon), Harrow-on-the-Hill, West Mersea (near Colchester), Flatford Bridge, Amberley Castle, etc., interspersed with several figure studies.

A vote of thanks was passed to Colonel Gale.

ORDINARY MEETING.

Held at No. 66, Russell Square, on Tuesday, December 11th, 1900.
Mr. Thomas R. Dallmeyer, F.R.A.S., President, in the Chair.

The Minutes of the last Meeting were read and confirmed.

The certificates of the following candidates for membership were read for the first time :—

Henry E. Davis, Camera Club, Charing Cross Road, W.C.
John L. Emerson, 25, High Street, Halstead.
Charles G. Emery, Springfield, Heston Road, Isleworth.
A. Montiville Evans, 25, Alma Road, St. Alban's.
H. M. Lomas, 3, Selbourne Villas, Minehead.
Harry Quilter, St. Martin's, Leicester.
A. F. Robinson, West Hill, Woking.
Fred. C. Shardlow, St. Martin's, Leicester.

The following were duly elected members of the Society :—

Mrs. Alderson, 59, Church Street, Isleworth.
A. M. Balgny, Weyhill, Andover.
Lawson Leigh Ballantine-Dykes, the Wellington Club, 1, Grosvenor Place, S.W.
Rev. A. H. Blake, M.A., Warden, St. Thomas' Diocesan Home, Basingstoke.
Robert Bourke, 175, Belle Vue Road, Leeds.
John Henry Chambers, 22, Glen View, Clover Hill, Halifax.
Ralph R. Clapp, 153, Earlham Grove, Forest Gate, E.
John Page Croft, 24, Quadrant Chambers, New Street, Birmingham.
Miss Catherine S. Edmonds, Abingdon Studio, 118, Westbourne Grove, W.
Harry Edwin, Iona, Chester Road, Northwood, Middlesex.
John H. Gash, 115, Albion Street, Leeds.
Dr. Chas. F. Grindrod, Wyche Side, Malvern.
Joseph Hartley, 2, South Road, Waterloo, Liverpool.
Captain J. S. Henderson, Wellesley, Lorne Street, Reading.
Cherry Kearton, Southcote, Merstham, Surrey.
P. Mackenzie, Royal Survey Department, Bangkok, Siam.
Edward C. Pitt-Johnson, Exeter College, Oxford.
Thomas Cunningham Porter, Eton, Windsor.
Croot Stone, 20, Lichfield Road, Cricklewood, N.W.
Alfred Taylor, 113, Vandyke Street, Liverpool.
Harry Wade, 29, Blackfriars Street, Manchester.
R. Fellows Wilson, 118, New Bond Street, W.

The PRESIDENT announced that the Blairgowrie and District Photographic Association had been admitted to affiliation.

The following presents were received, and a vote of thanks was passed to the donors of the same :—

"Instruction in photography," by Lieut. Abney, 1st edition, 1871 (printed for

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private circulation only). *Presented by* Col. J. Scully, I.M.S., through Major-General J. Waterhouse, I.S.C.

Stereoscopic Daguerreotype slide, given to the donor by the late Mr. Walter Woodbury about 20 years ago. *Presented by* Lewis Wolff, Esq., through Duncan Robertson, Esq.

Photograph of a lion enlarged to life size from a portion of a quarter plate. *Presented by* John F. East, Esq.

Parcel of Exhibition Catalogues. *Presented by* Walter D. Welford, Esq.

The following members were elected auditors of the accounts of the Society for the year ending December 31st, 1900:—Messrs. H. Vivian Hyde and Frank E. Seary.

The following note was read by Major-General J. Waterhouse, I.S.C., Vice-President, on behalf of the author:—

ON A METHOD OF ATTACHING A COLOURED GLASS PLATE IN FRONT OF ANY LENS IN A CONVENIENT WAY.

By Lieutenant-General J. F. Tennant, R.E., F.R.S.

I have thought that this contrivance might interest members of the Society as it facilitates experiments with coloured screens, and all the essentials can be readily made at home. A Thornton-Pickard shutter is used as the foundation, and the whole, as shown, consists of two plates of ebonite rectangular in form, and with rectangular openings in them suitably fastened to the front of the shutter. The outer plate has the smaller opening, and when both are fixed on to the shutter, a sort of cell is formed, in which the glasses under experiment can be slipped and removed without disturbing in any way the camera or the lens attached to it. The pieces of glass too are rectangular, and thus all turning or cutting into circles and fixing into metallic cells is avoided. The original apparatus was made of cardboard, and was purposely built to hold the glass quite loosely, so that not only could it be more easily removed, but that somewhat thicker glasses could readily be used: in the apparatus exhibited, however, the workman has been pleased to *fit* the glass more closely so as to remove the imaginary defect of shake. I think it will be seen that this little arrangement offers great conveniences to the experimenter, and it can be readily modified in various ways so as to leave the essential accessibility and freedom of dealing with the screen.

Mr. E. F. Chapman, representing the Atmospheric Gas Company, of Leeds, demonstrated a new method of artificial lighting for portraiture, and by means of which prints on gelatino-chloride printing-out paper could be produced by an exposure of about half-an-hour at a distance of six to ten inches from one of the lights. In the course of the demonstration Mr. Chapman said: This light is in no way dangerous, and is accompanied by no unpleasant smell although there is sufficient smell by which to detect an escape. Each burner gives a light of 125 candle power, as shown by a photometer, and to show that the actinic ray of the light is very great, I may say that with two burners on one side of a sitter and one burner on the other side, the three burners together giving 375 candle power approximately, it is possible to obtain with an ordinary studio lens and a Barnet medium plate the same results as with a 2,000 candle power arc lamp. The 12 photographs distributed were all 4-second exposures, Barnet medium plate, at $f/11{\cdot}3$. If the gas escape there is no danger whatever, and, indeed, it is almost impossible for an accident to take place under any circumstances, as I will presently show by performing some experiments which would involve the most serious danger in the case of any other system of gas lighting. The analysis of the gas by volume as burned shows that it consists of oxygen 20·46 parts, nitrogen 77·68 parts, and only 1·86 part of combustible hydrocarbon gas. There is no condensation in the pipes; in five hours, passing 10 feet of gas per hour through a U tube embedded in ice and salt, about a teaspoonful of fluid was condensed, and that was found to be water, there being no condensation of hydrocarbons. The gas is derived from a mixture of 75 per cent. of benzoline and 25 per cent. of paraffin in a carburetter; a current of air is passed through the cylinder with a fan worked by any suitable means such as a water turbine, clockwork, hot-air engine, or a small gas engine worked by the gas; in the case of a gas engine no air fan is wanted, the engine sucking the necessary air through the carburetter by the suction of the piston in the engine cylinder. The cost, which never varies, is 1*s.* 6*d.* for the mixture, and taking into consideration power to drive the necessary air draught (say) 3*d.* = 1*s.* 9*d.*, equivalent to 1,000 cubic feet of coal gas for light, heat, and power, and the system can not only be used for ordinary domestic and photographic purposes, but also for driving motors and gas engines. The only necessary apparatus for photographic purposes is a carburetter and air-pressure of some sort—such as may be provided by a bicycle pump, only half-an-inch of pressure being required for 10 lights. The burners are fitted with incandescent mantles. A Welsbach mantle when used with this gas is about 50 per cent. more durable than when used with coal gas, both as regards the substance and the illuminating power. A mantle after being burnt a few hours can be taken off the burner by the hand and stood up on end on a table or thrown one or two feet in the air and caught by the hand without breaking it. The quantity of hydrocarbon consumed is about one quart in sixty hours for each burner, at a cost of 3*d.* per burner. There is no coal gas used in the process.

A vote of thanks was passed to Mr. Chapman.

The following paper was read by the author:—

THE McDONOUGH-JOLY PROCESS OF COLOUR PHOTOGRAPHY.

By H. Snowden Ward.

Probably every member of the Society who is present is fully acquainted with the history and the technical outlines of the process which has hitherto been known in Britain under the name of Dr. Joly; but as no account of the process is to be found in the Society's *Journal* I venture to give some very brief details in order that they may become a part of the Society's archives.*

Everyone now-a-days knows the trichromatic process—in which three separate negatives are made, each of which registers the effects of one primary colour of light. From each of these negatives a positive is made, in suitably stained gelatine and by placing these positives over each other, a transparency is obtained which renders the colours of the original objects very beautifully.

By a modification of the process three printing blocks can be produced, from which impressions in three suitably coloured printing inks are made one over another, giving us the well-known trichromatic print so often seen on magazine covers and Christmas number supplements.

No one who has seen the very magnificent transparencies produced by Mr. F. E. Ives, Mr. Sanger Shepherd, the brothers Lumière and others by this triple negative process, will attempt to deny its exquisite beauty. Nor can anyone fail to admit that by the work of Mr. Sanger Shepherd, in determining and placing upon the market standard light-filters, its adoption by photographers generally has been greatly

* The references to this process which I find in the Society's *Journal* are:—

Vol. XXI, p. 68. Technical Meeting, November 10th, 1896, Mr. E. J. Wall showed photo-mechanical prints made in America, by a method in which three blocks were made to represent the three colour-sensation records, and from these a triple print was made by letterpress printing. The prints were deposited in the Society's collection.

Vol. XXII, p. 133. Photo-mechanical Meeting, December 21st, 1897, Mr. W. Gamble showed two Joly slides and a taking screen.

Vol. XXII, p. 168. Photo-mechanical Meeting, January 18th, 1897, Mr. W. Gamble gave a very brief outline of the process, showed taking and viewing screens and a few lantern slides, and stated that Dr. Joly had patented means of applying the process to photo-mechanical work.

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facilitated. But any time during the past forty years there have been men who dreamed of a natural-colour process in which only one negative and one print would be needed, which might produce, without any complicated labour on the part of the photographer, either transparencies, photographic prints, or photo-mechanical prints.

As far as is recorded, the first man to suggest a practical method of achieving this result was Monsieur Louis Ducos du Hauron, to whom the Progress Medal of this Society was awarded last year for his pioneer work.* He suggested that by using a transparent light-filter consisting of minute parallel lines in the three colours suggested for the separate negatives in trichromatic work, a single negative might be made which would record the three primary light-impressions, as on three separate negatives cut up into extremely fine strips. The lines of the light-filter were (to name the colours eventually adopted) alternately red, green, violet; red, green, violet; so that the first, fourth, seventh, and tenth lines were all red, and formed part of the red impression negative: the second, fifth, eighth, and eleventh were green and formed part of the green impression negative; while the third, sixth, ninth, twelfth, and so on, were violet, and formed part of the violet impression negative.

From such a negative a positive transparency could be made, which could be placed in contact with a light-filter ruled similarly to the one through which the negative was taken, with the result that a reproduction in colour, of the original object, would be seen. In any given part of the picture the colours which had not acted upon the negative would be represented by lines of silver deposit in the positive, which would cover up the corresponding lines of the light-filter used for viewing, and prevent their being seen. The colours which had acted upon the negative would be represented by transparent lines in the positive, through which the corresponding colours of the viewing light-filter would be visible.

The want of knowledge of plates sufficiently sensitive to the red and green rays prevented Du Hauron's suggestion being successfully put into practice; and the next thing recorded of the process is in the patent of James McDonough applied for in the United States, in 1892.

In this he claimed the invention of a method whereby specks of coloured transparent matter might be scattered under or over the film of a gelatine dry-plate. In the case where the film was first coated on the glass, and the coloured matter (say stained gelatine) scattered over the face, the plate would be exposed in the camera in the ordinary way. Where (say) blue light fell, it would penetrate the blue dots of the coloured matter, and be absorbed by the red and the green. The silver, on development, would be reduced under the blue spots, and when the plate was viewed as a transparency it would be a true negative or reverse of the original. How Mr. McDonough intended to work this process I do not know, for it is inconceivable that he expected two light-filters, made of fortuitously scattered particles to coincide so that he could remove the colouring from his first negative, make a transparency therefrom and use another colour-system as a viewing-screen. It is possible that he intended to use the first negative, with its colouring intact, as an ordinary negative, from which he would make a contact print or enlargement upon another plate, similarly coloured with scattered particles. In such case, the coloured particles which had not originally passed the light would be supported by transparent parts of the film; on being exposed to white light a portion of it would pass through them. They would only pass their own colour, absorbing the complementary rays, so that the light passing through them would only pass through the particles of the same colour on the new plate which was to form a positive. The development of this second plate would show a colour positive of a sort, but the practical difficulties of such a process are so enormous, and will be so obvious on thinking over the matter that I need not attempt to point them out. They seem to have been sufficient to make Mr. McDonough abandon the process in favour of a better scheme, which was the same as had been

* The suggestion made by Du Hauron of a colour-record negative taken through a screen ruled with bands of red, yellow and blue, was published in *Les Couleurs en Photographie; Solution du Problème*, p. 54. (Paris: Marion, 1869.)

suggested long before by Du Hauron, and as had occurred, almost simultaneously, to Dr. John Joly, of Dublin.*

There seems every reason to believe that both Dr. Joly and Mr. McDonough† evolved the idea quite independently of each other, and without knowledge of the work of Du Hauron. Moreover, a third investigator, Mr. McFarlane Anderson, appears to have devised the same idea about the same time,‡ and to have carried it a stage further than either Joly or McDonough in one particular direction, for he suggested the use of a black-and-white screen instead of a coloured light-filter in making the negative. His screen was to have the black line double the width of the transparent one, so that in whatever position it stood, it would expose one-third and cover two-thirds of the plate. With the screen in a given position he would make an exposure through (say) a red light-filter which was placed conveniently before, behind, or between the combinations of the lens. Then, moving the screen to the extent of the width of the transparent line, he exposed for the green, with a green light-filter. Lastly, moving the screen still further, so that its lines covered the two exposures already made, he gave his third exposure through a blue light-filter. The principal intention in this case was, I believe, to use the method for photo-mechanical work. It has the advantage that light-filters for application to the lens can be made purer, truer to the ideal colours, and more transparent than is possible when they have to be ruled in lines upon glass. It has, however, the disadvantage that three exposures are necessary.

* A patent of Mr. McDonough (which I had not found at the time of reading this paper) explains that his intention was to scatter coloured particles over a bichromated film. The result of exposure was to be that wherever light passed through a coloured particle, the bichromated film would be rendered insoluble; so that development by washing-out, as in the carbon process, would wash away the gelatine carrying those particles which had not allowed the light to pass. In this way a positive in colour would be obtained: from which other positives in colour could be made by contact printing. [H. S. W.]

† The following patents bear upon the subject:—

1892. McDonough. Photography in colours. No. 5,597 British; Nos. 471,186 and 471,187 U.S.
1893, April 17. Joly. Photography in colours. No. 7,743 Brit. (abandoned).
1894, July 7. Joly. Photography in colours. No. 13,196 Brit. (abandoned).
1894, July 23. Joly. Photography in colours. No. 14,161 Brit. (cancelled by amended patent).
1894, December 28. McDonough (see 1898. No. 611,457 U.S.).
1895, March 26. Joly. Colour photography. 6,214 Brit. (abandoned).
1895, October 16. Joly. Photographic screens (of dyed silk fibre). 19,388 Brit.
1895, October 21. Joly. Photographic film or plate. 19,711 Brit. (abandoned).
1896, January 2. Joly. Mounting of transparencies. 93 Brit.
1896, February 29. Joly. Storing and viewing transparencies. 4,621 Brit. (abandoned).
1896, April 11. Joly. Ruling lines in fluid pigments. 7,671 Brit. (abandoned).
1896, April 17. Joly. Ink reservoir for pens. 8,114 Brit. (abandoned).
1896, April 22. Joly. Mounting pens for ruling machines. 8,441 Brit. (abandoned).
1896, May 2. Joly. Ruling machines. 9,290 Brit. (abandoned).
1896, June 9. McDonough. Colour photography. 12,645 Brit.
1896, June 19. Joly. Colour sensitive photographic surface. 13,554 Brit.
1896, June 23. McDonough. Photographic screens (light filters). 13,895 Brit.
1896, August 15. Joly. Drawing or ruling pen. 18,097 Brit. (abandoned).
1896. McDonough. 561,685 U.S., printing frame; 561,686 U.S., screen; 561,687 U.S., photographic negative; all included in 12,645 Brit.
1896. McDonough. 562,642 U.S., colour screen; is the same as 13,895 Brit.
1897, March 15. Joly. Photography. 6,704 Brit. (abandoned).
1897, May 10. Joly. Colour photography. 11,612 Brit. (abandoned).
1897, June 9. Joly. Producing pictures, etc. 14,101 Brit. (abandoned).
1897, July 27. Joly. Colour screen photography. A changing-box with bag changing arrangement, for camera back. 17,632 Brit.
1897, July 30. Joly. Colour photography (ruling machine). 17,900 Brit.
1897, December 31. Joly. Printing. 30,863 Brit. (abandoned).
1898, February 17. Joly. Photography. 4,044 Brit. (abandoned).
1898, April 4. Joly. Colour photography (a method for avoiding line-effect by cross-lines of translucent ribs). 7,971 Brit.
1898. McDonough. Colour photography on paper. 20,417 Brit. 611,457 U.S. Application filed December 28, 1894.

‡ McFarlane Anderson's U.S. patent, No. 559,051; 1896. Application filed May 1st, 1895.

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The outline of the process was obviously and theoretically correct, and the problems to which Dr. Joly and Mr. McDonough addressed themselves were :—

1. To perfect the rulings of transparent trichromatic lined screens, with a view to quality and to small cost.
2. To perfect a plate which should be sensitive in due ratio to each of the colours of the screen.
3. To devise a method whereby the colour-record negatives might be printed on paper, by photographic means, giving true-colour results.
4. To devise a similar method for machine printing.

The ruling of the screen raised many problems, mechanical, chemical, and optical. It was necessary to have the lines as wide as possible, for economy in ruling and ease of registration in printing or in mounting a transparency against the viewing screen. On the other hand it was necessary for the ruling to be as fine as possible, to prevent the line-effect being so far visible as to mar the picture. After much independent experimenting Dr. Joly adopted a screen of 240 lines and Mr. McDonough chose 300 lines to the inch. As I have had some three months' working with the McDonough standard, I may say that I find no practical difficulty in registering transparencies of that fineness; while those who see the small pictures on the walls, and the projections on the screen will admit that the lines are practically invisible *from any position where a picture of ordinary scale can be comfortably viewed as a whole.*

The selection of colours and plates was very difficult. Eventually Dr. Joly chose to work with the Spectrum plate, and with a light-filter on the lens to cut down the light to meet the conditions of the plate and his ruled screen. The McDonough people found no plate which satisfied them, so they adopted a plate of Seed's make, which they specially orthochromatise for their own purpose. They, too, use a light-filter (which they call a "chromatic balance shutter") on the lens. This chromatic balance shutter will be shown to you in the lantern. It is somewhat similar to an iris diaphragm, and is adjustable to cut out more or less of the blue rays as the light varies from sun to shade. I shall show you comparative slips to demonstrate the effect of this balance shutter.

The difficulty of ruling screens economically was a very serious one, for though a few shillings in the cost of a taking screen would be of little importance, it is necessary that the viewing-screens should be very cheap to make them available for the ordinary photographer. Dr. Joly patented a method of making such screens (or light-filters) by laying dyed silk across a glass support; and he made a still more interesting and promising suggestion. This was to take a great number of sheets of coloured gelatine, alternating red, green and violet, and lay them in a great pile, like the leaves of a book, until the edges of the sheets should cover an area equal to that desired for the colour screen. Then by cutting sections across the edges (at right angles to the surfaces of the sheets) it would be possible to make a great number of thin films each consisting of the desired coloured lines. I do not know what difficulty caused these plans to be abandoned, but I imagine it was connected with the securing of equal width and even spacing of the lines across a wide sheet. Mr. McDonough confined his attention to ruling methods, and Mr. Tripp, his coadjutor and successor, has further developed machines for ruling large surfaces.

The selection of dyes which have the correct absorption, which are sufficiently transparent, and sufficiently permanent, was perhaps the greatest difficulty of all in connection with the screens. It seems to have been reasonably well overcome. The pictures I show you will speak for the quality. As for the permanency, my own experience of Dr. Joly's screens only extends to a couple of years, in which time I see no difference; and Mr. Crewdson tells me that there are screens in Mr. McDonough's laboratory which have been exposed to the light for five or six years, but which still seem good and brilliant. In any case, if the present transparent colours are not absolutely permanent, and if a picture fades in a few years, the transparency will remain uninjured and will only require binding up with a new viewing screen. The colours used on paper, for photographic or photo-mechanical printing, present much less difficulty in this direction than those for transparencies, and there is no obvious reason why the paper prints should not be as permanent as any chromo, except in cases where the photographic image may introduce a difficulty. In such case the print will have just the same permanence as a similar photographic print without colour.

The production of paper prints has not, I believe, been successfully carried out by Dr. Joly. McDonough's people, on the other hand, have produced the photo-mechanical prints now on the walls; by ruling paper with red, green, and violet lines, and by printing over those rulings a single impression, in black ink, from a metal block. The black print has exactly the same function as the lines of silver deposit in the transparency, viz.:—to mask those portions of colour which form no part of the picture; allowing the lines forming the picture to show. There is, of course, a difficulty in obtaining registration, and it has been suggested that while single prints, selected from large numbers, may be satisfactory, it is impossible that any long run should be so. In the case of the prints I show, every one is the result of four separate impressions on a hand press, three printings of a ruled block for the coloured lines, and a fourth in black from the "key" or picture block. These prints are taken from some thousands which I have seen, and which run very fairly evenly; and if such results are obtained with hand-feeding, I ask,—Where is the insuperable difficulty when printing in a machine which shall take the paper from the roll or in sheets and print the lines and key in immediately successive operations?

The production of a suitable block, by a line-etching process, from a negative which is not in pure line, but may give in every line complete gradation from greatest density to greatest transparency of which the plate is capable, is a problem for which I can see no solution. I have asked Mr. Tripp how it is done, but he, unacquainted with photo-mechanical technique, could not fully understand, much less explain, the difficulty I raised. He pointed to the prints, as proof that the difficulty *has*, somehow, been overcome, and I can only do the same.

I frankly show you such prints as have reached this country; examples of the faults, as well as the advantages of the process. The little reproduction of a piece of wool-work probably shows the thing at its worst, because the original is dull and sombre, and the tendency of the process is to somewhat sadden all the colours. This can be overcome to a great extent by methods which will suggest themselves to any colour-printer, who is also a student of colour

The Egyptian scene is from a negative by Mr. Crewdson, a man who knew nothing whatever about photography until he took up this colour process, and almost immediately sailed for Egypt with a camera. The print shows the same saddening of the colours, but it also shows something else which has immediately appealed, without any suggestion of mine, to three separate painters to whom I have shown this subject. That is, that while the whole is lowered in tone, the relative values are very well preserved. This is not the case to the same extent with the trichromatic printing process, because in superposing pigments we introduce many difficulties from want of transparency in the inks, and from the fact that one must be over the other in the darker parts of the picture.

In the trichromatic process, whether by superposed stained films or by successive ink printings we add shade to shade, in the McDonough-Joly method, as in Ives' triple projection method, or in the photochromoscope, we add light to light. One method obeys the law of mixed pigments, whereby all mixtures tend toward black: the other obeys the law of mixed lights, whereby all mixtures tend toward white. This is a digression from the immediate question of tone-values, and the true scale of tone is, as it seems to me, a very important feature of the McDonough-Joly process.

This little Egyptian print will bear careful inspection, and I specially direct your attention to such details as the shadow thrown by the left-hand door-post upon the post to the right. Compare this soft blue shadow with the solid black in the opening and the sunlight on the front of the wall, and I think you will see a quality seldom found in monochrome photograms of sunlight effects.

The third print, of a Mexican youth, shows the applicability of the process to newspaper or poster work. The original of this was made with a 300-line screen, and the block (in half-tone) was made from an enlargement.

Of the pure photographic process on paper I have not seen a single example. My friend and fellow member, Mr. W. M. Warneuke, tells me that he has seen specimens which were far from satisfactory. The method of their production is simple in principle. Paper is ruled with the three colours in lines just as for printing upon by letterpress. This can be done from the roll, and the printed paper re-rolled to be ready for coating with, say, an ordinary gelatino-chloride print-out emulsion. Over this it is necessary to register the line-negative in such a way that each line falls over its proper colour on the paper, then print, tone, and fix in the ordinary way. The cost of such ruling need not add more than a few shillings to a quire of paper, and while there are difficulties in the way of registering, I think it is not impossible to print register marks on the paper, which will correspond with similar ones transferred to the negative by the taking screen, and which the photographer can easily adjust.

The process as it now stands only requires, in addition to any photographer's existing outfit:—1. A taking screen; 2. A chromatic balance shutter; 3. Special plates; 4. Viewing screens or specially prepared paper. And possibly 5. Re-arrangement of the dark-room light to make it safe for the specially sensitive plates.

Among the disadvantages of the process is the length of exposure necessary; and it is on this account that the examples hitherto shown in this country have usually been stuffed parrots, sturdy flowers, or examples of "still life." At worst the exposure is only the same as is needed for the red element alone of the trichromatic method; and very great progress has recently been made in the direction of shortening exposure. I am told that in sunshine snap shots of moving objects, such as the Dewey reception parade both in Broadway, New York, and on the Hudson River, were amply exposed without sign of motion in the marching sailors or the manœuvring vessels. I have seen a portrait of a gentleman, made in a studio, and have been assured by the subject himself that it was taken while he was actually walking across the studio. And I think I shall be able to show you more than one of Mr. Crewdson's pictures, made in Paris in November and the end of October, with a lens working only at *f*/8, that may fairly be called snap-shots.

My own attempts to fix a ratio of sensitiveness have been of the most rough-and-ready kind, but roughly speaking the exposure needed is about five times that suggested by Watkins for the slowest Isochromatic plate on his list. Taking the Watkins meter as a guide I should put the plate number as about 2. Working with *f*/8, I have given such exposures as:—

Thames from Tower of London, 10 a.m. late in October, dull, cloudy day, 15 seconds. Exterior views at Westminster Abbey, 3 p.m. late in October, slightly watery sunshine, 6 seconds. View of North Cloisters, Westminster, 3 p.m. to 4 p.m. late in October; good October sunshine, one hour. These exposures were dictated by Mr. Crewdson's experience, and proved correct.

A large number of slides were shown: and the "Chromatic balance shutter," when projected on the screen, was described as follows:—

Chromatic balance shutter. The shutter is shown under the following varying conditions; (*a*) Closed for extremely blue, diffused light; (*b*) Partly open, normal position for most purposes; (*c*) Fully open for brilliant sunshine.

As a matter of fact the extremes of opening and closing are seldom used. It will be noted that the more diffused the light the stronger, relatively, are the blue rays.

In the present state of manufacture, when taking-screens and plates vary somewhat, it is necessary for producing the most theoretically perfect effect, to adjust the balance shutter to its own taking-screen, and to test, and possibly readjust, for each new batch of plates. To test it, take three strips of coloured paper, a red, a green, and a blue, not necessarily exactly the same colours as are used in the taking-screen. Place these on a white paper, and photograph the whole, giving about correct exposure, and with the balance shutter set for the light then prevailing. Develop the plate and examine it with a pocket magnifier. If the white paper shows a homogeneous deposit of silver the balance shutter was correctly adjusted. If, however, blue be too

f. 18r

strong, the balance shutter must be further closed; if the red be too strong, the shutter must be opened. Having fixed the position, which ought always to be done by the testing of two plates, the determination need not be re-made until another batch of plates comes in. The distance between sun position and shade position is always about the same, so that having determined one, the other naturally follows. It may be possible to produce plates of such uniformity as to render these tests unnecessary, or else to mark every batch with a balance shutter number, which will tell the photographer all he needs to know.

Mr. Leslie E. Clift was pleased that he had had an opportunity of seeing the examples of the process, but although they were very interesting he was glad that it was not claimed that the colours of nature were really reproduced. He thought Mr. Snowden Ward said in his paper that photo-engravers would have great difficulty in making a line block from a McDonough-Joly negative?

Mr. Snowden Ward said that from a negative which gave every gradation from transparency to opacity in one line, he could see no method of making a line block that would reproduce that effect. In lantern projection a blue subject, for instance, would have the other two lines masked out, there would be parts where the blue was masked out to give black, parts where the blue was partly obliterated by density of line, and other parts where the same blue line would be fully shown, and he could not see any line-etching method which would give anything but an abrupt break at some point in the line. Prints exhibited in the room showed that the difficulty had been overcome in some way, and he was assured that no hand work had been employed.

Mr. Sanger Shepherd did not see why it should not be quite simple to produce a photo-mechanical block in which the density of the line should be represented by a variation in width exactly corresponding to the screen placed over the transparent projected pictures, and this method should be more perfect theoretically than the crossed line screen. Mr. Snowden Ward had shown some trial exposures on a colour chart with different plates, and also one on a "Spectrum" plate with the "Absolutus" filter. The latter, of course, did not render the colouring correctly, because the "Absolutus" filter only represented colours according to their luminosity; and in order to get the red, green and blue lines of the Joly screen represented by equal density in the negative the filter must be adjusted to the particular density of each individual line. He had made a great many filters adjusted to the Joly plate, some of them sealed of the same type as the "Absolutus," and others adjustable. He thought he was one of the first to show an adjustable filter for the Joly process, and there was very little trouble in so adjusting a filter as to get all three lines equally represented in a photograph of a white object. He had seen some prints made by Mr. Joly himself, and in his opinion they were more than equal to the American examples shown by Mr. Snowden Ward.

Mr. T. Bolas wished to say a few words about the discriminative line in connection with the photo-mechanical question. It seemed to him that the problem should be capable of solution on the principles laid down by Mr. Ives. Suppose there were crossing the line of the negative a series of other and opaque lines; if the latter had no width at all there would be a full passage of light; if they had a width equal to their own pitch there would be complete opacity; and it appeared to him that the line should be capable of division on this principle just in the same way that Mr. Ives' plate was divisible into optical pyramids, only in this case the elements crossing the line would be *quasi* cylindrical (or as a cylindrical lens would render Mr. Ives's dots) and not pyramidal. So far as the lantern slides were concerned, the results had certainly surprised him, but as Mr. Clift had said, this process—in common with all three-colour methods—did not give an absolute reproduction of colour. He was inclined to think that their excellence must be largely due to the superiority of the American ruled screens.

F. 18v

Mr. Snowden Ward said the tests with the British filter were merely carried out for the purpose of obtaining a rough idea of the effects of different filters, and not for the purpose of securing satisfactory colour results, or with any suggestion that these filters were intended for such purpose.

A vote of thanks was passed to Mr. Snowden Ward.

AFFILIATION OF PHOTOGRAPHIC SOCIETIES.

Lectures, etc., Available for Circulation.

[The Secretary is prepared to book any of the following lectures, on hearing from the Societies that may wish to borrow them, with the exception of those (marked with an asterisk) which are undergoing revision.]

Lantern Slide Making. By Mr. John A. Hodges, F.R.P.S. With lantern slide illustrations.

Negative Making. By Mr. Chapman Jones, F.I.C., F.C.S., F.R.P.S. With lantern slide illustrations.

*Pictorial Photography. By Mr. A. Horsley Hinton. With lantern slide illustrations.

Architectural Photography. By Mr. H. W. Bennett, F.R.P.S. With lantern slide illustrations.

*Intensification and Reduction. By Mr. John McIntosh. With lantern slide illustrations.

Hand Camera Work. By Mr. W. Thomas, F.R.P.S. With lantern slide illustrations.

Portraiture. By Mr. Harold Baker. With lantern slide illustrations.

Elementary Photographic Optics. By Mr. R. Child Bayley, F.R.P.S. With lantern slide illustrations.

*Bromide Enlarging. By Mr. John H. Gear, F.R.P.S. With lantern slide illustrations.

The Carbon Process in Practice. By the Autotype Company. (Specially written for the Affiliation.) With numerous illustrations.

The Photographing of Flowers. By Mr. H. T. Malby, F.R.P.S. Illustrated by about 50 lantern slides.

Defects and their Remedies. By Mr. E. Dockree. With lantern slide illustrations.

Methods of Control in Photographic Printing. By Mr. G. J. T. Walford. With lantern slide illustrations.

Elementary Chemistry for Photographers. By Mr. C. F. Townsend, F.R.P.S.

*Notes on Rejlander and his work. By Mr. A. H. Wall. Illustrated by slides from many of his pictures.

Orthochromatic Photography. By Mr. E. Sanger Shepherd, F.R.P.S. With lantern slide illustrations.

A set of 67 lantern slides, entitled "Linen and its Production," with explanatory notes by the Brechin Photographic Association.

A set of slides by the Toronto Camera Club.

Royal Meath and County Wicklow. A set of 100 slides with descriptive notes by members of the South London Photographic Society.

Scenes on the Pennsylvania Railroad. A set of 60 slides with notes lent by Mr. W. H. Rau, of Philadelphia.

A set of 100 slides selected from several sets previously in circulation. This set will, in the spring, be sent to the Affiliated Societies abroad, but may be borrowed by Societies at home in the meantime.

The Belgian Excursion, 1899. Leaders, William F. Slater and Walter D. Welford. A set of slides contributed by the party, and accompanied by descriptive notes.

ABSTRACTS.

On the Action of Chromate of Silver in Collodio-phosphate of Silver Emulsions, and the Photo-chemical Behaviour of Mixtures of Silver phosphate and Silver chloride Collodion Emulsions. *E. Valenta.* (*Photog. Correspondenz*, 38, 449.)—The author has previously described a silver printing-out paper of remarkable sensitiveness; and a series of experiments have shown him that printing-out papers of this kind possess a very considerable range of gradation, and negatives very full of contrast are required in order to produce brilliant prints. In this respect the collodio-phosphate papers greatly surpass albumenised paper, and if it is desired to produce papers which shall work like albumenised paper, some substance must be added to the emulsion, which will shorten the scale of gradation, and bring about a greater differentiation of the various gradations of tone from each other. In this respect, also, the collodio-phosphate emulsion is similar to the chloro-citrate emulsions, and it appears that the addition of chromic acid to the phosphate emulsion enables papers to be obtained, which, as regards gradation, are very like albumenised paper.

The chromic acid also reduces the sensitiveness of these emulsions, but by the addition of a suitable quantity, the emulsion will give brilliant printing papers, even when they are very much more sensitive than albumenised paper.

The same result can, however, be brought about without the use of chromic acid or its salts by mixing a silver phosphate emulsion with a chloro-citrate emulsion in suitable proportions.

The author prepares two emulsions as follows :—

I. *Phosphate Emulsion.*

A.	Collodion 4 per cent.	150 cc.
	Ether	30 ,,
	Phosphoric acid (D=1·26) ...	2 grammes.
	Citric acid	5 ,,
	Alcohol	10 cc.
	Glycerine alcohol	2 ,,
B.	Silver nitrate	7 grammes.
	Water	8 cc.
	Alcohol	15 ,,

II. *Collodio Chloro-citrate Emulsion.*

Of similar composition to the phosphate emulsion only it contains, instead of the phosphoric acid, equivalent quantities of strontium and lithium chlorides in the proportion of 2 : 1.

An examination of the sensitiveness and range of gradation of the two emulsions with a scale photometer showed that the phosphate emulsion was about four or five times more sensitive than the chloro-citrate, and that the range of gradation of the former stood as 25 degrees to 13 degrees of the latter. Freshly sensitised albumenised paper showed a lower degree of sensitiveness than the chloro citrate emulsion and a range of gradation from 18 degrees on the photometer.

The following mixtures of both emulsions were made—

1. 10 cc. of I, with 90 cc. II.
2. 20 ,, ,, I, ,, 80 ,, II.
3. 30 ,, ,, I, ,, 70 ,, II.
4. 50 ,, ,, I, ,, 50 ,, II.

The papers coated with these emulsions gave the following results :—

No.	Sensitiveness as compared with collodio-chloride of silver emulsion.	Range of gradation.
1	2·5	17°
2	3·8	18°
3	4·1	20°
4	4·7	Over 20°

These experiments show that by the addition of a phosphate emulsion to collodio-chloride emulsions it is very easy to increase the sensitiveness of the latter and at the same time increase the range of gradation. J. R. G.

Intensifying and Toning Platinum Prints. *By Raimund Rapp.* (*Eder's Jahrbuch für Photographie*, **1900**, 90, *and B. J.* **47**, 819.)—To intensify a print, wash it thoroughly and immerse it in the following bath :—

Cold saturated solution of gallic acid	50 c.c.
Water	50 „
10 per cent. solution of silver nitrate	2 „
Glacial acetic acid	10 to 20 drops.

The print will intensify slowly, and, when it has acquired sufficient depth, wash it in two or three changes of water to which a little acetic acid has been added. The silver, which has been deposited on the image, is then converted to platinum by using the following bath :—

Potassium chloro-platinite	1 gramme.
Phosphoric acid	15 c.c.
Water	600 „

Wash the print once more.

As the process is based upon physical development, by the introduction of various modifications other tones may be secured after the print has been treated with the gallic acid and silver bath. For reddish tints use the well-known uranium intensification formula very much diluted. The bath may be reduced to one-tenth of its ordinary strength. Green tones may be obtained by treating a red or brown uranium toned print with a 25 per cent. solution of sulphate of iron. If a print toned green in this way is immersed in water acidulated with hydrochloric acid, it will acquire an intense blue tone. Less pronounced green and blue tones may be obtained by immersing the silver intensified print in the following bath :—

Water	50 c.c.
Ferricyanide of potassium solution (1 : 50)	20 „
Ammonio-citrate of iron solution (1 : 50)	10 „
Uranium nitrate solution (1 : 50)	10 „
Glacial acetic acid	10 „

The prints gradually acquire a green tone, but, if the subsequent washing is too prolonged, they turn blue. A very fine deep blue tone may be obtained with the following gold bath :—

Water	1 litre.
Nitrate of lead	15 grammes.
Sulphocyanide of ammonium	40 „

After filtration add :—

Chloride of gold solution (1 : 50)	20 c.c.

The prints may remain several hours in the bath, during which time intensity of the tone increases. Wash for about an hour.

PALLADIUM TONING. *By J. Joë.* (*Photographische Wochenblatt and B. J.*, **47**, 804.) The author gives details of a palladium toning process for silver prints. Plain salted paper is recommended as the most suitable, but commercial gelatino-chloride and collodio-chloride papers may also be used, in which case sepia tones prevail, unless combined palladium and gold toning is resorted to. There is much similarity between palladium and platinum toning, with regard to colour and other characteristics. The prints do not lose so much in fixing as when toned with iridium, and they should be printed to about the same depth as for platinum toning. It is very important that the prints should be thoroughly washed before toning and before fixing. Insufficient washing before fixing is followed by yellow stain, a fact which may be taken advantage of if the effect of an engraving upon toned paper is aimed at. It is also important that the bath should be acid. A strong bath favours black tones, and a weaker bath sepia and brown. The following formula is recommended :—

Potassium palladio-chloride	1 gramme.
Table salt	10 grammes.
Citric acid	10 ,,
Water	2 litres.

After taking the print from the frame, immerse it in salt and water, thoroughly wash it before toning, and again thoroughly wash before fixing.

The following bath gives very agreeable chocolate brown tones :—

Potassium palladio-chloride	1 gramme.
Molybdate of ammonia	10 grammes.
Citric acid	10 ,,
Water	2½ litres.

Palladium is an expensive toning process on account of the high price of the metal, but there is some compensation in the fact that more prints may be toned with a given quantity of it than with gold.

BOOKS, ETC., RECEIVED.

LOCKYER'S PHOTOGRAPHIC PREPARATIONS.

We are informed by Mr. J. E. Lockyer that he is introducing his toning solution in dry form. The packet we have received contains 4 grains of gold chloride in a small sealed tube, and a bottle holding 60 grains of ammonium sulphocyanide, both of which are made up to 16 ounces with water, equal parts being taken to form the toning solution. Mr. Lockyer is also putting up gold chloride in tubes and bulbs in quantities of 7½, 15, 30, and 60 grains, besides the 4 grain tube alluded to above, and a "special" recrystallised sulphite of soda, which is said to be quite free from sulphate.

PENROSE LITHO-ZINC PLATES.

Messrs. Penrose and Co. call attention to the fact that they are now making patent zinc plates for lithography, having acquired the license to work the patents of the inventor, Mr. G. H. Block. It is claimed that the results are quite equal to, if not better than, the work done upon stone, whilst the saving in expense is very considerable. Four different kinds of zinc plate will be issued, suitable for various classes of work, and there are several gauges that may be selected from.

CARBONA P.O.P.

This paper, recently placed upon the market by Messrs. Griffin and Sons, is introduced as a paper particularly capable of giving "artistic" photographs. It is made with matt and glossy surfaces, and gives an effect bearing some resemblance to pigment or carbon prints. The directions give formulæ for separate and combined toning and fixing,

f. 20v

and the paper, if these are followed, may be manipulated with ease, some very pleasing tones being obtainable.

Ruby Christia.

Messrs. Thomas Christy and Co. write that they have now resumed the manufacture of this well known medium for dark room illumination, large Government contracts having for a long time compelled them to discontinue its supply. With enlarged works they are again in a position to supply the material.

Attendances of Officers and Members of Council during 1900.

Council Meetings. Number of possible attendances.	Committee Meetings. Number of possible attendances.	Name.	Number of Attendances at Council Meetings.	Number of Attendances at Committee Meetings.
10	27	T. R. Dallmeyer (President)	10	7
10	—	The Earl of Crawford (Vice-Presidents.)	—	—
10	12	Chapman Jones (Vice-Presidents.)	10	12
10	17	Maj.-Gen. J. Waterhouse (Vice-Presidents.)	7	11
10	—	Sir H. Trueman Wood (Vice-Presidents.)	2	—
10	30	G. Scamell (Treasurer)	7	16
10	27	John A. Hodges (Hon. Sec.)	7	21
10	—	H. Wilmer (Librarian)...	3	—
10	—	F. Ince (Solicitor)	—	—
10	—	Sir W. de W. Abney, K.C.B. (Editor).	—	—
10	5	T. Bedding	10	3
10	3	T. Bolas	10	3
10	—	C. H. Bothamley	2	—
10	8	F. A. Bridge	8	6
10	—	A. Cowan	8	—
10	—	W. E. Debenham	8	—
8	—	W. B. Ferguson	6	—
10	17	Rev. F. C. Lambert	8	7
10	24	A. Mackie	7	20
10	12	J. W. Marchant	10	10
8	—	Prof. R. Meldola	5	—
8	2	E. S. Shepherd...	3	—
10	12	J. A. Sinclair	8	7
10	—	J. Spiller	6	—
10	—	J. W. Swan	—	—
7	4	W. Thomas	3	1
10	21	J. J. Vezey	10	18
10	2	E. J. Wall	8	1
10	23	H. Snowden Ward	9	22
10	11	J. B. B. Wellington	10	6

The undermentioned gentlemen are members of certain Committees, but not officers or members of the Council.

—	2	F. Bishop	—	2
—	2	J. J. Elliott	—	2
—	10	H. Vivian Hyde	—	4
—	12	J. C. S. Mummery	—	6

Note.—The attendances of the Selecting and Hanging Committee at the New Gallery are not recorded and are not included in the above.

f. 21r

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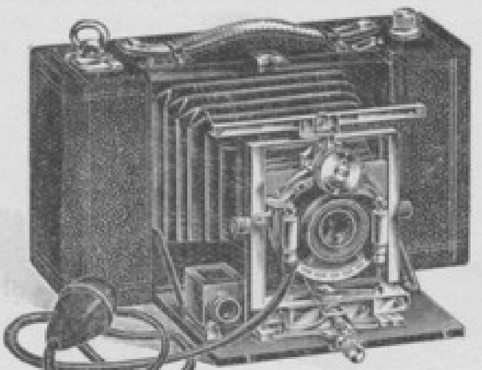

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The Royal Photographic Society.

