The Journal and Transactions

OF THE

PHOTOGRAPHIC SOCIETY

OF

GREAT BRITAIN.

NEW SERIES, VOL. V., No. 9.] FRIDAY, JUNE 24, 1881. [PRICE 3*d*.

PATRONS.
HER MAJESTY THE QUEEN
AND
HIS ROYAL HIGHNESS THE PRINCE OF WALES.

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The next Meeting of the Society will be at the Exhibition Soirée, which will take place on SATURDAY EVENING, OCTOBER 8th, 1881, at the Gallery, 5A, PALL MALL EAST.

TRANSACTIONS.

ORDINARY MEETING HELD AT 5A, PALL MALL EAST,

ON TUESDAY, JUNE 14th, 1881,

JAMES GLAISHER, F.R.S., President, in the Chair.

The Minutes of the last Meeting having been read and confirmed,

The following were then duly elected Members of the Society :—

William James Cox, 39, Curzon Street, Derby.
Lieutenant Charles E. Gladstone, R.N., Royal Naval College, Greenwich.

The President then called upon Mr. F. Galton, F.R.S., to read his paper.

COMPOSITE PORTRAITURE.

By Francis Galton, F.R.S.

I propose to draw attention to-night to the results of recent experiments and considerable improvements in a process of which I published the principles three years ago, and which I have subsequently exhibited more than once.*

I have shown that, if we have the portraits of two or more different persons, taken in the same aspect and under the same conditions of light and shade, and that if we put them into different optical lanterns converging on the same screen and carefully adjust them—first, so as to bring them to the same scale, and, secondly, so as to superpose them as accurately as the conditions admit—then the different faces will blend surprisingly well into a single countenance. If they are not very dissimilar, the blended result will always have a curious air of individuality, and will be unexpectedly well defined; it will exactly resemble none of its components, but it will have a sort of family likeness to all of them, and it will be an ideal and an averaged portrait. I have also shown that the image on the screen might be photographed then and there, or that the same result may be much more easily obtained by a method of successive photography, and I have exhibited many specimens made on this principle. Photo-lithographs of some of these will be found in the Proceedings of the Royal Institution, as illustrations of a lecture I gave there "On Generic Images" in 1879.

It will be convenient that I should again explain the best of the many processes that I have already published. It is simple enough in idea, but difficult to carry out with the scrupulous accuracy of adjustment that is needed for really good results. I have, therefore, little doubt that some photographers may have tried it, or some other of my plans, and failed, and that they consequently have abandoned what I believe is likely to become a new and not unimportant branch of their art. My old plan was this:—I reduced the photographs by a copying-camera to a uniform scale, so that in each case the distance between the line that intersected the pupils of the eyes and that which lay between the lips was the same. Then I took each paper portrait in turn, and laid it face downwards on a glass illuminated by a mirror below as in a retouching-frame, and I adjusted the portrait to certain cross lines drawn on the glass. Suppose the aspect of the portraits to be full face, then the fiducial lines that I used were two horizontal ones, at the standard distance apart between pupils and lips, and a vertical line that intersected them. I adjusted the portrait until it was correctly placed as regards the two horizontal lines, and then I slid it horizontally until the vertical line was exactly half way between the two pupils. Holding it firmly in this position, I pressed a bar that carried two needle points down on the portrait; they pricked two holes, which afterwards served as "register-marks." After all the portraits had been similarly treated in turn, I strung them together like leaves in a book, by threads passing through the two holes; then they were ready to be operated on. Suppose I had six portraits, and that, under the given conditions of light, lens, and scale of reduction, it would require 60 seconds of exposure to make a good copy of any one of the six, then I allotted only 10 seconds to each separate portrait, thus dividing the 60 seconds equally among them. I fixed the book of portraits against the wall in front of the camera, and when all was ready I uncapped the lens for 10 seconds; then I turned down the first portrait and exposed the second, and uncapped the lens again for 10 seconds, and so on until the six portraits had been gone through.