The Society consists of three classes of Members—namely, Ordinary Members, Fellows, and Honorary Fellows. Anyone who is interested in either the science or the art of Photography is eligible for election as an Ordinary Member. The Fellowship, on the contrary, is open only to those Members who are able to satisfy the Council that they have ability in one or other of the many branches of the subject.

N.B.—For further particulars and forms of application for membership, address The Secretary, Royal Photographic Society, 66, *Russell Square, London, W.C.*

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to Erasmus Galton from F.G.

Photographic Records of Pedigree Stock.

By Francis Galton, *D.C.L. (Oxf.), Hon. Sc.D.(Camb.), F.R.S.*

[Plate].

It is my purpose shortly to communicate with the Councils of some of the Societies who publish stud or herd books, urging the systematic collection of photographs of pedigree stock and of more information about them than is now procurable. Believing that if my proposals were carried into effect, they would greatly facilitate the study of heredity, I desire, before approaching the Societies, to submit my intended proposals to the criticism of a scientific body, and none seems more appropriate for the purpose than the Zoological Section of the British Association.

I have lately shown how the general knowledge that offspring can inherit peculiarities from their ancestry as well as from their parents may be superseded by a definite law whose nature was first suggested to me by theoretical considerations. Being subsequently in a position to verify its accordance with a large number of pertinent facts, I submitted the results to the Royal Society in a communication entitled 'On the Average Contribution of each Several Ancestor to the Total Heritage of the Offspring.'[1] My theory was thoroughly examined from fresh points of view by Professor Karl Pearson, F.R.S., in one of his remarkable 'Contributions to the Mathematical Theory of Evolution,'[2] in which he showed that the theory accorded with other observations, and accounted for other conclusions that had already been reached. Assuming, then, that the Ancestral Law may be accepted as at least approximately true, it will be serviceable now in showing the relative importance and range of the data which breeders must take into account, if they pursue their art with thoroughness. The law is that, *speaking on the average*, the two parents contribute between them one-half of the total heritage of the offspring, that the four grand-parents contribute between them one-quarter, the eight great grand-parents one-eighth, and so on. Consequently, since $\frac{1}{2}+\frac{1}{4}+\frac{1}{8}+\frac{1}{16}+\&c.=1$, the whole of the heritage is accounted for. The same law may be stated in another form, namely, that each parent contributes *on the average* one-quarter, each grand-parent one-sixteenth, each great grand-parent one sixty-fourth, and so on. It is a property of the first series of fractions that each term is equal to the sum of all those that follow ($\frac{1}{2}$ being equal to $\frac{1}{4}+\frac{1}{8}+\frac{1}{16}+\&c.$; $\frac{1}{4}$ to $\frac{1}{8}+\frac{1}{16}+\&c.$), therefore it results that if genealogical knowledge should cease with the grand-parents, inasmuch as they contributed one-quarter, another quarter of the heritage will remain indetermined; if it ceases with the great grand-parents one-eighth will remain indetermined; if with the next ascending grade, one-sixteenth, &c.

It must be understood that the law is intended to apply only to what may be called *plain* heredity, that is to cases where qualities are capable of blending freely, or, if they refuse to blend, where they present themselves as alternative possibilities. The necessary modifications have yet to be investigated when it has to be applied to hybrid heredity, and to those partial forms of hybridism which occur in cross breeding, where

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The ancestral law proves the importance of a much more comprehensive system of records than now exists. A breeder ought to be in a position to compare the records of all the near ancestry of the animals he proposes to mate together, in respect to the qualities in which he is interested. More especially he ought to have access to photographs which indicate form and general attitude far more vividly than verbal descriptions. But the information in stud and herd books is too meagre for the requirements of the breeder, while the photographs published in newspapers and elsewhere are inadequate for making complete genealogical collections.

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f. 2

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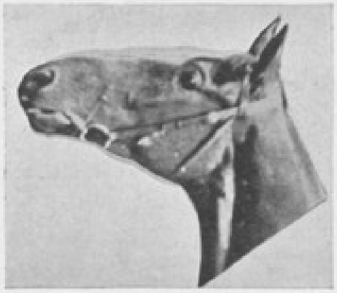

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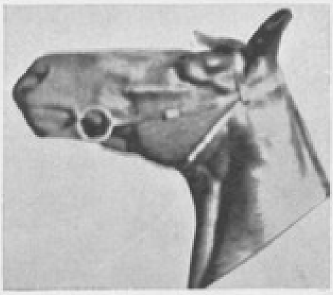
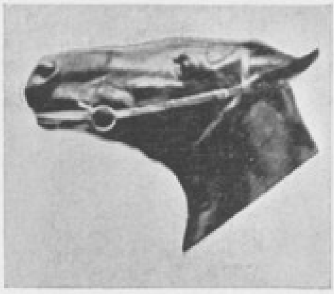
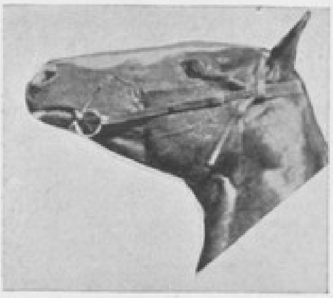


C, 12.—Raconteur.



E, 12.—Speedwell.



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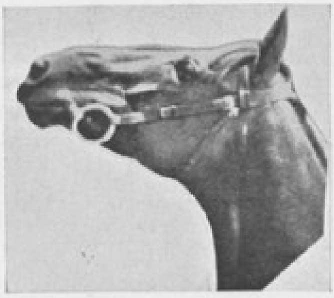
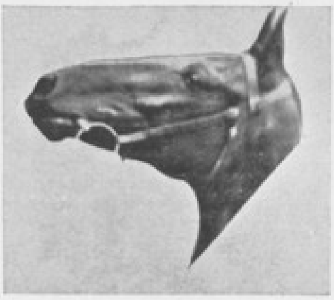
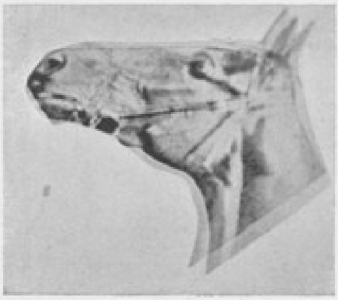


D, 12.—St Marnock



F, 12.—Salebela.



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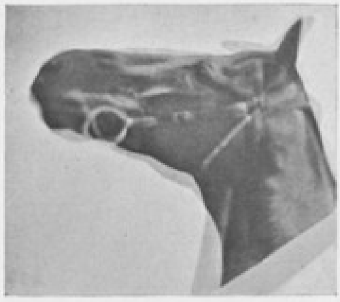

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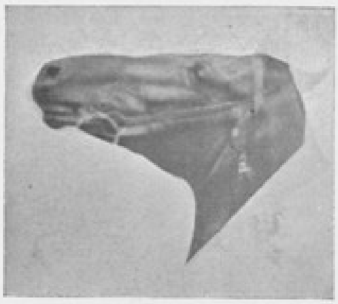

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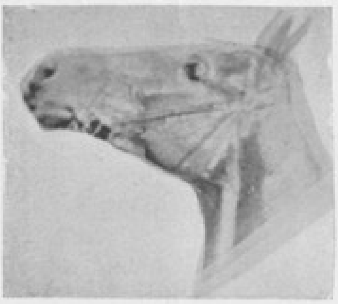
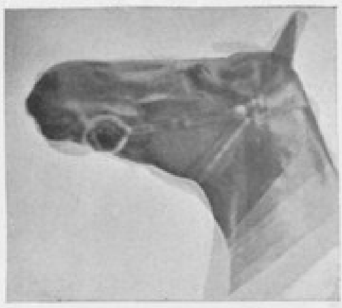
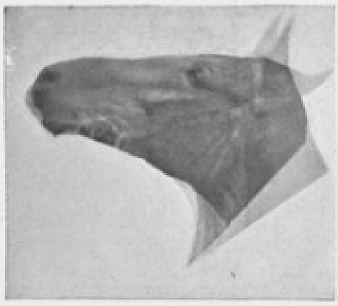


A, 4; B, 4; C, 1; D, 1; E, 1; F, 1.



C, 4: D, 4; A, 1; B, 1; E, 1; F, 1.



E, 4; F, 4; A, 1; B, 1; C, 1 D, 1.

Photographed by FRANCIS GALTON, F.R.S., to illustrate his memoir submitted to the British Association, Bristol, 1898, on 'Photographic Records of Pedigree Stock.'

It may be said that, even if all the ancestral photographs were spread in full view on a table, no human brain could combine into a single mental image the peculiarities in feature even of the two parents, and of the four grandparents, in the proportion laid down by the ancestral law. There is, however, a method by which a substitute for a mental picture may be obtained, which may possibly prove serviceable in practice. It is by *compositing* the photographs, allotting to each its appropriate time of exposure. I submit a few composites which I have made of the heads of racehorses: the component portraits are from the earlier numbers of the 'Racing Illustrated.' I enlarged them to an uniform scale, cut them out to get rid of the confusion introduced by a variety of background, and then combined them in various proportions. Especially I took six, those of (A) Sir Visto, (B) Solaro, (C) Raconteur, (D) St. Marnock, (E) Speedwell, and (F) Salebeia, which will henceforth be distinguished by those letters. With the plate, stop, and the two small electric lamps that I used for illumination, it required an exposure of 240 seconds, say of 12 units of time, each consisting of 20 seconds, to give a good copy of any one of the portraits, so I proceeded as follows:—First, I made a composite of A and B, allowing 6 units of exposure to each of them, or 12 units in all; then I made another composite of A, B, and the four others, allowing 4 units to A, 4 units to B, and 1 unit to each of the four others, forming a total as before of 12 units. So while the composite which I will call A 6, B 6, illustrates the combined features of the two parents, that of A 4, B 4, C 1, D 1, E 1, F 1 illustrates those of two parents and four grand-parents in the proportions laid down by the ancestral law. I proceeded similarly with C, D and with C, D and the other four, and again with E, F and with E, F and the other four; I submit these six composites. Of course the process could be extended indefinitely, working backwards to include as many previous generations of ancestry as desired, and it might be equally well applied to portraits of other animals than horses, including men and women, whose features combine unexpectedly well in composites, though one sex be bearded and the other not. This is not the place to enter further into the details of composite making, which I have now reduced to a very simple process whose accuracy is evidenced by the identity of the composites that have been re-made at different times from the same components. The specimens I submit would have been better if they had been made from the original photographs and not from photo-process copies of them, still they will serve to gauge the amount of information which composites are likely to give to the breeder. They should be carefully scrutinised and compared, when more differences and points of interest will be found than are apparent at a first glance.

A photograph considered merely as a portrait tells about as much of an animal as can be gathered from a single view of it; it defines the contour, the slope of the shoulders, the set of the head, the forms and the positions of the limbs, but this is by no means all that is obtainable from a photograph. It may be so taken that measurements made upon the photograph, after certain corrections have been applied to them, will be nearly as good as those made on the animal itself. Now, measurements are of the highest importance, for science is based on numerical data and the science of heredity is no exception to the general rule. Its progress depends primarily upon the power of procuring large collections of measurements of the same parts, which admit of being combined in any proportions by simple arithmetic. It matters little what limb, or bodily part, or faculty is the subject of measurement, because laws which are true for one particular quality, and for one particular race of animals or plants, will presumably apply with small modifications to any other quality and race. Therefore it would be no unworthy occupation for a scientific man to devote years of labour to carefully measuring each of many parts in the photographs of offspring and their ancestry, and to discuss the results by the elaborate methods of the higher statistics.

The photographs of which I speak are assumed to have been taken under the following conditions. They would represent side views of the animals and therefore be comparable on equal terms so far as position is concerned. The animals would have been photographed at a distance of *not less* than thirty feet from the camera, in order to avoid sensible distortion of the portrait. They should be taken while standing on hard ground, that the feet may be clearly shown. The height of the camera above the ground and its distance from the animal should be roughly measured and noted. Lastly, two direct measurements of the height of the animal should be made, one at its withers, the other at its croup. The photograph now becomes more than a mere picture, because the recorded data, together with others afforded by the photograph itself, supply corrections that will cause the measurements made upon it to correspond with more or less accuracy to those made on the animal itself. Of course, their correspondence would not be so exact as it would be in photographs taken in a 'hippometric' laboratory provided with marked lines on the ground and walls, but such a laboratory is impracticable on many grounds. Thoroughbred horses are so easily frightened in unfamiliar places and at unfamiliar objects that the best plan is to photograph them leisurely among their accustomed surroundings. It is difficult and dangerous to apply tapes and calipers, which tickle and irritate, for thoroughbred horses are exceedingly sensitive, timid, fidgety, and often vicious, while they are supple and sudden in their movements of offence. Measurements of the two vertical heights, made in the usual way, are comparatively easy to manage.

I find, moreover, that vertical measurements of all kinds may be made quickly and accurately without touching the objects at all, by means of a simple instrument which I roughly put together for trial. Its principle is that of a collimator, with additions and modifications. It seems very suitable for use at agricultural and other shows where many animals are collected.

Though many useful measurements can be made on a plain photograph, it would be a decided gain to select two, three or more important osseous protuberances, which can be easily felt, and to mark their positions by sticking on the animal small wafers of sufficiently adhesive paper—say, one quarter of an inch in diameter. The corresponding marks on the photographs will be too small to attract notice, but they are easily found when looked for, and afford excellent points from which to measure. I may add that measurements I have made, and had made, both on horses and on their photographs, show that the relative dimensions of horses differ considerably. If some five different measurements were made on an adult racehorse, it would be as easy to identify him by a 'Bertillon process' as it is to identify prisoners.

The corrections that are required for measurements on photographs can be found by appropriate tables. After these have been calculated in some detail once for all, their application would be simple. The needed corrections refer (1) to slight deviations in the position of the animal from that which would have yielded an exact side view of him, (2) to the camera having been obviously tilted, (3) to different degrees of nearness of different parts of his body. It is curious in how many different ways these corrections may be determined when the range of available measures is slightly increased. I have already discussed the question for a different series of data in 'Photographic Measurements of Horses and other Animals' (*Nature*, Jan. 6, 1898), which shows the general character of the problem, but I cannot enter into details now. The primary question is, will photographers and grooms take the proposed measurements with sufficient correctness, and are any additions to them feasible? To settle this question, experiments should be concentrated by various persons upon the same quiet and well measured animals. These ought to determine the trustworthiness of the results ascording to the data in use, and would show the minimum of effort that is necessary to afford the required degree of accuracy. I should be content if the average error in the calculated height and length of the horse did not exceed one inch, or say one-and-a-half per cent.

It remains to consider what has hitherto been taken for granted—the best method of starting a systematic collection of photographs of pedigree stock. My proposal is to suggest to the principal Societies which publish stud or herd books, that they should proceed as follows:

(1) To arrange with a photographer to store such negatives as the Society may hand over to his charge; he undertaking to supply prints from them to the public at a moderate cost and under reasonable regulations.

(2) To invite owners of pure-bred stock to send to the Society with which they are in connection, a negative photographic plate of each of the animals which they use for breeding, on the understanding that if the negative be accepted by the Society it will be handed over to the photographer.

(3) Only those negatives will be considered suitable for acceptance (*a*) which are of good quality, and which do not transgress specified limits of size; (*b*) which are strictly side views; (*c*) which have been taken at a distance from the animal of not less than 30 feet, and (*d*) which show the animal standing on hard ground.

(4) The following information is to be stamped or written on the negative in such a way as to be clearly legible in the prints, (1) the name and sex of the animal, (2) year of its birth, (3) year and month of taking the photograph, (4) heights at its withers and croup, (5) height of camera and its distance from the animal.

(5) The Society shall order an asterisk to be affixed to the name of each animal entered in its stud or herd book, when the photographic negatives of its sire and dam have been accepted.

It seems to me that a system such as this would be efficient, self-supporting and acceptable to all parties. Breeders would be pleased that photographs of their animals should be publicly recognised as serviceable for the advancement of their art. Owners of valuable animals are almost sure to order photographs of them on their own account, so the gift of the negatives to the Society would deprive them of nothing. The asterisks applied to the names of the offspring would be a valued distinction, and would help to introduce the system. Later on, when they had become common, the absence of an asterisk would excite suspicion and require explanation. Lastly, the printing of the photographs would be self supporting. I have already expressed a belief that the custom would arise of printing a separate pamphlet for every important stallion or bull containing its photograph and those of its nearer ancestry, together with other appropriate information. Larger publications of a more costly kind would doubtless be issued under the auspices of each Society, to correspond with an awakened demand for fuller information on the antecedents of pedigree stock.

As regards useful additions to the printed matter in stud and herd books, I would now merely allude to the need for them, and to the propriety of carefully reconsidering how much of real utility could be asked for from breeders that they would supply willingly and truthfully. The measurements of adult animals, of which I spoke, would be appropriate entries. An accumulation even of these during two or three generations would be exceedingly valuable, considering how many coherent results in the science of heredity have been derived from observations of human stature, though limited to comparatively small numbers of parents and their offspring.

The amount of money annually spent in rearing pedigree stock is enormous ; so is the care and thought bestowed upon it, and so also is its national importance. The non-preservation of adequate records of pedigree stock is a cruel waste of opportunity, and has been most prejudicial to the acquirement of a sound knowledge of the art of breeding. If the scheme I have sketched be found feasible, it will cause much to be noted that has hitherto been overlooked, and much that is commonly observed, to be placed permanently on record instead of being ill remembered and soon wholly forgotten.

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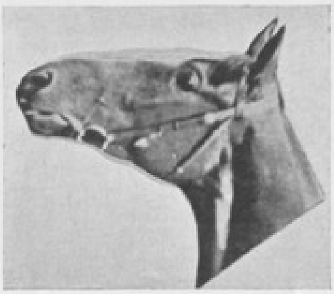

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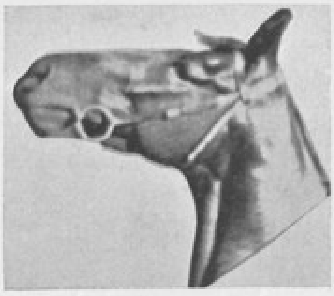

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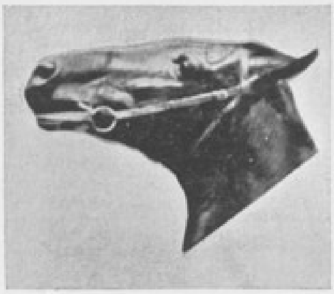

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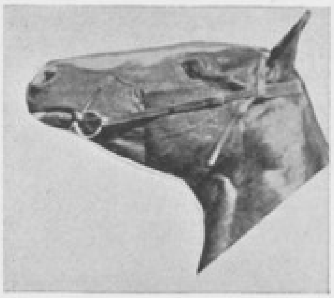
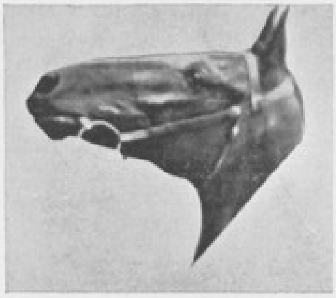
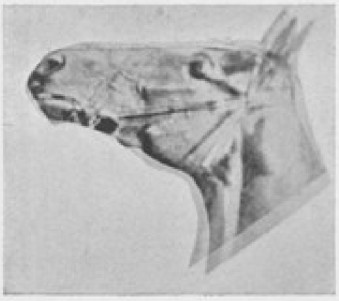


B, 12.—Solaro

D, 12.—St Marnock



F, 12.—Salebeia.



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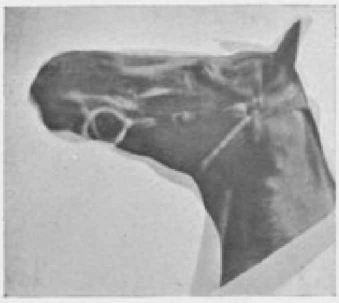
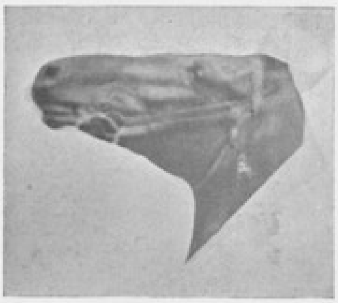


C, 6; D, 6.



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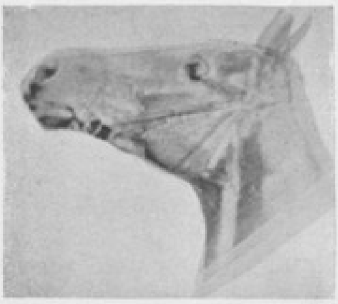
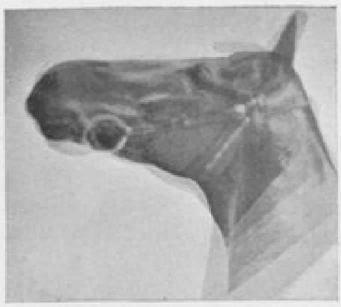


A, 4; B, 4; C, 1; D, 1; E, 1; F, 1.



C, 4: D, 4; A, 1; B, 1; E, 1; F, 1.

E, 4; F, 4; A, 1; B, 1; C, 1 D, 1.

Photographed by FRANCIS GALTON, F.R.S., to illustrate his memoir submitted to the British Association, Bristol, 1898, on 'Photographic Records of Pedigree Stock.'

It may be said that, even if all the ancestral photographs were spread in full view on a table, no human brain could combine into a single mental image the peculiarities in feature even of the two parents, and of the four grandparents, in the proportion laid down by the ancestral law. There is, however, a method by which a substitute for a mental picture may be obtained, which may possibly prove serviceable in practice. It is by *compositing* the photographs, allotting to each its appropriate time of exposure. I submit a few composites which I have made of the heads of racehorses : the component portraits are from the earlier numbers of the 'Racing Illustrated.' I enlarged them to an uniform scale, cut them out to get rid of the confusion introduced by a variety of background, and then combined them in various proportions. Especially I took six, those of (A) Sir Visto, (B) Solaro, (C) Raconteur, (D) St. Marnock, (E) Speedwell, and (F) Salebeia, which will henceforth be distinguished by those letters. With the plate, stop, and the two small electric lamps that I used for illumination, it required an exposure of 240 seconds, say of 12 units of time, each consisting of 20 seconds, to give a good copy of any one of the portraits, so I proceeded as follows :—First, I made a composite of A and B, allowing 6 units of exposure to each of them, or 12 units in all ; then I made another composite of A, B, and the four others, allowing 4 units to A, 4 units to B, and 1 unit to each of the four others, forming a total as before of 12 units. So while the composite which I will call A 6, B 6, illustrates the combined features of the two parents, that of A 4, B 4, C 1, D 1, E 1, F 1 illustrates those of two parents and four grand-parents in the proportions laid down by the ancestral law. I proceeded similarly with C, D and with C, D and the other four, and again with E, F and with E, F and the other four ; I submit these six composites. Of course the process could be extended indefinitely, working backwards to include as many previous generations of ancestry as desired, and it might be equally well applied to portraits of other animals than horses, including men and women, whose features combine unexpectedly well in composites, though one sex be bearded and the other not. This is not the place to enter further into the details of composite making, which I have now reduced to a very simple process whose accuracy is evidenced by the identity of the composites that have been re-made at different times from the same components. The specimens I submit would have been better if they had been made from the original photographs and not from photo-process copies of them, still they will serve to gauge the amount of information which composites are likely to give to the breeder. They should be carefully scrutinised and compared, when more differences and points of interest will be found than are apparent at a first glance.

A photograph considered merely as a portrait tells about as much of an animal as can be gathered from a single view of it ; it defines the contour, the slope of the shoulders, the set of the head, the forms and the positions of the limbs, but this is by no means all that is obtainable from a photograph. It may be so taken that measurements made upon the photograph, after certain corrections have been applied to them, will be nearly as good as those made on the animal itself. Now, measurements are of the highest importance, for science is based on numerical data and the science of heredity is no exception to the general rule. Its progress depends primarily upon the power of procuring large collections of measurements of the same parts, which admit of being combined in any proportions by simple arithmetic. It matters little what limb, or bodily part, or

f. 8v

faculty is the subject of measurement, because laws which are true for one particular quality, and for one particular race of animals or plants, will presumably apply with small modifications to any other quality and race. Therefore it would be no unworthy occupation for a scientific man to devote years of labour to carefully measuring each of many parts in the photographs of offspring and their ancestry, and to discuss the results by the elaborate methods of the higher statistics.

The photographs of which I speak are assumed to have been taken under the following conditions. They would represent side views of the animals and therefore be comparable on equal terms so far as position is concerned. The animals would have been photographed at a distance of *not less* than thirty feet from the camera, in order to avoid sensible distortion of the portrait. They should be taken while standing on hard ground, that the feet may be clearly shown. The height of the camera above the ground and its distance from the animal should be roughly measured and noted. Lastly, two direct measurements of the height of the animal should be made, one at its withers, the other at its croup. The photograph now becomes more than a mere picture, because the recorded data, together with others afforded by the photograph itself, supply corrections that will cause the measurements made upon it to correspond with more or less accuracy to those made on the animal itself. Of course, their correspondence would not be so exact as it would be in photographs taken in a 'hippometric' laboratory provided with marked lines on the ground and walls, but such a laboratory is impracticable on many grounds. Thoroughbred horses are so easily frightened in unfamiliar places and at unfamiliar objects that the best plan is to photograph them leisurely among their accustomed surroundings. It is difficult and dangerous to apply tapes and calipers, which tickle and irritate, for thoroughbred horses are exceedingly sensitive, timid, fidgety, and often vicious, while they are supple and sudden in their movements of offence. Measurements of the two vertical heights, made in the usual way, are comparatively easy to manage.

I find, moreover, that vertical measurements of all kinds may be made quickly and accurately without touching the objects at all, by means of a simple instrument which I roughly put together for trial. Its principle is that of a collimator, with additions and modifications. It seems very suitable for use at agricultural and other shows where many animals are collected.

Though many useful measurements can be made on a plain photograph, it would be a decided gain to select two, three or more important osseous protuberances, which can be easily felt, and to mark their positions by sticking on the animal small wafers of sufficiently adhesive paper—say, one quarter of an inch in diameter. The corresponding marks on the photographs will be too small to attract notice, but they are easily found when looked for, and afford excellent points from which to measure. I may add that measurements I have made, and had made, both on horses and on their photographs, show that the relative dimensions of horses differ considerably. If some five different measurements were made on an adult racehorse, it would be as easy to identify him by a 'Bertillon process' as it is to identify prisoners.

The corrections that are required for measurements on photographs can be found by appropriate tables. After these have been calculated in some detail once for all, their application would be simple. The needed corrections refer (1) to slight deviations in the position of the animal from that which would have yielded an exact side view of him, (2) to the camera having been obviously tilted, (3) to different degrees of nearness of different parts of his body. It is curious in how many different ways these corrections may be determined when the range of available measures is slightly increased. I have already discussed the question for a different series of data in 'Photographic Measurements of Horses and other Animals' (*Nature*, Jan. 6, 1898), which shows the general character of the problem, but I cannot enter into details now. The primary question is, will photographers and grooms take the proposed measurements with sufficient correctness, and are any additions to them feasible? To settle this question, experiments should be concentrated by various persons upon the same quiet and well measured animals. These ought to determine the trustworthiness of the results according to the data in use, and would show the minimum of effort that is necessary to afford the required degree of accuracy. I should be content if the average error in the calculated height and length of the horse did not exceed one inch, or say one-and-a-half per cent.

It remains to consider what has hitherto been taken for granted—the best method of starting a systematic collection of photographs of pedigree stock. My proposal is to suggest to the principal Societies which publish stud or herd books, that they should proceed as follows :

(1) To arrange with a photographer to store such negatives as the Society may hand over to his charge ; he undertaking to supply prints from them to the public at a moderate cost and under reasonable regulations.

(2) To invite owners of pure-bred stock to send to the Society with which they are in connection, a negative photographic plate of each of the animals which they use for breeding, on the understanding that if the negative be accepted by the Society it will be handed over to the photographer.

(3) Only those negatives will be considered suitable for acceptance (*a*) which are of good quality, and which do not transgress specified limits of size ; (*b*) which are strictly side views ; (*c*) which have been taken at a distance from the animal of not less than 30 feet, and (*d*) which show the animal standing on hard ground.

(4) The following information is to be stamped or written on the negative in such a way as to be clearly legible in the prints, (1) the name and sex of the animal, (2) year of its birth, (3) year and month of taking the photograph, (4) heights at its withers and croup, (5) height of camera and its distance from the animal.

(5) The Society shall order an asterisk to be affixed to the name of each animal entered in its stud or herd book, when the photographic negatives of its sire and dam have been accepted.

It seems to me that a system such as this would be efficient, self-supporting and acceptable to all parties. Breeders would be pleased that photographs of their animals should be publicly recognised as serviceable for the advancement of their art. Owners of valuable animals are almost sure to order photographs of them on their own account, so the gift of the negatives to the Society would deprive them of nothing. The asterisks applied to the names of the offspring would be a valued distinction, and would help

f. 9v

to introduce the system. Later on, when they had become common, the absence of an asterisk would excite suspicion and require explanation. Lastly, the printing of the photographs would be self supporting. I have already expressed a belief that the custom would arise of printing a separate pamphlet for every important stallion or bull containing its photograph and those of its nearer ancestry, together with other appropriate information. Larger publications of a more costly kind would doubtless be issued under the auspices of each Society, to correspond with an awakened demand for fuller information on the antecedents of pedigree stock.

As regards useful additions to the printed matter in stud and herd books, I would now merely allude to the need for them, and to the propriety of carefully reconsidering how much of real utility could be asked for from breeders that they would supply willingly and truthfully. The measurements of adult animals, of which I spoke, would be appropriate entries. An accumulation even of these during two or three generations would be exceedingly valuable, considering how many coherent results in the science of heredity have been derived from observations of human stature, though limited to comparatively small numbers of parents and their offspring.

The amount of money annually spent in rearing pedigree stock is enormous; so is the care and thought bestowed upon it, and so also is its national importance. The non-preservation of adequate records of pedigree stock is a cruel waste of opportunity, and has been most prejudicial to the acquirement of a sound knowledge of the art of breeding. If the scheme I have sketched be found feasible, it will cause much to be noted that has hitherto been overlooked, and much that is commonly observed, to be placed permanently on record instead of being ill remembered and soon wholly forgotten.

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Galton Frank
on
Pedigree Stock

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1897

Duplicate

From the PROCEEDINGS OF THE ROYAL SOCIETY, VOL. 62.

AN EXAMINATION INTO THE REGISTERED SPEEDS OF AMERICAN TROTTING HORSES, WITH REMARKS ON THEIR VALUE AS HEREDITARY DATA.

BY

FRANCIS GALTON, D.C.L., F.R.S.

[*From the* PROCEEDINGS OF THE ROYAL SOCIETY, Vol. 62.]

"An Examination into the Registered Speeds of American Trotting Horses, with Remarks on their value as Hereditary Data." By FRANCIS GALTON, D.C.L., F.R.S. Received November 29,—Read December 16, 1897.

It is strange that the huge sums spent on the breeding of pedigree stock, whether of horses, cattle, or other animals, should not give rise to systematic publications of authentic records in a form suitable for scientific inquiry into the laws of heredity. An almost solitary exception to the disregard, shown by breeders and owners, of exact measurements for publication in stud books, exists in the United States with respect to the measured speed of "trotters" and "pacers" under defined conditions. The performance of 1 mile by a trotter, harnessed to a two-wheeled vehicle, carrying a weight of not less than 150 lbs. inclusive of the driver, in 2 minutes 30 seconds qualifies him for entry in the Trotting Register, giving him, as it were, a pass-degree into a class of horses whose several utmost speeds or "records" are there published. To avoid prolixity I will not speak particularly of pacers (pace = amble), since what will be said of the trotters applies in general principle to them also.

The great importance attached to high speed, and the watchfulness of competitors, have resulted in evolving a method of timing trotters which is generally accepted as authoritative. The length of the track is scrupulously measured, and numerous other conditions are attended to, that shall ensure the record being correct, with an attempted exactitude to the nearest quarter of a second. A race against time, even if exact to the nearest quarter of a second, is by no means so close a measure of the speed of a horse relatively to his competitors, as the differential method of ordinary races. The speed of 1 mile in 2′ 30″, or of 1760 yards in 150 seconds, is equivalent to about 12 yards in 1 second. Now, the length of a horse when extended at full trot is half as long again as his height at the withers—as I gather from the instantaneous photographs of Muybridge—and consequently is hardly ever as much as

3 yards. Therefore at a 2′ 30″ speed a horse travels through his whole length in a quarter of a second. In an ordinary English race a winner by half a length gains a notable victory, while a neck or even a head in advance is sufficient to establish his priority. Therefore the record of the speed of a horse to the nearest quarter of a second is by no means an absurd refinement. It is, of course, very difficult under the exciting circumstances of a race to measure time with such precision as that. I tested the value of these entries as follows:—If quarter seconds were noted with exactness the entries of 0, $\frac{1}{4}$, $\frac{1}{2}$, and $\frac{3}{4}$ would be approximately equal in number; they would also be equal if they were set down at random without bias, but if there be a bias towards favourite numbers its effects would be apparent. I extracted a few hundred entries, and found the relative frequency of the 0, $\frac{1}{4}$, $\frac{1}{2}$, and $\frac{3}{4}$ to be almost exactly as 1, 3, 2, and 1. Consequently the $\frac{1}{4}$ is on the average three times as great a favourite as either the 0 or the $\frac{3}{4}$, and the $\frac{1}{2}$ is twice as much a favourite as they are. It is evident that the $\frac{1}{4}$ seconds are not strictly trustworthy, but it may well be urged that their entry is preferable to their total disregard.

I was informed that a trifling laxity was tolerated when a horse had just but only just failed to qualify, an allowance of $\frac{1}{4}$ of a second in his favour being commonly made. So that a speed of 2′ 30$\frac{1}{4}$″ would usually be reckoned as 2′ 30″. I shall return to this point further on.

The system of timing and of registering records began more than fifty years ago, and was developed and improved by degrees. In 1892 a considerable change was made in the conditions by the introduction of bicycle wheels with pneumatic tyres, which produced a gain of speed, the amount of which is much discussed, but which a prevalent opinion rates at 5 seconds in the mile. Thenceforward the records are comparable on nearly equal terms. All trotting performances up to the 2′ 30″ standard are registered in the large and closely printed volumes of 'Wallace's Year Book,' published under the authority of the American Trotting Association. Vols. 8—12 refer to the years 1892-6, and it is from the entries in these that the following remarks are based.

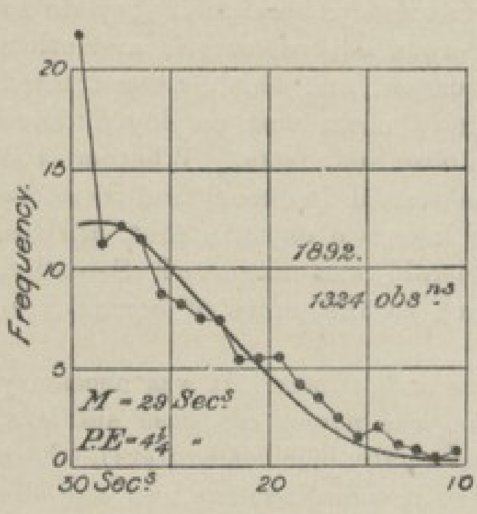
The object of my inquiry was to test the suitability of these trotting (and pacing) records for investigations into the laws of heredity. Their trustworthiness was of course one point to be ascertained, another was to obtain a just notion of the proper principle on which marks for speed should be awarded, as, for instance, in the following example:—Suppose a particular ancestor, whom we will call A, of a certain horse has a record of 2′ 30″, and that another ancestor in the same degree, whom we will call B, has a record of 2′ 10″, how are their joint influences to be estimated? Will it be the

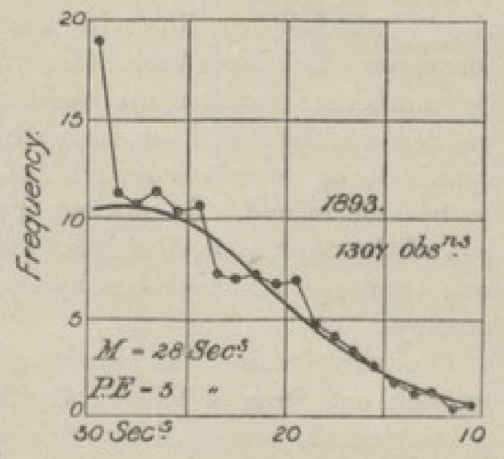
f. 3r

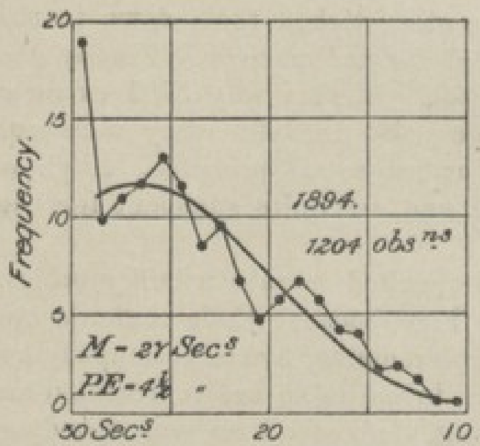
same on the average as that of two horses each having the speed of 2′ 20″, or will it be something altogether different? In short, is the arithmetical the most appropriate mean or not? It would be a strong presumption in the affirmative, if the relative frequency of the various speeds should correspond approximately with those determined by the normal law of frequency, because if they do so they would fall into line with numerous anthropometric and other measures which have been often discussed, and which, when treated by methods in which the arithmetic mean was employed, have yielded results that accord with observed facts. Whether the speeds do or do not occur with the normal frequency had therefore to be ascertained. So my inquiry had two objects: first, did the run of the observations suggest a tolerably smooth curve? Secondly, was that curve a tolerable approach to the curve of normal frequency?

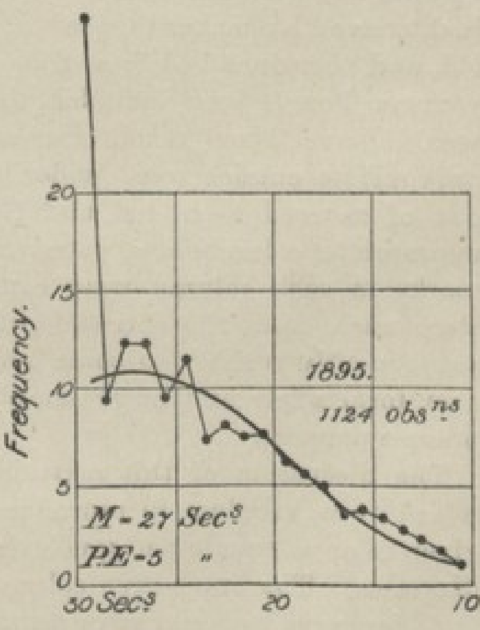
The investigation was troublesome and tedious. It was necessary to pick out from a large collection the names of those stallions, geldings, and mares (all three being equally efficient trotters), whose records had been made in the year under consideration, and who also had arrived at maturity, that is, who were not less than five years old, and therefore had had time to show their full powers. Had younger horses been included, the frequency of the slower records would have been much increased. Assisted by a friend, the appropriate entries were underlined in the printed volumes, then one of us read them out, and the other ticked them down in the appropriate column of a page ruled for the purpose. Finally the marks in each column were counted. In this way 5705 extracts were made from the entries for the years 1892–96; they were not subsequently verified, so some few omissions are probable. Anyhow they form a fair and large sample, and are quite sufficient for the present purpose.

The discussion of this material resulted in rather bulky tables, which it is needless to reproduce here, because their contents are given in an adequate and much simpler manner by the accompanying diagrams. The successive columns in the table are represented in the diagrams by imaginary columns that stand on corresponding bases. They run as follows:—The first column, counting from the left, contains the percentage value of all observations recorded as 2′ 29·0″, 29¼″, 29½″, or 29¾″; that is of all under 30 down to 29 inclusive (the minutes being here omitted for brevity). The second column referred to 28·0″, 28¼″, 28½″, and 28¾″, and so on with the rest. Consequently the dot in the diagram which indicates the percentage number of observations, according to the side scale, stands in the middle of its own imaginary column. For example, that of the 2′ 28″ set stands vertically above the point that lies half way between 28 and 29 on the scale along the base. The dots are connected by thin lines to show the trace or curve of the observations. The smooth curves are those of normal frequency, calculated from the values of the mean (M) and of the probable error (P.E.), which are given in the diagrams.



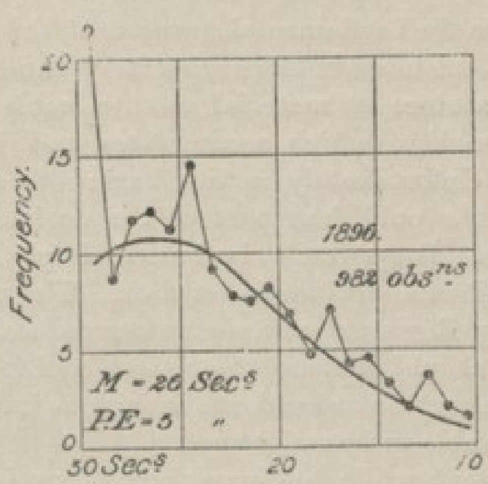






Leaving aside for the moment the strange pinnacle that rises on the extreme left of every diagram, we see that the traces of the observations run very roughly, but not intolerably so. In each diagram they seem to be disposed about a fundamentally smooth curve. Considering the smallness of the interval, namely, only 1 second, that separates the observations assigned to each pair of successive columns, together with the experience derived from other kinds of statistical curves, it seems to me that the run of the obser-

f. 45



vations is good enough to certify their general trustworthiness. As regards the pinnacle it is a different matter, and is one which when beginning work, as I did, on the 1892 entries only, was very perplexing. However, by persevering with the other years it became increasingly plain that the pinnacle was a false maximum; in 1896 it was certain that the true maximum lay well within the portion of the curve included in the diagram. The explanation of the pinnacle then became obvious; it was that the tolerance granted to those horses who failed by only a little to qualify themselves, was extended considerably beyond the quarter second for which I was prepared.* The cases of 2′ 30·0″ were few; they do not appear in the diagram, but their addition would be quite insufficient to remove the difficulty. If the pinnacle were distributed among *two* adjacent columns outside and to the left of the diagram it would smooth away the incongruity, so I suspect that cases of "under 2′ 32″ and down to 2′ 30″" are habitually rated at a trifle less than 2′ 30″. Consequently I had no hesitation in wholly disregarding the entries that helped to make the pinnacle, namely, the whole of those contained in the first column to the left in every one of the diagrams. The course thereupon became clear and straightforward. When fixing upon the mean for each year, I was somewhat biassed by the entries in the adjacent years; similarly as to the probable error. Now that the curves are drawn I see that somewhat better fits might have been made, but they are close enough to show the existence of a fair amount of correspondence between the observed values and those calculated according to the law of normal frequency. It is near enough to remove hesitation in working with the arithmetic mean.

* [*Jan.* 20.—I have since learnt that the conditions of timing are too rigorous to justify this inference; also that the very numerous efforts simply to secure a standard record, and thenceforward to cease training, may be a chief cause of the pinnacle.]

I now come to the fundamental purpose of this memoir, which is to point out the existence in the registers of the American Trotting Association, of a store of material most valuable to inquirers into the laws of heredity, which accumulates and increases in value year by year. Unfortunately it lies buried to a hopeless depth, partly because the published part of the registers refers only to standard trotters. It appears to be buried simply through the omission of having its importance insisted on. The published volumes of the 'Trotting Register' contain numerous elaborate tables, but lacks one that should include the names and pedigrees of those horses concerning whose antecedents enough is known to make their pedigrees serviceable to investigators.

It is hardly worth while to discuss hereditary influence on speed, in the case of any horses, unless the records of at least their sires and of their dams, and those of each of their four grandparents, as well as their own record, are all known. Even in this case (according, at least, to my own theory) one quarter of the hereditary influences are unknown and have to be inferred. It is practically impossible to make an adequate collection of the names of horses who fulfil the above conditions out of the entries in the 'Trotting Register,' each search requiring many cross references and occupying a long time, while the number of futile searches before attaining a success is great. On the other hand, the breeders and possessors of these notably bred horses must be familiar with the required facts, and would assuredly be delighted to have them known. There need, therefore, be little difficulty in obtaining materials for the much desired table. In the meantime I am sending circulars to the chief breeders in America in hopes of making a start.

The great need for genealogical data of an exact numerical kind, by those who prosecute inquiries into the laws of heredity, is the justification that I offer for submitting these remarks to the Royal Society.

HARRISON AND SONS, Printers in Ordinary to Her Majesty, St. Martin's Lane.

f.1

SUGGESTIONS FOR IMPROVING THE LITERARY SYTLE OF SCIENTIFIC MEMOIRS.

BY
FRANCIS GALTON,
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SUGGESTIONS FOR IMPROVING THE LITERARY STYLE OF SCIENTIFIC MEMOIRS.

BY FRANCIS GALTON, D.C.L., HON. SC.D.CAMBRIDGE, F.R.S.

[Read April 29th, 1908.]

THE memoirs published by scientific societies are blamed with justice for being more difficult of comprehension than need be, owing to a want of simplicity in their language, of clearness of expression, and of logical arrangement. Forcible remarks in this sense were publicly made, by more than one person, at and about the time of the last Anniversary Meeting of the Royal Society. This opinion had also been held by myself for many past years, during which I have chafed at the impediment caused by rugged and careless writing to my honest endeavour to keep abreast with the advances of modern science. Success in this, under the most favourable conditions, and in only one branch of science, would occupy the spare energies of most men. It is a cruel addition to their labours that the information they need should be contained in crabbedly written memoirs.

It has been my lot to serve on the councils of many scientific societies, and to have had more MSS "referred" to me than I could now enumerate. My experience is that an undue proportion of them had to be read more than once, and to be puzzled over in parts, before it was possible to justly comprehend what their authors had in their minds to say.

It must not be imagined for a moment that I pose as a literary critic. I am far too sensible of my own grave deficiencies to assume that position. But a man need not be a cobbler in order to know when his shoe pinches. My standpoint is merely that I find many scientific memoirs difficult to understand, owing to the bad style in which they are written, and that I am conscious of a rare relief when one of an opposite quality comes to my hand.

Having become a Fellow of the Royal Society of Literature through the invitation of the Council, I seize the opportunity of asking its powerful help in considering methods by which this grave defect may be lessened. To this end, I will proffer some suggestions of my own, which I hope will be well discussed, and may induce others to assist in this crusade. If useful conclusions should be reached, it would be open to Fellows of scientific societies to press for reforms, under the consciousness that the proposed methods for obtaining them had been carefully considered, and were not simply the crude offspring of their individual brains. I ask for nothing that lies outside of the purview of the Royal Society of Literature. It is not proposed by me that the Society in its corporate capacity should thrust advice upon the scientific societies, who might resent interference, but merely that it should discuss certain general principles, leaving action upon them to other hands, in the way just described.

I now proceed to speak of some of the literary defects, other than bad grammar and faulty syntax, that make scientific memoirs difficult to understand. One of the most prominent is a superfluous use of technical expressions that have not yet become naturalised among scientific men. It is impossible to avoid the use of technical words, but their number should be minimised. It is especially needful to do so in the opening paragraphs of a memoir, whose function is to explain the object of the writer in the plainest possible language. If it be necessary to use unfamiliar technical words, their meaning ought to be defined in a foot-note. The opening paragraphs of a memoir should be intelligible to any man who is conversant not only with the branch of science to which it belongs, but to allied branches also. A similar remark applies to the concluding paragraphs, in which the author summarises his results. The intending reader will then be able to judge for himself whether or no the memoir falls within his own province and merits his further study. Owing to a want of care in writing the opening paragraphs, it has not infrequently occurred to myself, and doubtless to others, to have been perplexed about the exact purpose of a paper until it has been half read through.

Some *veto* is desirable before a Society gives its "imprimatur" to newly coined words, for many of them fail to express their meaning, and very many are unnecessarily cumbrous. The way in which the *veto* might be applied will be explained later on, I now am merely calling attention to its need. To take one example of bad nomenclature, the contrasted terminations of the two Mendelian words "domi-

nant" and "reces*sive*" imply a distinction which does not exist. Re*cedent* would have been unobjectionable on that ground.

The nomenclature of modern chemistry seems preposterous to outsiders, even after making liberal allowance for inherent difficulties. I copy one of these chemical words from a paper now lying on my table, it is "Dimethylbutanetricarboxylate," and is not the longest that might have been adduced. But it suffices for an example. It is of course understood that these are what have been termed "portmanteau" words, in which a great deal of meaning is packed, but they are overlarge even for portmanteaux; they might more justly be likened to Saratoga trunks, or to furniture vans. It is with the greatest diffidence that I suggest that a single letter might sometimes suffice to show what is now delegated to one or two syllables; if so, the word would be shortened in proportion. In certain barbarian languages this is a familiar process.

Long English words and circuitous expressions are a nuisance to readers, and convey the idea that the writer had not that firm grasp of his subject which every one ought to have before he takes up his pen. Clear views are naturally expressed in brief and incisive language. The power of the English tongue when limited to the use of words of one or two syllables is remarkably great. Excellent instances of this are to be found in the writings of Tennyson. I will quote some marvellously graphic descriptions from his *Palace of Art*, which refer to certain well-known pictures, and are written under the above limitations.

"One showed an iron coast and angry waves,
You seemed to hear them rise and fall,
And roar rock-thwarted in their bellowing caves—
Beneath the windy wall.
And one, a full-fed river winding slow,
By herds upon an endless plain,
The ragged rims of thunder brooding low,
And shadow streaks of rain."

There are about twenty gems like this in the *Palace of Art*.

The to-and-fro arguments in the *Two Voices* are equally concentrated and forcible.

"The memory of the withered leaf
In endless time is scarce more brief
Than of the garnered autumn sheaf.
Go vexed spirit, sleep in trust;
The right ear that is filled with dust
Hears little of the false or just."

Or again—

"Yea, said the voice, thy dream was good,
While thou abodest in the bud,
It was the stirring of the blood.
If Nature put not forth her power,
About the opening of the flower,
Who is it that could live an hour?
Then comes the check, the change, the fall,
Pain rises up, old pleasures pall,
There is one remedy for all."

The comparative rarity among the English of a keen sense of the difference between good and bad literary style is a great obstacle to the reform I desire. It is especially noticeable among the younger scientific men, whose education has been over-specialised and little concerned with the "Humanities." The literary sense is far more developed in France, where a slovenly paper ranks with a disorderly dress, as a sign of low breeding.

I have had occasion to read many memoirs in manuscript, on subjects where I was fairly at home, in which there was nothing especially recondite, but the expressions used in them were so obscure, the grammar so bad, and the arrangement so faulty, that they were scarcely intelligible on a first reading; nevertheless the writers could hardly be made to perceive their shortcomings. I have heard equally bad reports relating to essays sent by candidates for Fellowships at Colleges in one at least of our Universities. The writers of them may have been, and probably were, successful investigators, but their powers of literary exposition were of a sadly low order; so low that they could hardly be made to realise their deficiencies. The preliminary culture of students in science, seems usually to have been very imperfect.

Sufficient has now been said as to the need of reform and of the difficulties to be overcome in affecting it. It becomes our next duty to consider the steps that should be taken towards that end. The power of reform lies largely in the hands of the councils of the scientific societies, who can withhold the publication of memoirs presented to them, or accept the memoirs under such limitations as they please. A Society gives much, consequently the Council who represents it has a right to exact much in return. The Society supplies a stage from which a writer can disseminate his views, and have them subjected to the criticism of experts. It defrays the cost of publication of the memoirs, and, under occasional circumstances, that of preparing expensive plates. Therefore the Society, or its Council on its behalf, may fairly demand that the memoirs should be written in a style that is creditable to their journals; that they should be lucid, logical, and as easy for its members (who pay for the publication) to understand as the nature of the subject permits. I suggest that Councils should require a report on the literary sufficiency of every proffered memoir, before discussing whether it should be accepted for publication. It is hardly necessary to bring to remembrance that it is the universal practice of Councils of Scientific Societies to "refer" every memoir that is submitted to them. One, two, or more referees are selected among those of their Fellows who are able to give a trustworthy opinion on the merits of the paper. The referees are each supplied with a schedule on which numerous searching questions are printed, which they are requested to answer confidentially. Their reports are read to the Council, which then proceeds to discuss the question whether or no the memoir should be published as it stands, or subject to some restriction, or be rejected altogether. What I now suggest is that the printed reference paper should include questions as to the literary suitability of the memoir. They might be such as—"Do you consider the memoir to be (1) clearly expressed, (2) free from superfluous technical words, (3) orderly in arrangement, (4) of appropriate length. (5) State whether any new terms are used in the memoir, mention what they are and whether you consider them appropriate. (6) Add such general remarks on its literary style as you think would be useful to the Council when considering its publication."

I do not presume to anticipate what action a Council might take if the answers to these questions were more or less unfavourable, as much would depend on other considerations. What I want is that the members of the Council should not be left in the dark, as they usually now are, on one important element of goodness or badness in the memoir, before they consider the question of its publication. Also that they should appreciate the widely felt desire for literary reform.

There is yet another way in which scientific societies might be made to realise the occurrence of literary faults in the memoirs that they publish, namely, by occasional articles containing a selection of passages that are conspicuous for shortcomings.

I now crave your opinions on these suggestions, and hope that you will be able to offer other recommendations that may help in accomplishing the very important object in view; namely, that of improving the literary style of future Memoirs published by Scientific Societies.

DISCUSSION.

Sir Edward Brabrook.—I have pleasure in supporting the proposal of Mr. Francis Galton. I have had some experience, far less of course than his, as a referee of scientific MSS, and it fully accords with his. I associate myself, therefore, with his observations as to the *rôle* the Royal Society of Literature should take up in this matter. It is within the rightful functions of the Society to take note of words that are not yet dictionary words, and see to their proper applications, but to do so would be a difficult matter. As Mr. Galton says, the chemists are greatly addicted to coining long words. The report of the Leicester meeting of the British Association just issued gives us a portmanteau word of thirty-five letters—"chloroketodimethyltetrahydrobenzene"—and I have seen some worse than that. That, however, is not the main point. The use of difficult technical language cannot be avoided. What is wanted is to urge the authors of papers to write good English; many of them sadly fail in this respect. Mr. Galton's suggestion as to the addition of a question to the referee paper is excellent. I think it would be quite the right thing for the Council to send a copy of his paper to the various scientific societies, and recommend that suggestion to them for adoption. I agree with the view expressed by a committee of the British Association, which might indeed itself have been put into better English, "that the opportunity furnished by the necessity for writing an account of what a student has done and seen in his laboratory work ought to be utilised in relation to the teaching of English composition."

Sir Archibald Geikie.—The complaints so forcibly and temperately urged by Mr. Galton in the paper to which we have listened will awaken much sympathy, not only in the general public, but among a large number of men of science. I do not appear here with a brief in defence of the scientific societies, though I think that some strong pleas might be pressed in their favour. Looking at the question, however, as a matter affecting the English language and literature, I am bound to confess that the strictures contained in the paper are by no means without foundation.

It seems to me that no candid reader can compare the scientific memoirs published at the present day with those which appeared a hundred years ago, without coming to the conclusion that, in average literary quality, the modern writings stand decidedly on a lower level than their predecessors, and that the deterioration in this respect is on the increase. The earlier papers were for the most part conceived in a broader spirit, arranged more logically, and expressed in a better style than those of to-day. They show their authors to have been generally men of culture, who would have shrunk with horror from the slipshod language which is now so prevalent.

If it be asked what reason can be assigned for this change, various causes may be suggested. In former days, when life was less strenuous than it has now become, the number of men of science was comparatively small, and they belonged in no small measure to the leisured classes of the community. They were not constantly haunted by the fear of losing their claims to priority of discovery, if they did not at once publish what they had discovered. They were content to wait, sometimes for years, before committing their papers to the press. And no doubt the printing of their papers was likewise a leisurely process, during which ample opportunity was afforded for correction and improvement.

But this quiet, old-fashioned procedure has been hustled out of existence by the more impatient habits and requirements of the present day. The struggle for priority is almost as keen as the struggle for existence. As soon as a new observation is believed to have been made, the happy author of it too often dashes off a paper, in more or less legible manuscript, and forwards it without delay to some scientific society or journal for publication. In such hurried contributions attention to literary considerations finds little or no place.

Besides this too common haste in production, another and more serious cause for the defects of which Mr. Galton complains is to be found in the continually augmenting specialisation of science. Advance in every department of inquiry leads into more and more detailed studies. It becomes increasingly difficult, even for men whose lives are devoted to the pursuit of science, to keep in touch with the progress of more than one province of investigation, or even one section of a province. Details thus come to acquire, in the eyes of many earnest and enthusiastic workers, an interest and importance at least as great as can belong to the broad deductions or principles up to which they lead. These authors in their paternal fondness for the details which they have patiently and toilsomely elaborated, often crowd them into their papers, which consequently look sometimes more like leaves torn out of field note-books or laboratory journals than reasoned presentations of the results of research. It would probably be found that, as a rule, such excessive exposition of the details of the several steps in an inquiry is as unnecessary from the scientific point of view, as it is repellent from the literary side.

Closely connected with this specialisation and augmentation of detail is the increase in the number of new technical terms with which the papers in every department of science now bristle. The multiplication of such terms is admittedly a necessary accompaniment of the development of scientific research. It is obvious that each new fact brought to light in the investigation of nature should be precisely defined by some word or phrase having a definite, unambiguous signification, and preferably capable of being adopted with but slight modification into any modern language. The plea that the vernacular tongue should, where possible, be employed for this purpose is met with the objection that the language of science ought, as far as possible, to be cosmopolitan, and that those terms are most suitable which can be most easily adapted into the vocabularies of other countries. Hence the preference for coining new compounds from Greek and Latin. Lovers of the purity of the English language and the dignity of English literature may not unnaturally be grieved to see such a flood of novel and often, it must be confessed, uncouth words coming into use at a rate with which the most industrious lexicographers cannot keep pace. But the flood is inevitable, and must increase in volume, nor is its gathering strength to be stemmed by any protest. All that, perhaps, may be reasonably insisted upon is that each new term shall be absolutely necessary, shall not be unduly cacophonous, and shall not be compounded from more than one language nor framed in defiance of the grammar of the tongue, whether living or dead, from which it is borrowed.

Many men of science share Mr. Galton's regret that it is becoming more and more difficult or even impossible to follow with full intelligence and sympathy the advances made in departments of investigation with which one is not personally in touch. The difficulty is probably inseparable from the rapidity of the increase of knowledge in all domains of nature. But there can be little doubt that it is in no small degree aggravated by the multiplication of technical terms which do not always explain themselves, and for which no explanation is afforded in the papers where they are so rampant. It is becoming every year a more accepted practice that in writing a scientific paper an author has only to consider the fraternity of his own branch of science. If his colleagues understand him, it does not matter whether or not he is comprehended outside their circle. He forgets the interests not only of the general public but also of his fellow-labourers in other fields of research, many of whom would gladly keep themselves informed of the progress of inquiry in departments lying beyond their own special purview, but who are, in too many instances, deterred by the formidable terminological barriers that must first be surmounted. The growing isolation of scientific workers within their own fields of investigation is an evil which may, perhaps, be inevitable, but which, undoubtedly, is much to be deplored. Anything which can be done to lessen it is worthy of the most serious consideration. Since the language of the biologists is becoming increasingly unintelligible to the physicists, and that of the physicists not less so to the biologists, Mr. Galton's suggestion might be usefully adopted, that where necessary or desirable a scientific paper should include a brief summary of its general purport expressed in simple untechnical language. Such a concession to the ignorance of the general reader would probably be welcomed by a large body of scientific men.

It must not be supposed that scientific societies are wholly blind to the evils which have been pointed out in the interesting paper that has been read this afternoon. They are by no means negligent as to the form and style of the papers submitted to them. On the contrary, they have an elaborate system of committees and referees acting under the jurisdiction of the Councils, and no paper is sanctioned for publication without having been subjected to this process of examination. Moreover, the secretaries or assistant secretaries are usually vested with editorial powers, which are exercised as an additional control over the production of the papers. If the original condition of some contributions were compared with their ultimate published form, it would be seen how much care has been bestowed upon their improvement. In more than one learned society attention has recently been called from the Presidential chair to the defective form in which papers are too frequently presented. We must hope that from these and other efforts towards amelioration some good will follow. While in the publications of a scientific society literary excellence will always be subordinated to scientific merit, there is surely no reason why the two qualities should not be more generally combined than they at present are. Such a combination will, perhaps, be most likely to be effected when the writers of scientific papers come to realise that it will be in their own interest, as well as in that of their scientific brethren at large, and still more of the outside public, to present such a summary of their work as may be intelligible, and even interesting, to any ordinary cultivated reader.

Mr. Crackanthorpe, K.C. (who was invited to speak by the chairman), said the most interesting remark he had to make was in regard to the health of the author of the paper just read by Mr. Pember. He had seen Mr. Galton that day, and had found him quite cheerful, but confined to his room. There was reason to believe that he would very soon be completely his old self, and able to resume the beneficent work to which he had devoted most of the years of his life. (Applause.)

The first point made in Mr. Galton's paper was that a scientific memoir should be "simple in its language, clear in its expression, and logical in its arrangement." These were virtues which every prose composition should possess, whether written or spoken. They should be aimed at alike by the man of science and the layman; by the learned and the unlearned; by the leader-writer in the daily press; and the orator on the platform. Schopenhauer had pointed out that the first requisite for the art of writing was to have something to say; and the second, to have clearly thought out the subject in hand. Then, what was called "literary style" would come of itself. There was an old French saying—"the style was the man." At all events, it was, or ought to be, an expression of the natural mood of the man at the moment of his writing.

Mr. Galton's next point was that a scientific memoir should not use unfamiliar technical words without explaining them in a foot-note, nor more of such words than was absolutely necessary. He (Mr. Crackanthorpe) agreed, although he thought the first of these cautions was rather vague. It might be asked, Unfamiliar to whom? There were, for instance, many technical words which were unfamiliar to him (the speaker), but no doubt quite familiar to Mr. Galton. Where was the line to be drawn? One would hardly expect to find in a scientific work a glossary of terms such as an Englishman looked for in a collection of Burns' Poems. Every scientific writer was surely entitled to assume that his reader had some technical knowledge—otherwise his explanations would be endless. At the same time, if an explanation were given, care should be taken to make it adequate. He would illustrate what he meant by an example. Anyone taking up one of the numerous books on Heredity, now appearing in the British and German markets, would come across the word "chromosome." He met the other day with this word in a very valuable treatise just published, "with stainable body" added by way of explanation. Was this adequate? The white tablecloth, now in that room, was a "stainable body" (in the mechanical sense); and so were a hundred other everyday things. If any explanation was wanted, should not the reader have been told, either in a foot-note or an appendix, how colouring matter served to detect the presence of minute particles of matter otherwise invisible even to the microscope-aided eye? Then, the explanation would have been alive.

He might mention by the way, that this same word "chromosome" violated one of the canons laid down in the paper. It was, like the "recessive" of the Mendelians, an instance of "bad nomenclature," because it was wrongly formed. The word should, in strictness, not have been "chromosome," but "chromatosome," since the Greek for "colour" was not *chromos* but *chroma*.

As to the second of Mr. Galton's cautions, viz. against the use of more technical words than necessary, he would illustrate the point by reference to the "idants" and "ids" of Weismann. It appeared that the nucleated masses into which a dividing cell broke up consisted of several parts. To these Weismann gave the names of "idants"; and since "idants" were theoretically decomposable into particles more minute, he gave to these last the name of "ids." One wondered why he stopped there. He should have gone on to subdivide his "ids" into "i's," and these again into mere dots, giving to each a technical name, thus recalling the old lines:

> "Big fleas have little fleas upon their backs to bite 'em,
> And these again have lesser fleas, and so *ad infinitum*."

(Laughter.)

In this connection he desired entirely to associate himself with what he understood to fall from Sir Archibald Geikie, and to protest against the employment of incomprehensible terms to indicate things the existence of which was incapable of scientific proof.

Mr. Galton had, at the end of his paper, suggested that the shortcomings of the writers of scientific memoirs might now and then be published as a warning to others. He (Mr. Crackanthorpe) could not help thinking that this would be rather hard measure, even though no names were mentioned. He was quite sure that Mr. Galton himself, who was one of the most kind-hearted of men, would never lend himself to any such action. Would not his object be attained if the faulty memoir were returned to its author for revision, and this were, if necessary, repeated again and again until a flawless edition was reached? Then, when the memoir came to be published by the learned society to which it was presented, there would be nothing to offend the most fastidious ear.

Mr. E. H. Pember, K.C.—He sympathised fully with the motives which had prompted Mr. Galton's very suggestive paper. But he doubted whether any drastic steps could be taken to bring about an improvement which everybody must desire. Indeed, what was asked for amounted to little less than a wide distribution of something approaching to literary genius among the writers of scientific papers. This might be encouraged, but it could not be compelled. It would be impossible to establish a direct literary censorship over productions which might be extremely valuable though extremely ill-written. The writers would resent it, and the discouragement, still more the rejection, of important communications, would be too high a price to pay even for the luxury of a fine style. Indirect encouragement of good composition would be preferable to penalties upon bad. It was the desire, he hoped he might say that it was the intention, of the Royal Society of Literature, by putting itself into communication with educational centres throughout the kingdom, and possibly by other methods, to do something substantial in that direction. It was too true that the present standard of prose style was somewhat decadent. When one compared the twentieth with the eighteenth century, the condition of our own epoch left much to be desired. To mention only a very few names, Hume in History, Blackwood in Law, Bishop Berkeley and Sir Thomas Browne in Philosophy, were all living proofs of the truth that profundity in thought and exactness in exposition were not only consistent with, but enhanced by, a clear and elegant style. In the nineteenth century Huxley, Darwin, Mill, and Macaulay were all examples of the same healthy combination. He expressed an opinion that the banishment of the classical languages from general education was one source of the evil, and he trusted that something might be done not only to retain, but to extend, the study of them. Meanwhile, towards the end desired, suasion, and not an aggressive censorship, must be acknowledged to be the working means.

Mr. Percy W. Ames, Secretary.—Mr. Galton has added one more to his many public services by calling attention to the need of improved literary form in the papers in which scientific discoveries are presented to the world. The practical suggestions he has made would, if adopted, make a general and considerable step in this direction, and immediately secure one desirable object. It is important that the Councils of the various societies should be informed whether the papers submitted for publication are clearly expressed, and so have the opportunity of rejecting or referring back those that are deficient in this respect, but unless a competent committee undertakes the laborious task of literary correction, in some cases practically re-writing the memoir, such rejection may result occasionally in the loss of valuable contributions. Sir Archibald Geikie has told us that in the Royal Society this report and correction are provided for. Mr. Galton has invited discussion on ways and means for securing a better literary style for such memoirs in the future, and has referred to the necessity for more adequate preliminary training, and on this point I venture to make an observation. It would not be practicable to require students of science to follow the best plan for acquiring a good style of composition, namely, to obtain a first-hand acquaintance with the classics of English literature, though such labour would bring its own reward. Time is short, the practical interrogation of Nature is absorbing; we must not expect investigators of physical phenomena to turn aside into the "quiet and still air," as Milton called it, of literary study, however delightful, and it is not necessary. The object is not to seek the elegance of an Addison or a Ruskin, still less the art of the poet, though something might be said in favour of imitating the attractive ease and simplicity of Charles Lamb, De Quincey, and Thackeray. The remedy I suggest as effective is not so foreign to the main purpose of the life-work of a man of science as the study of general English literature would be. It is simply to give more time and attention to the specific study of scientific method. Too often it is the case that the author of a badly written memoir is the "calculator of distances, or analyser of compounds, or labeller of species," and nothing more. Herbert Spencer claimed for the study of science that it exercises the memory with understanding, cultivates the judgment, continually appeals to individual reason, develops independence of character, requires perseverance and self-renunciation, contributes sincerity, and gives moral, intellectual, and religious culture.

All this is more than is wanted for the purpose in hand; but that exactness of statement and that simplicity of expression, which are desired, arise from clearness of thought and an orderly habit of mind, qualities which are developed by fidelity to the principles of scientific method. That these should be thoroughly understood by everyone engaged in scientific research will not be disputed, and they are best mastered by coming into close touch with the most eminent teachers through the works in which they have applied them. It should, I think, be made compulsory for every scientific student, irrespective of his specialty, to master one or more of the works of Darwin, Huxley, Tyndall, and Herbert Spencer. The discipline so afforded would soon reveal itself in more systematic thinking and in greater precision of expression.

Mr. Emanuel Green, who presided in the unavoidable absence of the Earl of Halsbury, expressed the thanks of the meeting to Mr. Galton for his paper, and to Mr. Pember for reading it.

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f. 125

SUGGESTIONS FOR IMPROVING THE LITERARY STYLE OF SCIENTIFIC MEMOIRS.

BY FRANCIS GALTON, D.C.L., HON. SC.D.CAMBRIDGE, F.R.S.

[Read April 29th, 1908.]

THE memoirs published by scientific societies are blamed with justice for being more difficult of comprehension than need be, owing to a want of simplicity in their language, of clearness of expression, and of logical arrangement. Forcible remarks in this sense were publicly made, by more than one person, at and about the time of the last Anniversary Meeting of the Royal Society. This opinion had also been held by myself for many past years, during which I have chafed at the impediment caused by rugged and careless writing to my honest endeavour to keep abreast with the advances of modern science. Success in this, under the most favourable conditions, and in only one branch of science, would occupy the spare energies of most men. It is a cruel addition to their labours that the information they need should be contained in crabbedly written memoirs.

It has been my lot to serve on the councils of many scientific societies, and to have had more MSS "referred" to me than I could now enumerate. My experience is that an undue proportion of them had to be read more than once, and to be puzzled over in parts, before it was possible to justly comprehend what their authors had in their minds to say.

It must not be imagined for a moment that I pose as a literary critic. I am far too sensible of my own grave deficiencies to assume that position. But a man need not be a cobbler in order to know when his shoe pinches. My standpoint is merely that I find many scientific memoirs difficult to understand, owing to the bad style in which they are written, and that I am conscious of a rare relief when one of an opposite quality comes to my hand.

Having become a Fellow of the Royal Society of Literature through the invitation of the Council, I seize the opportunity of asking its powerful help in considering methods by which this grave defect may be lessened. To this end, I will proffer some suggestions of my own, which I hope will be well discussed, and may induce others to assist in this crusade. If useful conclusions should be reached, it would be open to Fellows of scientific societies to press for reforms, under the consciousness that the proposed methods for obtaining them had been carefully considered, and were not simply the crude offspring of their individual brains. I ask for nothing that lies outside of the purview of the Royal Society of Literature. It is not proposed by me that the Society in its corporate capacity should thrust advice upon the scientific societies, who might resent interference, but merely that it should discuss certain general principles, leaving action upon them to other hands, in the way just described.

I now proceed to speak of some of the literary defects, other than bad grammar and faulty syntax, that make scientific memoirs difficult to understand. One of the most prominent is a superfluous use of technical expressions that have not yet become naturalised among scientific men. It is impossible to avoid the use of technical words, but their number should be minimised. It is especially needful to do so in the opening paragraphs of a memoir, whose function is to explain the object of the writer in the plainest possible language. If it be necessary to use unfamiliar technical words, their meaning ought to be defined in a foot-note. The opening paragraphs of a memoir should be intelligible to any man who is conversant not only with the branch of science to which it belongs, but to allied branches also. A similar remark applies to the concluding paragraphs, in which the author summarises his results. The intending reader will then be able to judge for himself whether or no the memoir falls within his own province and merits his further study. Owing to a want of care in writing the opening paragraphs, it has not infrequently occurred to myself, and doubtless to others, to have been perplexed about the exact purpose of a paper until it has been half read through.

Some *veto* is desirable before a Society gives its "imprimatur" to newly coined words, for many of them fail to express their meaning, and very many are unnecessarily cumbrous. The way in which the *veto* might be applied will be explained later on, I now am merely calling attention to its need. To take one example of bad nomenclature, the contrasted terminations of the two Mendelian words "domi-

nant" and "reces*sive*" imply a distinction which does not exist. R*ecedent* would have been unobjectionable on that ground.

The nomenclature of modern chemistry seems preposterous to outsiders, even after making liberal allowance for inherent difficulties. I copy one of these chemical words from a paper now lying on my table, it is "Dimethylbutanetricarboxylate," and is not the longest that might have been adduced. But it suffices for an example. It is of course understood that these are what have been termed "portmanteau" words, in which a great deal of meaning is packed, but they are overlarge even for portmanteaux; they might more justly be likened to Saratoga trunks, or to furniture vans. It is with the greatest diffidence that I suggest that a single letter might sometimes suffice to show what is now delegated to one or two syllables; if so, the word would be shortened in proportion. In certain barbarian languages this is a familiar process.

Long English words and circuitous expressions are a nuisance to readers, and convey the idea that the writer had not that firm grasp of his subject which every one ought to have before he takes up his pen. Clear views are naturally expressed in brief and incisive language. The power of the English tongue when limited to the use of words of one or two syllables is remarkably great. Excellent instances of this are to be found in the writings of Tennyson. I will quote some marvellously graphic descriptions from his *Palace of Art*, which refer to certain well-known pictures, and are written under the above limitations.

"One showed an iron coast and angry waves,
You seemed to hear them rise and fall,
And roar rock-thwarted in their bellowing caves—
Beneath the windy wall.
And one, a full-fed river winding slow,
By herds upon an endless plain,
The ragged rims of thunder brooding low,
And shadow streaks of rain."

There are about twenty gems like this in the *Palace of Art.*

The to-and-fro arguments in the *Two Voices* are equally concentrated and forcible.

"The memory of the withered leaf
In endless time is scarce more brief
Than of the garnered autumn sheaf.
Go vexed spirit, sleep in trust;
The right ear that is filled with dust
Hears little of the false or just."

Or again—

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Who is it that could live an hour?
Then comes the check, the change, the fall,
Pain rises up, old pleasures pall,
There is one remedy for all."

The comparative rarity among the English of a keen sense of the difference between good and bad literary style is a great obstacle to the reform I desire. It is especially noticeable among the younger scientific men, whose education has been over-specialised and little concerned with the "Humanities." The literary sense is far more developed in France, where a slovenly paper ranks with a disorderly dress, as a sign of low breeding.

I have had occasion to read many memoirs in manuscript, on subjects where I was fairly at home, in which there was nothing especially recondite, but the expressions used in them were so obscure, the grammar so bad, and the arrangement so faulty, that they were scarcely intelligible on a first reading; nevertheless the writers could hardly be made to perceive their shortcomings. I have heard equally bad reports relating to essays sent by candidates for Fellowships at Colleges in one at least of our Universities. The writers of them may have been, and probably were, successful investigators, but their powers of literary exposition were of a sadly low order; so low that they could hardly be made to realise their deficiencies. The preliminary culture of students in science, seems usually to have been very imperfect.

Sufficient has now been said as to the need of reform and of the difficulties to be overcome in affecting it. It becomes our next duty to consider the steps that should be taken towards that end. The power of reform lies largely in the hands of the councils of the scientific societies, who can withhold the publication of memoirs presented to them, or accept the memoirs under such limitations as they please. A Society gives much, consequently the Council who represents it has a right to exact much in return. The Society supplies a stage from which a writer can disseminate his views, and have them subjected to the criticism of experts. It defrays the cost of publication of the memoirs, and, under occasional circumstances, that of preparing expensive plates. Therefore the Society, or its Council on its behalf, may fairly demand that the memoirs should be written in a style that is creditable to their journals; that they should be lucid, logical, and as easy for its members (who pay for the publication) to understand as the nature of the subject permits. I suggest that Councils should require a report on the literary sufficiency of every proffered memoir, before discussing whether it should be accepted for publication. It is hardly necessary to bring to remembrance that it is the universal practice of Councils of Scientific Societies to "refer" every memoir that is submitted to them. One, two, or more referees are selected among those of their Fellows who are able to give a trustworthy opinion on the merits of the paper. The referees are each supplied with a schedule on which numerous searching questions are printed, which they are requested to answer confidentially. Their reports are read to the Council, which then proceeds to discuss the question whether or no the memoir should be published as it stands, or subject to some restriction, or be rejected altogether. What I now suggest is that the printed reference paper should include questions as to the literary suitability of the memoir. They might be such as—"Do you consider the memoir to be (1) clearly expressed, (2) free from superfluous technical words, (3) orderly in arrangement, (4) of appropriate length. (5) State whether any new terms are used in the memoir, mention what they are and whether you consider them appropriate. (6) Add such general remarks on its literary style as you think would be useful to the Council when considering its publication."

I do not presume to anticipate what action a Council might take if the answers to these questions were more or less unfavourable, as much would depend on other considerations. What I want is that the members of the Council should not be left in the dark, as they usually now are, on one important element of goodness or badness in the memoir, before they consider the question of its publication. Also that they should appreciate the widely felt desire for literary reform.

There is yet another way in which scientific societies might be made to realise the occurrence of literary faults in the memoirs that they publish, namely, by occasional articles containing a selection of passages that are conspicuous for shortcomings.

I now crave your opinions on these suggestions, and hope that you will be able to offer other recommendations that may help in accomplishing the very important object in view; namely, that of improving the literary style of future Memoirs published by Scientific Societies.

Duplicate

The Royal Institute of Public Health.

LONDON CONGRESS, 1905.

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Section B.

ANTHROPOMETRY AT SCHOOLS.

BY

FRANCIS GALTON, D.C.L., Hon. D.Sc. Camb., F.R.S.

Anthropometry, or the art of measuring the physical and mental faculties of human beings, enables a shorthand description of any individual to be given by recording the measurements of a small sample of his dimensions and qualities. These will sufficiently define his bodily proportions, his massiveness, strength, agility, keenness of sense, energy, health, intellectual capacity, and mental character, and will substitute concise and exact numerical values for verbose and disputable estimates. Its methods necessarily differ for different faculties: some measurements are made by the foot-rule, others by scales, others by the watch; health is measured by the frequency and character of illness; the remainder by performances in the school or on the playground. Anthropometry furnishes the readiest method of ascertaining whether a boy is developing normally or otherwise, and how far the average conditions of pupils at one institution differ from those at others. Though partially practised at every school—for example, in all examinations—its powers are far from being generally understood, and its range is much too restricted. But as an interest in anthropometry has arisen and progressed during recent years, it is to be expected that the good sense of school authorities, assisted by the expert knowledge of medical men, anthropologists, and statisticians, will gradually introduce improvements in its methods and enlargements of its scope.

It is not, however, so much about this that I wish to speak, as on our present deplorable want of knowledge of the true worth of anthropometric warnings and forecasts. We do not possess enough material in the form of life-histories to enable us to frame answers in definite and appropriate figures to such elementary questions as these: How far does success or failure in youth foretell success or failure in later years? What is the prophetic value of anthropometry at school in respect to health, strength, and energy in after-life? How far are the observations, then, made useful in indicating the career to which a boy is naturally best fitted? What are his permanently weak and strong points? Is he, for instance, more or less likely than others to break down under a tropical climate? What becomes of the boys? In what proportion do they rise above the level of the station in which they were born, and in what proportion do they fall below it? The late Sir James Paget published a brief but most suggestive memoir entitled, "What becomes of the Medical Students?" During his long tenure of a professorship at St. Bartholomew's, his lectures were attended by about a thousand medical pupils, and the subsequent history of each was traced by his zealous assistants. Their successes and failures were then classified by Sir J. Paget in an ingenious and instructive way. It makes one heartily wish that similar investigations could be carried out into the after-careers of all who were educated at our public schools. Most laudable attempts have been made at many of them to compile registers, which are very useful as clues for further search, but far too scanty—at least, in all cases that I know of—for statistical deductions. The question now to be considered is the best way of accumulating a sufficient store of material to serve the above purposes in the future.

The conditions differ so widely in different places of education that it is almost necessary to limit the reply to one class of them. For this purpose it will be convenient to consider what might be done at the public schools. The question how the same general principles might be adapted to others must stand over for the present.

The first conclusion to be emphasized is that no programme for anthropometry in any school can be considered complete unless it provides for the collection of data during the after-lives of their pupils.

The difficulties of continuing records are many, and for the most part obvious, but I believe they might be overcome in the great public schools, to which I now confine my remarks, by the process about to be described. It is one that would prompt all the parties concerned to stimulate one another; it would work automatically, and it might be carried on without sensible charge on the funds of the institution. Some one of the masters who had a disposition for the work could be selected to perform the function of registrar, and be partly, if not entirely, remunerated for his extra labour through fees, collected in the way hereafter to be described.

There are certain small preliminary expenses, which would be met by a charge of a few shillings on leaving school for the privilege of keeping the name on the books. A large envelope would then be provided for each boy, to contain his anthropometric record up to date, and subsequent documents, which would be stored in perpetuity and become the property of the school. I reckon that the average thickness of each filled envelope would be less than ½ inch, so the records of 100 boys could stand side by side, like thin books, on a shelf 4 feet long. Each boy would also have two opposite pages of a ledger allotted to him, for brief entries from time to time. Access to all these documents would be permitted under reasonable restrictions.

It should be carefully impressed on every boy that communications will be welcomed from him all his life through, but under strict limitations as to their form and frequency, in order to reduce to a minimum the trouble of dealing with them. As regards form, the experience of all statisticians is strongly in favour of communications of this character being written on printed schedules, in reply to a few well-considered questions, only so much space being allowed for each reply as is really needful. The schedules must also contain a moderate amount of extra space for additional remarks. Printed questions check prolixity, bring to mind points of importance that might otherwise have been neglected, and ensure uniformity of arrangement.

As to frequency, yearly returns would be far too troublesome, and they are quite unnecessary. A four-yearly interval seems as good as any other that can be suggested, while it has the unique advantage of possessing one exceptional day—February 29 in each leap-year—which has thus far been unappropriated to any special purpose. I urge in all seriousness that it would be an excellent novelty to observe February 29 as a day of reminiscence—a rarer kind of Saint's day—wider and differing in its objects to those of the traditional Yule-time, which refer chiefly to family gatherings. It might be a day for each person to recall with affection and gratitude the friends and benefactors who had influenced his life for good, a recognised opportunity for reviving the friendships of early years by visits and letters. The sentiment that I wish to underlie its observance is exactly expressed by Wordsworth's well-known lines:

> "The child is father to the man,
> And I would wish my days to be
> Bound each to each by natural piety."

The celebration of the day in schools would be much concerned with the works of living men who were formerly pupils, but then engaged in the battle of life. Their doings would be spoken of, and hearty sympathies evoked. Affection and duty should co-operate in maintaining the bands of fellowship between school and former scholars; in short, its maintenance should be considered a "pious" object.

If these ideas should haply take root and thrive, it would become a common question between men at the beginning of each leap-year: "Shall you send returns to your old school?" This would serve both as a reminder and as an opening to pleasant talks about past times—about the successes and failures of contemporaries, and what had become of them; would lead to the renewal of not a few dim friendships that might otherwise have lapsed, merely through want of opportunity for keeping them up.

Uniformity of date in receiving the returns is desirable on other grounds. It would arouse a wholesome competition among the registrars of the several schools to compile their respective four-yearly digests in the best way they could, both from a scientific and a literary point of view. The simultaneity of the appearance of these digests throughout the country would compel public attention. They would be subject, as a whole, to comparison and criticism, through which their quality would improve on each successive occasion. Statisticians would, of course, take them simultaneously in hand, as containing a large aggregation of fresh, well-ordered, and trustworthy material, eminently suitable for their purposes.

It is an essential feature of my proposal to vest the initiative of sending in the records with the former pupils, thereby relieving the school authorities of the burden of hunting out changed addresses and of writing imploring letters. Consequently, the date for making the returns should be such that no person is likely to forget it. It will now be understood that the suggestion of February 29 has solid advantages.

The customary proceeding to which I look forward is that early in each leap-year every old pupil would bethink himself, and be reminded by others, that it is time to prepare his returns. He would write to his former school, asking that a blank schedule be sent to the address given by him, and enclosing a statutory fee, calculated to cover the whole cost of trouble, materials, printing, and postage. The blank schedule would be forwarded, and the date of his application and his address would be entered in the ledger. When the filled-up return had reached the school, it would be noted in the ledger, slipped into the appropriate envelope, and be acknowledged by a few friendly words on a postcard. Finally, a copy of the four-yearly digest, containing among other things a list of contributors, would be posted to the same address on its publication.

I do not propose to enter into the character of the questions to be printed on the schedule, which, as already remarked, require very careful consideration, and should be framed, as far as practicable, on a uniform plan for all schools. Suffice it to say that the questions would take cognisance of only a few simple physical facts, and would principally relate to health, profession, preferments, marriage, and children. The two sides of a quarto sheet of paper would afford more than ample space for all that need be recorded by a person concerning his history during the past four years. Therefore, if he lived forty years after leaving school, the contents of his envelope would be limited to (1) ten sheets of after-life history, (2) his anthropometric record while at school (written in a thin copy-book, with blank pages at the end), (3) one sheet of family history (asked for from his parents when he was about to leave school, and probably repeated later), and (4) a few photographs. It was on this basis that I reckoned the average thickness of each filled envelope to be less than ¼ inch.

Whenever an old pupil revisits his school after a long interval, the opportunity should be taken of repeating and recording a few simple measurements, such as his height, weight, eyesight, and strength, writing them on the blank pages at the end of the copy-book containing his anthropometric record; also of asking him to look over the pages in the ledger that are adjacent to his own, which contain the names of his former schoolfellows, and to give such information as he can to fill up any long-continued blanks—it may be by the notice of a death and its cause. Such information, written and signed on a separate slip, would be noted in the ledger, and put into the appropriate envelope.

The scheme thus outlined would interest all the parties concerned. The former pupil would acquire a much more vivid appreciation than at present of his continued relationship to his old school. Knowing that his earlier life-history was stored there, he would be the more disposed to continue it up to date and to pay his small share of the cost of the entire procedure. Moreover, he would shrink from acquiring the reputation of being indifferent to the wishes of the school, by abstaining from sending his returns. The school authorities would rejoice in the possession of the whole history of those over whose early development they exercised large control. Anthropologists would know where to lay hands on a mass of material suitable for comparing the health, bodily qualities, and scholastic achievements in early life with the health, vigour, and achievements afterwards. Statisticians would possess a four-yearly census, out of which unexpected conclusions would probably be derived. Lastly, some few of the records would be invaluable to future biographers. There will, of course, be many failures to send, but a very great deal would be secured that must otherwise have been lost, quite sufficient to warrant the experiment.

It is the behaviour of a brute beast, such as a dog or a cat, to lavish care on its puppies or kittens for a while and afterwards to cast them off entirely; yet no more prolonged interest used in former times to be shown at most schools and colleges to their old pupils. A far more humane spirit has fortunately arisen of late years, and is apparently established. The effect of the present proposals would be to encourage it, and to prolong and intensify the kindly fellowship between past and present pupils and their school, and to make it serve more than sentimental purposes. The addition of a scientific motive could not fail to invest that relation with a more durable and business-like character, and to open a way to fields of research of no small importance that have hitherto been unduly neglected.

Probability, the Foundation of Eugenics

THE HERBERT SPENCER LECTURE

DELIVERED ON JUNE 5, 1907

BY FRANCIS GALTON, F.R.S.