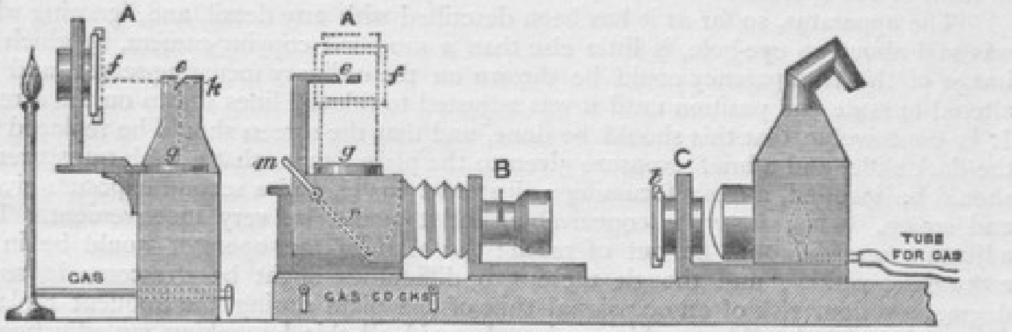
The method I now use is much better; it leads to more accurate results, and is easier to manage. I will exhibit and explain the apparatus as it stands, and will indicate some improvements as I go on. The apparatus is here. I use it by gaslight, and employ rapid plates, which, however, under the conditions of a particularly small aperture and the character of the light, require 60 seconds of total exposure. The apparatus is 4 feet long and 6½ inches broad; it lies with its side along the edge of

* "Composite Portraits," Journal of the Anthropological Institute, 1878, reprinted in *Nature* and in the *Photographic News*, and translated in the *Révue Scientifique*. Also "Generic Images," Proceedings of the Royal Institution, 1879.

the table at which I sit, and it is sloped towards me, so that, by bending my neck slightly, I can bring my eye to an eyehole, where I watch the effect of the adjustments which my hands are free to make. The entire management of the whole of these is within an easy arm's length, and I complete the process without shifting my seat.

The apparatus consists of three parts, A, B, and C. A is rigidly fixed; it contains the dark slide and the contrivances by which the position of the image can be viewed; the eyehole, *e*, already mentioned, being part of A. B is a travelling carriage that holds the lens, and is connected by bellows-work with A. In my apparatus it is pushed out and in, and clamped where desired, but it ought to be moved altogether by pinion and rack-work. The lens I use is a I B Dallmeyer. Its focal length is

DIAGRAM SHOWING THE ESSENTIAL PARTS.



A The body of the camera, which is fixed.
B Lens on a carriage, which can be moved to and fro.
C Frame for the transparency, on a carriage that also supports the lantern; the whole can be moved to and fro.
r The reflector inside the camera.
m The arm outside the camera attached to the axis of the reflector; by moving it, the reflector can be moved up or down.
g A ground-glass screen on the roof, which receives the image when the reflector is turned down, as in the diagram.
e The eye-hole through which the image is viewed on *g*; a thin piece of glass immediately below *e*, reflects the illuminated fiducial lines in the transparency at *f*, and gives them the appearance of lying upon *g*,—the distances *f k* and *g k* being made equal, the angle *f k g* being made a right angle, and the plane of the thin piece of glass being made to bisect *f k g*.
f Framework, adjustable, holding the transparency with the fiducial lines on it.
t Framework, adjustable, holding the transparency of the portrait.

appropriate to the size of the instrument, and I find great convenience in a lens of wide aperture when making the adjustments, as I then require plenty of light; but, as to the photography, the smaller the aperture the better. The hole in my stop is only two-tenths of an inch in diameter, and I believe one-tenth would be better.*

C is a travelling carriage that supports the portraits in turn, from which the composite has to be made. I work directly from the original negatives with trans mitted light; but prints could be used with light falling on their face. For convenience

* A small aperture helps to remove a cause of fog, due to the source of illumination not lying in the same plane as the transparency, but behind it. Suppose an isolated black dot, *p*, in the transparency call its image on the focussing-screen *m*; let the point in the illuminated surface that lies in the prolongation of *m p* be called *p′*. Then to produce an image free from fog, every ray proceeding from *p* towards the lens ought to be intercepted by *p*. This, however, can never be the case under the above conditions. Since *m* is in focus for rays proceeding from *p*, the focus for rays proceeding from the more distant point *p′* will be somewhere in front of *m*. Consequently those rays from *p′* that skirt *p*, and that fall upon the outer portions of the lens, will be brought to a focus in front of *m*; they will diverge as they proceed further, and will form a blur of light that will overspread *m*,—in short they will cause fog. Owing to this cause, we cannot have perfect sharpness in any contour line in the image of a transparency that is illuminated by a mirror reflecting the light of the sky, or by a distant sheet of white paper. The remedy is two-fold; one part consists in bringing the source of illumination as near as possible to the transparency, and the other is to use a very small aperture. I rest the transparency against a ground-glass plate not too strongly illuminated from behind, and which itself becomes the source of illumination. I do not like to put the *face* of the transparency against the *face* of the ground glass without introducing certain other difficulties; thus the image somewhat loses its definition, and the grain of the ground glass, or any dust on its surface, is photographed with unwelcome precision. As for the aperture, I use a stop the hole in which is too small to admit an ordinary lead pencil.