OXFORD
AT THE CLARENDON PRESS
LONDON
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1907

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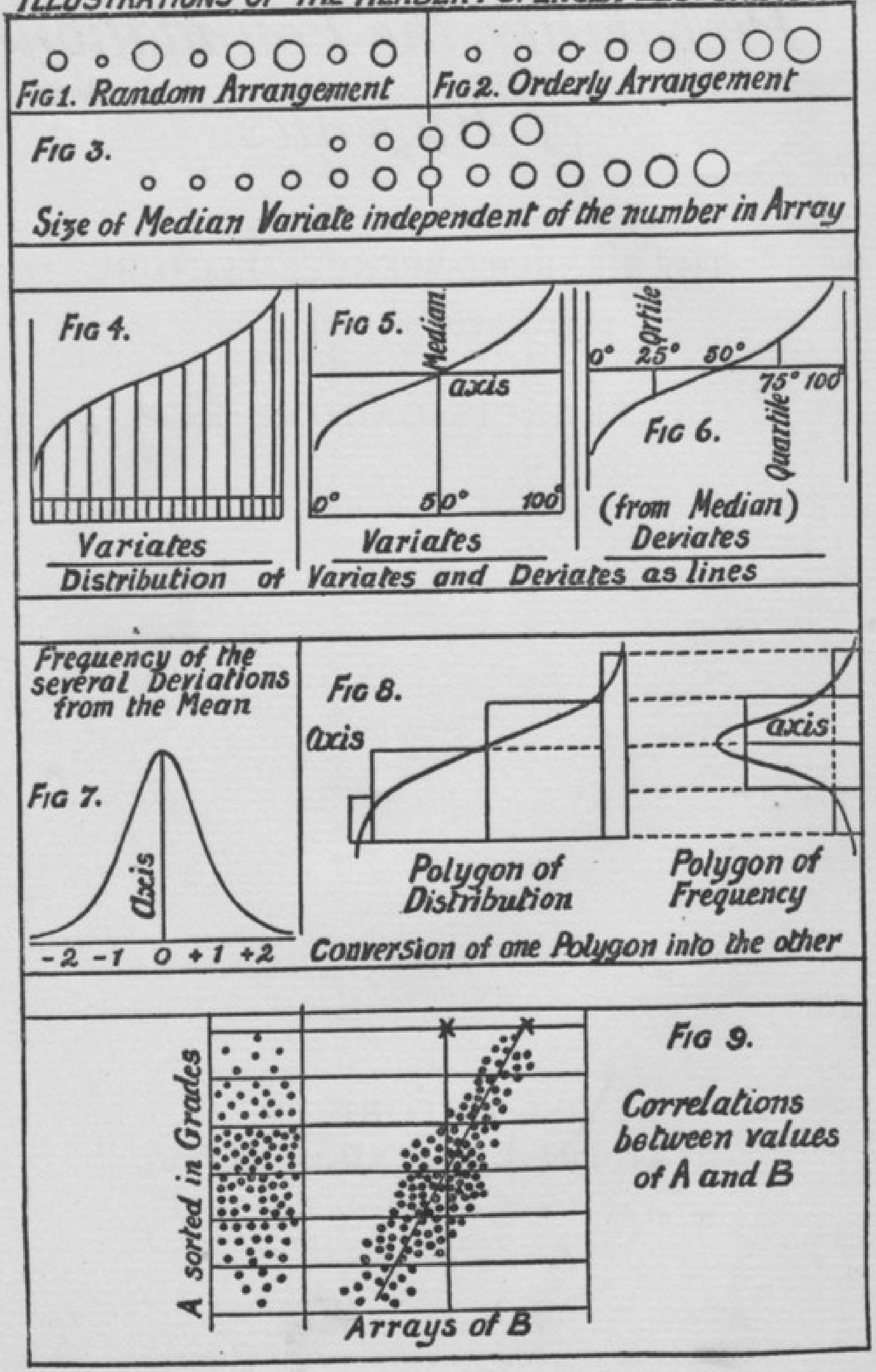

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HENRY FROWDE, M.A.
PUBLISHER TO THE UNIVERSITY OF OXFORD
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PROBABILITY, THE FOUNDATION OF EUGENICS

THE request so honourable to myself, to be the Herbert Spencer lecturer of this year, aroused a multitude of vivid recollections. Spencer's strong personality, his complete devotion to a self-imposed and life-long task, together with rare gleams of tenderness visible amidst a wilderness of abstract thought, have left a unique impression on my mind that years fail to weaken.

I do not propose to speak of his writings; they have been fully commented on elsewhere, but I desire to acknowledge my personal debt to him, which is large. It lies in what I gained through his readiness to discuss any ideas I happened to be full of at the time, with quick sympathy and keen criticism. It was his custom for many afternoons to spend an hour or two of rest in the old smoking room of the Athenaeum Club, strolling into an adjoining compartment for a game of billiards when the table was free. Day after day on those afternoons I enjoyed brief talks with him, which were often of exceptional interest to myself. All that kind of comfort and pleasure has long ago passed from

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me. Among the many things of which age deprives us, I regret few more than the loss of contemporaries. When I was young I felt diffident in the presence of my seniors, partly owing to a sense that the ideas of the young cannot be in complete sympathy with those of the old. Now that I myself am old it seems to me that my much younger friends keenly perceive the same difference, and I lose much of that outspoken criticism which is an invaluable help to all who investigate.

History of Eugenics.

It must have surprised you as it did myself to find the new word 'Eugenics' in the title both of the Boyle lecture, delivered in Oxford about a fortnight ago, and of this. It was an accident, not a deliberate concurrence, and I accept it as a happy omen. The field of Eugenics is so wide that there is no need for myself, the second lecturer, to plant my feet in the footsteps of the first; on the contrary, it gives freedom by absolving me from saying much that had to be said in one way or another. I fully concur in the views so ably presented by my friend and co-adjutor Professor Karl Pearson, and am glad to be dispensed from further allusion to subjects that formed a large portion of his lecture, on which he is a far better guide and an infinitely higher authority than myself.

In giving the following sketch of the history of Eugenics I am obliged to be egotistical, because I kindled the feeble flame that struggled doubtfully for a time until it caught hold of adjacent stores of suitable material, and became a brisk fire, burning freely by itself, and again because I have had much to do with its progress quite recently.

The word 'Eugenics' was coined and used by me in my book *Human Faculty*, published as long ago as 1883, which has long been out of print; it is, however, soon to be re-published in a cheap form. In it I emphasized the essential brotherhood of mankind, heredity being to my mind a very real thing; also the belief that we are born to act, and not to wait for help like able-bodied idlers, whining for doles. Individuals appear to me as finite detachments from an infinite ocean of being, temporarily endowed with executive powers. This is the only answer I can give to myself in reply to the perpetually recurring questions of 'Why? whence? and whither?' The immediate 'whither?' does not seem wholly dark, as some little information may be gleaned concerning the direction in which Nature, so far as we know of it, is now moving. Namely towards the evolution of mind, body, and character in increasing energy and co-adaptation.

I have often wondered that the poem of Hyperion, by Keats—that magnificent torso of an incompleted work—has not been placed in the very forefront of past speculations on evolution. Keats is so thorough that he makes the very Divinities to be its product. The earliest gods such as Coelus, born out of Chaos,

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are vague entities, they engender Saturn, Oceanus, Hyperion, and the Titan brood, who supersede them. These in their turn are ousted from dominion by their own issue, the Olympian Gods. A notable advance occurs at each successive stage in the quality of the Divinities. When Hyperion, newly terrified by signs of impending overthrow, lies prostrate on the earth 'his ancient mother, for some comfort yet,' the voice of Coelus from the universal space, thus 'whispered low and solemn in his ear . . . yet do thou strive for thou art capable . . . my life is but the life of winds and tides, no more than winds and tides can I prevail, but thou canst.' I have quoted only disjointed fragments of this wonderful poem, enough to serve as a reminder to those who know it, but will add ten consecutive lines from the speech of the fallen Oceanus to his comrades, which give a summary of evolution as here described:

As Heaven and Earth are fairer, fairer far
Than Chaos and black Darkness, though once chiefs,
And as we show beyond that Heaven and Earth
In form and shape compact and beautiful,
In Will, in action free, companionship,
And thousand other signs of purer life;
So on our heels a fresh perfection treads
A power more strong in beauty, born of us
And fated to excel us, as we pass
In glory that old Darkness.

He ends with 'this is the truth, and let it be your balm.' The poem is a noble conception, founded on the crude cosmogony of the ancient Greeks.

The ideas have long held my fancy that we men may be the chief, and perhaps the only executives on earth. That we are detached on active service with, it may be only illusory, powers of free-will. Also that we are in some way accountable for our success or failure to further certain obscure ends, to be guessed as best we can. That though our instructions are obscure they are sufficiently clear to justify our interference with the pitiless course of Nature, whenever it seems possible to attain the goal towards which it moves, by gentler and kindlier ways. I expressed these views as forcibly as I then could in the above-mentioned book, with especial reference to improving the racial qualities of mankind, in which the truest piety seems to me to reside in taking action, and not in submissive acquiescence to the routine of Nature. It was thought impious at one time to attach lightning conductors to churches, as showing a want of trust in the tutelary care of the Deity to whom they were dedicated; now I think most persons would be inclined to apply some contemptuous epithet to such as obstinately refused, on those grounds, to erect them.

The direct pursuit of studies in Eugenics, as to what could practically be done, and the amount of change in racial qualities that could reasonably be anticipated, did not at first attract investigators. The idea of effecting an improvement in that direction was too much in advance of the march of popular imagination, so I had to wait. In the meantime I occupied myself with collateral problems, more especially with that of dealing measurably with faculties that are variously distributed in a large population. The results were published in my 'Natural Inheritance' in 1889, and I shall have occasion to utilize some of them later on, in this very lecture. The publication of that book proved to be more timely than the former. The methods were greatly elaborated by Professor Karl Pearson, and applied by him to Biometry. Professor Weldon of this University, whose untimely death is widely deplored, aided powerfully. A new science was thus created primarily on behalf of Biometry, but equally applicable to Eugenics, because their provinces overlap.

The publication of *Biometrika*, in which I took little more than a nominal part, appeared in 1901.

Being myself appointed Huxley Lecturer before the Anthropological Institute in 1901 I took for my title 'The possible improvement of the Human Breed under the existing conditions of Law and Sentiment' (*Nature*, November 1, 1901, *Report of the Smithsonian Institute, Washington*, for the same year).

The next and a very important step towards Eugenics was made by Professor Karl Pearson in his Huxley lecture of 1903 entitled 'The Laws of Inheritance in Man' (*Biometrika*, vol. iii). It contains a most valuable compendium of work achieved and of objects in view; also the following passage (p. 159), which is preceded by forcible reasons for his conclusions:

We are ceasing as a nation to breed intelligence as we did fifty to a hundred years ago. The mentally better stock in the nation is not reproducing itself at the same rate as it did of old; the less able, and the less energetic are more fertile than the better stocks. No scheme of wider or more thorough education will bring up, in the scale of intelligence, hereditary weakness to the level of hereditary strength. The only remedy, if one be possible at all, is to alter the relative fertility of the good and the bad stocks in the community.

Again in 1904, having been asked by the newly-formed Sociological Society to contribute a memoir, I did so on 'Eugenics, its definition, aim, and scope'. This was followed up in 1905 by three memoirs, 'Restrictions in Marriage,' 'Studies in National Eugenics,' and 'Eugenics as a factor in Religion', which were published in the Memoirs of that Society with comments thereon by more than twenty different authorities (*Sociological Papers*, published for the Sociological Society (Macmillan), vols. i and ii). The subject of Eugenics being thus formally launched, and the time appearing ripe, I offered a small endowment to the University of London to found a Research Fellowship on its behalf. The offer was cordially accepted, so Eugenics gained the recognition of its importance by the University of London, and a home for its study in University College. Mr. Edgar Schuster, of this University, became Research Fellow in 1905, and I am much indebted to his care in nurturing the young undertaking and for the memoirs he has contributed, part of which must remain for a short time longer unpublished.

When the date for Mr. Schuster's retirement approached it was advisable to utilize the experience so far gained in reorganizing the Office. Professor Pearson and myself, in consultation with the authorities of the University of London, elaborated a scheme at the beginning of this year, which is a decided advance, and shows every sign of vitality and endurance. Mr. David Heron, a Mathematical Scholar of St. Andrews, is now a Research Fellow; Miss Ethel Elderton, who has done excellent and expert work from the beginning, is deservedly raised to the position of Research Scholar; and the partial services of a trained Computer have been secured. An event of the highest importance to the future of the Office is that Professor Karl Pearson has undertaken, at my urgent request, that general supervision of its work which advancing age and infirmities preclude me from giving. He will, I trust, treat it much as an *annexe* to his adjacent biometric laboratory, for many studies in Eugenics might, with equal propriety, be carried on in either of them, and the same methods of precise analysis which are due to the mathematical skill and untiring energy of Professor Pearson are used in both. The Office now bears the name of the Eugenics Laboratory, and its temporary home is in 88 Gower Street. The phrase 'National Eugenics' is defined as 'the study of agencies under social control that may improve or impair the racial qualities of future generations, either physically or mentally'.

The Laboratory has already begun to publish memoirs on its own account, and I now rest satisfied in the belief that, with a fair share of good luck, this young Institution will prosper and grow into an important centre of research.

Application of Theories of Probability to Eugenics.

Eugenics seeks for quantitative results. It is not contented with such vague words as 'much' or 'little', but endeavours to determine 'how much' or 'how little' in precise and trustworthy figures. A simple example will show the importance of this. Let us suppose a class of persons, called *A*, who are afflicted with some form and some specified degree of degeneracy, as inferred from personal observations, and from family history, and let class *B* consist of the offspring of *A*. We already know only too well that when the grade of *A* is very low, that of the average *B* will be below par and mischievous to the community, but how mischievous will it probably be? This question is of a familiar kind, easily to be answered when a sufficiency of facts have been collected. But a second question arises, What will be the trustworthiness of the forecast derived from averages when it is applied to individuals? This is a kind of question that is not familiar, and rarely taken into account, although it too could be answered easily as follows. The average mischief done by each *B* individual to the community may for brevity be called M: the mischiefs done by the several individuals differ more or less from M by amounts whose average may be called D. In other words D is the average amount of the individual deviations from M. D thus becomes the measure of untrustworthiness. The smaller D is, the more precise the forecast, and the stronger the justification for taking such drastic measures against the propagation of class B as would be consonant to the feelings if the forecast were known to be infallible. On the other hand, a large D signifies a corresponding degree of uncertainty, and a risk that might be faced without reproach through a sentiment akin to that expressed in the maxim 'It is better that many guilty should escape than that one innocent person should suffer'. But that is not the sentiment by which natural selection is guided, and it is dangerous to yield far to it.

There can be no doubt that a thorough investigation of the kind described, even if confined to a single grade and to a single form of degeneracy, would be a serious undertaking. Masses of trustworthy material must be collected, usually with great difficulty, and be afterwards treated with skill and labour by methods that few at present are competent to employ. An extended investigation into the good or evil done to the state by the offspring of many different classes of persons, some of civic value, others the reverse, implies a huge volume of work sufficient to occupy Eugenics laboratories for an indefinite time.

Object Lessons in the Methods of Biometry.

I propose now to speak of those fundamental principles of the laws of Probability that are chiefly concerned in the newer methods of Biometry, and consequently of Eugenics. Most persons of ordinary education seem to know nothing about them, not even understanding their technical terms, much less appreciating the cogency of their results. This popular ignorance so obstructs the path of Eugenics that I venture to tax your attention by proposing a method of partly dispelling it. Let me first say that no one can be more conscious than myself of the large amount of study that is required to qualify a man to deal adequately with the mathematical methods of Biometry, or that any man can hope for much success in that direction unless he is possessed of appropriate faculties and a strong brain. On the other hand, I hold an opinion likely at first sight to scandalize biometricians and which I must justify, that the fundamental ideas on which abstruse problems of Probability are based admit of being so presented to any intelligent person as to be grasped by him, even though he be quite ignorant of mathematics. The conditions of doing so are that the lessons shall be as far as possible 'Object lessons', in which real objects shall be handled as in the Kindergarten system, and simple operations performed and not only talked about. I am anxious to make myself so far understood, that some teachers of science may be induced to elaborate the course that I present now only in outline. It seems to me suitably divisible into a course of five lessons of one hour each, which would be sufficient to introduce the learner into a new world of ideas, extraordinarily wide in their application. A proper notion of what is meant by Correlation requires some knowledge of the principal features of Variation, and will be the goal towards which the lessons lead.

To most persons Variability implies something indefinite and capricious. They require to be taught that it, like Proteus in the old fable, can be seized, securely bound, and utilized; that it can be defined and measured. It was disregarded by the old methods of statistics, that concerned themselves solely with Averages. The average amount of various measurable faculties or events in a multitude of persons was determined by simple methods, the individual variations being left out of account as too difficult to deal with. A population was treated by the old methods as a structureless atom, but the newer methods treat it as a compound unit. It will be a considerable intellectual gain to an otherwise educated person, to fully understand the way in which this can be done, and this and such like matters the proposed course of lessons is intended to make clear. It cannot be expected that in the few available minutes more than an outline can be given here of what is intended to be conveyed in perhaps thirty-fold as much time with the aid of profuse illustrations by objects and diagrams. At the risk of being wearisome, it is, however, necessary to offer the following syllabus of what is proposed, for an outline of what teachers might fill in.

The object of the first lesson would be to explain and illustrate Variability of Size, Weight, Number, &c., by exhibiting samples of specimens that had been marshalled at random (Fig. 1), or arrayed in order of their magnitude (Fig. 2). Thus when variations of length were considered, objects of suitable size, such as chestnuts, acorns, hazel-nuts, stones of wall fruit, might be arrayed as beads on a string. It will be shown that an 'Array' of Variates of any kind falls into a continuous series. That each variate differs little from its neighbours about the middles of the Arrays, but that such differences increase rapidly towards their extremities. Abundant illustration would be required, and much handling of specimens.

Arrays of Variates of the same class strung together, differing considerably in the number of the objects they each contain, would be laid side by side and their middlemost variates or 'Medians' (Fig. 3) would be compared. It would be shown that as a rule the Medians become very similar to one another when the numbers in the Arrays are large. It must then be dogmatically explained that double accuracy usually accompanies a four-fold number, a treble accuracy a nine-fold number, and so on.

(This concludes the first lesson, during which the words and significations of Variability, Variate, Array, and Median will have been learnt.)

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The second lesson is intended to give more precision to the idea of an Array. The variates in any one of these strung loosely on a cord, should be disposed at equal distances apart in front of an equal number of compartments, like horses in the front of a row of stalls (Fig. 4), and their tops joined. There will always be one more side to the row of stalls than there are objects, otherwise a side of one of the extreme stalls would be wanting. Thus there are two ways of indicating the portion of a particular variate, either by its *serial number* as 'first', 'second', 'third', or so on, or by *degrees* like those of a thermometer. In the latter case the sides of the stalls serve as degrees, counting the first of them as 0°, making one more graduation than the number of objects, as should be. The difference between these two methods has to be made clear, and that while the serial position of the Median object is always the same in any two Arrays whatever be the number of variates, the serial positions of their subdivisions cannot be the same, the ignored half interval at either end varying in width according to the number of variates, and becoming considerable when that number is small.

Lines of proportionate length will then be used drawn on a black board, and the limits of the Array will be also drawn, at a half interval from either end. The base is then to be divided centesimally.

Next join the tops of the lines with a smooth curve, and wipe out everything except the curve, the Limit at either side, and the Centesimally divided Base (Fig. 5).

This figure forms a Scheme of Distribution of Variates. Explain clearly that its shape is independent of the number of Variates, so long as they are sufficiently numerous to secure statistical constancy.

Show numerous schemes of variates of different kinds, and remark on the prevalent family likeness between the bounding curves. (Words and meanings learnt—Schemes of Distribution, Centesimal graduation of base.)

The third lesson passes from Variates, measured upwards from the base, to Deviates measured upwards or downwards from the Median, and treated as positive or negative values accordingly (Fig. 6).

Draw a Scheme of Variates on the black board, and show that it consists of two parts; the median which represents a constant, and the curve which represents the variations from it. Draw a horizontal line from limit to limit, through the top of the Median, to serve as Axis to the Curve. Divide the Axis centesimally, and wipe out everything except Curve, Axis, and Limits. This forms a Scheme of Distribution of Deviates. Draw ordinates from the axis to the curve at the 25th and 75th divisions. These are the 'Quartile' deviates.

At this stage the Genesis of the theoretical Normal curve might be briefly explained and the generality of its application; also some of its beautiful properties of reproduction. Many of the diagrams already shown would be again employed to show the prevalence of approximately normal distributions. Exceptions of strongly marked Skew curves would be exhibited and their genesis briefly explained.

It will then be explained that while the ordinate at *any* specified centesimal division in two normal curves measures their relative variability, the Quartile is commonly employed as the unit of variability under the almost grotesque name of 'Probable Error', which is intended to signify that the length of any Deviate in the system is as likely as not to exceed or to fall short of it. This, by construction, is the case of either Quartile.

(New words and meanings—Scheme of Distribution of Deviates, Axis, Normal, Skew, Quartile, and Probable Error.)

In the fourth lesson it has to be explained that the Curve of Normal Distribution is not the direct result of calculation, neither does the formula that expresses it lend itself so freely to further calculation, as that of Frequency. Their shapes differ; the first is an Ogive, the second (Fig. 7) is Bell-shaped. In the curve of Frequency the Deviations are reckoned from the Mean of all the Variates, and not from the Median. Mean and Median are the same in Normal Curves, but may differ much in others. Either curve can be transformed into the other, as is best exemplified by using a Polygon (Fig. 8) instead of the Curve, consisting of a series of rectangles differing in height by the same amounts, but having widths respectively representative of the frequencies of 1, 3, 3, 1. (This is one of those known as a binomial series, whose

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genesis might be briefly explained.) If these rectangles are arrayed in order of their widths, side by side, they become the equivalents of the ogival curve of Distribution. Now if each of these latter rectangles be slid parallel to itself up to either limit, their bases will overlap and they become equivalent to the bell-shaped curve of Frequency with its base vertical.

The curve of Frequency contains no easily perceived unit of variability like the Quartile of the Curve of Distribution. It is therefore not suited for and was not used as a first illustration, but the formula that expresses it is by far the more suitable of the two for calculation. Its unit of variability is what is called the 'Standard Deviation,' whose genesis will admit of illustration. How the calculations are made for finding its value is beyond the reach of the present lessons. The calculated ordinates of the normal curve must be accepted by the learner much as the time of day by his watch, though he be ignorant of the principles of its construction. Much more beyond his reach are the formulae used to express quasi-normal and skew curves. They require a previous knowledge of rather advanced mathematics.

(New words and ideas—Curve of Frequency, Standard Deviation, Mean, Binomial Series.)

The fifth and last lesson deals with the measurement of Correlation, that is, with the closeness of the relation between any two systems whose variations are due partly to causes common to both, and partly to causes special to each. It applies to nearly every social relation, as to environment and health, social position and fertility, the kinship of parent to child, of uncle to nephew, &c. It may be mechanically illustrated by the movements of two pulleys with weights attached, suspended from a cord held by one of the hands of three different persons, 1, 2, and 3. No. 2 holds the middle of the cord, one half of which then passes round one of the pulleys up to the hand of No. 1; the other half similarly round the other pulley up to the hand of No. 3. The hands of Nos. 1, 2 and 3 move up and down quite independently, but as the movements of both weights are simultaneously controlled in part by No. 2, they become 'correlated'.

The formation of a table of correlations on paper ruled in squares, is easily explained on the blackboard (Fig. 9). The pairs of correlated values A and B have to be expressed in units of their respective variabilities. They are then sorted into the squares of the paper,—vertically according to the magnitudes of A, horizontally according to those of B—, and the Mean of each partial array of B values, corresponding to each grade of A, has to be determined. It is found theoretically that where variability is normal, the Means of B lie practically in a straight line on the face of the Table, and observation shows they do so in most other cases. It follows that the average deviation of a B value bears a constant ratio to the deviation of the corresponding A value. This ratio is called the 'Index of Correlation', and is expressed by a single figure. For example: if the thigh-bone of many persons deviate 'very much' from the usual length of the thigh-bones of their race, the average of the lengths of the corresponding arm-bones will differ 'much', but not 'very much', from the usual length of arm-bones, and the ratio between this 'very much' and 'much' is constant and in the same direction, whatever be the numerical value attached to the word 'very much'. Lastly, the trustworthiness of the Index of Correlation, when applied to individual cases, is readily calculable. When the closeness of correlation is absolute, it is expressed by the number 1·0, and by 0·0, when the correlation is nil.

(New words and ideas—Correlation and Index of Correlation.)

This concludes what I have to say on these suggested Object lessons. It will have been tedious to follow in its necessarily much compressed form but will serve, I trust, to convey its main purpose of showing that a very brief course of lessons, copiously illustrated by diagrams and objects to handle, would give an acceptable introduction to the newer methods employed in Biometry and in Eugenics. Further, that when read leisurely by experts in its printed form, it would give quite sufficient guidance for elaborating details.

Influence of Collective Truths upon Individual Conduct.

We have thus far been concerned with Probability, determined by methods that take cognizance of Variations, and yield exact results, thereby affording a solid foundation for action. But the stage on which human action takes place is a superstructure into which emotion enters, we are guided on it less by Certainty and by Probability than by Assurance to a greater or lesser degree. The word Assurance is derived from *sure,* which itself is an abbreviation of *secure,* that is of *se- cura,* or without misgiving. It is a contented attitude of mind largely dependent on custom, prejudice, or other unreasonable influences which reformers have to overcome, and some of which they are apt to utilize on their own behalf. Human nature is such that we rarely find our way by the pure light of reason, but while peering through spectacles furnished with coloured and distorting glasses.

Locke seems to confound certainty with assurance in his forcible description of the way in which men are guided in their daily affairs (*Human Understanding,* iv, 14, par. 1):

Man would be at a great loss if he had nothing to direct him but what has the certainty of true knowledge. For that being very short and scanty, he would be often utterly in the dark, and in most of the actions of his life, perfectly at a stand, had he nothing to guide him in the absence of clear and certain knowledge. He that will not eat till he has demonstration that it will nourish him, he that will not stir till he infallibly knows the business he goes about will succeed, will have little else to do but to sit still and perish.

A society may be considered as a highly complex organism, with a consciousness of its own, caring only for itself, establishing regulations and customs for its collective advantage, and creating a code of opinions to subserve that end. It is hard to over-rate its power over the individual in regard to any obvious particular on which it emphatically insists. I trust in some future time that one of those particulars will be the practice of Eugenics. Otherwise the influence of collective truths on individual conduct is deplorably weak, as expressed by the lines:—

For others' follies teach us not,
Nor much their wisdom teaches,
But chief of solid worth is what
Our own experience preaches.

Professor Westermark, among many other remarks in which I fully concur, has aptly stated (*Sociological Papers*, published for the Sociological Society. Macmillan, 1906, vol. ii, p. 24), with reference to one obstacle which prevents individuals from perceiving the importance of Eugenics, 'the prevalent opinion that almost anybody is good enough to marry is chiefly due to the fact that in this case, cause and effect, marriage and the feebleness of the offspring, are so distant from each other that the *near-sighted eye* does

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not distinctly perceive the connexion between them.' (The Italics are mine.)

The enlightenment of individuals is a necessary preamble to practical Eugenics, but social opinion is the tyrant by whose praise or blame the principles of Eugenics may be expected hereafter to influence individual conduct. Public opinion may, however, be easily directed into different channels by opportune pressure. A common conviction that change in the established order of some particular codes of conduct would be impossible, because of the shock that the idea of doing so gives to our present ideas, bears some resemblance to the conviction of lovers that their present sentiments will endure for ever. Conviction, which is that very Assurance of which mention has just been made, is proved by reiterated experience to be a highly fallacious guide. Love is notoriously fickle in despite of the fervent and genuine protestations of lovers, and so is public opinion. I gave a list of extraordinary variations of the latter in respect to restrictions it enforced on the freedom of marriage, at various times and places (*Sociological Papers*, quoted above). Much could be added to that list, but I will not now discuss the effects of public opinion on such a serious question. I will take a much smaller instance which occurred before the time to which the recollections of most persons can now reach, but which I myself recall vividly. It is the simple matter of hair on the face of male adults. When I was young, it was an unpardonable offence for any English person other than a cavalry officer, or perhaps some one of high social rank, to wear a moustache. Foreigners did so and were tolerated, otherwise the assumption of a moustache was in popular opinion worse than wicked, for it was atrociously bad style. Then came the Crimean War and the winter of Balaclava, during which it was cruel to compel the infantry to shave themselves every morning. So their beards began to grow, and this broke a long established custom. On the return of the army to England the fashion of beards spread among the laity, but stopped short of the clergy. These, however, soon began to show dissatisfaction, they said the beard was a sign of manliness that ought not to be suppressed and so forth; and at length the moment arrived. A distinguished clergyman, happily still living, 'bearded' his Bishop on a critical occasion. The Bishop yielded without protest, and forthwith hair began to sprout in a thousand pulpits where it had never appeared before within the memory of man.

It would be no small shock to public sentiment if our athletes in running public races were to strip themselves stark naked, yet that custom was rather suddenly introduced into Greece. Plato says (Republic V, par. 452, Jowett's translation):

> Not long ago the Greeks were of the opinion, which is still generally received among the barbarians, that the sight of a naked man was ridiculous and improper, and when first the Cretans and the Lacedaemonians introduced naked exercises, the wits of that day might have ridiculed them. . . .

Thucydides (I. 6) also refers to the same change as occurring 'quite lately'.

Public opinion is commonly far in advance of private morality, because society as a whole keenly appreciates acts that tend to its advantage, and condemns those that do not. It applauds acts of heroism that perhaps not one of the applauders would be disposed to emulate. It is instructive to observe cases in which the benevolence of public opinion has outstripped that of the Law —which, for example, takes no notice of such acts as are enshrined in the parable of the good Samaritan. A man on his journey was robbed, wounded, and left by the wayside. A priest and a Levite successively pass by and take no heed of him. A Samaritan follows, takes pity, binds his wounds, and bears him to a place of safety. Public opinion keenly condemns the priest and the Levite, and praises the Samaritan, but our criminal law is indifferent to such acts. It is most severe on misadventure due to the neglect of a definite duty, but careless about those due to absence of common philanthropy. Its callousness in this respect is painfully shown in the following quotations (Kenny, *Outlines of Criminal Law*, 1902, p. 121, per Hawkins in Reg. v. Paine, *Times*, February 25, 1880):

> If I saw a man who was not under my charge, taking up a tumbler of poison, I should not be guilty of any crime by not stopping him. I am under no legal obligation to protect a stranger.

That is probably what the priest and the Levite of the parable said to themselves.

A still more emphatic example is in the *Digest of Criminal Law*, by Justice Sir James Stephen, 1887, p. 154. Reg. v. Smith, 2 C. and P., 449:

> *A* sees *B* drowning and is able to help him by holding out his hand. *A* abstains from doing so in order that *B* may be drowned, and *B* is drowned. *A* has committed no offence.

It appears, from a footnote, that this case has been discussed in a striking manner by Lord Macaulay in his notes on the Indian Penal Code, which I have not yet been able to consult.

Enough has been written elsewhere by myself and others to show that whenever public opinion is strongly roused it will lead to action, however contradictory it may be to previous custom and sentiment. Considering that public opinion is guided by the sense of what best serves the interests of society as a whole, it is reasonable to expect that it will be strongly exerted in favour of Eugenics when a sufficiency of evidence shall have been collected to make the truths on which it rests plain to all. That moment has not yet arrived. Enough is already known to those who have studied the question to leave no doubt in their minds about the general results, but not enough is quantitatively known to justify legislation or other action except in extreme cases. Continued studies will be required for some time to come, and the pace must not be hurried. When the desired fullness of information shall have been acquired, then, and not till then, will be the fit moment to proclaim a 'Jehad,' or Holy War against customs and prejudices that impair the physical and moral qualities of our race.

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FOLLOWED BY AN ABSTRACT OF AN EARLIER MEMOIR

EUGENICS: ITS DEFINITION, SCOPE AND AIMS.

MEMOIRS COMMUNICATED TO THE SOCIOLOGICAL SOCIETY

BY

FRANCIS GALTON, F.R.S., D.C.L., &c.

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I.

RESTRICTIONS IN MARRIAGE

By FRANCIS GALTON, F.R.S., D.C.L., Sc.D.

Read before the Sociological Society, on Tuesday, February 14th, at a meeting in the School of Economics and Political Science (University of London), Clare Market, W.C., Dr. E. WESTERMARCK in the Chair.

It is proposed in the following remarks to meet an objection that has been repeatedly urged against the possible adoption of any system of Eugenics,* namely, that human nature would never brook interference with the freedom of marriage.

In my reply, I shall proceed on the not unreasonable assumption, that when the subject of Eugenics shall be well understood, and when its lofty objects shall have become generally appreciated, they will meet with some recognition both from the religious sense of the people and from its laws. The question to be considered is, how far have marriage restrictions proved effective, when sanctified by the religion of the time, by custom, and by law? I appeal from arm-chair criticism to historical facts.

To this end, a brief history will be given of a few

* Eugenics may be defined as the science which deals with those social agencies that influence, mentally or physically, the racial qualities of future generations.

widely-spread customs in successive paragraphs. It will be seen that with scant exceptions they are based on social expediency, and not on natural instincts. Each paragraph might have been expanded into a long chapter had that seemed necessary. Those who desire to investigate the subject further can easily do so by referring to standard works in anthropology, among the most useful of which, for the present purpose, are Frazer's *Golden Bough*, Westermarck's *History of Marriage*, Huth's *Marriage of Near Kin*, and Crawley's *Mystic Rose*.

1. Monogamy. It is impossible to label mankind by one general term, either as animals who instinctively take a plurality of mates, or who consort with only one, for history suggests the one condition as often as the other. Probably different races, like different individuals, vary considerably in their natural instincts. Polygamy may be understood either as having a plurality of wives; or, as having one principal wife and many secondary but still legitimate wives, or any other recognised but less legitimate connections; in one or other of these forms it is now permitted—by religion, customs, and law—to at least one-half of the population of the world, though its practice may be restricted to a few, on account of cost, domestic peace, and the insufficiency of females. Polygamy holds its ground firmly throughout the Moslem world. It exists throughout India and China in modified forms, and it is entirely in accord with the sentiments both of men and women in the larger part of negro Africa. It was regarded as a matter of course in the early Biblical days. Jacob's twelve children were born of four mothers all living at the same time, namely, Leah, and her sister, Rachel, and their respective handmaids Bilhah and Zilpah. Long afterwards, the Jewish kings emulated the luxurious habits of neighbouring potentates and carried polygamy to an extreme degree. For Solomon, see I. Kings, xi. 3. For his son Rehoboam, see II. Chron., xi. 21. The history of the subsequent practice of the custom among the Jews is obscure, but the Talmud contains no law against polygamy. It must have ceased in Judæa by the time of the Christian Era. It was not then allowed in either Greece or Rome. Polygamy

was unchecked by law in profligate Egypt, but a reactionary and ascetic spirit existed, and some celibate communities were formed in the service of Isis, who seem to have exercised a large though indirect influence in introducing celibacy into the early Christian church. The restriction of marriage to one living wife subsequently became the religion and the law of all Christian nations, though licence has been widely tolerated in royal and other distinguished families, as in those of some of our English kings. Polygamy was openly introduced into Mormonism by Brigham Young, who left seventeen wives, and fifty-six children. He died in 1877; polygamy was suppressed soon after. (*Encyc. Brit.*, xvi. 827.)

It is unnecessary for my present purpose to go further into the voluminous data connected with these marriages in all parts of the world. Enough has been said to show that the prohibition of polygamy, under severe penalties by civil and ecclesiastical law, has been due not to any natural instinct against the practice, but to consideration of social well-being. I conclude that equally strict limitations to freedom of marriage might, under the pressure of worthy motives, be hereafter enacted for Eugenic and other purposes.

2. ENDOGAMY, or the custom of marrying exclusively *within* one's own tribe or caste, has been sanctioned by religion and enforced by law, in all parts of the world, but chiefly in long settled nations where there is wealth to bequeath and where neighbouring communities profess different creeds. The details of this custom, and the severity of its enforcement, have everywhere varied from century to century. It was penal for a Greek to marry a barbarian, for a Roman patrician to marry a plebeian, for a Hindu of one caste to marry one of another caste, and so forth. Similar restrictions have been enforced in multitudes of communities, even under the penalty of death.

A very typical instance of the power of law over the freedom of choice in marriage, and which was by no means confined to Judæa, is that known as the Levirate. It shows that family property and honour were once held by the Jews to dominate over individual preferences. The Mosaic law actually *compelled* a man to marry the widow of his brother if he left no male issue. (Deuteron. xxv.) Should the brother refuse, "then shall his brother's wife come unto him in the presence of the elders, and loose his shoe from off his foot, and spit in his face; and she shall answer and say, so shall it be done unto the man that doth not build up his brother's house. And his name shall be called in Israel the house of him that hath his shoe loosed." The form of this custom survives to the present day and is fully described and illustrated under the article "Halizah" (=taking off, untying) in the *Jewish Cyclopedia.* Jewish widows are now almost invariably remarried with this ceremony. They are, as we might describe it, "given away" by a kinsman of the deceased husband, who puts on a shoe of an orthodox shape which is kept for the purpose, the widow unties the shoe, spits, but now on the *ground*, and repeats the specified words.

The duties attached to family property led to the history, which is very strange to the ideas of the present day, of Ruth's advances to Boaz under the advice of her mother. "It came to pass at midnight" that Boaz "was startled (see marginal note in the Revised Version) and turned himself, and behold a woman lay at his feet," who had come in "softly and uncovered his feet and laid her down." He told her to lie still until the early morning and then to go away. She returned home and told her mother, who said, "Sit still, my daughter, until thou know how the matter will fall, for the man will not rest until he have finished the thing this day." She was right. Boaz took legal steps to disembarrass himself of the claims of a still nearer kinsman, who "drew off his shoe"; so Boaz married Ruth. Nothing could be purer, from the point of view of those days, than the history of Ruth. The feelings of the modern social world would be shocked if the same thing were to take place now in England.

Evidence from the various customs relating to endogamy show how choice in marriage may be dictated by religious custom. That is, by a custom founded on a religious view of family property and family descent. Eugenics deal with what is more valuable than money or lands, namely the heritage of a high character, capable brains, fine physique, and vigour; in short, with all that is most desirable for a family to possess as a birthright. It aims at the evolution and preservation of high races of men, and it as well deserves to be strictly enforced as a religious duty, as the Levirate law ever was.

3. EXOGAMY is, or has been, as widely spread as the opposed rule of endogamy just described. It is the duty enforced by custom, religion, and law, of marrying *outside* one's own clan, and is usually in force amongst small and barbarous communities. Its former distribution is attested by the survival in nearly all countries of ceremonies based on "marriage by capture." The remarkable monograph on this subject by the late Mr. McLennan is of peculiar interest. It was one of the earliest, and perhaps the most successful, of all attempts to decipher pre-historic customs by means of those now existing among barbarians, and by the marks they have left on the traditional practices of civilised nations, including ourselves. Before his time those customs were regarded as foolish, and fitted only for antiquarian trifling. In small fighting communities of barbarians, daughters are a burden; they are usually killed while infants, so there are few women to be found in a tribe who were born in it. It may sometimes happen that the community has been recently formed by warriors who have brought no women, and who, like the Romans in the old story, can only supply themselves by capturing those of neighbouring tribes. The custom of capture grows; it becomes glorified, because each wife is a living trophy of the captor's heroism; so marriage within the tribe comes to be considered an unmanly, and at last a shameful act. The modern instances of this among barbarians are very numerous.

4. AUSTRALIAN MARRIAGES. The following is a brief clue, and apparently a true one, to the complicated marriage restrictions among Australian bushmen, which are enforced by the penalty of death, and which seem to be partly endogamous

in origin and partly otherwise. The example is typical of those of many other tribes that differ in detail.

A and B are two tribal classes; 1 and 2 are two other and independent divisions of the tribe (which are probably by totems). Any person taken at random is equally likely to have either letter or either numeral, and his or her numeral and letter are well known to all the community. Hence the members of the tribe are sub-classed into four sub-divisions, A1, A2, B1, B2. The rule is that a man may marry those women only whose letter and numeral are both different to his own. Thus, A1 can marry only B2, the other three sub-divisions A1, A2, and B1 being absolutely barred to him. As to the children, there is a difference of practice in different parts: in the cases most often described, the child takes its father's letter and its mother's numeral, which determines class by paternal descent. In other cases the arrangement runs in the contrary way, or by maternal descent.

The cogency of this rule is due to custom, religion and law, and is so strong that nearly all Australians would be horrified at the idea of breaking it. If any one dared to do so, he would probably be clubbed to death.

Here then is another restriction to the freedom of marriage which might with equal propriety have been applied to the furtherance of some form of Eugenics.

5. Taboo. The survival of young animals largely depends on their inherent timidity, their keen sensitiveness to warnings of danger by their parents and others, and to their tenacious recollection of them. It is so with human children, who are easily terrified by nurses' tales, and thereby receive more or less durable impressions.

A vast complex of motives can be brought to bear upon the naturally susceptible minds of children, and of uneducated adults who are mentally little more than big children. The constituents of this complex are not sharply distinguishable, but they form a recognisable whole that has not yet received an appropriate name, in which religion, superstition, custom, tradition, law and authority all have part. This group of motives will for the present purpose be entitled "immaterial," in contrast to material ones. My contention is that the experience of all ages and all nations shows that the immaterial motives are frequently far stronger than the material ones, the relative power of the two being well illustrated by the tyranny of taboo in many instances, called as it is by different names in different places. The facts relating to taboo form a voluminous literature, the full effect of which cannot be conveyed by brief summaries. It shows how, in most parts of the world, acts that are apparently insignificant have been invested with ideal importance, and how the doing of this or that has been followed by outlawry or death, and how the mere terror of having unwittingly broken a taboo may suffice to kill the man who broke it. If non-eugenic unions were prohibited by such taboos, none would take place.

6. PROHIBITED DEGREES. The institution of marriage, as now sanctified by religion and safeguarded by law in the more highly civilised nations, may not be ideally perfect, nor may it be universally accepted in future times, but it is the best that has hitherto been devised for the parties primarily concerned, for their children, for home life, and for society. The degrees of kinship within which marriage is prohibited, is with one exception quite in accordance with modern sentiment, the exception being the disallowal of marriage with the sister of a deceased wife, the propriety of which is greatly disputed and need not be discussed here. The marriage of a brother and sister would excite a feeling of loathing among us that seems implanted by nature, but which further inquiry will show, has mainly arisen from tradition and custom.

We will begin by giving due weight to certain assigned motives. (1) Indifference and even repugnance between boys and girls, irrespectively of relationship, who have been reared in the same barbarian home. (2) Close likeness, as between the members of a thorough-bred stock, causes some sexual indifference: thus highly bred dogs lose much of their sexual desire for one another, and are apt to consort with mongrels. (3) Contrast is an element in sexual attraction which has not yet been discussed quantitatively. Great resemblance creates indifference, and great dissimilarity is repugnant. The maximum of attractiveness must lie somewhere between the two, at a point not yet ascertained. (4) The harm due to continued interbreeding has been considered, as I think, without sufficient warrant, to cause a presumed strong natural and instinctive *repugnance* to the marriage of near kin. The facts are that close and continued interbreeding invariably does harm after a few generations, but that a single cross with near kinsfolk is practically innocuous. Of course a sense of repugnance might become correlated with any harmful practice, but there is no evidence that it is *repugnance* with which interbreeding is correlated, but only *indifference*, which is equally effective in preventing it, but quite another thing. (5) The strongest reason of all in civilised countries appears to be the earnest desire not to infringe the sanctity and freedom of the social relations of a family group, but this has nothing to do with instinctive sexual repugnance. Yet it is through the latter motive alone, so far as I can judge, that we have acquired our apparently instinctive horror of marrying within near degrees.

Next as to facts. History shows that the horror now felt so strongly did not exist in early times. Abraham married his half-sister Sarah, "she is indeed the sister, the daughter of my father, but not the daughter of my mother, and she became my wife." (Gen. xx., 12). Amram, the father of Moses and Aaron, married his aunt, his father's sister Jochabed. The Egyptians were accustomed to marry sisters. It is unnecessary to go earlier back in Egyptian history than to the Ptolemies, who, being a new dynasty, would not have dared to make the marriages they did in a conservative country, unless popular opinion allowed it. Their dynasty includes the founder, Ceraunus, who is not numbered; the numbering begins with his son Soter, and goes on to Ptolemy XIII., the second husband of Cleopatra. Leaving out her first husband, Ptolemy XII., as he was a mere boy, and taking in Ceraunus, there are thirteen Ptolemies to be considered. Between them, they contracted eleven incestuous marriages, eight with whole sisters, one with a half-sister, and two with nieces. Of course, the object was to keep the royal line pure, as was done by the ancient Peruvians. It would be tedious to follow out the laws enforced at various times and in the various states of Greece during the classical ages. Marriage was at one time permitted in Athens between half-brothers and half-sisters, and the marriage between uncle and niece was thought commendable in the time of Pericles, when it was prompted by family considerations. In Rome the practice varied much, but there were always severe restrictions. Even in its dissolute period, public opinion was shocked by the marriage of Claudius with his niece.

A great deal more evidence could easily be adduced, but the foregoing suffices to prove that there is no instinctive repugnance felt universally by man to marriage within the prohibited degrees, but that its present strength is mainly due to what I called immaterial considerations. It is quite conceivable that a non-eugenic marriage should hereafter excite no less loathing than that of a brother and sister would do now.

7. Celibacy. The dictates of religion in respect to the opposite duties of leading celibate lives, and of continuing families, have been contradictory. In many nations it is and has been considered a disgrace to bear no children, and in other nations celibacy has been raised to the rank of a virtue of the highest order. The ascetic character of the African portion of the early Christian church, as already remarked, introduced the merits of celibate life into its teaching. During the fifty or so generations that have elapsed since the establishment of Christianity, the nunneries and monasteries, and the celibate lives of Catholic priests, have had vast social effects, how far for good and how far for evil need not be discussed here. The point I wish to enforce is the potency, not only of the religious sense in aiding or deterring marriage, but more especially the influence and authority of ministers of religion in enforcing celibacy. They have notoriously used it when aid has been invoked by members of the family on grounds that are not religious at all, but merely of family expediency. Thus, at some times and in some Christian nations, every girl who did

not marry while still young, was practically compelled to enter a nunnery from which escape was afterwards impossible.

It is easy to let the imagination run wild on the supposition of a whole-hearted acceptance of Eugenics as a national religion; that is of the thorough conviction by a nation that no worthier object exists for man than the improvement of his own race; and when efforts as great as those by which nunneries and monasteries were endowed and maintained should be directed to fulfil an opposite purpose. I will not enter further into this. Suffice it to say, that the history of conventual life affords abundant evidence on a very large scale, of the power of religious authority in directing and withstanding the tendencies of human nature towards freedom in marriage.

Conclusion.—Seven different subjects have now been touched upon. They are monogamy, endogamy, exogamy, Australian marriages, taboo, prohibited degrees and celibacy. It has been shown under each of these heads how powerful are the various combinations of immaterial motives upon marriage selection, how they may all become hallowed by religion, accepted as custom and enforced by law. Persons who are born under their various rules live under them without any objection. They are unconscious of their restrictions, as we are unaware of the tension of the atmosphere. The subservience of civilised races to their several religious superstitions, customs, authority and the rest, is frequently as abject as that of barbarians. The same classes of motives that direct other races direct ours, so a knowledge of their customs helps us to realise the wide range of what we may ourselves hereafter adopt, for reasons as satisfactory to us in those future times, as theirs are or were to them at the time when they prevailed.

Reference has frequently been made to the probability of Eugenics hereafter receiving the sanction of religion. It may be asked, "how can it be shown that Eugenics fall within the purview of our own?" It cannot, any more than the duty of making provision for the future needs of oneself and family, which is a cardinal feature of modern civilisation, can be deduced from the Sermon on the Mount. Religious precepts, founded on the ethics and practice of olden days, require to be reinterpreted to make them conform to the needs of progressive nations. Ours are already so far behind modern requirements that much of our practice and our profession cannot be reconciled without illegitimate casuistry. It seems to me that few things are more needed by us in England than a revision of our religion, to adapt it to the intelligence and needs of the present time. A form of it is wanted that shall be founded on reasonable bases and enforced by reasonable hopes and fears, and that preaches honest morals in unambiguous language, which good men who take their part in the work of the world, and who know the dangers of sentimentalism, may pursue without reservation.

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II.

STUDIES IN NATIONAL EUGENICS

By Francis Galton, F.R.S., D.C.L., Sc.D.

Communicated at a meeting of the Sociological Society held in the School of Economics and Political Science (University of London), Clare Market, W.C., on Tuesday, February 14th, 1905.

It was stated in the *Times*, January 26, 1905, that at a meeting of the Senate of the University of London, Mr. Edgar Schuster, M.A., of New College, Oxford, was appointed to the Francis Galton Research Fellowship in National Eugenics. "Mr. Schuster will in particular carry out investigations into the history of classes and families, and deliver lectures and publish memoirs on the subjects of his investigations."

Now that this appointment has been made, it seems well to publish a suitable list of subjects for eugenic inquiry. It will be a programme that binds no one, not even myself, for I have not yet had the advantage of discussing it with others, and may hereafter wish to largely revise and improve what is now provisionally sketched. The use of this paper lies in its giving a general outline of what, according to my present view, requires careful investigation, of course not all at once, but step by step, at possibly long intervals.

I. Estimation of the average quality of the offspring of married couples, from their personal and ancestral data. This includes questions of fertility, and the determination of the "probable error" of the estimate for individuals, according to the data employed.

(a) "Biographical Index to Gifted Families," modern and recent, for publication. It might be drawn up on the same principle as my "Index to Achievements of Near Kinsfolk of Some of the Fellows of the Royal Society" (see "Sociological Papers," Vol. I., p. 85). The Index refers only to facts creditable to the family, and to such of these as have already appeared in publications, which are quoted as authority for the statements. Other biographical facts that may be collected concerning these families are to be preserved for statistical use only.

(b) Biographies of capable families, who do not rank as "gifted," are to be collected, and kept in MS., for statistical use, but with option of publication.

(c) Biographies of families, who, as a whole, are distinctly below the average in health, mind, or physique, are to be collected. These include the families of persons in asylums of all kinds, hospitals, and prisons. To be kept for statistical use only.

(d) Parentage and progeny of representatives of each of the social classes of the community, to determine how far each class is derived from, and contributes to, its own and the other classes. This inquiry must be carefully planned beforehand.

(e) Insurance office data. An attempt to be made to carry out the suggestions of Mr. Palin Egerton, "Sociological Papers," Vol. I., p. 62, of obtaining material that the authorities would not object to give, and whose discussion might be advantageous to themselves as well as to Eugenics. The matter is now under consideration, so more cannot be said.

II. Effects of action by the State and by Public Institutions.

(f) Habitual criminals. Public opinion is beginning to regard with favour the project of a prolonged segregation of habitual criminals, for the purpose of restricting their opportunities for (1) continuing their depredations, and (2) producing low class offspring. The inquiries spoken of above (see *c*) will measure the importance of the latter object.

(g) Feeble-minded. Aid given to Institutions for the feeble-minded are open to the suspicion that they may eventually promote their marriage and the production of offspring like themselves. Inquiries are needed to test the truth of this suspicion.

(h) Grants towards higher education. Money spent in the higher education of those who are intellectually unable to profit by it lessens the

sum available for those who can do so. It might be expected that aid systematically given on a large scale to the more capable would have considerable eugenic effect, but the subject is complex and needs investigation.

(*i*) Indiscriminate charity, including out-door relief. There is good reason to believe that the effects of indiscriminate charity are notably non-eugenic. This topic affords a wide field for inquiry.

III. Other influences that further or restrain particular classes of marriage.

The instances are numerous in recent times in which social influences have restrained or furthered freedom of marriage. A judicious selection of these would be useful, and might be undertaken as time admits. I have myself just communicated to the Sociological Society a memoir entitled "Restrictions in Marriage," in which remarkable instances are given of the dominant power of religion, law and custom. This will suggest the sort of work now in view, where less powerful influences have produced statistical effects of appreciable amount.

IV. Heredity.

The facts after being collected are to be discussed, for improving our knowledge of the laws both of actuarial and of physiological heredity, the recent methods of advanced statistics being of course used. It is possible that a study of the effect on the offspring of differences in the parental qualities may prove important.

It is to be considered whether a study of Eurasians, that is, of the descendants of Hindoo and English parents, might not be advocated in proper quarters, both on its own merits as a topic of national importance and as a test of the applicability of the Mendelian hypotheses to men. Eurasians have by this time intermarried during three consecutive generations in sufficient numbers to yield trustworthy results.

V. Literature.

A vast amount of material that bears on Eugenics exists in print, much of which is valuable and should be hunted out and catalogued. Many scientific societies, medical, actuarial, and others, publish such material from time to time. The experiences of breeders of stock of all kinds, and those of horticulturists, fall within this category.

VI. Co-operation.

After good work shall have been done and become widely recognised, the influence of eugenic students in stimulating others to contribute to their inquiries may become powerful. It is too soon to speculate on this, but every good opportunity should be seized to further co-operation, as well as the knowledge and application of Eugenics.

VII. Certificates.

In some future time, dependent on circumstances, I look forward to a suitable authority issuing Eugenic certificates to candidates for them. They would imply a more than an average share of the several qualities of at least goodness of constitution, of physique, and of mental capacity. Examinations upon which such certificates might be granted are already carried on, but separately; some by the medical advisers of insurance offices, some by medical men as to physical fitness for the army, navy and Indian services, and others in the ordinary scholastic examinations. Supposing constitution, physique and intellect to be three independent variables (which they are not), the men who rank among the upper third of each group would form only one twenty-seventh part of the population. Even allowing largely for the correlation of those qualities, it follows that a moderate severity of selection in each of a few particulars would lead to a severe all-round selection. It is not necessary to pursue this further.

The above brief memorandum does not profess to deal with more than the pressing problems in Eugenics. As that science becomes better known, and the bases on which it rests are more soundly established, new problems will arise, especially such as relate to its practical application. All this must bide its time; there is no good reason to anticipate it now. Of course, useful suggestions in the present embryonic condition of Eugenic study would be timely, and might prove very helpful to students.

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DISCUSSION

DR. A. C. HADDON SAID:

We have been greatly favoured this afternoon in listening to one who has devoted his life to science and has just presented us, in so able a paper, with the conclusions of his mature age. Future generations will hold the name of Mr. Galton in high reverence for the work he has done in so firmly establishing the theory of evolution, and I consider that we have listened to a memorable paper which will mark a definite stage in the history of the subject with which Mr. Galton's name will remain imperishably associated. It is refreshing, if Mr. Galton will allow me to say so, to find a man of his years formulating such a progressive policy, for this is generally supposed to be a characteristic of younger men, but he has done so because all his life he has been studying evolution. He has seen what evolution has accomplished amongst the lower animals; he has seen what man can do to improve strains of animals and plants by means of careful selection; and he foresees what man may do in the future to improve his own species by more careful selection. It is possible for people to change their customs, ideas and ideals. We are always accustomed to regard the savages as conservative, and so they are, but, as a matter of fact, savages do change their views. In Australia we find that different tribes have different marriage customs and different social regulations, and it will be generally found that the change in marriage custom or social control is nearly always due to betterment in their physical conditions. The tribes which, as some of us believe, have the more primitive marital arrangements, are those which live in the least favoured countries; and the tribes who have adopted father-right are those who live under more favourable conditions. In Melanesia, Africa, and in India, social customs vary a very great deal, and this proves that even their marriage customs are not in any way hide-bound, and that social evolution is taking place. When circumstances demand a change, then a change takes place, perhaps more or less automatically, being due to a sort of natural selection. There are thinking people among savages, and we have evidence that they do consider and

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discuss social customs, and even definitely modify them; but, on the whole, there appears to be a general trend of social factors that cause this evolution. There is no reason why social evolution should continue to take place among ourselves in a blind sort of way, for we are intelligent creatures, and we ought to use rational means to direct our own evolution. Further, with the resources of modern civilisation, we are in a favourable position to accelerate this evolution. The world is gradually becoming self-conscious, and I think Mr. Galton has made a very strong plea for a determined effort to attempt a conscious evolution of the race.

Dr. F. W. MOTT said:

I have to say, I think it is of very great importance to the nation to consider this subject of Eugenics very seriously. Being engaged as pathologist to the London County Council Asylums, I see the effect of heredity markedly on the people admitted into the Asylums. The improvement of the stock can in my opinion be brought about in two ways:—(1) By segregation, to some extent carried on at present, which in some measure, checks the reproduction of the unfit; and (2) by encouraging the reproduction of the fit. Checking the reproduction of the unfit is quite as important as encouraging the reproduction of the fit. This, in my opinion, could be effected to some extent, by taking the defective children and keeping them under control, at least a certain number that are at present allowed to have social privileges. It would be for their own welfare and the welfare of the community; and they would suffer no hardship if taken when quite young. This is included in the question of Eugenics which Mr. Galton has brought forward, and has shown his practical sympathy with, by establishing a Fellowship, which will, no doubt, do great good in placing the subject on a firm basis, and also in getting a wide intellectual acceptance of the principle. It seems to me the first thing required is that it should become generally known that it is to the advantage of the individual and of the race to have a healthy heritage. Whether any practical steps could be taken to forward this principle when it has a widespread acceptance, is a question; and I consider that any State interference would be harmful at first, but it would be proper for the State to encourage setting up registry offices where not only a form would be given, with particulars as to marriage, but also a form that would give a bill of health to the contracting parties; and that bill of health should be of some value not only to the possessors, but to their children. If children had a good heritage, there is no doubt it would have actuarial value, in the matter, for instance, of obtaining life insurance policies at a more reasonable rate; also in obtaining municipal and government employment, because the chances of paying pensions to people who have a good heritage, is very much less. It seems to me that the subject is one of national importance,

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and this Society, by spreading the views of Mr. Galton, will do, not only a very great work for individuals, but for the race as a whole.

Mr. A. E. CRAWLEY said:

Mr. Galton's remarkable and suggestive paper shows how anthropological studies can be made fruitful in practical politics. Sociology should be founding its science of eugenics upon anthropology, psychology, and physiology. I hope that it will avoid socialistic dreams and that, while chiefly considering the normal individual, it will not forget the special claims of those abnormal persons whom we call geniuses. In a well-ordered state they should be considered before the degenerate and the diseased.

With regard to one or two minor matters: I should like to ask the author if he has examined the evidence for McLennan's examples of marriage by capture. It is not, perhaps, a very important point, but anthropological theories are often houses of cards, and I doubt the existence of a single real case of capture as an institution. As to exogamy, it is important to understand that in the great majority of cases it is really endogamous, that is to say, the favourite marriage in exogamy is between first cousins, and the only constant prohibition is that against the marriage of brothers and sisters. Exogamy, in fact, as Dr. Howitt, Dr. Frazer, and myself agree, reduces to this one principle. McLennan, the inventor of exogamy, never understood the facts, and the term is meaningless. If, as I have suggested in *Nature*, the normal type of primitive marriage was the bisectional exogamy seen in Australia, which amounts to cross-cousin marriage, two families A and B intermarrying for generation after generation—we have found a theory of the origin of the tribe, an enlarged dual family, and we have also worked out a factor which may have done much to fix racial types. Lewis Morgan suggested something of the latter notion as a result of his consanguine family.

I am still persuaded that one or two forms of union are mere "sports," group-marriage, for instance, which is as rare as the marriage of brother and sister. Neither of these can be regarded as the primal type of union, though anthropologists have actually so regarded them. I think we may take it as certain that there are two permanent polar tendencies in human nature, first against union within the same home, and secondly against too promiscuous marriage.

In questions like this, I think it is most important to avoid confusing sexual with matrimonial concerns. It seems to me, on the evidence of history and anthropology, that polygamy is the result of such a confusion. For efficiency and individuality, monogamy is the best foundation of the family. Mr. Galton has not, I think, shown any cause for concluding that the prohibition of polygamy is due to social considerations. Schopenhauer indeed suggested the adoption of polygamy as a solution of the problem created by the preponderance of females, and as likely to do away with what he thought to be a false position, that of the lady—a position due to Christian and chivalrous sentimentalism. His suggestion, by the way, shows the same confusion between sexual and domestic matters, but it certainly would solve many social difficulties. The sexual impulse in men seems to have several normal outlets. In spite of defects the ancient Greeks in their best period seem to show the results of an unconscious eugenic tradition; and I believe the same is true of the Japanese.

Mr. Galton's suggestions as to the part religion may play in these matters seem to me to be excellent. Religion can have no higher duty than to insist upon the sacredness of marriage, but, just as the meaning and content of that sacredness were the result of primitive science, so modern science must advise as to what this sacredness involves for us in our vastly changed conditions, complicated needs, and increased responsibilities.

Dr. ALICE DRYSDALE VICKERY said:

There appeared to her three essentials to success in any attempt to improve the standard of health and development of the human race. These were (1), the economic independence of women, so as to render possible the exercise of selection, on the lines of natural attraction, founded on mental, moral, social, physical and artistic sympathies, both on the feminine and masculine side; (2), the education of the rising generation, both girls and boys, so as to impress them with a sense of their future responsibilities as citizens of the world, as co-partners in the regulation of its institutions, and as progenitors of the future race; (3), an intelligent restriction of the birth-rate, so that children should only be born in due proportion to the requirements of the community, and under conditions which afforded a reasonable prospect of the efficient development of the future citizens.

The present economic dependence of women upon men was detrimental to the physical, intellectual and moral growth of woman, as an individual. It falsified and distorted her views of life, and, as a consequence, her sense of duty. It was above all prejudicial to the interests of the coming generation, for it tended to diminish the free play and adequate development of those maternal instincts on which the rearing and education of children mainly depended. The economic independence of women was desirable in the interests of a true monogamic marriage, for without this economic independence, the individuality of woman could not exercise that natural selective power in the choice of a mate, which was probably a main factor in the spiritual evolution of the race. Where the sympathetic attraction between those concerned was only superficial, instead of being deeply interwoven in all their mutual interests and tastes, the apparent monogamic relation only too frequently masked an unavowed polygamy, or polyandry, or perhaps both. Therefore it would forward truly monogamic marriage if greater facilities should be afforded for the coming together of those who were spontaneously and pre-eminently attracted to each other.

In respect of limitations of offspring, we had to consider both organic and social criteria. For the determination of these, physiologist must combine with sociologist. From the individual and family point of view, we wanted guidance in determining the size of family adapted to given conditions, and from the social point of view we wanted guidance in determining the numbers of population adapted to a given region at a given time. Incidentally it was here worth noting that in the case of Great Britain, the present birth-rate of 28 per 1000, with death-rate of 15 per 1000, gave an excess of 13 per 1000, compared with a birth-rate of 36 per 1000, and death-rate of 23 per 1000, shown by the vital statistics of 1877; but yet the lower contemporary birth-rate gave the same, or a rather higher, yearly increase, *i.e.*, rather over 400,000 per annum; and with this annual increment of between 400,000 and 500,000, we had to remember that there fell upon the nation the burden of supporting over a million paupers, and a great number of able-bodied unemployed. It seemed, therefore, desirable that sociologists should investigate the conditions and criteria of an optimum increase of population. The remarkable local and class differences in the birth-rate were well known. If the birth-rate of 18 per 1000 and death-rate of 15 per 1000 which prevailed in Kensington could be made universal throughout the United Kingdom, it would give, from our total population of 42 millions, a yearly increment beginning at 130,000. Incidentally she wished to call attention to a paper by M. Gabriel Giroud which went to show that the food supplies of the human race are insufficient, and that one-third of the world's inhabitants exist habitually in a condition of semi-starvation.

The propositions which she desired to submit, were (1), that sexual selection, as determined by the individuality of the natural woman, embodies eugenic tendencies, but that these tendencies are more or less countered and even reversed by a process of matrimonial social selection determined by the economic dependence of woman in contemporary occidental society—in short, that eugenics may be promoted by assuring an income to young women; (2), that artificial control of the birth-rate is a condition of eugenics.

MR. SKRINE SAID:

Mr. Galton, in treating of monogamy, says that polygamy is now permitted to at least one half of the human race. I have lived for twenty-one years amongst polygamists, and having come home to Europe I seem to see conditions prevailing which are not in essence dissimilar. The conclusion I have arrived at is that monogamy is purely a question of social sanction, a question, as it were, of police. In regard to endogamy we may trace back its origin to periods before the dawn of history. The origin of caste and endogamous marriage is due, I believe, to the rise of powerful or intellectual families, which everywhere tend to draw to themselves less powerful families. The higher family was looked up to, and it was thought an honour to marry within it. And thus a small group was formed by a combined process of social and sexual selection. The history of certain group formations determined by this sort of marriage selection might be compiled from that royal stud book, the *Almanac de Gotha.* There is, it is true, the method of evading the selective process by the custom of morganatic marriage, but that only proves the rule. Mr. Galton has not touched on polyandry; that, I think, may be interpreted as one of the devices for limiting population, and can be accounted for, I believe, by scarcity of land.

Dr. WESTERMARCK, speaking from the Chair, SAID:

The members of the Sociological Society have to-day had an opportunity to listen to a most important and suggestive paper, followed by a discussion in which, I am sure, all of us have taken a lively interest. For my own part, I beg to express my profound sympathy and regard for Mr. Galton's ardent endeavours to draw public attention to one of the most important problems with which social beings, like ourselves, could be concerned. Mr. Galton has to-day appealed to historical facts to prove that restrictions in marriage have occurred and do occur, and that there is no reason to suppose that such restrictions might not be extended far beyond the limits drawn up by the laws of any existing civilised nation. I wish to emphasise one restriction not yet touched upon. The husband's and father's function in the family is generally recognised to be to protect and support his wife and children, and many savages take this duty so seriously that they do not allow any man to marry who has not previously given some proof of his ability to fulfil it. Among various Bechuana and Kafir tribes, the youth is not allowed to take a wife until he has killed a rhinoceros. Among the Dyaks of Borneo, and other peoples in the Malay Archipelago, no one can marry unless he has acquired a certain number of human heads by killing members of foreign tribes. Among the Arabs of Upper Egypt the man must undergo an ordeal of whipping by the relations of his bride, and if he wishes to be considered worth having, he must receive the chastisement, which is sometimes exceedingly severe, with an expression of enjoyment. I do not say that these particular methods are to be recommended, but the idea underlying them is certainly worthy of imitation. Indeed we find in Germany and Austria, in the nineteenth century, laws forbidding persons in actual receipt of poor-law relief to contract marriages, and in many cases the legislators went further still and prohibited all marriages until the contracting parties could prove that they possessed the means of supporting a family. Why could not some such laws become universal, and why could not the restrictions in marriage be extended also to persons who, in all probability, would become parents of diseased and feeble offspring? I say, "in all probability," because I do not consider certainty to be required. We cannot wait till biology has said its last word about the laws of heredity. We do not allow lunatics to walk freely about, even though there be merely a suspicion that they may be dangerous. I think that the doctor ought to have a voice in every marriage which is contracted. It is argued, of course, that to interfere here would be to intrude upon the individual's right of freedom. But men are not generally allowed to do mischief simply in order to gratify their own appetites. It will be argued that they will do mischief even though the law prevent them. Well, this holds true of every law, but we do not maintain that laws are useless because there are persons who break them. There will always in this world be offspring of diseased and degenerated parents, but the law may certainly in a very considerable degree restrict their number by preventing such persons from marrying. I think that moral education also might help to promote the object of eugenics. It seems that the prevalent opinion, that almost anybody is good enough to marry, is chiefly due to the fact that in this case the cause and effect, marriage and the feebleness of the offspring, are so distant from each other that the near-sighted eye does not distinctly perceive the connection between them. Hence no censure is passed on him who marries from want of foresight, or want of self-restraint, and by so doing is productive of offspring doomed to misery. But this can never be right. Indeed there is hardly any other point in which the moral consciousness of civilised men still stands in greater need of intellectual training than in its judgments on cases which display want of care or foresight. Much progress has in this respect been made in the course of evolution, and it would be absurd to believe that we have yet reached the end of this process. It would be absurd to believe that men would for ever leave to individual caprice the performance of the most important and, in its consequences, the most far-reaching function which has fallen to the lot of mankind.

DR. DRYSDALE SAID:

He would like to ask the Chairman if he was aware that some of the restrictions he had referred to were actually in force in England? In some of the great English banks, for instance, clerks are not allowed to marry until their salary has reached a certain level. But for his part he thought the principle unsound. Would it not be better to say to these young men that they might marry, but that they must restrict the number of their children?

WRITTEN COMMUNICATIONS

PROFESSOR YVES DELAGE *(Professor of Biology in the University of Paris)*, in a letter to Mr. Galton, wrote:

I am delighted with the noble and very interesting enterprise which you are undertaking.

I have no doubt that if in all countries the men who are at the head of the intellectual movement would give it their support, it would in the end triumph over the obstacles which are caused by indifference, routine, and the sarcasms of those who only see in any new idea the occasion for exercising a satirical spirit, in which they cloak their ignorance and hardness of heart. . . . . . .

We should translate "eugenics" into French by "eugonie" or "eugenèse." Could you not, while there is still time, modify the English term into "eugonics" or "eugenesis," in order that it might be the same in both languages?

I see with pleasure that you have had the tact to attack the question on the side by which it can be determined.

Many years ago I had myself examined the subject that you prosecute at this moment, but I had thought only of compulsory, or rather prohibitive means of attaining the object. . . . . . .

You are entirely right in laying aside, at least at the outset, all compulsory or prohibitive means, and in seeking only to initiate a movement of opinion in favour of eugenics, and in trying to modify the mental attitude towards marriage, so that young people, and especially parents, will think less of fortune and social conditions, and more of physical perfection, moral well-being, and intellectual vigour. Social opinion should be modified, so that the opprobrium of mésalliance falls not on the union of the noble with the plebeian, or of the rich with the poor, but on the mating of physical, intellectual, and moral qualities, with the defects of these.

As you have so well put it, public opinion and social convention have a considerable prohibitive force. You will have rendered an incalculable service if you direct these towards eugenics.

The thing is difficult, and will need sustained effort. To impress the public, not only men of science must be asked to help, but those of renown in literature in all countries.

From Dr. HAVELOCK ELLIS.

The significance of Mr. Galton's paper lies less in what is said than what is implied. The title, "Restrictions in Marriage," bristles with questions. We need to know precisely what is meant by "marriage." Among us to-day marriage is a sexual union recognised by law, which is not necessarily entered into for the procreation of children, and, as a matter of fact, frequently remains childless. Mr. Galton seems, however, to mean a sexual union in which the offspring are the essential feature. The distinction is important, for the statements made about one kind of marriage would not hold good for the other. Then, again, by "restrictions" do we mean legal enactments or voluntary self-control?

Mr. Galton summarises some of the well-known facts which show the remarkable elasticity of the institution of marriage. By implication he asks whether it would not be wise further to modify marriage by limiting or regulating procreation, thus introducing a partial or half monogamy, which may perhaps be called—borrowing a term from botany—*hemigamy*. I may point out that a fallacy seems to underlie Mr. Galton's implied belief that the hemigamy of the future, resting on scientific principles, can be upheld by a force similar to that which upheld the sexual taboos of primitive peoples. These had a religious sanction which we can never again hope to attain. No beliefs about benefits to posterity can have the powerful sanction of savage taboos. Primitive marriage customs are not conventions which every one may preach for the benefit of others and any one dispense with for himself.

There is one point in Mr. Galton's paper which I am definitely unable to accept. It seems to be implicitly assumed that there is an analogy between human eugenics and the breeding of domestic animals. I deny that analogy. Animals are bred for points, and they are bred by a superior race of animals, not by themselves. These differences seem fundamental. It is important to breed, let us say, good sociologists; that, indeed, goes without saying. But can we be sure that, when bred, they will rise up and bless us? Can we be sure that they will be equally good in the other relations of life, or that they may not break into fields for which they were not bred and spread devastation? Only a race of supermen, it seems to me, could successfully breed human varieties and keep them strictly chained up in their several stalls.

And if it is asserted that we need not breed for points but for a sort of general all-round improvement, then we are very much in the air. If we cannot even breed fowls which are both good layers and good table birds, is it likely that we can breed men who will not lose at other points what they gain at one? (Moreover, the defects of a quality seem sometimes scarcely less valuable than the quality itself.) We know, indeed, that there are good stocks and bad stocks, and my own small observations have suggested to me that we have scarcely yet realised how subtle and far-reaching hereditary influences are. But the artificial manipulation of human stocks, or the conversion of bad into good, is still all very dubious.

It would be something, however, if we could put a drag on the propagation of definitely bad stocks, by educating public opinion and so helping forward the hemigamy, or whatever it is to be called, that Mr. Galton foresees. When two stocks are heavily tainted, and both tainted in the same direction, it ought to be generally felt that union, for the purposes of procreation, is out of the question. There ought to be a social conscience in such matters. When, as in a case known to me, an epileptic woman conceals her condition from the man she marries, it ought to be felt that an offence has been committed serious enough to annul the marriage contract. At the same time, we must avoid an extreme scrupulosity. It is highly probable that a very slight taint may benefit rather than injure a good stock. There are many people whose intellectual ability, and even virtues as good citizens, seem to be intimately bound up with the stimulating presence of some obscure "thorn in the flesh," some slight congenital taint. To sum up: (1) let us always carefully define our terms; (2) let us, individually as a nation, do our best to accumulate data on this matter, following, so far as we can, the example so nobly set us by Mr. Galton; (3) let us educate public opinion as to the immense gravity of the issues at stake; but (4) in the present state of our knowledge, let us be cautious about laying down practical regulations which may perhaps prove undesirable, and in any case are impossible to enforce.

From Mr. A. H. HUTH

(Author of "The Marriage of Near Kin").

Every one will sympathise with Mr. Galton in his desire to raise the Human Race. He is not the first, and he will not be the last. Long ago the Spartans practised what Mr. Galton has christened "Eugenics"; and in more modern times Frederick I. of Prussia tried something of the sort. I have often thought that if the human race knew what was good for them, they would appoint some great man as Dictator with absolute power for a time. At the expense of some pain to individuals, some loss of liberty for say one generation, what might not be done! Preferably, they should choose me: not because I think myself superior to others, but I would rather make the laws than submit myself to them!

Mr. Galton shows very clearly, and, I think, indisputably, that people do submit to restrictions on marriage of very different kinds, much as if they were laws of nature. Hence the deduction is drawn: that since people submit without (in most cases) a murmur, to restrictions which do not benefit the race, why not artificially produce the same thing in a manner that will benefit the race?

There are, however, two difficulties: One, the smaller, that in our present state of civilisation people will not accept, as they did in the childhood of their race, the doctrine of authority. The other is that all the restrictions on marriage cited by Mr. Galton, with the one exception of celibacy, to which I shall come later, only impeded, but did not prevent marriage. Every man could marry under any of the restrictions, and only very few women could not lawfully be joined to him in matrimony.

Now, what is Mr. Galton's contention? He wishes to hasten the action of the natural law of improvement of the race which works by selection. He wishes to do as breeders have done in creating superior races by the selection of mates. He recognises that, unhappily, we cannot compel people to mate as the scientist directs: they must be persuaded to do so by some sort of creed, which, however, he does not (at least in this paper) expressly define. You could not make a creed that your choice of a wife should be submitted to the approval of a high priest or of a jury. You would not, again, submit the question from a quasi-religious point of view to the like authorities, as to whether you are to marry at all or not. Mr. Galton does indeed point out that people were doomed to celibacy in religious communities: but here you have either a superior authority forcing you to take the vows, or you have the voluntary taking of the vows. Would the undesirable, the weak, the wicked, the frivolous—any of those beings who ought not to propagate their species—take these vows? I fear not. Only the best, those who have strength of mind, the unselfish—in short, only those who should propagate their species—would take the vows with any prospect of respecting them.

I have said that Mr. Galton is seeking to hasten a natural process. We all know the Darwinian law of the selection of the fittest; and also that other law of sexual selection which is constantly going on. I think that even within historical times they have told. I think that if you study the portraits which have come down to us (excluding of course the idealistic productions of the Greeks and some others), if you study even the prints of the grosser multitude, and then walk down any of the more populous streets of London, you will find that you have reason to congratulate the race on a decided general improvement in looks and figure. We have also undoubtedly improved in health and longevity; but this may be due, as also the improvement in looks may be partly due, to improvement in the conditions of life. But with all this, with all these natural forces working untiringly, effectively, and imperceptibly for the improvement of the race, our whole aims as a social body, all our efforts are directed to thwart this natural improvement, to reverse its action, and cause the race not to endeavour to better its best, but to multiply its worst.

The whole tendency of the organised world has been to develop from the system of the production of a very numerous offspring ill fitted to survive, to the production of much fewer offspring better fitted to survive, and guarded at the expense of the parents until they were started in life. This law so permeates the world, and is so general, that it is even true of the higher and lower plains of humanity. The better classes, the more educated, and those capable of greater self-denial, will not marry till they see their way to bring up children in health and comfort and give them a start in life. The lower class, without a thought for the morrow, the wastrels, the ignorant, the selfish, and thoughtless, marry and produce children. Under the ordinary law of nature, of course, the natural result would follow: the children of the more desirable class, though fewer, would survive in greater proportion than the more numerous progeny of the less desirable class, and the race would not deteriorate. But here legislation, and still worse, the so-called philanthropist steps in. Burdens are heaped upon the prudent; they are taxed and bullied, the means which they have denied themselves to save for their own children are taken from them and given to idle vagabonds, in order that their children may be preserved to grow up and reproduce their like. Not only are these children carefully maintained at the costs of the more prudent, but their wretched parents are fed and coddled also at the expense of the more worthy, and saved against themselves to produce more of the—shall I call them kakogenetics? Not content with this, we freely import from the sweepings of Europe, and add them to our breeding stock.

In the days when England made her greatness, she did not suffer from the cankers of wild philanthropy and a promiscuous alien immigration.

From Dr. MAX NORDAU.

The shortness of the time at my disposal, and the vastness of the subject treated by Mr. Galton, do not permit me to deal with the paper as it deserves. I must limit myself to a few "*obiter dicta,*" for the somewhat dogmatic form of which I crave the indulgence of the Sociological Society.

Theoretically, everybody must hail Eugenics. It is a fine and obviously desirable ideal, to direct the evolution of the individual and the race towards the highest possible type of humanity. Practically, however, the matter is so obscure and complicated that it can only be approached with hesitation and misgivings.

We often hear people, even scientists, say: "We breed our domestic animals and useful plants with the greatest care, while no selection and foresight is exercised in the case of the noblest creature—Man." This allusion to the methods of breeding choice cattle implies a biological fallacy. The breeder knows exactly what he wants to develop in his stock: now it is swiftness, now it is staying-power; here it is flesh, there it is wool; in this case it is abundance of milk, in that a capacity for transforming, quickly and completely, food into muscle and fat of a high market value. The breeder is working out the one quality he is aiming at, at the cost of *other* qualities which would be of value to the animal, if not to its owner. The selection practised by the breeder in view of a certain aim, creates new types that may be economically superior, but are biologically inferior. To put it flatly: our vaunted thoroughbreds, the triumph of selection exercised for many generations, may be wonderfully adapted to the one particular end they are destined for; they may flatter our utilitarianism and fetch high prices, but their general vital power is diminished, they are less resistant to the injuries of life, they are subject to diseases far less frequently, or not at all, met with in non-selected animals of their kind; and if not constantly fostered and protected by man, they would be unable to hold their own in the struggle for life.

It is clear that we cannot apply the principles of artificial breeding to man. Which quality of his are we to develop by selection? Of course, there is the ready answer: "*Mens sana in corpore sano.*" But this is so general and vague a rule that it means nothing when it comes to practical application. There is no recognised standard of physical and intellectual perfection. Do you want inches? In that case, you have to shut out from your selection Frederick the Great and Napoleon I., who were undersized; Thiers, who was almost a dwarf; and the Japanese as a nation, as they are considerably below the average of some European races. Yet in all other respects than tallness they are very recommendable specimens of our species. What is your ideal of beauty? Is it a white skin, clear eyes and fair hair? Then you must favour the northern type and exclude the Italian, Spaniard, Greek, etc., from your selection, which would not be to the taste of these nations.

If from somatic we turn to intellectual perfection, we encounter the same difficulties. Some highly gifted individuals have inductive, others deductive talents. You cannot easily have in the same man a great mathematician and a great poet, an inventor and a statesman. You must make up your mind whether you wish to breed artists or scientists, warriors or speculative philosophers. If you say you will breed each of these intellectual categories, each of those physical types, then it amounts to confessing that you will let things pretty much have their own way and that you renounce guiding Nature and directing consciously the species towards an ideal type. If you admit that you have *no* fixed standard of beauty and mental attainment, of physical and intellectual perfection, to propose as the aim of eugenic selection; if your artificial man-breeding is not destined to develop certain well-defined organic qualities, to the detriment of others, then Eugenics means simply that people about to marry should choose handsome, healthy young individuals; and this, I am sorry to say, is a mere triviality, as already, without any scientific consciousness or intervention, people ARE attracted by beauty, health and youth, and repulsed by the visible absence of these qualities.

The principle of sexual selection is the natural promoter of Eugenics; it is a constant factor in biology, and undoubtedly at work in mankind. The immense majority of men and women marry the best individual among those that come within their reach. Only a small minority is guided in its choice by considerations of a social and economical order, which may determine selections to which the natural instinct would object. But even such a choice, contrary as it seems to the principle of Eugenics, might be justified to a certain extent. The noble Ernest Renan would never have been chosen for his physical appearance by any young woman of natural taste; nor would Darmesteter, the great philologist, who was afflicted with gibbosity. Yet these men had high qualities that were well worth being perpetuated in the species. A young and beautiful woman could put in a plausible plea for her marrying an elderly rich financier or nobleman of not very pleasing appearance. In both cases her own organic qualities may vouchsafe fair offspring which will better develop in economically and socially favourable surroundings than it would have done in poverty and obscurity, even if the father had been a much finer specimen of man.

It seems to me that the problem must be approached from another side. There have been pure human races in pre-historical times. Actually every European nation represents a mixture, different in its *proportion* only, of ALL the races of Europe and probably some of Asia and Northern Africa. Probably every European has in his ancestry, representatives of a great number of human types, good and indifferent ones. He is the bearer of all the potentialities of the species. By atavism, any one of the ancestral types may revive in him. Place him in favourable conditions, and there is a fair chance of his developing his potentialities and of his growing into resemblance with the best of his ancestors. The essential thing, therefore, is not so much the selection of particular individuals (every individual having probably latent qualities of the best kind) as the creating of favourable conditions for the development of the good qualities. Marry Hercules with Juno, and Apollo with Venus, and put them in slums—their children will be stunted in growth, rickety and consumptive. On the other hand, take the miserable slum-dwellers *out* of their noxious surroundings, house, feed, clothe them well, give them plenty of light, air and leisure, and their grand-children, perhaps already their children, will reproduce the type of the fine, tall Saxons and Danes of whom they are the offspring.

If Eugenics is only to produce a few Grecian Gods and Goddesses in the sacred circle of the privileged few, it has a merely artistico-æsthetical but no politico-ethnological interest. Eugenics, in order to modify the aspect and value of the nation, must ameliorate not some select groups, but the bulk of the people, and this aim is not to be attained by trying to influence the love-life of the masses. It can be approached only by elevating their standard of life. Redeem the millions of their harrowing care, give them plenty of food and rational hygienics, and allow their natural sympathies to work out their matrimonial choice, and you will have done all the Eugenics that is likely to strengthen, embellish and ennoble the race. In one word: Eugenics, to be largely efficient, must be considered, not as a biological, but as an economical question.

One word more as to the restriction of marriage. There is no doubt that laws and customs have had at all times and in all places, the effect of narrowing the circle within which the matrimonial selection could take place. But I believe it would be an error to conclude that therefore it would be within the power of the legislator to modify these laws and customs, and to create new restrictions unknown before our own time. The old marriage laws and customs had the undisputed authority of religion; they were considered as divine institutions, and superstitious fears prevented transgression. This religious sanction would be absent from modern restricted laws, and in the case of a conflict between passion or desire and legal prohibition, *this* would weigh as a feather against *that*. In a low state of civilisation, the masses obey traditional laws without questioning their authority. Highly differentiated cultured persons have a strong critical sense; they ask of everything the reason why, and they have an irrepressible tendency to be their own lawgivers. These persons would not submit to laws restricting marriage for the sake of vague Eugenics, and if they could not marry under such laws in England, they would marry abroad, unless you dream of a uniform legislation in all countries of the globe, which would indeed be a bold dream.

From Professor A. POSADA

(Professor of Constitutional Law in the University of Oviedo).

Without entering into a discussion of the bases on which Mr. Galton has raised Eugenics as a science I find many very acceptable points of view in all that is proposed by this eminent sociologist.

The history of matrimonial relationship in itself discloses most interesting results. The relative character of its forms, the transitory condition of its laws, the very history of these would seem to show that the reflex action of opinion influences the being and constitution of the human family.

Granting this, and assuming that the actual conditions of the matrimonial regime—especially those that bear upon the manner of contract—

must not be considered as the final term of evolution (since they are far from being ideal), one cannot do less than encourage all that is being done to elucidate the positive nature of matrimonial union and the positive effects resultant from it, whether such union was effected with regard, or disregard, to the exigencies of generation and its influence on descendants.

Marriage is actually contracted either for love or for gain—more often than not the woman marries because she does not enjoy economic independence. In such circumstances, physiological considerations, the influence of heredity, both physiological and moral, have little or no weight—perhaps because they are neither sufficiently known or demonstrated in such a manner that the disastrous effects of their disregard can induce direct motives of conduct.

On this account I think that we should:

(1) Work to elucidate, in as scientific a manner as possible, the requirements of progressive selection in marriage, and we should rigorously demonstrate the consequences of such unions as are decidedly prejudicial to vigorous and healthy offspring.

(2) We should disseminate a knowledge of the conclusions ascertained by scientific investigation and rational statistics, so that these could be gradually assimilated by public opinion and converted into legal and moral obligations, into determinative motives of conduct.

But we must bear in mind that one cannot expect a transformation of actual criteria of sexual relationship, from the mere establishment of a science of eugenics, nor even from the propagation of its conclusions; the problem is thus seen to be very complex.

The actual criteria applied to sexual relationships—especially to those here alluded to—depend on general economic conditions, by virtue of which marriage is contracted under the influence of a multitude of secondary social predispositions, that have no regard to the future of the race; and it is useless to think that any propaganda would be sufficient to overcome the exigencies of economic conditions. On the other hand the actual education of both the woman and the man leaves much to be desired, and more particularly in regard to sexual relationship. And it would be futile to think of any effectual transformation in family life, while both the man and woman do not each of them equally exact, by virtue of an invulnerable repugnance to all that injures morality—a purity of morals in the future spouse.

The day that the woman will refuse as husband the man of impure life, with a repugnance equal to that usually felt by man towards impure womanhood, we shall have made a great step towards the transformation of actual marriage—to the gain of future generations.

FROM PROFESSOR SERGI

(Director of the Museum and Laboratory of Anthropology, University of Rome).

As an abstract proposition, I believe Mr. Galton's proposal is entirely right and has many attractions. But, nevertheless, it seems to me to be not easily practicable and perhaps even impossible.