of description I will confine myself to the first instance only, and will therefore speak of C as the carriage that supports the frame that holds the negative transparencies. C can be pushed along the board and be clamped anywhere, and it has a rack and pinion adjustment; but it should have been made movable by rack and pinion along the whole length of the board. The frame for the transparencies has the same movements of adjustment as those in the stage of a microscope. It rotates round a hollow axis, through which a beam of light is thrown, and independent movements in the plane, at right angles to the axis, can be given to it in two directions, at right angles to one another, by turning two separate screws. The beam of light is furnished by three gas-burners, and it passes through a condenser. The gas is supplied through a flexible tube that does not interfere with the movements of C, and it is governed by a stop-cock in front of the operator.

The apparatus, so far as it has been described with any detail, and, ignoring what was said about an eye-hole, is little else than a modified copying-camera, by which an image of the transparency could be thrown on the ordinary focussing-screen, and be altered in scale and position until it was adjusted to fiducial lines drawn on the screen. It is conceivable that this should be done, and that the screen should be replaced by the dark slide, and a brief exposure given to the plate; then, that a fresh transparency should be inserted, a fresh focussing adjustment made, and a second exposure given, and so on. This, I say, is conceivable, but it would be very inconvenient. The adjusting screws would be out of reach; the head of the operator would be in an awkward position; and though these two difficulties might be overcome in some degree, a serious risk of an occasional shift of the plate during the frequent replacement of the dark slide would remain. I avoid all this by making my adjustments while the plate continues in position with its front open. I do so through the help of a reflector temporarily interposed between it and the lens. I do not use the ordinary focussing-screen at all in making my adjustments, but one that is flush, or nearly so, with the roof of the camera. When the reflector is interposed, the image is wholly cut off from the sensitized plate, and is thrown upwards against this focussing-screen, *g*. When the reflector is withdrawn, the image falls on the plate. It is upon this focussing-screen in the roof that I see the fiducial lines by which I make all the adjustments. Nothing can be more convenient than the position of this focussing-screen for working purposes. I look down on the image as I do upon a book resting on a sloping desk, and all the parts of the apparatus are within an easy arm's length.

My reflector in my present instrument is, I am a little ashamed to confess, nothing better than a piece of looking-glass fixed to an axle within the camera, near its top left-hand edge. One end of the axle protrudes, and has a short arm; when I push the arm back, the mirror is raised; when I push it forward, it drops down. I used looking-glass because the swing action is very true, and as my apparatus was merely a provisional working model, made of soft wood, I did not like to use sliding arrangements which might not have acted truly, or I should certainly have employed a slide with a rectangular glass prism, on account of the perfect reflection it affords. And let me say, that a prism of 2 inches square in the side is quite large enough for adjustment purposes, for it is only the face of the portrait that is wanted to be seen. I chose my looking-glass carefully, and selected a piece that was plane and parallel. It has not too high a polish, and therefore does not give troublesome double reflections. In fact, it answers very respectably, especially when we consider that perfection of definition is thrown away on composites. I thought of a mirror silvered on the front of the glass, but this would soon tarnish in the gas-light, so I did not try it. For safety against the admission of light unintentionally, I have a cap to the focussing-screen in the roof, and a slide in the fixed body of the instrument immediately behind the reflector and before the dark slide. Neither of these will be wanted when the reflector is replaced by a prism, set into one end of a sliding block that had a large horizontal hole at the other end, and a sufficient length of solid wood between the two to block out the passage of light both upwards and downwards, whenever the block is passing through the half-way position.

As regards the fiducial lines, they might be drawn on the glass screen; but black lines are not, I find, the best. It is far easier to work with illuminated lines; and it is important to be able to control their brightness. I produce these lines by means of a vertical transparency, set in an adjustable frame, connected with A, and having a gas-light behind it. Below the eye-hole *e*, through which I view the glass screen *g*, is a thin piece of glass set at an angle of 45°, which reflects the fiducial lines and gives them the appearance of lying on the screen, the frame being so adjusted that the distance from the thin piece of glass to the transparency and to the glass screen *g* is the same. I thus obtain beautiful fiducial lines, which I can vary from extreme faintness to extreme brilliancy, by turning the gas lower or higher, according to the brightness of the image of the portrait, which itself depends on the density of the transparency that I am engaged upon. This arrangement seems as good as can be. It affords a gauge of the density of the negative, and enables me to regulate the burners behind it, until the image of the portrait on *g* is adjusted to a standard degree of brightness.