The sexual relations are vital in the life of all animal species. Any restrictions, to be at all tolerable, must irrefutably demonstrate a great and conspicuous gain. But, unfortunately, we are ignorant of the consequences of restrictions in marriage relations.

It is important in this connection to bear in mind that in modern societies there are certain unmistakable new tendencies at work. These tendencies are all in the direction of dissolving the old restrictions, both religious and social. They constitute, in fact, a movement towards what is called "free love." Now this tendency runs, it seems to me, counter to Mr. Galton's proposals, and makes it particularly difficult to initiate any restrictions of a new form and character.

It is, I believe, an illusion to expect that from any intellectual convictions there may arise a conscious inhibition of sex relations in the population generally. Instances are not wanting of men of high culture marrying women who are the daughters of insane and epileptic parents.

But notwithstanding these objections, which I hold to be a most serious obstacle, and even perhaps fatal to the practical application of Mr. Galton's eugenic principles, nevertheless I believe the studies which, in the second of his two papers to the Sociological Society, he proposes to institute will be both interesting and useful.

FROM DR. R. S. STEINMETZ

(Lecturer on Sociology in the University of Leyden).

I quite agree with Mr. Galton and others (*e. g.*, Dr. Schallmeyer, of Munich, author of "Vererbung und Auslese im Lebenslauf der Volker," 1903) that one of the highest objects of Applied Sociology is the promotion of eugenic marriages. I think there is no worthier object of discussion for a sociological society than that of the means of this promotion. To be sure, the thorough and real knowledge of the true, not the expressed and the reputed motives, for introducing restrictions on marriage might be a means to this end. What we want to know is the real objective cause of these restrictions; there need not, of course, have been any conscious motive at all.

Coming to detailed examination of some points in Mr. Galton's paper on "Restrictions in Marriage," I would ask, is it certain that prohibition of polygamy in Christian nations was due "to considerations of social well-being," as Mr. Galton has it? Surely other causes were also at work. I think, where the number of adult men and women are nearly equal, monogamy is the natural result; polygamy is only possible when by wars and other causes, this proportion is reversed, and when other circumstances, as social inequality, allow some men to take more women than one.

A special distribution of labour between men and women may contribute to this result, but cannot be the cause of it, as every man wants the assistance of more women when he may get them. And in respect of sexual relations, it has to be observed that many men are polygamous in intention, and are only deterred by practical difficulties.

Social inequality, poverty, successful wars are the condition of polygamy. Economical or sexual wants drive men to it.

When these conditions are no longer fulfilled, monogamy will replace it. This is furthered by any rise in the position of women, by the freer play of the purer sentiments between the sexes, and by at least official or public chastity. I believe I am so far in agreement with Westermarck's views on the question. Christianity was very ascetic, as is attested by St. Paul's expressions in the Epistle to the Corinthians. By these ascetic tendencies Christian morals were opposed to polygamy. This tendency was enforced by the Christian ebionistic sympathies, by which all the fathers of the church were governed. Asceticism and social equality can both make for monogamy. Monogamy is certainly in accordance with one very mighty human instinct, that of jealousy; therefore it is the only democratic form of marriage. And I think it is the only one in harmony with the higher sentiments between the sexes, and with a right moral relation between offspring and parents.

But, in considering it, we should never forget that it is largely traversed by irregular love, whether this be sentimental or more sensual, and also by very general prostitution in all ages and classes.

So we must be very cautious in deducing from the fact of monogamy any conclusions as to new and rational marriage regulations, desirable as they may be.

Generally, the term endogamy is employed in a narrower sense than the prohibition of Greeks to marry barbarian women (concubinage with them was allowed, so the restriction was not severe).

I do not consider that Mr. Galton's view of the causes and conditions of endogamy and exogamy is in strict accordance with the results of "anthropology" (the Continental term is "ethnology"); Mr. Galton thinks exogamy is usually to be found in "small and barbarous communities," but combined with the marriage restrictions by blood-ties, and the very general horror of incest, which are only its expression, exogamy is by far the commonest rule of the Chinese; and the Hindus are exogamous in the strict sense, and in the other sense all civilised nations are exogamous, marriage between close kindred being prohibited (Post, "Grundr. Ethn. Jurispr.," 1897, pp. 37-42).

The possibility of the complicated Australian marriage system, of which we know not yet the real motives and causes, does not at all warrant the conclusion that "with equal propriety" it might be applied "to the furtherance of some form of eugenics" among the Australians or among us. The conclusion from the Australians to us stands in need of demonstration. It cannot be assumed. Is it certain that motives of the same strength as those unknown may be found?

The motives for the horror of incest, we do not yet know quite certainly. Perhaps they are the result of very deep-seated and fundamental causes, which suggest the gravest caution in postulating their analogies.

As yet we are even incapable of restraining the very deplorable neo-Malthusian tendencies in the higher classes and some others in all civilised nations, nor those very generally and strongly operating in the eastern United States, in France, in English Australia. We are powerless against the dangers in this direction with which we are threatened by the widely spread feministic movement.*

The race-love of civilised men and women is regretfully feeble. The real problem is first to enforce it. At present the care for future man, the love and respect of the race, are quite beyond the pale of the morals of even the best.

The nobility of old, yea, the patriarchial family generally, entertained a real love and care for the qualities of their offspring. So, perhaps, the turn for this feeling may come again. The intensification of economic and social life will raise the demands on everybody's mental and bodily capabilities; the better knowledge of the hereditary qualities and their signification in attaining the highest degree of capacity will perhaps, and, I think should, in some degree inevitably waken the care for the qualities of one's own offspring.

I put much more hope on this resultant of intensified social demands, of increase and spreading of pathological knowledge, and of evermore enlightened egoism than on public morals embracing the future of the race. Improved care for one's own offspring according to science may possibly come. The result will be a change in our ideas, morals, and morality.

The next measures that then could be taken by the legislator seem to be those formulated by Dr. Schallmeyer in his excellent paper, "Infection als Morgengaber."

Meantime the chief force for progress in eugenic studies is, I think, the accomplishment of the life work of Mr. Galton, and the next is his establishment of a Research Fellowship in National Eugenics.

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* For my own opinions on this, *vide* "Die neueren Forschungen zur Geschichte der menschlichen Familie," *Zeitschrift für Socialwissenschaft*, 1899; *cf.* my "Der Nachwuchs der Begabten" and "Feminismus und Rasse," *Zeitschrift für Socialwissenschaft*, 1904.

It is a shameful reflection for Continental universities that this whole range of studies is neglected by them, and may be fittingly compared to their traditional narrowing of the whole field of social science to economics.

FROM SIR RICHARD TEMPLE.

NOTE I.—STUDIES IN NATIONAL EUGENICS.

Topic I.—It seems to me that definitions of "gifted" and "capable" are required. Are the "gifted" to be those who perform the initiative reasoning, out of which the practical results arise? Are the "capable" to be those who bring into effect the reasoning of the "gifted"? It has always seemed to me that the work accomplished in the world is due to both classes in an equal degree. Neither can be effective without the other. Both are equally important. The success of either demands mental powers of a very high order. I am not at all sure that it is going too far to say of an equally high order. Then there are those who combine in themselves both the capacities, the initiative reasoning and the bringing into effect. Where are these to be placed? Many who possess the one in an eminent degree also possess the other; but, as reasoning and giving effect each requires so much thought and absorbs so much energy and time, the majority have not the opportunity to perform both. I suggest that, as regards family eugenics, both the "gifted" and the "capable" be, if the above definitions are to stand, taken as divisions of one class of mankind. This should be the safest method of bringing the inquiry to a practical result, because of the tendency, so strong in human beings, to look on their own description of work as that which is of the most importance to their kind. The great practical difficulty in the inquiry on the lines indicated, that impresses itself on me is that, especially among women—owing to their place in the world's work,—qualities essential to usefulness are frequently present in individuals who are otherwise possessed of no specially high mental qualities, and are therefore "unknown," and in no way remarkable: such qualities as initiative, discretion, "common sense," perseverance, patience, even temper, energy, courage, and so on, without which the "gifted" and "capable" are apt to be of no practical value to the world. I suggest that progress represents the sum of individual capacities, past and present, at any given period among any given population in any given environment. Then again, in the prosecution of Eugenics by statistics of achievement, there is another great difficulty, which may be best expressed in the words of the Preacher in Ecclesiastes: "I returned, and saw under the sun, that the race is not to the swift, nor the battle to the strong, neither yet bread to the wise, nor yet riches to men of understanding, nor yet favour to men of skill: but time and chance happeneth to them all." Existing social conditions and prejudices, all the

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world over, will force eugenical philosophy to take root very slowly. This is, perhaps, as it should be, in view of the above practical reflection.

Topic VI.—It would appear that a beginning has been made, as regards men, in the Rhodes Scholarships.

Note II.—Restrictions in Marriage.

In one sense, Eugenics is the oldest and most universal philosophy in the world, of which the convention called marriage is the outward and visible sign. Everywhere, among all peoples in all times, marriage has originated for the enforcement and maintenance of real or supposed eugenics. The object of the convention has been fundamentally always the same, the direct personal advantage in some tangible form of a group in its environment. All that can be done by individual philosophers is to give marriage a definite turn in a direction deemed beneficial, because human beings in a mass, in a matter affecting every individual, act upon instinct—defining instinct as unconscious reasoning. In human affairs the outward and visible sign of instinct is custom. By reasoning, instinct can be given a definite direction, and hence a definite form can be given to a custom. This has often been accomplished, but, so far as I can apprehend history, reasoning has only succeeded in creating instinct and thus custom, when the masses subjected to its pressure have been able to see the direct personal advantage to be gained by the line taken. This is the practical point that the eugenical philosopher has to keep ever before him. A custom can be created. The questions for the philosopher are what should be created and how it should be created.

All forms of marriage are due fundamentally to considerations of well-being. Exogamy exists where it is thought important to abnormally increase the numbers of a group. Endogamy exists where it is thought important in a settled community to reserve property and social standing or power for a limited group. Monogamy, polygamy, polyandry are all attempts to maintain social well-being in a form that has seemed obviously advantageous to different groups of human beings. Religion, taboo, and the prohibited degrees are all methods of enforcing custom by moral force. The Australian marriage system is merely a primitive, and therefore complicated, method of enforcing custom. But the human instinct as to incest is something going very deep down, as there is the same kind of instinct in some of the "higher" animals of the two sexes when stabled together, *e.g.*, horses, elephants. Celibacy seems to be due to different causes in different circumstances, according as to whether it is enforced or voluntary. In the former case it is a method of enforcing marriage customs maintained for the supposed common good. In the latter it is due to asceticism, itself an universal instinct based on a philosophy of personal advantage.

The restrictions enforced by marriage customs have led to hypergamy, a *mariage de convenance* exchanging position and property, but really an unreasoning form of eugenics adopted because of the supposed personal advantage, and this has led, in one disastrous form, to female infanticide in a distinctly harmful degree. All the restrictions of marriage are modified in uncivilised communities by promiscuity before marriage and in civilised communities by hetairism. The greater the restrictions the more systematic has hetairism become. Illegitimacy has taken on many almost unrecognisable forms in various parts of the world. It really represents the result of rebellion against convention. Every one of these considerations materially affect any proposition for a reform of Eugenics. Caste is the outward manifestation of an endogamic marriage system introduced by the "intellectuals" of a people for the personal advantage of their own group within the nation, and imitated without reasoning by other groups. This system of endogamic marriage, adopted for the real or supposed advantage of a group, has brought about national disaster, for it has made impossible the instinct of nationality, or the larger group, and has brought the peoples adopting it into perpetual subjection to others possessing the instinct of nationality. Its existence and practical effect is a standing warning to the eugenical philosopher, which should point out to him the extreme care that is necessary in consciously directing eugenics into any given channel.

From Professor TÖNNIES

(Professor of Philosophy in the University of Kiel).

I fully agree with the scope and aims of Mr. Galton's "Eugenics," and consequently with the essence of the two papers proposed. But with respect to details, I have certain objections and illustrations, which I now try to explain.

1. There can be no doubt but the three kinds of accomplishments are desirable in mankind; physical, mental and moral ability. Surely the three, or as Mr. Galton classifies them, constitution—which I understand to imply moral character—physique and intellect, are not independent variables, but if they to a large extent are correlate, on the other hand they also tend to exclude each other, strong intellect being very often connected with a delicate health as well as with poor moral qualities, and *vice versa.* Now the great question, as it appears to me, will be, whether Eugenics is to favour one kind of these excellencies at the *cost of* another one, or of both the other, and which should be preferred under any circumstances.

2. Under existing social conditions it would mean a cruelty to raise the average intellectual capacity of a nation to that of its better moiety of the present day. For it would render people so much more conscious of the dissonance between the hopeless monotony of their toil and the lack of

recreation, poorness of comfort, narrowness of prospects, under which they are even now suffering severely, notwithstanding the dulness of the great multitude.

3. The rise of intellectual qualities also involves, under given conditions, a danger of further decay of moral feeling, nay, of sympathetic affections generally. Town life already produces a race of cunning rascals. Temptations are very strong, indeed, to outrun competitors by reckless astuteness and remorseless tricks. Intelligence promotes egotism and pleasure-seeking, very much in contradiction to the interests of the race.

4. A strong physique seems to be correlate with some portions of our moral nature, but not with all. Refinement of moral feeling and tact are more of an intellectual nature, and again combine more easily with a weak frame and less bodily power.

5. I endorse what Mr. Galton shows—that marriage selection is very largely conditioned by motives based on religious and social consideration; and I accept, as a grand principle, the conclusion that the same class of motives may, in time to come, direct mankind to disfavour unsuitable marriages, so as to make at least some kinds of them impossible or highly improbable, and this would mean an enormous benefit to all concerned, and to the race in general. But I very much doubt if a sufficient unanimity may be produced upon the question—which marriages *are* unsuitable?

6. Of course this unanimity may be promoted by a sufficient study of the effects of heredity. This is the proper and most prominent task of Eugenics, as Mr. Galton luminously points out in his six topics to be taken in hand under the Research Fellowship. Highly though I appreciate the importance of this kind of investigation, to which my own attention has been directed at a very early date, I am apt to believe, however, that the *practical* outcome of them will not be considerable. Our present knowledge, scanty and incoherent as it is, still suffices already to make certain marriages, which are especially favoured by social convention, by religion and by custom, appear to sober-thinking men, highly unsuitable. Science is not likely to gain an influence equivalent to, or even outweighing, those influences that further or restrain particular classes of marriage. On the other hand the voice of Reason, notably with respect to hygienic as well as moral considerations, is often represented by *parents* in contradiction to inclinations or even passions of their offspring (especially daughters); and the prevailing individualistic tendencies of the present age, greatly in favour of individual choice and of the natural right of Love, mostly, or at least very often, dumb that voice of Reason and render it more and more powerless. Eugenics has to contend against the two fronts: against the *mariage de convenance* on the one side, the *mariage de passion* on the other.

7. But this applies chiefly to the upper strata of society, where a certain influence of scientific results may be presumed on principle with greater likelihood than among the multitude. Mr. Galton wishes the

national importance of Eugenics to be introduced into the national conscience like a new religion. I do not believe that this will be possible, unless the conditions of every day existence were entirely revolutionised beforehand. The function of Religion has always been to give *immediate* relief to pressing discomforts, and to connect it with hopeful prospects of an *individual* life to come. The life of the race is a subject entirely foreign to popular feelings, and will continue to be so, unless the mass should be exempt from daily toil and care, to a degree which we are unable to realise at present.

8. However, the first and main point is to secure the general *intellectual* acceptance of Eugenics as a hopeful and most important study. I willingly and respectfully give my fullest sympathy and approval to this claim.

I have tried to express my sentiments here as evoked by the two most interesting papers. I have been obliged to do so in great haste, and consequently, as I am aware, in very bad English, for which I must apologise.

From Professor AUGUST WEISMANN.

It has given me great pleasure to learn that a Sociological Society has been formed in England, and to see that so many distinguished names are associated with its inauguration and proceedings.

As for the request that I should send "an expression of my views on the subject" of Mr. Galton's two papers, I fear I can have nothing to say that will be at all new.

I think there is one question, however, of very great importance which has not yet, so far as I know, been investigated, and to which the statistical method alone can supply an answer. It is this:—Whether, when a hereditary disease like tuberculosis has made its appearance in a family it is afterwards possible for it to be entirely banished from this or that branch of the family; or whether, on the contrary, the progeny of these members of the family who appear healthy must not sooner or later produce a tuberculous offspring?

I am fully aware that there exists already a great mass of statistical matter on the subject of "tuberculosis," but I cannot say that it seems to me sufficient, thus far, to justify a sure conclusion.

Speaking for myself, I am disposed, both on theoretic grounds and in view of known facts, to opine that a complete purification and re-establishment of such a family is quite possible in the cases of slighter infection.

For I believe that hereditary transmission in such cases depends upon an infected condition of the seed, germ, or generative cell; that it is conceivable that single generative cells of the parent may remain free from bacilli; that an entirely healthy child may be developed from one such

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generative cell, and that from this sound shoot an entirely healthy branch of the family may grow in time.

I would almost go so far as to say that if this were *not* the case, then there could hardly be a family on earth to-day unaffected by hereditary disease.

Let me ask the Sociological Society to accept this note as merely an indication of my willingness to make at least a very small contribution to the list of those sociological problems which the Society aims at solving.

From the Hon. V. LADY WELBY.

It is obvious that in the question of eugenic restrictions in marriage there are two opposite points of view from which we may work: (1) that of making the most of the race, which concentrates interest, not on the parents—who are then merely, like the organism itself, the germ carriers—but always on the children (in their turn merely race-bearers); and (2) that of making the most of the individual, and thus raising the standard of the whole by raising that of its parts. The problem is to combine these in the future more adequately than has been attempted in the past.

In a small contribution to the discussion on Mr. Galton's first paper I appealed to women to realise more clearly their true place and gift as representing that original racial motherhood, out of which the masculine and feminine characters have arisen. It seems advisable now to take somewhat wider ground.

When, in the interests of an ascending family ideal, we emphasise the need for restrictions on marriage which shall embody all those, as summarised in Mr. Galton's paper, to which human societies have already submitted, we have to consummate a further marriage—one of ideas; we have to combine what may appear to be incompatible aims. In the first place, in order to foster all that makes for a higher and nobler type of humanity than any we have yet known how to realise, we must face the fact that some sacrifice of emotion become relatively unworthy is imperative. Else we weaken "the earnest desire not to infringe the sanctity and freedom of the social relations of a family group." But the sacrifice is of an emotion which has ceased to make for Man and now makes for Self or for reversion to the sub-human.

We are always confronted with a practical paradox. The marriage which makes for the highest welfare of the united man and woman may be actually inimical to the children of that union. The marriage which makes for the highest type of family and its highest and fullest development may often mean, and must always tend to mean, the inhibition of much that makes for individual perfection.

And since the children in their turn will be confronted by the same initial difficulty it may be desirable not only to define our aim and the best methods of reaching it, but to suggest one or two simple prior considerations which are seldom taken into account. One of these is the fact that, speaking generally, human development is a development of the higher brain and its new organ, the hand. It may, I suppose, be said that the rest of the organism has not been correspondingly developed, but remains essentially on the animal level. What especially concerns us here is that this includes the uterine system, which has even tended to retrograde. Here, surely, we have the key to many social and ethical difficulties in the marriage question.

This relatively enormous complexity of brain, disturbing, or at least altering the organic balance, coupled with the sexual incompleteness of the individual, has cost us dear. All such special developments involving comparative overgrowth must do this. In this case we have gained, of course, a priceless analytical, constructive, and elaborative faculty. But there seem to be many indications that we have correspondingly lost a direct and trustworthy reaction to the stimuli of nature in its widest sense, a reaction that should deserve the name of intuition as representing a practically unerring instinct. An eugenic advance secured by an increase of moral sensitiveness on the subject of parentage may well tend to restore on a higher level these primordial responses to excitation of all kinds. But of course it will still rest with education, in all senses and grades, either (as, on the whole, at present) to blunt or distort them, or to interpret and train them into directed and controlled efficiency.

At present our mental history seems to present a curious anomaly. On the one hand we see what, compared with the animal and even with the lower intellectual human types, is an amazing development of logical precision, ordered complexity of reasoning, rigorous validity of conclusion, all ultimately depending for their productive value on the validity of the presuppositions from which they start. On the other hand, this initial validity can but seldom, if ever, be proved experimentally or by argument, or be established by universal experience. Thus the very perfection of the rational development is always liable to lead us further and further astray. The result we see in endless discussions which tend rather to divide than to unite us by hardening into opposed views of what we take for reality, and to confuse or dim the racial outlook and hinder the racial ascent.

It is to be hoped then that one result of the creation of a eugenic conscience will be a restoration of the human balance, bringing about an immensely increased power of revising familiar assumptions and thus of rightly interpreting experience and the natural world. This must make for the solution of pressing problems which at present cannot even be worthily stated. For there is no more significant sign of the present dead-lock resulting from the anomaly just indicated, than the general neglect of the question of effective expression, and therefore of its central value to us; that is, what we are content vaguely to call its meaning.

Such a line of thought may seem, for the very reason of this neglect, far enough from the subject to be dealt with,—from the question of restrictions in marriage. But in the research, studies, and discussions which ought to precede any attempt in the direction of giving effect to an aroused sense of eugenic responsibility, surely this factor will really be all-important. It must be hoped that such discussion will be carried on by those in whom what, for convenience sake, I would call the mother-sense, or the sense of human, even of vital origin and significance, is not entirely overlaid by the priceless power of co-ordinating subtle trains of abstract reasoning. For this supreme power easily defeats itself by failing to examine and rectify the all-potent starting point of its activities, the simple and primary assumption.

I have admitted that the foregoing suggestions—offered with all diffidence—seem to be far from the present subject of discussion, with which, indeed, I have not attempted directly to deal. I would only add that this is not because such questions have not the deepest interest for me, as for all who in any degree realise their urgency.

We shall have to discuss, though I hope in some cases privately, such questions as the influence on descendants of the existence or the lack of reverent love and loyalty between parents, not as "acquired characters," in the controversial sense, but as giving full play to the highest currents of our mental and spiritual life. We shall have to consider the possibilities of raising the whole moral standard of the race, so that the eugenic loyalty shown in instinctive form on the sub-human plane should be reproduced in humanity consciously, purposively, and progressively. Finally, we shall have to reconsider the two cults of Self and Happiness, which we are so prone to make ultimate. The truly eugenic conscience will look upon self as a means and an instrument of consecrated service; and happiness not as an end or an ideal to strive for, since such striving ignobly defeats its own object, but—as sorrow or disappointment may also become—a means or a result of purifying and energising the human activities to an extent as yet difficult to speak of.

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CONTRIBUTORY NOTES

Brief communications were contributed by, amongst others:

Professor B. ALTAMIRA (of the University of Oviedo), who wrote:—"The subjects of Mr. Galton's communications are very interesting, and there should be some very valuable information forthcoming on the forms of marriage (endogamy, exogamy, etc.) to be unearthed from the actual juridical manners and customs of Spain."

Mr. F. CARREL, who wrote:—"I should like to ask Mr. Galton whether the general practice of eclectic mating might not tend to the production of a very inferior residual type, always condemned to mate together until eliminated from an existence in which they would be too unfitted to participate; and, if so, whether such a system can be adopted without inflicting suffering upon the more or less slowly disappearing residuum?"

Mrs. FAWCETT, who wrote:—"Mr. Galton evidently realises that he has a gigantic task before him, that of raising up a new standard of conduct on one of the most fundamental of human relations. At present, the great majority of men and women, otherwise conscientious, seem to have no conscience about their responsibility for the improvement or deterioration of the race. One frequently observes cases of men suffering from mortal and incurable disease who apparently have no idea that it is wrong to have children who will probably enter life heavily handicapped by inherited infirmity. Two thirds of what is called the social evil would disappear of itself, if responsibility for the welfare of the coming generation found its fitting place in the conscience of the average man. I wish all success to Mr. Francis Galton's efforts."

Professor J. G. MCKENDRICK, who wrote:—"Mr. Galton is opening up a subject of great interest and importance—more especially in its relation to improving the physical, mental, and pure qualities of the race. At present much is carried on by haphazard, and I fear the consequence is that we see indications of degeneration in various directions. I heartily wish much success to those who are carrying on investigations of these important problems. We are all indebted to Mr. Galton for his valuable and deeply suggestive papers."

Professor J. H. MUIRHEAD, who wrote:—"I think Mr. Galton's suggestions for the advance of the study and practice of Eugenics most important, and hope our Society may do something to forward the subject."

Professor E. B. POULTON, who wrote:—"I entirely agree with the aims Mr. Galton has in view and profoundly admire his papers on this subject. I think they unfold great possibilities for the human race."

The Hon. BERTRAND RUSSELL, who wrote:—"I have read Mr. Galton's two papers in abstract with much interest, and agree entirely with the view that marriage customs might be modified in a eugenic direction."

Mr. C. A. WITCHELL (*Author of "The Cultivation of Man"*), who wrote:—"There is one factor operating in the selection of husbands and wives which will be extremely difficult to bring within the purview of eugenics, and which is yet supreme in its influence. The union of the sexes in its higher form is not a matter of passion, but of the more powerful and enduring sentiment which we call love. The capturing of mates is not confined to mankind; the polygamous birds exhibit it. But there are birds that sing to win a mate—these have a delayed courtship; and in man this is developed to still nobler ideals. Let a man look around him at a public ball. Would he choose for mother of his children the woman who of all present has the greatest physical attractions? Nothing of the kind. The one he chooses (by instinct) is the one who inspires him with a certain elevation of spiritual sentiment, who, indeed, freezes his physical nature out of his thought—whom he could hardly pay a compliment to, and yet whom he knows he would select from among them all. Why does he choose her? Has he not made selection through the assessors chosen by Nature—certain subtle and undefinable perceptions received through the senses of sight and hearing. These perceptions, fleet and instant messengers, have not been delayed by social distances. They have pierced all the flimsy armour of fashion, they have penetrated the shams of culture, and have told his inmost sense of consciousness—his soul—what hers is like. By that knowledge his soul has chosen hers; and unless science can analyse this subtle process of spiritual selection it must stand aside. By all means let eugenics advance! But let its exponents pause to analyse first what is now the most powerful factor governing the selection of the sexes, and seek to take advantage of it rather than to stifle it with mere physical agencies. To sterilise defective types is one thing; to eliminate the criminally weak and diseased is another —equally reasonable. But let us beware lest we do anything that may tend to obliterate by physical means the higher instinctive teachings of sexual selection."

A MEMBER OF THE SOCIOLOGICAL SOCIETY, who is a well-known writer, but wishes here to remain anonymous:—"My own views are on the side of the largest scope being given to what might be called interference in the matter, and for this reason I should even regret the abrogation of the sister-in-law disability, mistaken as it seems to me on its merits. I mean anything which keeps alive the sense that marriage is the affair of the State seems to me to have a certain value. When one knows, as I do, of a certain physician asking a patient, 'Were your parents first cousins?' and the affirmative answer, one feels certain that here is a realm of duty to which conscience has yet to awaken."

MR. GALTON'S REPLY.

This Society has cause to congratulate itself on the zeal and energy which has brought together so large a body of opinion. We have had verbal contributions from four eminent specialists in anthropology: Dr. Haddon, Dr. Mott, Mr. Crawley and Dr. Westermarck, and numerous written communications have been furnished by well-known persons. At the time that I am revising and extending these words no less than twenty-six contributions to the discussion are in print. Want of space compels me to confine my reply to those remarks that seem more especially to require it, and to do so very briefly, for Eugenics is a wide study, with an uncounted number of side issues into which those who discuss it are tempted to stray. If, however, sure advance is to be made, these issues must be thoroughly explored, one by one, and partial discussion should as far as possible be avoided. To change the simile, we have to deal with a formidable chain of strongholds, which must be severally attacked in force, reduced, and disposed of, before we can proceed freely.

In the first place, it is a satisfaction to find that no one impugns the conclusion which my memoir was written to justify, that history tells how restrictions in marriage, even of an excessive kind, have been contentedly accepted very widely, under the guidance of what I called "immaterial motives." This is all I had in view when writing it.

Certificates.—One of the comments on which I will remark is that if certificates were now offered to those who passed certain examinations into health, physique, moral and intellectual powers, and hereditary gifts, great mistakes would be made by the examiners. I fully agree that it is too early to devise a satisfactory system of marks for giving what might be styled "honour-certificates," because we do not yet possess sufficient data to go upon. On the other hand, there are persons who are exceptionally and unquestionably unfit to contribute offspring to the nation, such as those mentioned in Dr. Mott's bold proposals. The best methods of dealing with these are now ripe for immediate consideration.

Breeding for points.—It is objected by many that there cannot be unanimity on the "points" that it is most desirable to breed for. I fully discussed this objection in my memoir read here last spring, showing that some qualities such as health and vigour were thought by all to be desirable, and the opposite undesirable, and that this sufficed to give a first direction to our aims. It is a safe starting point, though a great deal more has to be inquired into as we proceed on our way. I think that some contributors to this discussion have been needlessly alarmed. No question has been raised by me of breeding men like animals for particular points, to the disregard of all-round efficiency in physical, intellectual (including moral), and hereditary qualifications. Moreover, as statistics have shown, the best qualities are largely correlated. The youths who became judges, bishops, statesmen, and leaders of progress in England could have furnished formidable athletic teams in their times. There is a tale, I know not how far founded on fact, that Queen Elizabeth had an eye to the calves of the legs of those she selected for bishops. There is something to be said in favour of selecting men by their physical characteristics for other than physical purposes. It would decidedly be safer to do so than to trust to pure chance.

The residue.—It is also objected that if the inferior moiety of a race are left to intermarry, their produce will be increasingly inferior. This is certainly an error. The law of "regression towards mediocrity" insures that their offspring, as a whole, will be superior to themselves; and if, as I sincerely hope, a freer action will be hereafter allowed to selective agencies than hitherto, the portion of the offspring so selected would be better still. The influences that now withstand the free action of selective agencies are numerous, they include indiscriminate charity.

Passion of love.—The argument has been repeated that love is too strong a passion to be restrained by such means as would be tolerated at the present time. I regret that I did not express the distinction that ought to have been made between its two stages, that of slight inclination and that of falling thoroughly into love, for it is the first of these rather than the second that I hope the popular feeling of the future will successfully resist. Every match-making mother appreciates the difference. If a girl is taught to look upon a class of men as tabooed, whether owing to rank, creed, connections, or other causes, she does not regard them as possible husbands and turns her thoughts elsewhere. The proverbial "Mrs. Grundy" has enormous influence in checking the marriages she considers indiscreet.

Eugenics as a factor in religion.—Remarks have been made concerning eugenics as a religion; this will be the subject of the brief memoir that follows these remarks.

It is much to be desired that competent persons would severally take up one or other of the many topics mentioned in my second memoir, or others of a similar kind, and work it thoroughly out as they would any ordinary scientific problem; in this way solid progress would be made. I must be allowed to re-emphasise my opinion that an immense amount of investigation has to be accomplished before a definite system of Eugenics can be safely framed.

III.

EUGENICS AS A FACTOR IN RELIGION.

Eugenics strengthens the sense of social duty in so many important particulars that the conclusions derived from its study ought to find a welcome home in every tolerant religion. It promotes a far-sighted philanthropy, the acceptance of parentage as a serious responsibility, and a higher conception of patriotism. The creed of eugenics is founded upon the idea of evolution; not on a passive form of it, but on one that can to some extent direct its own course. Purely passive, or what may be styled mechanical evolution, displays the awe-inspiring spectacle of a vast eddy of organic turmoil, originating we know not how, and travelling we know not whither. It forms a continuous whole from first to last, reaching backward beyond our earliest knowledge and stretching forward as far as we think we can foresee. But it is moulded by blind and wasteful processes, namely, by an extravagant production of raw material and the ruthless rejection of all that is superfluous, through the blundering steps of trial and error. The condition at each successive moment of this huge system, as it issues from the already quiet past and is about to invade the still undisturbed future, is one of violent internal commotion. Its elements are in constant flux and change, though its general form alters but slowly. In this respect, it resembles the curious stream of cloud that sometimes seems attached to a mountain top during the continuance of a strong breeze; its constituents are always changing, though its shape as a whole hardly varies. Evolution is in any case a grand phantasmagoria, but it assumes an infinitely more interesting aspect under the knowledge that the intelligent action of the human will is, in some small measure, capable of guiding its course. Man has the power of doing this largely so far as the evolution of humanity is concerned; he has already affected the quality and distribution of organic life so widely that the changes on the surface of the earth, merely through his disforestings and agriculture, would be recognisable from a distance as great as that of the moon.

As regards the practical side of eugenics, we need not linger to re-open the unending argument whether man possesses any creative power of will at all, or whether his will is not also predetermined by blind forces or by intelligent agencies behind the veil, and whether the belief that man can act independently is more than a mere illusion. This matters little in practice, because men, whether fatalists or not, work with equal vigour whenever they perceive they have the power to act effectively.

Eugenic belief extends the function of philanthropy to future generations, it renders its action more pervading than hitherto, by dealing with families and societies in their entirety, and it enforces the importance of the marriage covenant by directing serious attention to the probable quality of the future offspring. It sternly forbids all forms of sentimental charity that are harmful to the race, while it eagerly seeks opportunity for acts of personal kindness, as some equivalent to the loss of what it forbids. It brings the tie of kinship into prominence and strongly encourages love and interest in family and race. In brief, eugenics is a virile creed, full of hopefulness, and appealing to many of the noblest feelings of our nature.

APPENDIX

The following is an abstract of the paper communicated to the Sociological Society in 1904, under the title,

"EUGENICS: ITS DEFINITION, SCOPE, AND AIMS."*

Eugenics is the science which deals with all influences which improve the inborn qualities of a race; also those that develop them to the utmost advantage.

Postulating existing social groups and existing moral criteria, Eugenics aims at the reproduction of the best specimens of individual—in each of those groups in which the characteristic activity is not demonstrably anti-social (as in criminals).

The practice of Eugenics would thus raise the average quality of a nation to that of its better moiety of the present day: men of an order of ability which is now very rare would become more frequent, because the level out of which they rose would itself have risen.

In respect of practical measures, the endeavour is to bring as many influences as can be reasonably employed, to cause the useful classes of a community to contribute *more* than their proportion to the next generation.

The course of procedure which lies within the functions of a Sociological Society would be somewhat as follows:—

1. Dissemination of a knowledge of the laws of heredity so far as they are surely known, and promotion of their farther study. The knowledge of what may be termed the *actuarial* side of heredity has greatly advanced in recent years. The *average* closeness of kinship in each degree, now admits of exact definition and of being treated mathematically, like birth and death-rates, and the other topics with which actuaries are concerned.

2. Historical inquiry into the rates with which the various classes of society (classified according to civic usefulness) have contributed to the population at various times in ancient and modern nations. There is

* The paper, along with discussion thereon, is printed in full in "Sociological Papers," Vol. I.

strong reason for believing that national rise and decline is closely connected with this influence. It seems to be the tendency of civilisation to check fertility in the higher types, through numerous causes, some of which are well known, others are inferred, and others again are wholly obscure.

3. Systematic collection of facts showing the circumstances under which eugenic families have most frequently originated. The names of such families in England have yet to be learnt, and the conditions under which they have arisen.

The Sociological Society might advantageously initiate such an investigation, by issuing a carefully drafted questionnaire; and thus would accumulate a manuscript collection, of great use for statistical students, and hereafter capable of developing into a "golden book" of thriving families.

4. Influences affecting marriage. Anthropology shows that social influences of all kinds have immense power in modifying marriage customs and conventions. If unsuitable marriages, from the Eugenic point of view, were banned socially, or even regarded with the unreasonable disfavour which some attach to cousin-marriages, very few would be made.

5. Persistence in setting forth the national importance of Eugenics. There are three stages to be passed through. *Firstly*, it must be made familiar as an academic question, until its exact importance has been understood and accepted as a fact. *Secondly*, it must be recognised as a subject whose practical development deserves serious consideration; and *Thirdly*, it must be introduced into the national conscience, like a new religion. By the study and practice of Eugenics, Man may hope to co-operate with Nature in securing that humanity shall be represented by the highest races. What Nature does blindly, slowly and ruthlessly, man may do providently, quickly and kindly. As it lies within his power, so it becomes his duty to work in that direction; just as it is his duty to succour neighbours who suffer misfortune. The improvement of our stock is one of the highest objects that we can reasonably attempt. We are ignorant of the ultimate destinies of humanity, but we may feel perfectly sure that it is as noble a work to raise its level in the sense already explained, as it would be disgraceful to abase it.

The ideals of Eugenics may in time acquire something of religious sanction, but its details must first be worked out sedulously in the study. Over-zeal, leading to hasty action, would do harm by holding out expectations of a near golden age, which would certainly be falsified and cause the science to be discredited. The first and main point is to secure the general intellectual acceptance of Eugenics as a hopeful and most important study. Then let its principles work into the heart of the nation, who will gradually give practical effect to them in ways that we may not wholly foresee.

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from Francis Galton

Restrictions in Marriage.
Studies in National Eugenics.
Eugenics as a Factor in Religion.

FOLLOWED BY AN ABSTRACT OF AN EARLIER MEMOIR

EUGENICS: ITS DEFINITION, SCOPE AND AIMS.

MEMOIRS COMMUNICATED TO THE SOCIOLOGICAL SOCIETY

BY

FRANCIS GALTON, F.R.S., D.C.L., &c.

Extracted from the forthcoming publication of the Society:

SOCIOLOGICAL PAPERS VOL. II.

1905

The Sociological Society.

The Sociological Society was constituted in November 1903, at a meeting in the rooms of the Royal Statistical Society, at which were present representatives of all departments of social investigation as well as of practical interests—political, educational, philanthropic, religious, etc.

The aims of the Society are scientific, educational and practical. It seeks to promote investigation, and to advance education in the social sciences in their various aspects and applications. It aims at affording a common ground on which workers from all fields and schools concerned with social phenomena may meet—economist and historian, psychologist and moralist, anthropologist and archaeologist, geographer and naturalist; as also physician and alienist, criminologist and jurist, hygienist and educationist, philanthropist and social reformer, politician and cleric.

The Sociological Society prosecutes its work by means customary to an efficiently organised learned society, namely, Meetings for Papers and Discussions, the collection of relevant periodical and book literature and by publications. The Society is endeavouring to raise a fund of about £5,000 for a Reference Library of Sociology, and for the issue of sociological publications.

For papers and discussions, the Society annually holds at least six general meetings, at the School of Economics and Political Science (University of London) Clare Market, W.C. (near the Law Courts). These meetings are open free to all members of the Society.

The periodical publications are distributed to members free of charge. The Society will publish each year at least one volume of "Sociological Papers," similar to that containing the papers and discussions of 1904 (pp. xviii., + 292, Macmillan, 1905, 10/6 to non-subscribers).

The foreign correspondents of the Society include many of the most eminent Continental and American sociologists. Membership is open to all interested in sociological studies. Annual subscription, £1 1s (26 francs; 5 dollars; 21 marks); payable in advance on January 1st. Payment for Life Membership, £10 10s. (260 francs; 50 dollars; 210 marks).

Cheques in payment of subscription and of donations should be crossed Clydesdale Bank and forwarded to the Secretary, 5, Old Queen Street, Westminster, S.W., from whom further information can be had.

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Further particulars, Lists of Members, Copies of the Rules, &c., may be obtained on application to

THE SECRETARY,
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5, Old Queen Street,
Westminster, S.W.

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I.

RESTRICTIONS IN MARRIAGE

By Francis Galton, F.R.S., D.C.L., Sc.D.

Read before the Sociological Society, on Tuesday, February 14th, at a meeting in the School of Economics and Political Science (University of London), Clare Market, W.C., Dr. E. Westermarck in the Chair.

It is proposed in the following remarks to meet an objection that has been repeatedly urged against the possible adoption of any system of Eugenics,* namely, that human nature would never brook interference with the freedom of marriage.

In my reply, I shall proceed on the not unreasonable assumption, that when the subject of Eugenics shall be well understood, and when its lofty objects shall have become generally appreciated, they will meet with some recognition both from the religious sense of the people and from its laws. The question to be considered is, how far have marriage restrictions proved effective, when sanctified by the religion of the time, by custom, and by law? I appeal from arm-chair criticism to historical facts.

To this end, a brief history will be given of a few

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* Eugenics may be defined as the science which deals with those social agencies that influence, mentally or physically, the racial qualities of future generations.

widely-spread customs in successive paragraphs. It will be seen that with scant exceptions they are based on social expediency, and not on natural instincts. Each paragraph might have been expanded into a long chapter had that seemed necessary. Those who desire to investigate the subject further can easily do so by referring to standard works in anthropology, among the most useful of which, for the present purpose, are Frazer's *Golden Bough*, Westermarck's *History of Marriage*, Huth's *Marriage of Near Kin*, and Crawley's *Mystic Rose*.

1. Monogamy. It is impossible to label mankind by one general term, either as animals who instinctively take a plurality of mates, or who consort with only one, for history suggests the one condition as often as the other. Probably different races, like different individuals, vary considerably in their natural instincts. Polygamy may be understood either as having a plurality of wives; or, as having one principal wife and many secondary but still legitimate wives, or any other recognised but less legitimate connections; in one or other of these forms it is now permitted—by religion, customs, and law—to at least one-half of the population of the world, though its practice may be restricted to a few, on account of cost, domestic peace, and the insufficiency of females. Polygamy holds its ground firmly throughout the Moslem world. It exists throughout India and China in modified forms, and it is entirely in accord with the sentiments both of men and women in the larger part of negro Africa. It was regarded as a matter of course in the early Biblical days. Jacob's twelve children were born of four mothers all living at the same time, namely, Leah, and her sister, Rachel, and their respective handmaids Billah and Zilpah. Long afterwards, the Jewish kings emulated the luxurious habits of neighbouring potentates and carried polygamy to an extreme degree. For Solomon, see I. Kings, xi. 3. For his son Rehoboam, see II. Chron., xi. 21. The history of the subsequent practice of the custom among the Jews is obscure, but the Talmud contains no law against polygamy. It must have ceased in Judæa by the time of the Christian Era. It was not then allowed in either Greece or Rome. Polygamy

was unchecked by law in profligate Egypt, but a reactionary and ascetic spirit existed, and some celibate communities were formed in the service of Isis, who seem to have exercised a large though indirect influence in introducing celibacy into the early Christian church. The restriction of marriage to one living wife subsequently became the religion and the law of all Christian nations, though licence has been widely tolerated in royal and other distinguished families, as in those of some of our English kings. Polygamy was openly introduced into Mormonism by Brigham Young, who left seventeen wives, and fifty-six children. He died in 1877; polygamy was suppressed soon after (*Encyc. Brit.*, xvi. 827.)

It is unnecessary for my present purpose to go further into the voluminous data connected with these marriages in all parts of the world. Enough has been said to show that the prohibition of polygamy, under severe penalties by civil and ecclesiastical law, has been due not to any natural instinct against the practice, but to consideration of social well-being. I conclude that equally strict limitations to freedom of marriage might, under the pressure of worthy motives, be hereafter enacted for Eugenic and other purposes.

2. ENDOGAMY, or the custom of marrying exclusively *within* one's own tribe or caste, has been sanctioned by religion and enforced by law, in all parts of the world, but chiefly in long settled nations where there is wealth to bequeath and where neighbouring communities profess different creeds. The details of this custom, and the severity of its enforcement, have everywhere varied from century to century. It was penal for a Greek to marry a barbarian, for a Roman patrician to marry a plebeian, for a Hindu of one caste to marry one of another caste, and so forth. Similar restrictions have been enforced in multitudes of communities, even under the penalty of death.

A very typical instance of the power of law over the freedom of choice in marriage, and which was by no means confined to Judæa, is that known as the Levirate. It shows that family property and honour were once held by the Jews to dominate over individual preferences. The Mosaic law actually *compelled* a man to marry the widow of his brother if he left no male issue. (Deuteron. xxv.) Should the brother refuse, "then shall his brother's wife come unto him in the presence of the elders, and loose his shoe from off his foot, and spit in his face; and she shall answer and say, so shall it be done unto the man that doth not build up his brother's house. And his name shall be called in Israel the house of him that hath his shoe loosed." The form of this custom survives to the present day and is fully described and illustrated under the article "Halizah" (=taking off, untying) in the *Jewish Cyclopedia.* Jewish widows are now almost invariably remarried with this ceremony. They are as we might describe it, "given away" by a kinsman of the deceased husband, who puts on a shoe of an orthodox shape which is kept for the purpose, the widow unties the shoe, spits, but now on the *ground*, and repeats the specified words.

The duties attached to family property led to the history, which is very strange to the ideas of the present day, of Ruth's advances to Boaz under the advice of her mother. "It came to pass at midnight" that Boaz "was startled (see marginal note in the Revised Version) and turned himself, and behold a woman lay at his feet," who had come in "softly and uncovered his feet and laid her down." He told her to lie still until the early morning and then to go away. She returned home and told her mother, who said, "Sit still, my daughter, until thou know how the matter will fall, for the man will not rest until he have finished the thing this day." She was right. Boaz took legal steps to disembarrass himself of the claims of a still nearer kinsman, who "drew off his shoe"; so Boaz married Ruth. Nothing could be purer, from the point of view of those days, than the history of Ruth. The feelings of the modern social world would be shocked if the same thing were to take place now in England.

Evidence from the various customs relating to endogamy show how choice in marriage may be dictated by religious custom. That is, by a custom founded on a religious view of family property and family descent. Eugenics deal with what is more valuable than money or lands, namely the heritage of a high character, capable brains, fine physique, and vigour; in short, with all that is most desirable for a family to possess as a birthright. It aims at the evolution and preservation of high races of men, and it as well deserves to be strictly enforced as a religious duty, as the Levirate law ever was.

3. Exogamy is, or has been, as widely spread as the opposed rule of endogamy just described. It is the duty enforced by custom, religion, and law, of marrying *outside* one's own clan, and is usually in force amongst small and barbarous communities. Its former distribution is attested by the survival in nearly all countries, of ceremonies based on "marriage by capture." The remarkable monograph on this subject by the late Mr. McLennan is of peculiar interest. It was one of the earliest, and perhaps the most successful, of all attempts to decipher pre-historic customs by means of those now existing among barbarians, and by the marks they have left on the traditional practices of civilised nations, including ourselves. Before his time those customs were regarded as foolish, and fitted only for antiquarian trifling. In small fighting communities of barbarians, daughters are a burden; they are usually killed while infants, so there are few women to be found in a tribe who were born in it. It may sometimes happen that the community has been recently formed by warriors who have brought no women, and who, like the Romans in the old story, can only supply themselves by capturing those of neighbouring tribes. The custom of capture grows; it becomes glorified because each wife is a living trophy of the captor's heroism; so marriage within the tribe comes to be considered an unmanly, and at last a shameful act. The modern instances of this among barbarians are very numerous.

4. Australian Marriages. The following is a brief clue, and apparently a true one, to the complicated marriage restrictions among Australian bushmen, which are enforced by the penalty of death, and which seem to be partly endogamous

in origin and partly otherwise. The example is typical of those of many other tribes that differ in detail.

A and B are two tribal classes; 1 and 2 are two other and independent divisions of the tribe (which are probably by totems). Any person taken at random is equally likely to have either letter or either numeral, and his or her numeral and letter are well known to all the community. Hence the members of the tribe are sub-classed into four sub-divisions, A1, A2, B1, B2. The rule is that a man may marry those women only whose letter and numeral are both different to his own. Thus, A1 can marry only B2, the other three sub-divisions A1, A2, and B1 being absolutely barred to him. As to the children, there is a difference of practice in different parts: in the cases most often described, the child takes its father's letter and its mother's numeral, which determines class by paternal descent. In other cases the arrangement runs in the contrary way, or by maternal descent.

The cogency of this rule is due to custom, religion and law, and is so strong that nearly all Australians would be horrified at the idea of breaking it. If anyone dared to do so, he would probably be clubbed to death.

Here then is another restriction to the freedom of marriage which might with equal propriety have been applied to the furtherance of some form of Eugenics.

5. Taboo. The survival of young animals largely depends on their inherent timidity, their keen sensitiveness to warnings of danger by their parents and others, and to their tenacious recollection of them. It is so with human children, who are easily terrified by nurses' tales and thereby receive more or less durable impressions.

A vast complex of motives can be brought to bear upon the naturally susceptible minds of children, and of uneducated adults who are mentally little more than big children. The constituents of this complex are not sharply distinguishable, but they form a recognisable whole that has not yet received an appropriate name, in which religion, superstition, custom, tradition, law and authority all have part. This group of motives will for the present purpose be entitled "immaterial" in contrast to material ones. My contention is that the experience of all ages and all nations shows that the immaterial motives are frequently far stronger than the material ones, the relative power of the two being well illustrated by the tyranny of taboo in many instances, called as it is by different names in different places. The facts relating to taboo form a voluminous literature, the full effect of which cannot be conveyed by brief summaries. It shows how, in most parts of the world, acts that are apparently insignificant, have been invested with ideal importance, and how the doing of this or that has been followed by outlawry or death, and how the mere terror of having unwittingly broken a taboo, may suffice to kill the man who broke it. If non-eugenic unions were prohibited by such taboos, none would take place.

6. Prohibited Degrees. The institution of marriage, as now sanctified by religion and safeguarded by law in the more highly civilised nations, may not be ideally perfect, nor may it be universally accepted in future times, but it is the best that has hitherto been devised for the parties primarily concerned, for their children, for home life, and for society. The degrees of kinship within which marriage is prohibited, is with one exception quite in accordance with modern sentiment, the exception being the disallowal of marriage with the sister of a deceased wife, the propriety of which is greatly disputed and need not be discussed here. The marriage of a brother and sister would excite a feeling of loathing among us that seems implanted by nature, but which further inquiry will show, has mainly arisen from tradition and custom.

We will begin by giving due weight to certain assigned motives. (1) Indifference and even repugnance between boys and girls, irrespectively of relationship, who have been reared in the same barbarian home. (2) Close likeness, as between the members of a thorough-bred stock, causes some sexual indifference: thus highly bred dogs lose much of their sexual desire for one another, and are apt to consort with mongrels. (3) Contrast is an element in sexual attraction which has not yet been discussed quantitatively. Great resemblance creates indifference, and great dissimilarity is repugnant. The maximum of attractiveness must lie somewhere between the two, at a point not yet ascertained. (4) The harm due to continued interbreeding has been considered, as I think, without sufficient warrant, to cause a presumed strong natural and instinctive *repugnance* to the marriage of near kin. The facts are that close and continued interbreeding invariably does harm after a few generations, but that a single cross with near kinsfolk is practically innocuous. Of course a sense of repugnance might become correlated with any harmful practice, but there is no evidence that it is *repugnance* with which interbreeding is correlated, but only *indifference*, which is equally effective in preventing it, but quite another thing. (5) The strongest reason of all in civilised countries appears to be the earnest desire not to infringe the sanctity and freedom of the social relations of a family group, but this has nothing to do with instinctive sexual repugnance. Yet it is through the latter motive alone, so far as I can judge, that we have acquired our apparently instinctive horror of marrying within near degrees.

Next as to facts. History shows that the horror now felt so strongly did not exist in early times. Abraham married his half-sister Sarah, "she is indeed the sister, the daughter of my father, but not the daughter of my mother, and she became my wife." (Gen. xx., 12). Amram, the father of Moses and Aaron, married his aunt, his father's sister Jochabed. The Egyptians were accustomed to marry sisters. It is unnecessary to go earlier back in Egyptian history than to the Ptolemies, who, being a new dynasty, would not have dared to make the marriages they did in a conservative country, unless popular opinion allowed it. Their dynasty includes the founder, Ceraunus, who is not numbered; the numbering begins with his son Soter, and goes on to Ptolemy XIII., the second husband of Cleopatra. Leaving out her first husband, Ptolemy XII., as he was a mere boy, and taking in Ceraunus, there are thirteen Ptolemies to be considered. Between them, they contracted eleven incestuous marriages, eight with whole sisters, one with a half-sister, and two with nieces. Of course, the object was to keep the royal line pure, as was done by the ancient Peruvians. It would be tedious to follow out the laws enforced at various times and in the various states of Greece during the classical ages. Marriage was at one time permitted in Athens between half-brothers and half-sisters, and the marriage between uncle and niece was thought commendable in the time of Pericles, when it was prompted by family considerations. In Rome the practice varied much, but there were always severe restrictions. Even in its dissolute period, public opinion was shocked by the marriage of Claudius with his niece.

A great deal more evidence could easily be adduced, but the foregoing suffices to prove that there is no instinctive repugnance felt universally by man to marriage within the prohibited degrees, but that its present strength is mainly due to what I called immaterial considerations. It is quite conceivable that a non-eugenic marriage should hereafter excite no less loathing than that of a brother and sister would do now.

7. Celibacy. The dictates of religion in respect to the opposite duties of leading celibate lives, and of continuing families, have been contradictory. In many nations it is and has been considered a disgrace to bear no children, and in other nations celibacy has been raised to the rank of a virtue of the highest order. The ascetic character of the African portion of the early Christian church, as already remarked, introduced the merits of celibate life into its teaching. During the fifty or so generations that have elapsed since the establishment of Christianity, the nunneries and monasteries, and the celibate lives of Catholic priests, have had vast social effects, how far for good and how far for evil need not be discussed here. The point I wish to enforce is the potency, not only of the religious sense in aiding or deterring marriage, but more especially the influence and authority of ministers of religion in enforcing celibacy. They have notoriously used it when aid has been invoked by members of the family on grounds that are not religious at all, but merely of family expediency. Thus, at some times and in some Christian nations, every girl who did

not marry while still young, was practically compelled to enter a nunnery from which escape was afterwards impossible.

It is easy to let the imagination run wild on the supposition of a whole-hearted acceptance of Eugenics as a national religion; that is of the thorough conviction by a nation that no worthier object exists for man than the improvement of his own race; and when efforts as great as those by which nunneries and monasteries were endowed and maintained should be directed to fulfil an opposite purpose. I will not enter further into this. Suffice it to say, that the history of conventual life affords abundant evidence on a very large scale, of the power of religious authority in directing and withstanding the tendencies of human nature towards freedom in marriage.

Conclusion.—Seven different subjects have now been touched upon. They are monogamy, endogamy, exogamy, Australian marriages, taboo, prohibited degrees and celibacy. It has been shown under each of these heads how powerful are the various combinations of immaterial motives upon marriage selection, how they may all become hallowed by religion, accepted as custom and enforced by law. Persons who are born under their various rules live under them without any objection. They are unconscious of their restrictions, as we are unaware of the tension of the atmosphere. The subservience of civilised races to their several religious superstitions, customs, authority and the rest, is frequently as abject as that of barbarians. The same classes of motives that direct other races, direct ours, so a knowledge of their customs helps us to realise the wide range of what we may ourselves hereafter adopt, for reasons as satisfactory to us in those future times, as theirs are or were to them, at the time when they prevailed.

Reference has frequently been made to the probability of Eugenics hereafter receiving the sanction of religion. It may be asked, "how can it be shown that Eugenics fall within the purview of our own." It cannot, any more than the duty of making provision for the future needs of oneself and family, which is a cardinal feature of modern civilisation, can be deduced from the Sermon on the Mount. Religious precepts, founded on the ethics and practice of olden days, require to be reinterpreted to make them conform to the needs of progressive nations. Ours are already so far behind modern requirements that much of our practice and our profession cannot be reconciled without illegitimate casuistry. It seems to me that few things are more needed by us in England than a revision of our religion, to adapt it to the intelligence and needs of the present time. A form of it is wanted that shall be founded on reasonable bases and enforced by reasonable hopes and fears, and that preaches honest morals in unambiguous language, which good men who take their part in the work of the world, and who know the dangers of sentimentalism, may pursue without reservation.

f.38v

II.

STUDIES IN NATIONAL EUGENICS.

By Francis Galton, F.R.S., L.L.D., Sc.D.

Communicated at a meeting of the Sociological Society held in the School of Economics and Political Science (University of London), Clare Market, W.C., on Tuesday, February 14th, at 4 p.m.

It was stated in the *Times*, January 26, 1905, that at a meeting of the Senate of the University of London, Mr. Edgar Schuster, M.A., of New College, Oxford, was appointed to the Francis Galton Research Fellowship in National Eugenics. "Mr. Schuster will in particular carry out investigations into the history of classes and families, and deliver lectures and publish memoirs on the subjects of his investigations."

Now that this appointment has been made, it seems well to publish a suitable list of subjects for eugenic inquiry. It will be a programme that binds no one, not even myself, for I have not yet had the advantage of discussing it with others, and may hereafter wish to largely revise and improve what is now provisionally sketched. The use of this paper lies in its giving a general outline of what, according to my present view, requires careful investigation, of course not all at once, but step by step, at possibly long intervals.

I. Estimation of the average quality of the offspring of married couples, from their personal and ancestral data. This includes questions of fertility, and the determination of the "probable error" of the estimate for individuals, according to the data employed.

(a) "Biographical Index to Gifted Families," modern and recent, for publication. It might be drawn up on the same principle as my "Index to Achievements of Near Kinsfolk of Some of the Fellows of the Royal Society" (see "Sociological Papers," Vol. I., p. 85). The Index refers only to facts creditable to the family, and to such of these as have already appeared in publications, which are quoted as authority for the statements. Other biographical facts that may be collected concerning these families are to be preserved for statistical use only.

(b) Biographies of capable families, who do not rank as "gifted," are to be collected, and kept in MS., for statistical use, but with option of publication.

(c) Biographies of families, who, as a whole, are distinctly below the average in health, mind, or physique, are to be collected. These include the families of persons in asylums of all kinds, hospitals, and prisons. To be kept for statistical use only.

(d) Parentage and progeny of representatives of each of the social classes of the community, to determine how far each class is derived from, and contributes to, its own and the other classes. This inquiry must be carefully planned beforehand.

(e) Insurance office data. An attempt to be made to carry out the suggestions of Mr. Palin Egerton, "Sociological Papers," Vol. I., p. 62, of obtaining material that the authorities would not object to give, and whose discussion might be advantageous to themselves as well as to Eugenics. The matter is now under consideration, so more cannot be said.

II. Effects of action by the State and by Public Institutions.

(f) Habitual criminals. Public opinion is beginning to regard with favour the project of a prolonged segregation of habitual criminals, for the purpose of restricting their opportunities for (1) continuing their depredations, and (2) producing low class offspring. The enquiries spoken of above (see *c*) will measure the importance of the latter object.

(g) Feeble minded. Aid given to Institutions for the feeble minded are open to the suspicions that they may eventually promote their marriage and the production of offspring like themselves. Inquiries are needed to test the truth of this suspicion.

(h) Grants towards higher education. Money spent in the higher education of those who are intellectually unable to profit by it lessens the

sum available for those who can do so. It might be expected that aid systematically given on a large scale to the more capable would have considerable eugenic effect, but the subject is complex and needs investigation.

(*i*) Indiscriminate charity, including out-door relief. There is good reason to believe that the effects of indiscriminate charity are notably non-eugenic. This topic affords a wide field for inquiry.

III. Other influences that further or restrain particular classes of marriage.

The instances are numerous in recent times in which social influences have restrained or furthered freedom of marriage. A judicious selection of these would be useful, and might be undertaken as time admits. I have myself just communicated to the Sociological Society a memoir entitled "Restrictions in Marriage," in which remarkable instances are given of the dominant power of religion, law, and custom. This will suggest the sort of work now in view, where less powerful influences have produced statistical effects of appreciable amount.

IV. Heredity.

The facts after being collected are to be discussed, for improving our knowledge of the laws both of actuarial and of physiological heredity, the recent methods of advanced statistics being of course used. It is possible that a study of the effect on the offspring of differences in the parental qualities may prove important.

It is to be considered whether a study of Eurasians, that is, of the descendants of Hindoo and English parents, might not be advocated in proper quarters, both on its own merits as a topic of national importance and as a test of the applicability of the Mendelian hypotheses to men. Eurasians have by this time intermarried during three consecutive generations in sufficient numbers to yield trustworthy results.

V. Literature.

A vast amount of material that bears on Eugenics exists in print, much of which is valuable and should be hunted out and catalogued. Many scientific societies, medical, actuarial, and others, publish such material from time to time. The experiences of breeders of stock of all kinds, and those of horticulturists, fall within this category.

VI. Co-operation.

After good work shall have been done and become widely recognised, the influence of eugenic students in stimulating others to contribute to their inquiries may become powerful. It is too soon to speculate on this, but every good opportunity should be seized to further co-operation, as well as the knowledge and application of Eugenics.

VII. Certificates.

In some future time, dependent on circumstances, I look forward to a suitable authority issuing Eugenic certificates to candidates for them. They would imply a more than an average share of the several qualities of at least goodness of constitution, of physique, and of mental capacity. Examinations upon which such certificates might be granted are already carried on, but separately; some by the medical advisers of insurance offices, some by medical men as to physical fitness for the army, navy and Indian services, and others in the ordinary scholastic examinations. Supposing constitution, physique and intellect to be three independent variables (which they are not), the men who rank among the upper third of each group would form only one twenty-seventh part of the population. Even allowing largely for the correlation of those qualities, it follows that a moderate severity of selection in each of a few particulars would lead to a severe all-round selection. It is not necessary to pursue this further.

The above brief memorandum does not profess to deal with more than the pressing problems in Eugenics. As that science becomes better known, and the bases on which it rests are more soundly established, new problems will arise, especially such as relate to its practical application. All this must bide its time; there is no good reason to anticipate it now. Of course, useful suggestions in the present embryonic condition of Eugenic study would be timely, and might prove very helpful to students.

DISCUSSION

Dr. A. C. HADDON said:

We have been greatly favoured this afternoon in listening to one who has devoted his life to science and has just presented us, in so able a paper, with the conclusions of his mature age. Future generations will hold the name of Mr. Galton in high reverence for the work he has done in so firmly establishing the theory of evolution, and I consider that we have listened to a memorable paper which will mark a definite stage in the history of the subject with which Mr. Galton's name will remain imperishably associated. It is refreshing, if Mr. Galton will allow me to say so, to find a man of his years formulating such a progressive policy, for this is generally supposed to be a characteristic of younger men, but he has done so because all his life he has been studying evolution. He has seen what evolution has accomplished amongst the lower animals; he has seen what man can do to improve strains of animals and plants by means of careful selection; and he foresees what man may do in the future to improve his own species by more careful selection. It is possible for people to change their customs, ideas and ideals. We are always accustomed to regard the savages as conservative, and so they are, but, as a matter of fact, savages do change their views. In Australia we find that different tribes have different marriage customs and different social regulations, and it will be generally found that the change in marriage custom or social control is nearly always due to betterment in their physical conditions. The tribes which, as some of us believe, have the more primitive marital arrangements, are those which live in the least favoured countries; and the tribes who have adopted father-right are those who live under more favourable conditions. In Melanesia, Africa, and in India, social customs vary a very great deal, and this proves that even their marriage customs are not in any way hide-bound, and that social evolution is taking place. When circumstances demand a change, then a change takes place, perhaps more or less automatically, being due to a sort of natural selection. There are thinking people among savages, and we have evidence that they do consider and discuss social customs, and even definitely modify them; but, on the whole, there appears to be a general trend of social factors that cause this evolution. There is no reason why social evolution should continue to take place among ourselves in a blind sort of way, for we are intelligent creatures, and we ought to use rational means to direct our own evolution. Further, with the resources of modern civilisation, we are in a favourable position to accelerate this evolution. The world is gradually becoming self-conscious, and I think Mr. Galton has made a very strong plea for a determined effort to attempt a conscious evolution of the race.

Dr. F. W. MOTT said:

I have to say, I think it is of very great importance to the nation to consider this subject of Eugenics very seriously. Being engaged as pathologist to the London County Council Asylums, I see the effect of heredity markedly on the people admitted into the Asylums. The improvement of the stock can in my opinion be brought about in two ways:—(1) By segregation, to some extent carried on at present, which in some measure, checks the reproduction of the unfit; and (2) by encouraging the reproduction of the fit. Checking the reproduction of the unfit is quite as important as encouraging the reproduction of the fit. This, in my opinion, could be effected to some extent, by taking the defective children and keeping them under control, at least a certain number that are at present allowed to have social privileges. It would be for their own welfare and the welfare of the community; and they would suffer no hardship if taken when quite young. This is included in the question of Eugenics which Mr. Galton has brought forward, and has shown his practical sympathy with, by establishing a Fellowship, which will, no doubt, do great good in placing the subject on a firm basis, and also in getting a wide intellectual acceptance of the principle. It seems to me the first thing required is that it should become generally known that it is to the advantage of the individual and of the race to have a healthy heritage. Whether any practical steps could be taken to forward this principle when it has a widespread acceptance, is a question; and I consider that any State interference would be harmful at first, but it would be proper for the State to encourage setting up registry offices where not only a form would be given, with particulars as to marriage, but also a form that would give a bill of health to the contracting parties; and that bill of health should be of some value not only to the possessors, but to their children. If children had a good heritage, there is no doubt it would have actuarial value, in the matter, for instance, of obtaining life insurance policies at a more reasonable rate; also in obtaining municipal and government employment, because the chances of paying pensions to people who have a good heritage, is very much less. It seems to me that the subject is one of national importance, and this Society, by spreading the views of Mr. Galton, will do, not only a very great work for individuals, but for the race as a whole.

MR. A. E. CRAWLEY SAID:

Mr. Galton's remarkable and suggestive paper shows how anthropological studies can be made fruitful in practical politics. Sociology should be founding its science of eugenics upon anthropology, psychology, and physiology. I hope that it will avoid socialistic dreams and that, while chiefly considering the normal individual, it will not forget the special claims of those abnormal persons whom we call geniuses. In a well-ordered state they should be considered before the degenerate and the diseased.

With regard to one or two minor matters: I should like to ask the author if he has examined the evidence for McLennan's examples of marriage by capture. It is not, perhaps, a very important point, but anthropological theories are often houses of cards, and I doubt the existence of a single real case of capture as an institution. As to exogamy, it is important to understand that in the great majority of cases it is really endogamous, that is to say, the favourite marriage in exogamy is between first cousins, and the only constant prohibition is that against the marriage of brothers and sisters. Exogamy, in fact, as Dr. Howitt, Dr. Frazer, and myself agree, reduces to this one principle. McLennan, the inventor of exogamy, never understood the facts, and the term is meaningless. If, as I have suggested in *Nature*, the normal type of primitive marriage was the bisectional exogamy seen in Australia, which amounts to cross-cousin marriage, two families A and B intermarrying for generation after generation—we have found a theory of the origin of the tribe, an enlarged dual family, and we have also worked out a factor which may have done much to fix racial types. Lewis Morgan suggested something of the latter notion as a result of his consanguine family.

I am still persuaded that one or two forms of union are mere "sports," group-marriage, for instance, which is as rare as the marriage of brother and sister. Neither of these can be regarded as the primal type of union, though anthropologists have actually so regarded them. I think we may take it as certain that there are two permanent polar tendencies in human nature, first against union within the same home, and secondly against too promiscuous marriage.

In questions like this I think it is most important to avoid confusing sexual with matrimonial concerns. It seems to me, on the evidence of history and anthropology, that polygamy is the result of such a confusion. For efficiency and individuality, monogamy is the best foundation of the family. Mr. Galton has not, I think, shown any cause for concluding that the prohibition of polygamy is due to social considerations. Schopenhauer indeed suggested the adoption of polygamy as a solution of the problem created by the preponderance of females, and as likely to do away with what he thought to be a false position, that of the lady—a position due to Christian and chivalrous sentimentalism. His suggestion, by the way, shows the same confusion between sexual and domestic matters, but it certainly would solve many social difficulties. The sexual impulse in men seems to have several normal outlets. In spite of defects the ancient Greeks in their best period seem to show the results of an unconscious eugenic tradition; and I believe the same is true of the Japanese.

Mr. Galton's suggestions as to the part religion may play in these matters seem to me to be excellent. Religion can have no higher duty than to insist upon the sacredness of marriage, but, just as the meaning and content of that sacredness were the result of primitive science, so modern science must advise as to what this sacredness involves for us in our vastly changed conditions, complicated needs, and increased responsibilities.

Dr. ALICE DRYSDALE VICKERY

said there appeared to her three essentials to success in any attempt to improve the standard of health and development of the human race. These were (1), the economic independence of women, so as to render possible the exercise of selection, on the lines of natural attraction, founded on mental, moral, social, physical and artistic sympathies, both on the feminine and masculine side; (2), the education of the rising generation, both girls and boys, so as to impress them with a sense of their future responsibilities as citizens of the world, as co-partners in the regulation of its institutions, and as progenitors of the future race; (3), an intelligent restriction of the birth-rate so that children should only be born in due proportion to the requirements of the community, and under conditions which afforded a reasonable prospect of the efficient development of the future citizens.

The present economic dependence of women upon men was detrimental to the physical, intellectual and moral growth of woman, as an individual. It falsified and distorted her views of life, and as a consequence, her sense of duty. It was above all prejudicial to the interests of the coming generation, for it tended to diminish the free play and adequate development of those maternal instincts on which the rearing and education of children mainly depended. The economic independence of women was desirable in the interests of a true monogamic marriage, for without this economic independence, the individuality of woman could not exercise that natural selective power in the choice of a mate, which was probably a main factor in the spiritual evolution of the race. Where the sympathetic attraction between those concerned was only superficial, instead of being deeply interwoven in all their mutual interests and tastes, the apparent monogamic relation only too frequently masked an unavowed polygamy, or polyandry, or perhaps both. Therefore it would forward truly monogamic marriage if greater facilities should be afforded for the coming together of those who were spontaneously and preeminently attracted to each other.

In respect of limitations of offspring, we had to consider both organic and social criteria. For the determination of these, physiologist must combine with sociologist. From the individual and family point of view, we wanted guidance in determining the size of family adapted to given conditions, and from the social point of view we wanted guidance in determining the numbers of population adapted to a given region at a given time. Incidentally it was here worth noting that in the case of Great Britain, the present birth-rate of 28 per 1000, with death-rate of 15 per 1000, giving an excess of 13 per 1000, compared with a birth-rate of 36 per 1000, and death-rate of 23 per 1000, shown by the vital statistics of 1877, but yet the lower contemporary birth-rate gave the same, or a rather higher, yearly increase, *i.e.*, rather over 400,000 per annum, and with this annual increment of between 400,000 and 500,000, we had to remember that there fell upon the nation the burden of supporting over a million paupers, and a great number of able-bodied unemployed. It seemed, therefore, desirable that sociologists should investigate the conditions and criteria of an optimum increase of population. The remarkable local and class differences in the birth-rate were well known. If the birth-rate of 18 per 1000 and death-rate of 15 per 1000 which prevailed in Kensington could be made universal throughout the United Kingdom, it would give from our total population of 42 millions, a yearly increment beginning at 130,000. Incidentally she wished to call attention to a paper by M. Gabriel Giroud which went to show that the food supplies of the human race are insufficient, and that one-third of the world's inhabitants exist habitually in a condition of semi-starvation.

The propositions which she desired to submit, were (1), that sexual selection, as determined by the individuality of the natural woman, embodies eugenic tendencies, but that these tendencies are more or less countered and even reversed by a process of matrimonial social selection determined by the economic dependence of woman in contemporary occidental society—in short, that eugenics may be promoted by assuring an income to young women ; (2), that artificial control of the birth-rate is a condition of eugenics.

Mr. SKRINE said:

Mr. Galton, in treating of monogamy, says that polygamy is now permitted to at least one half of the human race. I have lived for twenty-one years amongst polygamists, and having come home to Europe I seem to see conditions prevailing which are not in essence dissimilar. The conclusion I have arrived at is that monogamy is purely a question of social sanction, a question, as it were, of police. In regard to endogamy we may trace back its origin to periods before the dawn of history. The origin of caste and endogamous marriage is due, I believe, to the rise of powerful or intellectual families, which everywhere tend to draw to themselves less powerful families. The higher family was looked up to, and it was thought an honour to marry within it. And thus a small group was formed by a combined process of social and sexual selection. The history of certain group formations determined by this sort of marriage selection might be compiled from that royal stud book, the *Almanac de Gotha*. There is, it is true, the method of evading the selective process by the custom of morganatic marriage, but that only proves the rule. Mr. Galton has not touched on polyandry; that, I think, may be interpreted as one of the devices for limiting population, and can be accounted for, I believe, by scarcity of land.

Dr. WESTERMARCK, speaking from the Chair, SAID:

The members of the Sociological Society have to-day had an opportunity to listen to a most important and suggestive paper, followed by a discussion in which, I am sure, all of us have taken a lively interest. For my own part, I beg to express my profound sympathy and regard for Mr. Galton's ardent endeavours to draw public attention to one of the most important problems with which social beings, like ourselves, could be concerned. Mr. Galton has to-day appealed to historical facts to prove that restrictions in marriage have occurred and do occur, and that there is no reason to suppose that such restrictions might not be extended far beyond the limits drawn up by the laws of any existing civilised nation. I wish to emphasise one restriction not yet touched upon. The husband's and father's function in the family is generally recognised to be to protect and support his wife and children, and many savages take this duty so seriously that they do not allow any man to marry who has not previously given some proof of his ability to fulfil it. Among various Bechuana and Kafir tribes, the youth is not allowed to take a wife until he has killed a rhinoceros. Among the Dyaks of Borneo, and other peoples in the Malay Archipelago, no one can marry unless he has acquired a certain number of human heads by killing members of foreign tribes. Among the Arabs of Upper Egypt the man must undergo an ordeal of whipping by the relations of his bride, and if he wishes to be considered worth having, he must receive the chastisement, which is sometimes exceedingly severe, with an expression of enjoyment. I do not say that these particular methods are to be recommended, but the idea underlying them is certainly worthy of imitation. Indeed we find in Germany and Austria, in the nineteenth century, laws forbidding persons in actual receipt of poor-law relief to contract marriages, and in many cases the legislators went further still and prohibited all marriages until the contracting parties could prove that they possessed the means of supporting a family. Why could not some such laws become universal, and why could not the restrictions in marriage be extended also to persons who, in all probability, would become parents of diseased and feeble offspring? I say, "in all probability," because I do not consider certainty to be required. We cannot wait till biology has said its last word about the laws of heredity. We do not allow lunatics to walk freely about, even though there be merely a suspicion that they may be dangerous. I think that the doctor ought to have a voice in every marriage which is contracted. It is argued, of course, that to interfere here would be to intrude upon the individual's right of freedom. But men are not generally allowed to do mischief simply in order to gratify their own appetites. It will be argued that they will do mischief even though the law prevent them. Well, this holds true of every law, but we do not maintain that laws are useless because there are persons who break them. There will always in this world be offspring of diseased and degenerated parents, but the law may certainly in a very considerable degree restrict their number by preventing such persons from marrying. I think that moral education also might help to promote the object of eugenics. It seems that the prevalent opinion, that almost anybody is good enough to marry, is chiefly due to the fact that in this case the cause and effect, marriage and the feebleness of the offspring, are so distant from each other that the near-sighted eye does not distinctly perceive the connection between them. Hence no censure is passed on him who marries from want of foresight, or want of self-restraint, and by so doing is productive of offspring doomed to misery. But this can never be right. Indeed there is hardly any other point in which the moral consciousness of civilised men still stands in greater need of intellectual training than in its judgments on cases which display want of care or foresight. Much progress has in this respect been made in the course of evolution, and it would be absurd to believe that we have yet reached the end of this process. It would be absurd to believe that men would for ever leave to individual caprice the performance of the most important and, in its consequences, the most far-reaching function which has fallen to the lot of mankind.

Dr. DRYSDALE

said he would like to ask the Chairman if he was aware that some of the restrictions he had referred to were actually in force in England? In some of the great English banks, for instance, clerks are not allowed to marry until their salary has reached a certain level. But for his part he thought the principle unsound. Would it not be better to say to these young men that they might marry, but that they must restrict the number of their children.

WRITTEN COMMUNICATIONS

Professor YVES DELAGE *(Professor of Biology in the University of Paris)*, in a letter to Mr. Galton, wrote:

I am delighted with the noble and very interesting enterprise which you are undertaking.

I have no doubt that if in all countries the men who are at the head of the intellectual movement would give it their support, it would in the end triumph over the obstacles which are caused by indifference, routine, and the sarcasms of those who only see in any new idea, the occasion for exercising a satirical spirit in which they cloak their ignorance and hardness of heart. . . . . . .

We should translate "eugenics" into French by "eugonie" or "eugenèse." Could you not, while there is still time, modify the English term into "eugonics" or "eugenesis," in order that it might be the same in both languages?

I see with pleasure that you have had the tact to attack the question on the side by which it can be determined.

Many years ago I had myself examined the subject that you prosecute at this moment, but I had thought only of compulsory, or rather prohibitive means of attaining the object. . . . . . .

You are entirely right in laying aside, at least at the outset, all compulsory or prohibitive means, and in seeking only to initiate a movement of opinion in favour of eugenics, and in trying to modify the mental attitude towards marriage, so that young people, and especially parents, will think less of fortune and social conditions, and more of physical perfection, moral wellbeing, and intellectual vigour. Social opinion should be modified, so that the opprobrium of mésalliance falls not on the union of the noble with the plebeian, or of the rich with the poor, but on the mating of physical, intellectual, and moral qualities, with the defects of these.

As you have so well put it, public opinion and social convention have a considerable prohibitive force. You will have rendered an incalculable service if you direct these towards eugenics.

The thing is difficult, and will need sustained effort. To impress the public, not only men of science must be asked to help, but those of renown in literature in all countries.

From Dr. HAVELOCK ELLIS.

The significance of Mr. Galton's paper lies less in what is said than what is implied. The title, "Restrictions in Marriage," bristles with questions. We need to know precisely what is meant by "marriage." Among us to-day marriage is a sexual union recognised by law, which is not necessarily entered into for the procreation of children, and, as a matter of fact, frequently remains childless. Mr. Galton seems, however, to mean a sexual union in which the offspring are the essential feature. The distinction is important, for the statements made about one kind of marriage would not hold good for the other. Then, again, by "restrictions" do we mean legal enactments or voluntary self-control?

Mr. Galton summarises some of the well-known facts which show the remarkable elasticity of the institution of marriage. By implication he asks whether it would not be wise further to modify marriage by limiting or regulating procreation, thus introducing a partial or half monogamy, which may perhaps be called—borrowing a term from botany—*hemigamy*. I may point out that a fallacy seems to underlie Mr. Galton's implied belief that the hemigamy of the future, resting on scientific principles, can be upheld by a force similar to that which upheld the sexual taboos of primitive peoples. These had a religious sanction which we can never again hope to attain. No beliefs about benefits to posterity can have the powerful sanction of savage taboos. Primitive marriage customs are not conventions which everyone may preach for the benefit of others and anyone dispense with for himself.

There is one point in Mr. Galton's paper which I am definitely unable to accept. It seems to be implicitly assumed that there is an analogy between human eugenics and the breeding of domestic animals. I deny that analogy. Animals are bred for points, and they are bred by a superior race of animals, not by themselves. These differences seem fundamental. It is important to breed, let us say, good sociologists; that, indeed, goes without saying. But can we be sure that, when bred, they will rise up and bless us? Can we be sure that they will be equally good in the other relations of life, or that they may not break into fields for which they were not bred and spread devastation? Only a race of supermen, it seems to me, could successfully breed human varieties and keep them strictly chained up in their several stalls.

And if it is asserted that we need not breed for points but for a sort of general all-round improvement, then we are very much in the air. If we cannot even breed fowls which are both good layers and good table birds, is it likely that we can breed men who will not lose at other points what they gain at one? (Moreover, the defects of a quality seem sometimes scarcely less valuable than the quality itself.) We know, indeed, that there are good stocks and bad stocks, and my own small observations have suggested to me that we have scarcely yet realised how subtle and far-reaching hereditary influences are. But the artificial manipulation of human stocks, or the conversion of bad into good, is still all very dubious.

It would be something, however, if we could put a drag on the propagation of definitely bad stocks, by educating public opinion and so helping forward the hemigamy, or whatever it is to be called, that Mr. Galton foresees. When two stocks are heavily tainted, and both tainted in the same direction, it ought to be generally felt that union, for the purposes of procreation, is out of the question. There ought to be a social conscience in such matters. When, as in a case known to me, an epileptic woman conceals her condition from the man she marries, it ought to be felt that an offence has been committed serious enough to annul the marriage contract. At the same time, we must avoid an extreme scrupulosity. It is highly probable that a very slight taint may benefit rather than injure a good stock. There are many people whose intellectual ability, and even virtues as good citizens, seem to be intimately bound up with the stimulating presence of some obscure "thorn in the flesh," some slight congenital taint. To sum up: (1) let us always carefully define our terms; (2) let us, individually as a nation, do our best to accumulate data on this matter, following, so far as we can, the example so nobly set us by Mr. Galton; (3) let us educate public opinion as to the immense gravity of the issues at stake; but (4) in the present state of our knowledge, let us be cautious about laying down practical regulations which may perhaps prove undesirable and in any case are impossible to enforce.

From Mr. A. H. HUTH.

(Author of "The Marriage of Near Kin").

Every one will sympathise with Mr. Galton in his desire to raise the Human Race. He is not the first, and he will not be the last. Long ago the Spartans practised what Mr. Galton has christened "Eugenics"; and in more modern times Frederick I. of Prussia tried something of the sort. I have often thought that if the human race knew what was good for them, they would appoint some great man as Dictator with absolute power for a time. At the expense of some pain to individuals, some loss of liberty for say one generation, what might not be done! Preferably, they should choose me: not because I think myself superior to others, but I would rather make the laws than submit myself to them!

Mr. Galton shows very clearly, and, I think, indisputably, that people do submit to restrictions on marriage of very different kinds, much as if they were laws of nature. Hence the deduction is drawn: that since people submit without (in most cases) a murmur, to restrictions which do not benefit the race, why not artificially produce the same thing in a manner that will benefit the race?

There are, however, two difficulties: One, the smaller, that in our present state of civilisation people will not accept, as they did in the childhood of their race, the doctrine of authority. The other is that all the restrictions on marriage cited by Mr. Galton, with the one exception of celibacy, to which I shall come later, only impeded, but did not prevent marriage. Every man could marry under any of the restrictions, and only very few women could not lawfully be joined to him in matrimony.

Now, what is Mr. Galton's contention? He wishes to hasten the action of the natural law of improvement of the race which works by selection. He wishes to do as breeders have done in creating superior races by the selection of mates. He recognises that, unhappily, we cannot compel people to mate as the scientist directs: They must be persuaded to do so by some sort of creed, which, however, he does not (at least in this paper) expressly define. You could not make a creed that your choice of a wife should be submitted to the approval of a High Priest or of a jury. You would not, again, submit the question from a quasi-religious point of view to the like authorities, as to whether you are to marry at all or not. Mr. Galton does indeed point out that people were doomed to celibacy in religious communities: but here you have either a superior authority forcing you to take the vows, or you have the voluntary taking of the vows. Would the undesirable, the weak, the wicked, the frivolous—any of those beings who ought not to propagate their species—take these vows? I fear not. Only the best, those who have strength of mind, the unselfish—in short, only those who should propagate their species—would take the vows with any prospect of respecting them.

I have said that Mr. Galton is seeking to hasten a natural process. We all know the Darwinian law of the selection of the fittest; and also that other law of sexual selection which is constantly going on. I think that even within historical times they have told. I think that if you study the portraits which have come down to us (excluding of course the idealistic productions of the Greeks and some others), if you study even the prints of the grosser multitude and then walk down any of the more populous streets of London, you will find that you have reason to congratulate the race on a decided general improvement in looks and figure. We have also undoubtedly improved in health and longevity; but this may be due, as also the improvement in looks may be partly due, to improvement in the conditions of life. But with all this, with all these natural forces working untiringly, effectively, and imperceptibly for the improvement of the race, our whole aims as a social body, all our efforts are directed to thwart this natural improvement, to reverse its action, and cause the race not to endeavour to better its best, but to multiply its worst.

The whole tendency of the organised world has been to develop from the system of the production of a very numerous offspring ill fitted to survive, to the production of much fewer offspring better fitted to survive, and guarded at the expense of the parents until they were started in life. This law so permeates the world, and is so general, that it is even true of the higher and lower plains of humanity. The better classes, the more educated, and those capable of greater self-denial, will not marry till they see their way to bring up children in health and comfort and give them a start in life. The lower class, without a thought for the morrow, the wastrels, the ignorant, the selfish, and thoughtless, marry and produce children. Under the ordinary law of nature, of course, the natural result would follow: the children of the more desirable class, though fewer, would survive in greater proportion than the more numerous progeny of the less desirable class, and the race would not deteriorate. But here legislation, and still worse, the so-called philanthropist steps in. Burdens are heaped upon the prudent, they are taxed and bullied, the means which they have denied themselves to save for their own children are taken from them and given to idle vagabonds in order that their children may be preserved to grow up and reproduce their like. Not only are these children carefully maintained at the costs of the more prudent, but their wretched parents are fed and coddled also at the expense of the more worthy, and saved against themselves to produce more of the—shall I call them kakogenetics. Not content with this, we freely import from the sweepings of Europe, and add them to our breeding stock.

In the days when England made her greatness, she did not suffer from the cankers of wild philanthropy and a promiscuous alien immigration.

From Dr. MAX NORDAU.

The shortness of the time at my disposal, and the vastness of the subject treated by Mr. Galton, do not permit me to deal with the paper as it deserves. I must limit myself to a few "*obiter dicta*," for the somewhat dogmatic form of which I crave the indulgence of the Sociological Society.

Theoretically, everybody must hail Eugenics. It is a fine and obviously desirable ideal, to direct the evolution of the individual and the race towards the highest possible type of humanity. Practically, however, the matter is so obscure and complicated that it can only be approached with hesitation and misgivings.

We hear often people, even scientists, say: "We breed our domestic animals and useful plants with the greatest care, while no selection and foresight is exercised in the case of the noblest creature—Man." This allusion to the methods of breeding choice cattle, implies a biological fallacy. The breeder knows exactly what he wants to develop in his stock, now it is swiftness, now it is staying-power; here it is flesh, there it is wool; in this case it is abundance of milk, in that a capacity for transforming, quickly and completely, food into muscle and fat of a high market value. The breeder is working out the one quality he is aiming at, at the cost of *other* qualities which would be of value to the animal, if not to its owner. The selection practised by the breeder in view of a certain aim, creates new types that may be economically superior, but are biologically inferior. To put it flatly: our vaunted thoroughbreds, the triumph of selection exercised for many generations, may be wonderfully adapted to the one particular end they are destined for, they may flatter our utilitarianism and fetch high prices, but their general vital power is diminished, they are less resistant to the injuries of life, they are subject to diseases far less frequently, or not at all, met with in non-selected animals of their kind, and if not constantly fostered and protected by man, they would be unable to hold their own in the struggle for life.

It is clear that we cannot apply the principles of artificial breeding to man. Which quality of his are we to develop by selection? Of course, there is the ready answer: "*Mens sana in corpore sano.*" But this is so general and vague a rule that it means nothing when it comes to practical application. There is no recognised standard of physical and intellectual perfection. Do you want inches? In that case, you have to shut out from your selection Frederick the Great and Napoleon I., who were undersized, Thiers, who was almost a dwarf, and the Japanese as a nation, as they are considerably below the average of some European races. Yet in all other respects than tallness they are very recommendable specimens of our species. What is your ideal of beauty? Is it a white skin, clear eyes and fair hair? Then you must favour the northern type and exclude the Italian, Spaniard, Greek, etc., from your selection, which would not be to the taste of these nations.