For convenience in enlarging or reducing, I take care that the intersection of the vertical fiducial line with that which passes through the pupils of the eyes shall correspond to the optical axis of the camera. Then, as I enlarge or reduce, that point in the image remains fixed. The uppermost horizontal fiducial line continues to intersect the pupils, and the vertical one continues to divide the face symmetrically. The mouth has alone to be watched. When the mouth is adjusted to the lower fiducial line, the scale is exact. It is a great help having to attend to no more than one varying element. The only inconvenience is that the image does not lie in the best position on the plate when the point between the eyes occupies its centre. This is easily remedied by using a larger back with a suitable inner frame. I have a more elaborate contrivance in my apparatus to produce the same result, which I need not stop to explain.

For success and speed in making composites, the apparatus should be solidly made, chiefly of metal, and all the adjustments ought to work smoothly and accurately. Good composites cannot be made without very careful adjustment in scale and position. An off-hand way of working produces nothing but failures.

I will first exhibit a very simple but instructive composite effect. I drew on a square card a circle of about 2½ inches in diameter, and two cross lines through its centre, cutting one another at right angles. Round each of the four points, 90° apart, where the cross cuts the circle, I drew small circles of the size of wafers and gummed upon each a disc of different tint. Finally I made a single black dot half-way between two of the arms of the cross. I then made a composite of the four positions of the card, as it was placed successively with each of its sides downwards. The result is a photograph having a sharply defined cross surrounded by four discs of precisely uniform tint, and between each pair of arms of the cross there is a very faint dot. This photograph shows many things. The fact of its being a composite is shown by the four faint dots. The equality of the successive periods of exposure is shown by the equal tint of the four dots. The accuracy of adjustment is shown by the sharpness of the cross being as great in the composite as in the original card. We see the smallness of the effect produced by any trait, such as the dot, when it appears in the same place in only one of the components: if this effect be so small in a series of only four components, it would certainly be imperceptible in a much larger series. Thirdly, the uniformity of resulting tint in the composite wafer is quite irrespective of the order of exposure. Let us call the four component wafers A, B, C, D, respectively, and the four composite wafers, 1, 2, 3, 4; then we see, by the diagram, that the order of exposure has differed in each case.

Composite.		Successive places of the Components.							
1	2	A	B	D	A	C	D	B	C
4	3	D	C	C	B	B	A	A	D

In 1 it has been A, D, C, B,
" 2 " B, A, D, C,
" 3 " C, B, A, D,
" 4 " D, C, B, A;

yet the result is identical. Therefore the order of exposure has no effect on the result.

I will next show a series consisting of two portraits considerably unlike to one another, and yet not so very discordant as to refuse to conform, and of two intermediate composites. In making one of the composites I gave two-thirds of the total time of exposure to the first portrait, and one-third to the second portrait. In making the other composite, I did the converse. It will be seen how good is the result in both cases, and how the likeness of the longest exposed portrait always predominates.

The next is a series of four composites. The first consists of 57 hospital patients suffering under one or other of the many forms of consumption. I may say that, with the aid of Dr. Mahomed, I am endeavouring to utilize this process to elicit the physiognomy of disease. The composite I now show is what I call a hotch-potch composite; its use is to form a standard whence deviations towards any particular sub-type may be conveniently gauged. It will be observed that the face is strongly marked, and that it is quite idealised. I claim for composite portraiture, that it affords a method of obtaining *pictorial averages*, which effects simultaneously for every point in a picture what a method of numerical averages would do for each point in the picture separately. It gives, in short, the average tint of every unit of area in the picture, measured from the fiducial lines as co-ordinates. Now every statistician knows, by experience, that numerical averages usually begin to agree pretty fairly when we deal with even twenty or thirty cases. Therefore we should expect to find that any groups of twenty or thirty men of the same class would yield composites bearing a considerable likeness to one another. In proof that this is the case, I exhibit three other composites: the one is made from the first 28 portraits of the 57, the second from the last 27, and the third is made from 36 portraits taken indiscriminately out of the 57. It will be observed that all the four composites are closely alike.

I will now show a few typical portraits I selected out of 82 male portraits of a different series of consumptive male patients; they were those that had more or less of a particular wan look, that I wished to elicit. The selected cases were about 18 in number, and from these I took 12, rejecting about 6 as having some marked peculiarity that did not conform well with the remaining 12. The result is a very striking face, thoroughly ideal and artistic, and singularly beautiful. It is, indeed, most notable how beautiful all composites are. Individual peculiarities are all irregularities, and the composite is always regular.

I show a composite of 15 female faces, also of consumptive patients, that gives somewhat the same aspect of the disease; also two others of only 6 in each, that have in consequence less of an ideal look, but which are still typical. I have here several other typical faces in my collection of composites; they are all serviceable as illustrations of this memoir, but, medically speaking, they are only provisional results.

I am indebted to Lieut. Leonard Darwin, R.E., for an interesting series of negatives of officers and privates of the Royal Engineers. Here is a composite of 12 officers; here is one of 30 privates. I then thought it better to select from the latter the men that came from the southern counties, and to again make a further selection of 11 from these, on the principle already explained. Here is the result. It is very interesting to note the stamp of culture and refinement on the composite officer, and the honest and vigorous but more homely features of the privates. The combination of these two, officers and privates together, gives a very effective physiognomy.

Let it be borne in mind that existing cartes-de-visite are almost certain to be useless. Among dozens of them it is hard to find three that fulfil the conditions of similarity of aspect and of shade. The negatives have to be made on purpose. I use a repeating back and a carte-de-visite plate, and get two good-sized heads on each plate, and of a scale that never gives less than four-tenths of an inch between the pupils of the eyes and the mouth. It is only the head that can be used, as more distant parts, even the ears, become blurred hopelessly.

It will be asked, of what use can all this be to ordinary photographers, even granting that it may be of scientific value in ethnological research, in inquiries into the physiognomy of disease, and for other special purposes? I think it can be turned to most interesting account in the production of family likenesses. The most unartistic productions of amateur photography do quite as well for making composites as those of the best professional workers, because their blemishes vanish in the blended result. All that amateurs have to do is to take negatives of the various members of their families in precisely the same aspect (I recommend either perfect full-face or perfect profile), and under precisely the same conditions of light and shade, and to send them to a firm provided with proper instrumental appliances to make composites from them. The result is sure to be artistic in expression and flatteringly handsome, and would be very interesting to the members of the family. Young and old and persons of both sexes can be combined into one ideal face. I can well imagine a fashion setting in to have these pictures.

Professional skill might be exercised very effectively in retouching composites. It would be easy to obliterate the ghosts of stray features that are always present when the composite is made from only a few portraits, and it would not be difficult to tone down any irregularity in the features themselves, due to some obtrusive peculiarity in one of the components. A higher order of artistic skill might be well bestowed upon the composites that have been made out of a large number of components. Here the irregularities disappear, the features are perfectly regular and idealised, but the result is dim. It is like a pencil drawing where many attempts have been made to obtaim the desired effect: such a drawing is smudged and ineffective; but the artist, under its guidance, draws his final work with clear bold touches, and then he rubs out the smudge. On precisely the same principle the faint but beautifully idealised features of these composites are, I believe, capable of forming the basis of a very high order of artistic work.

Mr. Warnerke said that when Mr. Galton published the first description of his process, five or six years ago, he tried it, and was much pleased with the result of his experiments, which were attended with considerable success. At that time the system was by no means so perfect as it was now, and did not permit of the use of separate negatives in the manner described that evening. The system he (Mr. Warnerke) used, as described by Mr. Galton at that time, was simply to put several sitters in exactly the same position, and expose one after the other on the same plate. He obtained very fine results, and many present at the meeting may have seen the example in his possession. After this he tried the plan of taking transparencies, and, by superimposing them, obtaining a composite portrait. He quite agreed with Mr. Galton's suggestion that composite portraiture might be adapted to the use of professional photographers, and might even be made very remunerative.

Captain Abney: The paper just read is one of great scientific value, and must have required much careful study: few people, perhaps, would have the time to give to the subject, which is one that Mr. Galton has made specially his own. In regard to the experiments of the composite of the four wafers, it was not superfluous in any case to mention the results obtained, as I should certainly have thought there might have been a difference according to the order of exposure. I presume Mr. Galton uses gelatine plates?

Mr. Galton: Yes.