If from somatic we turn to intellectual perfection, we encounter the same difficulties. Some highly gifted individuals have inductive, others deductive talents. You cannot easily have in the same man a great mathematician and a great poet, an inventor and a statesman. You must make up your mind whether you wish to breed artists or scientists, warriors or speculative philosophers. If you say you will breed each of these intellectual categories, each of those physical types, then it amounts to confessing that you will let things pretty much have their own way and that you renounce guiding Nature and directing consciously the species towards an ideal type. If you admit that you have *no* fixed standard of beauty and mental attainment, of physical and intellectual perfection to propose as the aim of eugenic selection, if your artificial man-breeding is not destined to develop certain well-defined organic qualities to the detriment of others, then Eugenics means simply that people about to marry should choose handsome, healthy young individuals, and this, I am sorry to say, is a mere triviality, as already, without any scientific consciousness or intervention, people ARE attracted by beauty, health and youth, and repulsed by the visible absence of these qualities.

The principle of sexual selection is the natural promoter of Eugenics; it is a constant factor in biology, and undoubtedly at work in mankind. The immense majority of men and women marry the best individual among those that come within their reach. Only a small minority is guided in its choice by considerations of a social and economical order, which may determine selections to which the natural instinct would object. But even such a choice, contrary as it seems to the principle of Eugenics, might be justified to a certain extent. The noble Ernest Renan would never have been chosen for his physical appearance by any young woman of natural taste; nor would Darmesteter, the great philologist, who was afflicted with gibbosity. Yet these men had high qualities that were well worth being perpetuated in the species. A young and beautiful woman could put in a plausible plea for her marrying an elderly rich financier or nobleman of not very pleasing appearance. In both cases her own organic qualities may vouchsafe fair offspring which will better develop in economically and socially favourable surroundings than it would have done in poverty and obscurity, even if the father had been a much finer specimen of man.

It seems to me that the problem must be approached from another side. There have been pure human races in pre-historical times. Actually every European nation represents a mixture, different in its *proportion* only, of ALL the races of Europe and probably some of Asia and Northern Africa. Probably every European has in his ancestry, representatives of a great number of human types, good and indifferent ones. He is the bearer of all the potentialities of the species. By atavism, any one of the ancestral types may revive in him. Place him in favourable conditions, and there is a fair chance of his developing his potentialities and of his growing into resemblance with the best of his ancestors. The essential thing, therefore, is not so much the selection of particular individuals (every individual having probably latent qualities of the best kind) as the creating of favourable conditions for the development of the good qualities. Marry Hercules with Juno, and Apollo with Venus and put them in slums—their children will be stunted in growth, rickety and consumptive. On the other hand, take the miserable slum-dwellers *out* of their noxious surroundings, house, feed, clothe them well, give them plenty of light, air and leisure, and their grand-children, perhaps already their children, will reproduce the type of the fine, tall Saxons and Danes of whom they are the offspring.

If Eugenics is only to produce a few Grecian Gods and Goddesses in the sacred circle of the privileged few, it has a merely artistico-æsthetical but no politico-ethnological interest. Eugenics, in order to modify the aspect and value of the nation, must ameliorate not some select groups, but the bulk of the people, and this aim is not to be attained by trying to influence the love-life of the masses. It can be approached only by elevating their standard of life. Redeem the millions of their harrowing care, give them plenty of food and rational hygienics, and allow their natural sympathies to work out their matrimonial choice, and you will have done all the Eugenics that is likely to strengthen, embellish and ennoble the race. In one word: Eugenics, to be largely efficient, must be considered, not as a biological, but as an economical question.

One word more as to the restriction of marriage. There is no doubt that laws and customs have had at all times and in all places, the effect of narrowing the circle within which the matrimonial selection could take place. But I believe it would be an error to conclude that therefore it would be within the power of the legislator to modify these laws and customs and to create new restrictions unknown before our own time. The old marriage laws and customs had the undisputed authority of religion, they were considered as divine institutions, and superstitious fears prevented transgression. This religious sanction would be absent from modern restricted laws, and in the case of a conflict between passion or desire and legal prohibition, *this* would weigh as a feather against *that*. In a low state of civilisation, the masses obey traditional laws without questioning their authority. Highly differentiated cultured persons have a strong critical sense, they ask of everything the reason why, and they have an irrepressible tendency to be their own lawgivers. These persons would not submit to laws restricting marriage for the sake of vague Eugenics, and if they could not marry under such laws in England, they would marry abroad, unless you dream of a uniform legislation in all countries of the globe, which would indeed be a bold dream.

From Professor A. POSADA.

(Professor of Constitutional Law in the University of Oviedo).

Without entering into a discussion of the bases on which Mr. Galton has raised eugenics as a science I find many very acceptable points of view in all that is proposed by this eminent sociologist.

The history of matrimonial relationship in itself discloses most interesting results. The relative character of its forms, the transitory condition of its laws, the very history of these would seem to show that the reflex action of opinion influences the being and constitution of the human family.

Granting this, and assuming that the actual conditions of the matrimonial regime—especially those that bear upon the manner of contract—

must not be considered as the final term of evolution (since they are far from being ideal), one cannot do less than encourage all that is being done to elucidate the positive nature of matrimonial union and the positive effects resultant from whether such union was effected with regard, or disregard, to the exigencies of generation and its influence on descendants.

Marriage is actually contracted either for love or for gain—more often than not the woman marries because she does not enjoy economic independence. In such circumstances, physiological considerations, the influence of heredity, both physiological and moral, have little or no weight—perhaps because they are neither sufficiently known or demonstrated in such a manner that the disastrous effects of their disregard can induce direct motives of conduct.

On this account I think that we should:

(1) Work to elucidate, in as scientific a manner as possible, the requirements of progressive selection in marriage, and we should rigorously demonstrate the consequences of such unions as are decidedly prejudicial to vigorous and healthy offspring.

(2) We should disseminate a knowledge of the conclusions ascertained by scientific investigation and rational statistics, so that these could be gradually assimilated by public opinion and converted into legal and moral obligations, into determinative motives of conduct.

But we must bear in mind that one cannot expect a transformation of actual criteria of sexual relationship, from the mere establishment of a science of eugenics, nor even from the propagation of its conclusions; the problem is thus seen to be very complex.

The actual criteria applied to sexual relationships—especially to those here alluded to—depend on general economic conditions, by virtue of which marriage is contracted under the influence of a multitude of secondary social predispositions, that have no regard to the future of the race; and it is useless to think that any propaganda would be sufficient to overcome the exigencies of economic conditions. On the other hand the actual education of both the woman and the man leaves much to be desired, and more particularly in regard to sexual relationship. And it would be futile to think of any effectual transformation in family life, while both the man and woman do not each of them equally exact, by virtue of an invulnerable repugnance to all that injures morality—a purity of morals in the future spouse.

The day that the woman will refuse as husband the man of impure life, with a repugnance equal to that usually felt by man towards impure womanhood, we shall have made a great step towards the transformation of actual marriage—to the gain of future generations.

From Professor SERGI.

(Director of the Museum and Laboratory of Anthropology, University of Rome).

As an abstract proposition, I believe Mr. Galton's proposal is entirely right and has many attractions. But, nevertheless, it seems to me to be not easily practicable and perhaps even impossible.

The sexual relations are vital in the life of all animal species. Any restrictions, to be at all tolerable, must irrefutably demonstrate a great and conspicuous gain. But, unfortunately, we are ignorant of the consequences of restrictions in marriage relations.

It is important in this connection to bear in mind that in modern societies there are certain unmistakable new tendencies at work. These tendencies are all in the direction of dissolving the old restrictions, both religious and social. They constitute, in fact, a movement towards what is called "free love." Now this tendency runs, it seems to me, counter to Mr. Galton's proposals, and makes it particularly difficult to initiate any restrictions of a new form and character.

It is, I believe, an illusion to expect that from any intellectual convictions there may arise a conscious inhibition of sex relations in the population generally. Instances are not wanting of men of high culture marrying women who are the daughters of insane and epileptic parents.

But notwithstanding these objections, which I hold to be a most serious obstacle, and even perhaps fatal to the practical application of Mr. Galton's eugenic principles, nevertheless I believe the studies which, in the second of his two papers to the Sociological Society, he proposes to institute will be both interesting and useful.

From Dr. R. S. STEINMETZ.

(Lecturer on Sociology in the University of Leyden).

I quite agree with Mr. Galton and others (*e. g.* Dr. Schallmeyer, of Munich, author of "Vererbung und Auslese im Lebenslauf der Volker," 1903) that one of the highest objects of Applied Sociology is the promotion of eugenic marriages. I think there is no worthier object of discussion for a sociological society than that of the means of this promotion. To be sure, the thorough and real knowledge of the true, not the expressed and the reputed motives, for introducing restrictions on marriage might be a means to this end. What we want to know is the real objective cause of these restrictions; there need not, of course, have been any conscious motive at all.

Coming to detailed examination of some points in Mr. Galton's paper on "Restrictions in Marriage," I would ask, is it certain that prohibition of polygamy in Christian nations was due "to considerations of social wellbeing," as Mr. Galton has it? Surely other causes were also at work. I think, where the number of adult men and women are nearly equal, monogamy is the natural result; polygamy is only possible when by wars and other causes, this proportion is reversed, and when other circumstances, as social inequality, allow some men to take more women than one.

A special distribution of labour between men and women may contribute to this result, but cannot be the cause of it, as every man wants the assistance of more women when he may get them. And in respect of sexual relations, it has to be observed that many men are polygamous in intention, and are only deterred by practical difficulties.

Social inequality, poverty, successful wars are the condition of polygamy. Economical or sexual wants drive men to it.

When these conditions are no longer fulfilled, monogamy will replace it. This is furthered by any rise in the position of women, by the freer play of the purer sentiments between the sexes, and by at least official or public chastity. I believe I am so far in agreement with Westermarck's views on the question. Christianity was very ascetic, as is attested by St. Paul's expressions in the Epistle to the Corinthians. By these ascetic tendencies Christian morals were opposed to polygamy. This tendency was enforced by the Christian ebionistic sympathies, by which all the fathers of the church were governed. Asceticism and social equality can both make for monogamy. Monogamy is certainly in accordance with one very mighty human instinct, that of jealousy; therefore it is the only democratic form of marriage. And I think it is the only one in harmony with the higher sentiments between the sexes, and with a right moral relation between offspring and parents.

But, in considering it, we should never forget that it is largely traversed by irregular love, whether this be sentimental or more sensual, and also by very general prostitution in all ages and classes.

So we must be very cautious in deducing from the fact of monogamy any conclusions as to new and rational marriage regulations, desirable as they may be.

Generally, the term endogamy is employed in a narrower sense than the prohibition of Greeks to marry barbarian women (concubinage with them was allowed, so the restriction was not severe).

I do not consider that Mr. Galton's view of the causes and conditions of endogamy and exogamy is in strict accordance with the results of "anthropology" (the continental term is "ethnology"); Mr. Galton thinks exogamy is usually to be found in "small and barbarous communities," but combined with the marriage restrictions by blood-ties, and the very general horror of incest, which are only its expression, exogamy is by far the commonest rule of the Chinese; and the Hindus are exogamous in the strict sense, and in the other sense all civilised nations are exogamous, marriage between close kindred being prohibited (Post, "Grundr. Ethn. Jurispr.," 1897, pp. 37-42),

The possibility of the complicated Australian marriage system, of which we know not yet the real motives and causes, does not at all warrant the conclusion that "with equal propriety" it might be applied "to the furtherance of some form of eugenics" among the Australians or among us. The conclusion from the Australians to us stands in need of demonstration. It cannot be assumed. Is it certain that motives of the same strength as those unknown may be found?

The motives for the horror of incest, we do not yet know quite certainly. Perhaps they are the result of very deep-seated and fundamental causes, which suggest the gravest caution in postulating their analogies.

As yet we are even incapable of restraining the very deplorable neo-Malthusian tendencies in the higher classes and some others in all civilised nations, nor those very generally and strongly operating in the eastern United States, in France, in English Australia. We are powerless against the dangers in this direction with which we are threatened by the widely spread feministic movement.*

The race-love of civilised men and women is regretfully feeble. The real problem is first to enforce it. At present the care for future man, the love and respect of the race, are quite beyond the pale of the morals of even the best.

The nobility of old, yea, the patriarchial family generally, entertained a real love and care for the qualities of their offspring. So, perhaps, the turn for this feeling may come again. The intensification of economic and social life will raise the demands on everybody's mental and bodily capabilities; the better knowledge of the hereditary qualities and their signification in attaining the highest degree of capacity will perhaps, and, I think should, in some degree inevitably waken the care for the qualities of one's own offspring.

I put much more hope on this resultant of intensified social demands, of increase and spreading of pathological knowledge, and of evermore enlightened egoism than on public morals embracing the future of the race. Improved care for one's own offspring according to science may possibly come. The result will be a change in our ideas, morals, and morality.

The next measures that then could be taken by the legislator seem to be those formulated by Dr. Schallmeyer in his excellent paper, "Infection als Morgengaber."

Meantime the chief force for progress in eugenic studies is, I think, the accomplishment of the life work of Mr. Galton, and the next is his establishment of a Research Fellowship in National Eugenics.

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* For my own opinions on this, *vide* "Die neueren Forschungen zur Geschichte der menschlichen Familie," *Zeitschrift für Socialwissenschaft*, 1899; *cf.* my "Der Nachwuchs der Begabten" and "Feminismus und Rasse," *Zeitschrift für Socialwissenschaft*, 1904.

It is a shameful reflection for continental universities that this whole range of studies is neglected by them and may be fittingly compared to their traditional narrowing of the whole field of social science to economics.

From Sir RICHARD TEMPLE.

Note I.—Studies in National Eugenics.

Topic I.—It seems to me that definitions of "gifted" and "capable" are required. Are the "gifted" to be those who perform the initiative reasoning, out of which the practical results arise? Are the "capable" to be those who bring into effect the reasoning of the "gifted"? It has always seemed to me that the work accomplished in the world is due to both classes in an equal degree. Neither can be effective without the other. Both are equally important. The success of either demands mental powers of a very high order. I am not at all sure that it is going too far to say of an equally high order. Then there are those who combine in themselves both the capacities, the initiative reasoning and the bringing into effect. Where are these to be placed? Many who possess the one in an eminent degree also possess the other; but, as reasoning and giving effect each requires so much thought and absorbs so much energy and time, the majority have not the opportunity to perform both. I suggest that, as regards family eugenics, both the "gifted" and the "capable" be, if the above definitions are to stand, taken as divisions of one class of mankind. This should be the safest method of bringing the inquiry to a practical result, because of the tendency, so strong in human beings, to look on their own description of work as that which is of the most importance to their kind. The great practical difficulty in the enquiry on the lines indicated, that impresses itself on me is that, especially among women—owing to their place in the world's work,—qualities essential to usefulness are frequently present in individuals who are otherwise possessed of no specially high mental qualities, and are therefore "unknown," and in no way remarkable: such qualities as initiative, discretion, "common sense," perseverance, patience, even temper, energy, courage, and so on, without which the "gifted" and "capable" are apt to be of no practical value to the world. I suggest that progress represents the sum of individual capacities, past and present, at any given period among any given population in any given environment. Then again, in the prosecution of Eugenics by statistics of achievement, there is another great difficulty, which may be best expressed in the words of the preacher in Ecclesiastes: "I returned, and saw under the sun, that the race is not to the swift, nor the battle to the strong, neither yet bread to the wise, nor yet riches to men of understanding, nor yet favour to men of skill: but time and chance happeneth to them all." Existing social conditions and prejudices, all the world over, will force eugenical philosophy to take root very slowly. This is, perhaps, as it should be, in view of the above practical reflection.

Topic VI.—It would appear that a beginning has been made, as regards men, in the Rhodes Scholarships.

Note II.—Restrictions in Marriage.

In one sense, Eugenics is the oldest and most universal philosophy in the world, of which the convention called marriage is the outward and visible sign. Everywhere, among all peoples in all times, marriage has originated for the enforcement and maintenance of real or supposed eugenics. The object of the convention has been fundamentally always the same, the direct personal advantage in some tangible form of a group in its environment. All that can be done by individual philosophers is to give marriage a definite turn in a direction deemed beneficial, because human beings in a mass, in a matter affecting every individual, act upon instinct—defining instinct as unconscious reasoning. In human affairs the outward and visible sign of instinct is custom. By reasoning, instinct can be given a definite direction, and hence a definite form can be given to a custom. This has often been accomplished, but, so far as I can apprehend history, reasoning has only succeeded in creating instinct and thus custom, when the masses subjected to its pressure have been able to see the direct personal advantage to be gained by the line taken. This is the practical point that the eugenical philosopher has to keep ever before him. A custom can be created. The questions for the philosopher are what should be created and how it should be created.

All forms of marriage are due fundamentally to considerations of well-being. Exogamy exists where it is thought important to abnormally increase the numbers of a group. Endogamy exists where it is thought important in a settled community to reserve property and social standing or power for a limited group. Monogamy, polygamy, polyandry, are all attempts to maintain social well-being in a form that has seemed obviously advantageous to different groups of human beings. Religion, taboo, and the prohibited degrees are all methods of enforcing custom by moral force. The Australian marriage system is merely a primitive, and therefore complicated, method of enforcing custom. But the human instinct as to incest is something going very deep down, as there is the same kind of instinct in some of the "higher" animals of the two sexes when stabled together, *e.g.*, horses, elephants. Celibacy seems to be due to different causes in different circumstances, according as to whether it is enforced or voluntary. In the former case it is a method of enforcing marriage customs maintained for the supposed common good. In the latter it is due to asceticism, itself an universal instinct based on a philosophy of personal advantage.

The restrictions enforced by marriage customs have led to hypergamy, a *mariage de convenance* exchanging position and property, but really an unreasoning form of eugenics adopted because of the supposed personal advantage, and this has led, in one disastrous form, to female infanticide in a distinctly harmful degree. All the restrictions of marriage are modified in uncivilised communities by promiscuity before marriage and in civilised communities by hetairism. The greater the restrictions the more systematic has hetairism become. Illegitimacy has taken on many almost unrecognisable forms in various parts of the world. It really represents the result of rebellion against convention. Every one of these considerations materially affect any proposition for a reform of Eugenics. Caste is the outward manifestation of an endogamic marriage system introduced by the "intellectuals" of a people for the personal advantage of their own group within the nation, and imitated without reasoning by other groups. This system of endogamic marriage, adopted for the real or supposed advantage of a group, has brought about national disaster, for it has made impossible the instinct of nationality, or the larger group, and has brought the peoples adopting it into perpetual subjection to others possessing the instinct of nationality. Its existence and practical effect is a standing warning to the eugenical philosopher, which should point out to him the extreme care that is necessary in consciously directing eugenics into any given channel.

From Professor TÖNNIES.

(Professor of Philosophy in the University of Kiel).

I fully agree with the scope and aims of Mr. Galton's "Eugenics," and consequently with the essence of the two papers proposed. But with respect to details, I have certain objections and illustrations, which I now try to explain.

1. There can be no doubt but the three kinds of accomplishments are desirable in mankind; physical, mental and moral ability. Surely the three, or as Mr. Galton classifies them, constitution—which I understand to imply moral character—physique and intellect, are not independent variables, but if they to a large extent are correlate, on the other hand they also tend to exclude each other, strong intellect being very often connected with a delicate health as well as with poor moral qualities, and *vice versa*. Now the great question, as it appears to me, will be, whether Eugenics is to favour one kind of these excellencies at the *cost of* another one, or of both the other, and which should be preferred under any circumstances.

2. Under existing social conditions it would mean a cruelty to raise the average intellectual capacity of a nation to that of its better moiety of the present day. For it would render people so much more conscious of the dissonance between the hopeless monotony of their toil and the lack of

recreation, poorness of comfort, narrowness of prospects, under which they are even now suffering severely, notwithstanding the dulness of the great multitude.

3. The rise of intellectual qualities also involves, under given conditions, a danger of further decay of moral feeling, nay, of sympathetic affections generally. Town life already produces a race of cunning rascals. Temptations are very strong indeed, to outrun competitors by reckless astuteness and remorseless tricks. Intelligence promotes egotism and pleasure-seeking, very much in contradiction to the interests of the race.

4. A strong physique seems to be correlate with some portions of our moral nature, but not with all. Refinement of moral feeling and tact are more of an intellectual nature, and again combine more easily with a weak frame and less bodily power.

5. I endorse what Mr. Galton shows—that marriage selection is very largely conditioned by motives based on religious and social consideration; and I accept, as a grand principle, the conclusion that the same class of motives may, in time to come, direct mankind to disfavour unsuitable marriages, so as to make at least some kinds of them impossible or highly improbable, and this would mean an enormous benefit to all concerned, and to the race in general. But I very much doubt, if a sufficient unanimity may be produced upon the question—which marriages *are* unsuitable?

6. Of course this unanimity may be promoted by a sufficient study of the effects of heredity. This is the proper and most prominent task of Eugenics, as Mr. Galton luminously points out in his six topics to be taken in hand under the Research Fellowship. Highly though I appreciate the importance of this kind of investigation, to which my own attention has been directed at a very early date, I am apt to believe, however, that the *practical* outcome of them will not be considerable. Our present knowledge, scanty and incoherent as it is, still suffices already to make certain marriages, which are especially favoured by social convention, by religion and by custom, appear to sober-thinking men, highly unsuitable. Science is not likely to gain an influence equivalent to, or even outweighing, those influences, that further or restrain particular classes of marriage. On the other hand the voice of Reason, notably with respect to hygienic as well as moral considerations, is often represented by *parents* in contradiction to inclinations or even passions of their offspring (especially daughters), and the prevailing individualistic tendencies of the present age, greatly in favour of individual choice and of the natural right of Love, mostly, or at least very often, dumb that voice of Reason and render it more and more powerless. Eugenics has to contend against the two fronts: against the *marriage de convenance* on the one side, the *marriage de passion* on the other.

7. But this applies chiefly to the upper strata of society, where a certain influence of scientific results may be presumed on principle with greater likelihood than among the multitude. Mr. Galton wishes the

national importance of Eugenics to be introduced into the national conscience like a new religion. I do not believe that this will be possible, unless the conditions of every day existence were entirely revolutionised beforehand. The function of Religion has always been to give *immediate* relief to pressing discomforts, and to connect it with hopeful prospects of an *individual* life to come. The life of the race is a subject entirely foreign to popular feelings, and will continue to be so, unless the mass should be exempt from daily toil and care, to a degree which we are unable to realise at present.

8. However, the first and main point is to secure the general *intellectual* acceptance of Eugenics as a hopeful and most important study. I willingly and respectfully give my fullest sympathy and approval to this claim.

I have tried to express my sentiments here as evoked by the two most interesting papers. I have been obliged to do so in great haste, and consequently, as I am aware, in very bad English, for which I must apologise.

From Professor AUGUST WEISMANN.

It has given me great pleasure to learn that a Sociological Society has been formed in England, and to see that so many distinguished names are associated with its inauguration and proceedings.

As for the request that I should send "an expression of my views on the subject" of Mr. Galton's two papers, I fear I can have nothing to say that will be at all new.

I think there is one question, however, of very great importance which has not yet, so far as I know, been investigated, and to which the statistical method alone can supply an answer. It is this:—Whether, when a hereditary disease like tuberculosis has made its appearance in a family it is afterwards possible for it to be entirely banished from this or that branch of the family, or whether, on the contrary, the progeny of these members of the family who appear healthy must not sooner or later produce a tuberculous offspring?

I am fully aware that there exists already a great mass of statistical matter on the subject of "tuberculosis," but I cannot say that it seems to me sufficient, thus far, to justify a sure conclusion.

Speaking for myself, I am disposed, both on theoretic grounds and in view of known facts, to opine that a complete purification and re-establishment of such a family is quite possible in the cases of slighter infection.

For I believe that hereditary transmission in such cases depends upon an infected condition of the seed, germ, or generative cell; that it is conceivable that single generative cells of the parent may remain free from bacilli; that an entirely healthy child may be developed from one such generative cell, and that from this sound shoot an entirely healthy branch of the family may grow in time.

I would almost go so far as to say that if this were *not* the case, then there could hardly be a family on earth to-day unaffected by hereditary disease.

Let me ask the Sociological Society to accept this note as merely an indication of my willingness to make at least a very small contribution to the list of those sociological problems which the Society aims at solving.

From the Hon. V. LADY WELBY.

It is obvious that in the question of eugenic restrictions in marriage there are two opposite points of view from which we may work: (1) that of making the most of the race, which concentrates interest, not on the parents—who are then merely, like the organism itself, the germ carriers—but always on the children (in their turn merely race-bearers); and (2) that of making the most of the individual and thus raising the standard of the whole by raising that of its parts. The problem is to combine these in the future more adequately than has been attempted in the past.

In a small contribution to the discussion on Mr. Galton's first paper I appealed to women to realise more clearly their true place and gift as representing that original racial motherhood, out of which the masculine and feminine characters have arisen. It seems advisable now to take somewhat wider ground.

When, in the interests of an ascending family ideal, we emphasise the need for restrictions on marriage which shall embody all those, as summarised in Mr. Galton's paper, to which human societies have already submitted, we have to consummate a further marriage—one of ideas; we have to combine what may appear to be incompatible aims. In the first place, in order to foster all that makes for a higher and nobler type of humanity than any we have yet known how to realise, we must face the fact that some sacrifice of emotion become relatively unworthy is imperative. Else we weaken "the earnest desire not to infringe the sanctity and freedom of the social relations of a family group." But the sacrifice is of an emotion which has ceased to make for Man and now makes for Self or for reversion to the sub-human.

We are always confronted with a practical paradox. The marriage which makes for the highest welfare of the united man and woman may be actually inimical to the children of that union. The marriage which makes for the highest type of family and its highest and fullest development may often mean, and must always tend to mean, the inhibition of much that makes for individual perfection.

And since the children in their turn will be confronted by the same initial difficulty it may be desirable not only to define our aim and the best methods of reaching it, but to suggest one or two simple prior considerations which are seldom taken into account. One of these is the fact that, speaking generally, human development is a development of the higher brain and its new organ, the hand. It may, I suppose, be said that the rest of the organism has not been correspondingly developed, but remains essentially on the animal level. What especially concerns us here is that this includes the uterine system, which has even tended to retrograde. Here, surely, we have the key to many social and ethical difficulties in the marriage question.

This relatively enormous complexity of brain, disturbing, or at least altering the organic balance, coupled with the sexual incompleteness of the individual, has cost us dear. All such special developments involving comparative overgrowth must do this. In this case we have gained, of course, a priceless analytical, constructive, and elaborative faculty. But there seem to be many indications that we have correspondingly lost a direct and trustworthy reaction to the stimuli of nature in its widest sense, a reaction that should deserve the name of intuition as representing a practically unerring instinct. An eugenic advance secured by an increase of moral sensitiveness on the subject of parentage may well tend to restore on a higher level these primordial responses to excitation of all kinds. But of course it will still rest with education, in all senses and grades, either (as, on the whole, at present) to blunt or distort them, or to interpret and train them into directed and controlled efficiency.

At present our mental history seems to present a curious anomaly. On the one hand we see what, compared with the animal and even with the lower intellectual human types, is an amazing development of logical precision, ordered complexity of reasoning, rigorous validity of conclusion, all ultimately depending for their productive value on the validity of the presuppositions from which they start. On the other hand, this initial validity can but seldom, if ever, be proved experimentally or by argument, or be established by universal experience. Thus the very perfection of the rational development is always liable to lead us further and further astray. The result we see in endless discussions which tend rather to divide than to unite us by hardening into opposed views of what we take for reality, and to confuse or dim the racial outlook and hinder the racial ascent.

It is to be hoped then that one result of the creation of a eugenic conscience will be a restoration of the human balance, bringing about an immensely increased power of revising familiar assumptions and thus of rightly interpreting experience and the natural world. This must make for the solution of pressing problems which at present cannot even be worthily stated. For there is no more significant sign of the present dead-lock resulting from the anomaly just indicated, than the general neglect of the question of effective expression, and therefore of its central value to us; that is, what we are content vaguely to call its meaning.

Such a line of thought may seem, for the very reason of this neglect, far enough from the subject to be dealt with,—from the question of restrictions in marriage. But in the research, studies, and discussions which ought to precede any attempt in the direction of giving effect to an aroused sense of eugenic responsibility, surely this factor will really be all-important. It must be hoped that such discussion will be carried on by those in whom what, for convenience sake, I would call the mother-sense, or the sense of human, even of vital origin and significance, is not entirely overlaid by the priceless power of co-ordinating subtle trains of abstract reasoning. For this supreme power easily defeats itself by failing to examine and rectify the all-potent starting point of its activities, the simple and primary assumption.

I have admitted that the foregoing suggestions—offered with all diffidence—seem to be far from the present subject of discussion, with which, indeed, I have not attempted directly to deal. I would only add that this is not because such questions have not the deepest interest for me as for all who in any degree realise their urgency.

We shall have to discuss, though I hope in some cases privately, such questions as the influence on descendants of the existence or the lack of reverent love and loyalty between parents, not as "acquired characters," in the controversial sense, but as giving full play to the highest currents of our mental and spiritual life. We shall have to consider the possibilities of raising the whole moral standard of the race, so that the eugenic loyalty shown in instinctive form on the sub-human plane, should be reproduced in humanity consciously, purposively, and progressively. Finally we shall have to reconsider the two cults of Self and Happiness, which we are so prone to make ultimate. The truly eugenic conscience will look upon self as a means and an instrument of consecrated service; and happiness not as an end or an ideal to strive for, since such striving ignobly defeats its own object, but—as sorrow or disappointment may also become—a means or a result of purifying and energising the human activities to an extent as yet difficult to speak of.

CONTRIBUTORY NOTES

Brief communications were contributed by, amongst others:

Professor B. ALTAMIRA (of the University of Oviedo), who wrote:—"The subjects of Mr. Galton's communications are very interesting, and there should be some very valuable information forthcoming on the forms of marriage (endogamy, oxogamy, etc.) to be unearthed from the actual juridical manners and customs of Spain."

Mr. F. CARREL, who wrote:—"I should like to ask Mr. Galton whether the general practice of eclectic mating might not tend to the production of a very inferior residual type, always condemned to mate together until eliminated from an existence in which they would be too unfitted to participate, and, if so, whether such a system can be adopted without inflicting suffering upon the more or less slowly disappearing residuum?"

Mrs. FAWCETT, who wrote:—"Mr. Galton evidently realises that he has a gigantic task before him, that of raising up a new standard of conduct on one of the most fundamental of human relations. At present, the great majority of men and women otherwise conscientious, seem to have no conscience about their responsibility for the improvement or deterioration of the race. One frequently observes cases of men suffering from mortal and incurable disease who apparently have no idea that it is wrong to have children who will probably enter life heavily handicapped by inherited infirmity. Two thirds of what is called the social evil would disappear of itself, if responsibility for the welfare of the coming generation found its fitting place in the conscience of the average man. I wish all success to Mr. Francis Galton's efforts."

Professor J. G. MCKENDRICK, who wrote:—"Mr. Galton is opening up a subject of great interest and importance—more especially in its relation to improving the physical, mental, and pure qualities of the race. At present much is carried on by haphazard, and I fear the consequence is that we see indications of degeneration in various directions. I heartily wish much success to those who are carrying on investigations of these important problems. We are all indebted to Mr. Galton for his valuable and deeply suggestive papers."

Professor J. H. Muirhead, who wrote:—"I think Mr. Galton's suggestions for the advance of the study and practice of Eugenics most important, and hope our Society may do something to forward the subject."

Professor E. B. Poulton, who wrote:—"I entirely agree with the aims Mr. Galton has in view and profoundly admire his papers on this subject. I think they unfold great possibilities for the human race."

The Hon. Bertrand Russell, who wrote:—"I have read Mr. Galton's two papers in abstract with much interest, and agree entirely with the view that marriage customs might be modified in a eugenic direction."

Mr. C. A. Witchell (*Author of "The Cultivation of Man"*), who wrote:—"There is one factor operating in the selection of husbands and wives which will be extremely difficult to bring within the purview of eugenics, and which is yet supreme in its influence. The union of the sexes in its higher form is not a matter of passion, but of the more powerful and enduring sentiment which we call love. The capturing of mates is not confined to mankind; the polygamous birds exhibit it. But there are birds that sing to win a mate—these have a delayed courtship; and in man this is developed to still nobler ideals. Let a man look around him at a public ball. Would he choose for mother of his children the woman who of all present has the greatest physical attractions? Nothing of the kind. The one he chooses (by instinct) is the one who inspires him with a certain elevation of spiritual sentiment, who, indeed, freezes his physical nature out of his thought—whom he could hardly pay a compliment to, and yet whom he knows he would select from among them all. Why does he choose her? Has he not made selection through the assessors chosen by Nature—certain subtle and undefinable perceptions received through the senses of sight and hearing. These perceptions, fleet and instant messengers, have not been delayed by social distances. They have pierced all the flimsy armour of fashion, they have penetrated the shams of culture, and have told his inmost sense of consciousness—his soul—what hers is like. By that knowledge his soul has chosen hers; and unless science can analyse this subtle process of spiritual selection it must stand aside. By all means let eugenics advance! But let its exponents pause to analyse first what is now the most powerful factor governing the selection of the sexes, and seek to take advantage of it rather than to stifle it with mere physical agencies. To sterilise defective

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types is one thing; to eliminate the criminally weak and diseased is another —equally reasonable. But let us beware lest we do anything that may tend to obliterate by physical means the higher instinctive teachings of sexual selection."

A MEMBER OF THE SOCIOLOGICAL SOCIETY, who is a well-known writer, but wishes here to remain anonymous:—"My own views are on the side of the largest scope being given to what might be called interference in the matter, and for this reason I should even regret the abrogation of the sister-in-law disability, mistaken as it seems to me on its merits. I mean anything which keeps alive the sense that marriage is the affair of the State seems to me to have a certain value. When one knows, as I do, of a certain physician asking a patient, 'Were your parents first cousins?' and the affirmative answer, one feels certain that here is a realm of duty to which conscience has yet to awaken."

MR. GALTON'S REPLY.

This Society has cause to congratulate itself on the zeal and energy which has brought together so large a body of opinion. We have had verbal contributions from four eminent specialists in anthropology: Dr. Haddon, Dr. Mott, Mr. Crawley and Dr. Westermarck, and numerous written communications have been furnished by well known persons. At the time that I am revising and extending these words no less than twenty-six contributions to the discussion are in print. Want of space compels me to confine my reply to those remarks that seem more especially to require it, and to do so very briefly, for Eugenics is a wide study, with an uncounted number of side issues into which those who discuss it are tempted to stray. If, however, sure advance is to be made, these issues must be thoroughly explored, one by one, and partial discussion should as far as possible be avoided. To change the simile, we have to deal with a formidable chain of strongholds, which must be severally attacked in force, reduced, and disposed of, before we can proceed freely.

In the first place, it is a satisfaction to find that no one impugns the conclusion which my memoir was written to justify, that history tells how restrictions in marriage, even of an excessive kind, have been contentedly accepted very widely, under the guidance of what I called "immaterial motives." This is all I had in view when writing it.

Certificates.—One of the comments on which I will remark is that if certificates were now offered to those who passed certain examinations into health, physique, moral and intellectual powers, and hereditary gifts, great mistakes would be made by the examiners. I fully agree that it is too early to devise a satisfactory system of marks for giving what might be styled "honour-certificates," because we do not yet possess sufficient data to go upon. On the other hand there are persons who are exceptionally and unquestionably unfit to contribute offspring to the nation, such as those mentioned in Dr. Mott's bold proposals. The best methods of dealing with these are now ripe for immediate consideration.

Breeding for points.—It is objected by many that there cannot be unanimity on the "points" that it is most desirable to breed for. I fully discussed this objection in my memoir read here last spring, showing that some qualities such as health and vigour were thought by all to be desirable, and the opposite undesirable, and that this sufficed to give a first direction to our aims. It is a safe starting point, though a great deal more has to be inquired into as we proceed on our way. I think that some contributors to this discussion have been needlessly alarmed. No question has been raised by me of breeding men like animals for particular points, to the disregard of all-round efficiency in physical, intellectual (including moral), and hereditary qualifications. Moreover, as statistics have shown, the best qualities are largely correlated. The youths who became judges, bishops, statesmen, and leaders of progress in England could have furnished formidable athletic teams in their times. There is a tale, I know not how far founded on fact, that Queen Elizabeth had an eye to the calves of the legs of those she selected for bishops. There is something to be said in favour of selecting men by their physical characteristics for other than physical purposes. It would decidedly be safer to do so than to trust to pure chance.

The residue.—It is also objected that if the inferior moiety of a race are left to intermarry, their produce will be increasingly inferior. This is certainly an error. The law of "regression towards mediocrity" insures that their offspring as a whole, will be superior to themselves, and if as I sincerely hope, a freer action will be hereafter allowed to selective agencies than hitherto, the portion of the offspring so selected would be better still. The influences that now withstand the free action of selective agencies are numerous, they include indiscriminate charity.

Passion of love.—The argument has been repeated that love is too strong a passion to be restrained by such means as would be tolerated at the present time. I regret that I did not express the distinction that ought to have been made between its two stages, that of slight inclination and that of falling thoroughly into love, for it is the first of these rather than the second that I hope the popular feeling of the future will successfully resist. Every match-making mother appreciates the difference. If a girl is taught to look upon a class of men as tabooed, whether owing to rank, creed, connections, or other causes, she does not regard them as possible husbands and turns her thoughts elsewhere. The proverbial "Mrs. Grundy" has enormous influence in checking the marriages she considers indiscreet.

Eugenics as a factor in religion.—Remarks have been made concerning eugenics as a religion; this will be the subject of the brief memoir that follows these remarks.

It is much to be desired that competent persons would severally take up one or other of the many topics mentioned in my second memoir, or others of a similar kind, and work it thoroughly out as they would any ordinary scientific problem; in this way solid progress would be made. I must be allowed to re-emphasise my opinion that an immense amount of investigation has to be accomplished before a definite system of Eugenics can be safely framed.

EUGENICS AS A FACTOR IN RELIGION.

Eugenics strengthens the sense of social duty in so many important particulars that the conclusions derived from its study ought to find a welcome home in every tolerant religion. It promotes a far-sighted philanthropy, the acceptance of parentage as a serious responsibility, and a higher conception of patriotism. The creed of eugenics is founded upon the idea of evolution; not on a passive form of it, but on one that can to some extent direct its own course. Purely passive, or what may be styled mechanical evolution, displays the awe inspiring spectacle of a vast eddy of organic turmoil, originating we know not how, and travelling we know not whither. It forms a continuous whole from first to last, reaching backward beyond our earliest knowledge and stretching forward as far as we think we can foresee. But it is moulded by blind and wasteful processes, namely, by an extravagant production of raw material and the ruthless rejection of all that is superfluous, through the blundering steps of trial and error. The condition at each successive moment of this huge system, as it issues from the already quiet past and is about to invade the still undisturbed future, is one of violent internal commotion. Its elements are in constant flux and change, though its general form alters but slowly. In this respect, it resembles the curious stream of cloud that sometimes seems attached to a mountain top during the continuance of a strong breeze; its constituents are always changing, though its shape as a whole hardly varies. Evolution is in any case a grand phantasmagoria, but it assumes an infinitely more interesting aspect under the knowledge that the intelligent action of the human will is, in some small measure, capable of guiding its course. Man has the power of doing this largely so far as the evolution of humanity is concerned; he has already affected the quality and distribution of organic life so widely that the changes on the surface of the earth, merely through his disforestings and agriculture, would be recognisable from a distance as great as that of the moon.

As regards the practical side of eugenics, we need not linger to re-open the unending argument whether man possesses any creative power of will at all, or whether his will is not also predetermined by blind forces or by intelligent agencies behind the veil, and whether the belief that man can act independently is more than a mere illusion. This matters little in practice, because men, whether fatalists or not, work with equal vigour whenever they perceive they have the power to act effectively.

Eugenic belief extends the function of philanthropy to future generations, it renders its action more pervading than hitherto, by dealing with families and societies in their entirety, and it enforces the importance of the marriage covenant by directing serious attention to the probable quality of the future offspring. It sternly forbids all forms of sentimental charity that are harmful to the race, while it eagerly seeks opportunity for acts of personal kindness, as some equivalent to the loss of what it forbids. It brings the tie of kinship into prominence and strongly encourages love and interest in family and race. In brief, eugenics is a virile creed, full of hopefulness, and appealing to many of the noblest feelings of our nature.

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APPENDIX

The following is an abstract of the paper communicated to the Sociological Society in 1904, under the title,

"EUGENICS: ITS DEFINITION, SCOPE, AND AIMS."*

Eugenics is the science which deals with all influences which improve the inborn qualities of a race; also those that develop them to the utmost advantage.

Postulating existing social groups and existing moral criteria, Eugenics aims at the reproduction of the best specimens of individual—in each of those groups in which the characteristic activity is not demonstrably anti-social (as in criminals).

The practice of Eugenics would thus raise the average quality of a nation to that of its better moiety of the present day: men of an order of ability which is now very rare would become more frequent, because the level out of which they rose would itself have risen.

In respect of practical measures, the endeavour is to bring as many influences as can be reasonably employed, to cause the useful classes of a community to contribute *more* than their proportion to the next generation.

The course of procedure which lies within the functions of a Sociological Society would be somewhat as follows:—

1. Dissemination of a knowledge of the laws of heredity so far as they are surely known, and promotion of their farther study. The knowledge of what may be termed the *actuarial* side of heredity has greatly advanced in recent years. The *average* closeness of kinship in each degree, now admits of exact definition and of being treated mathematically, like birth and death-rates, and the other topics with which actuaries are concerned.

2. Historical inquiry into the rates with which the various classes of society (classified according to civic usefulness) have contributed to the population at various times in ancient and modern nations. There is

* The paper, along with discussion thereon, is printed in full in "Sociological Papers," Vol. I.

strong reason for believing that national rise and decline is closely connected with this influence. It seems to be the tendency of civilisation to check fertility in the higher types, through numerous causes, some of which are well-known, others are inferred, and others again are wholly obscure.

3. Systematic collection of facts showing the circumstances under which eugenic families have most frequently originated. The names of such families in England have yet to be learnt, and the conditions under which they have arisen.

The Sociological Society might advantageously initiate such an investigation, by issuing a carefully drafted questionnaire; and thus would accumulate a manuscript collection, of great use for statistical students, and hereafter capable of developing into a "golden book" of thriving families.

4. Influences affecting marriage. Anthropology shows that social influences of all kinds have immense power in modifying marriage customs and conventions. If unsuitable marriages, from the Eugenic point of view, were banned socially, or even regarded with the unreasonable disfavour which some attach to cousin-marriages, very few would be made.

5. Persistence in setting forth the national importance of Eugenics. There are three stages to be passed through. *Firstly*, it must be made familiar as an academic question, until its exact importance has been understood and accepted as a fact. *Secondly*, it must be recognised as a subject whose practical development deserves serious consideration; and *Thirdly*, it must be introduced into the national conscience, like a new religion. By the study and practice of Eugenics, Man may hope to co-operate with Nature in securing that humanity shall be represented by the highest races. What Nature does blindly, slowly and ruthlessly, man may do providently, quickly and kindly. As it lies within his power, so it becomes his duty to work in that direction; just as it is his duty to succour neighbours who suffer misfortune. The improvement of our stock is one of the highest objects that we can reasonably attempt. We are ignorant of the ultimate destinies of humanity, but we may feel perfectly sure that it is as noble a work to raise its level in the sense already explained, as it would be disgraceful to abase it.

The ideals of Eugenics may in time acquire something of religious sanction, but its details must first be worked out sedulously in the study. Over-zeal, leading to hasty action, would do harm by holding out expectations of a near golden age, which would certainly be falsified and cause the science to be discredited. The first and main point is to secure the general intellectual acceptance of Eugenics as a hopeful and most important study. Then let its principles work into the heart of the nation, who will gradually give practical effect to them in ways that we may not wholly foresee.

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