Captain Abney: If you give exposure enough to bring out of the transparency one-fourth of the shades in the dark parts, the exposure might prove to be too much for the light parts; and when all the negative had been exposed, these light parts might begin to show a reverse action. However, this does not seem to have been the case, and apparently Mr. Galton has settled the point satisfactorily. There is one other point also in reference to his method of illuminating his negatives. It is sometimes very awkward if you get a bright spot behind a dark line, or *vice versâ*, for you get the image of the bright spot as well as the dark line. I will give an instance of this. The other day, in taking some transparencies of spectra, my assistant used a ground glass behind the negative, and he took for this purpose the focussing-screen of the camera, on which were ruled lines for regulating the sizes of negatives, and he was very much annoyed to get on his negatives dark streaks in the opaque portions. I traced these streaks to the images of the ruled lines. I would like to ask Mr. Galton if, instead of using ground glass, he has ever tried a solution of wax in ether at the back of the negative. This might, perhaps, be worth trying, as the surface given is extremely smooth and even. I am glad Mr. Galton has been so successful with the composite portrait of the officers and men of the Engineers. I was not aware the typical officer was so good looking as he appears to be, and I cannot help feeling a certain amount of satisfaction in being connected with the corps on this account alone [*laughter*].

Colonel WORTLEY: Perhaps Mr. Galton could explain how it is that in the composite portrait of officers and men combined, where the negatives of twelve officers and thirty men have been used, the expression of the features of the smaller number seem to predominate?

Mr. GALTON observed that Colonel Wortley was in error in regard to the proportions. The picture shown was made from the negatives of twelve officers, but only eleven men. Mr. Galton further observed as a curious instance of the way in which opinions differed as to likenesses, that he gave to an artist the composite portrait formed from two criminal boys to draw on the wood. The artist had never seen either of the boys, yet the picture he drew was the portrait, not of the composite, but of one of the boys whose picture formed a part of the composite. He (Mr. Galton) took this to exemplify the different opinions which people formed of likenesses, just as persons often disagreed as to which member of its family a child most resembled.

The PRESIDENT, in asking the Meeting to pass a vote of thanks to Mr. Galton, spoke of the pleasure which the paper had given, and congratulated Mr. Galton upon the success which had attended his many experiments. It was very singular to see how individual peculiarities were lost and merged into the type of persons upon whom the experiments had been made. Pictures of ethnological types, even of those belonging to our own country, would be very valuable, as indicating the characteristics of different races. He (the President) could only regret that the Photographic Society's contract with the Water Colour Society prevented Mr. Galton from exhibiting his experiments by means of the lantern, as that would have shown the results in a more striking way. However, as it was, Mr. Galton had given the Meeting a great treat, and he had no doubt a hearty vote of thanks would be passed. A vote of thanks was then passed to Mr. Galton by acclamation.

The following paper by Captain Abney, owing to the lateness of the hour, was taken as read:—

UTILIZATION OF THIN GELATINE PLATES.

By CAPTAIN ABNEY, R.E., F.R.S.

WHAT I am about to describe is not new, but it has scarcely been given sufficient prominence, I think, and it may be that it may aid in the utilization of thin gelatine plates, which otherwise would not be wasted. I have by me some gelatine plates which are very thinly coated,—so thin, indeed, that when the image is developed right through to the back of the plate the density is far less than it should be. It struck me that it was a pity to waste these plates, since they were exquisitely sensitive and bright, so long as the image was not forced in order to obtain density. The question arose as to whether sufficient density could be given before fixing, and the plates were so transparent that the density could be easily judged by transmitted light. Plates were exposed and developed with ferrous oxalate till an image, full of detail, was seen by reflected light. The plate was then washed and plunged in the alum (potash alum) bath for a couple of minutes, and then washed under the tap. Ordinary pyrogallic and citric acid intensifylng solution was then applied to the film and allowed to soak in for half a minute, and then ten drops of a twenty-grain solution of silver nitrate to each ounce of intensifier added. The image came up rapidly and easily, without the slightest tendency to stain at any part, and perfect opacity could have been obtained without the slightest difficulty. The plate was then rinsed and placed again in the alum bath, and then fixed in the usual manner. The negative was satisfactory in every particular, except in one which a very fastidious critic would have condemned. The gelatine had a *very slight* brown tone—not enough to interfere with the printing qualities, but still it was there, much less indeed than is seen with ordinary alkaline development. Another negative, treated with the iron intensifier, the formula for which I have often given in the *Journal*, was totally free from this tint, showing it to be a species of dye caused by the oxidation of the pyrogallic acid. Other negatives were developed with alkaline developer and intensified in the same manner, and exhibited the same increased density of image. I think the immersion in the alum bath an essential for success, I would treat an alkaline developed plate with pyrogallic in preference to an iron intensifier, and an iron developed negative with an iron intensifier. The reason will be apparent when it is considered what iron and pyrogallic acid might perchance form. I have not met with any stain due to this, but it is possible that it might occur.

Some of the plates experimented with were made of very soft gelatine, and it was curious to notice in some cases during intensification that the heat from the fingers caused the gelatine to run; but there were no blisters or frilling. When a dish was used, of course this disaster did not occur.

Local intensity can be given in this way with great success, and I exhibit a plate, one part of which has been intensified and the rest left in its normal condition. It will be seen that the vigour of the one part is extreme, whilst the weakness of the other is very great. It would be interesting if others would try this plan and report. It might happen that certain gelatines were apt to stain, whilst others were free from the tendency. All I can say is, that all the gelatines used by me have behaved properly, intensity being given without any stain whatever, except that very slight one already mentioned.

Mr. WARNERKE then made the following communications:—

IMPROVEMENT IN THE PROCESS DESCRIBED LAST MONTH;

AND

EXPERIMENT RELATIVE TO THE CAUSE OF SENSITIVENESS OF GELATINE EMULSION.

By LEON WARNERKE.

AT the last Meeting of this Society, I described a new process. It will be remembered that in this process a sheet of paper is covered with gelatine emulsion, exposed in the camera, developed with pyrogallol, and then cemented to the glass or other impermeable support; and by treatment with hot water, from the side opposite to that exposed to the light, the image is obtained; but this image is reversed, and, if required for silver printing, it could not be used without double transfer. Since that time I have succeeded in doing away with the necessity for double transfer by the following method:—After the sheet of gelatine-emulsion tissue is exposed and developed with pyrogallol, a glass plate is rubbed with talc (French chalk) and covered with plain collodion. When the collodion is set, but before drying, it is immersed in cold water and left until all greasiness disappears. The developed image is then put in contact with collodionized glass, all excess of water removed with an india-rubber squeegee, and the glass is left to rest for about ten minutes. It is next immersed in warm water, the paper which was used as the support of the tissue is peeled off, and all soluble gelatine removed by washing. Next, a sheet of gelatine, similar to that used by engravers for tracing, is immersed in cold water until softened, and, by means of the squeegee, cemented to the previously dried image. When this is dry, which takes place in the present season (June) in about two to four hours, it is easily peeled off, and in this condition it presents a flat sheet of gelatine, with the image permanently fixed. In order to render it proof against every possible accident that can occur during the printing, it is advisable to give a coating of collodion before the gelatine sheet is peeled off. Prepared in this way, a film negative being protected from both sides with collodion, it cannot be injured with damp, so that it may be washed with sponge or immersed in water for a short time. It can be printed from both sides, and it will be seen by the specimens sent round for inspection that the thickness of gelatine does not affect the sharpness in printing. It is useless to dwell upon the utility of having negatives upon the film rather than on glass.

Since my last communication regarding gelatine emulsion, several persons have tried the system, and some have complained that the developed image is rendered totally insoluble by the action of pyrogallol, or that the surface-film remains insoluble. This will always be found to be the case when gelatine emulsion is rendered insoluble by the addition of alum, or other treatment, during its preparation; therefore that kind of emulsion must be used which produces a film soluble in warm water. I found, however, that, even in the case of insolubility, the addition of acetic acid, and using the water warmer, are a great help.

I should like to communicate a phenomenon observed by myself, which may help towards the explanation of the cause of the sensibility of silver bromide. I found that if the surface of a gelatine emulsion plate—glass or paper—is pressed, those parts which are subjected to pressure are rendered less sensitive; this is made evident in the development. At first I thought that the pressed parts are not so easily acted upon by the developer; but the following experiment cleared up my doubts. I wrote on a sensitive plate some words with an ivory stylus—not scratched, then exposed it to light, and I wrote again after exposure in a similar manner: in the development the words written before the exposure appeared light on a dark ground, and very distinct; those written after the exposure, either were not visible at all, or appeared darker than the rest of the plate: and this experiment was tried many times. Reasoning that if the compression of the film diminishes the sensitiveness, the reverse action must have the effect of increasing it, I moistened with water the film before exposure, and the result gave the proof of my supposition.

The approaching summer recess, and my own intended absence from England, decided me to hasten the communication of this isolated observation, in order to contribute data showing that the previously expressed opinion by Captain Abney is strictly correct, viz., that the superior sensitiveness of bromide of silver in gelatine is not due to any difference in its chemical composition, but solely to the difference in its molecular conditions as compared with collodion emulsion. It is also evident that it will not be safe to rub or otherwise press the sensitive surface. In the experiments described I found that a very slight pressure produced a visible effect; in fact, my first observation was made on the sheet of tissue at the back of which a number was written with pencil.

Captain ABNEY: In a part of his communication Mr. Warnerke mentioned that acids caused insolubility in gelatine. I believe, however, that it is an accepted rule that the reverse is the case, *i.e.*, that they dissolve gelatine. Mr. Warnerke is right, however, for a very small quantity of hydrochloric acid will render gelatine insoluble, but if you increase the amount, the solubility increases very much. As to the diminution of sensitiveness by pressure, this is in confirmation of my own published experiments. In cases where you get a green or a blue bromide the sensitiveness is very great; but if pressure be exercised the bromide becomes red, which is much less sensitive, so much so that the rays which affected the film before no longer do so. It is evident that the change is due to molecular influence; indeed, I do not see there is any ground for the opinion that the change is a chemical one. Ozone and oxygen, aldehyde and paraldehyde, red and blue gold, all have the same chemical composition, but different molecular grouping. You can get a blue, red, or violet bromide; and it all depends how you treat that chemical substance, as to which molecular grouping you get. I may here say it seems theoretically impossible to get the highest state of sensitiveness in gelatine, because you cannot get the bromide of silver otherwise than in a strained condition. As Mr. Warnerke has stated, when this strain is taken off by moistening the film the sensitiveness is increased. I have always held, and shall continue to do so until proof to the contrary is brought forward, that sensitiveness is a matter of molecular grouping, and not of chemical change.

Mr. BERKELEY: Are we to understand from Mr. Warnerke's remarks, that gelatine emulsion before it is dried is more sensitive than after it is dried?

Mr. WARNERKE: No. It must first be dried, and then moistened.

Colonel WORTLEY: Has Captain Abney ever noticed that the sensitiveness of gelatine emulsion is affected by the mode in which it is shaken during preparation? I have noticed that it is far more sensitive when the mixing is done delicately than when it is done vigorously.

Captain ABNEY: I cannot say I have ever remarked this. In mixing the emulsion, I generally do it gently. I might add with regard to the effect of the presence of gelatine on bromide of silver, that you will get the greatest sensitiveness in bromide of silver when it is unrestrained by external influence. Gelatine acts as a restrainer in development, but it also restrains the bromide from taking the most sensitive form.

Colonel WORTLEY: Do you mean in the form as it is used on a Daguerrotype plate?

Captain ABNEY: Not quite, but something akin to that.

The PRESIDENT then presented the silver Progress Medal, which had been awarded to Mr. Willis for his platinotype process, to Mr. H. B. Berkeley, for transmission to Mr. Willis, who was now in America.

The PRESIDENT: The Progress Medal has been awarded to Mr. William Willis, jun., for his invention of the Platinotype process. The excellence of the results obtained by this process has been rendered evident by the specimens shown in our two last exhibitions, and in the very fine enlargements exhibited at the Society's meeting in April, 1880. These mark a new era in photography, the prints being produced in platinum black, and they are believed to be permanent. The mode of working was explained by Mr. Willis at the meeting of this Society in December, 1878, and no one who was then present can have forgotten the rapidity with which the platinum prints were developed before us. As early as 1832, Sir John Herschel communicated to the British Association at Oxford the fact that paper treated with platino-cyanide of potassium was sensitive to light, and that prints could be procured in a comparatively short time by development with nitrate of mercury. This is a perfectly different system of working; and there were difficulties in fixing the image, for it was stated that nearly all the "platinotypes slowly fade in the dark;" and the same observer also found that ferric oxalate was sensitive to light, and he developed prints so obtained with gold. In later times the quality of permanence has come to be regarded as one of the chief recommendations of the platinotype process, which in the hands of Mr. Willis has gradually progressed so far towards perfection that pure platinum-black, without silver or any other metal in admixture, constitutes the sole ingredient left in the texture of the paper. The artistic capabilities of the process have been already recognised by our Exhibition judges, who last year awarded a bronze medal to the Platinotype Company; and now I have the pleasure of handing the silver medal of "Progress" to Mr. Willis, as a token of the scientific value the members of this Society attach to his invention, and in the prosecution of which they wish him further success.

Mr. BERKELEY, on behalf of Mr. Willis, expressed his thanks, and stated that Mr. Willis was engaged in perfecting the process, and hoped before long to lay something new before the Society.

The PRESIDENT announced that the next Technical Meeting, which would be presided over by Captain Abney, would take place on June 28th. The first meeting had been presided over by Mr. Davis, and he (the President) hoped the second would be as successful as the first had been, every one who was present speaking of it as giving a spurt to the proceedings of the Society, which no doubt would prove beneficial.

The President then stated that the Judges of Awards for the 1881 Exhibition would be Mr. George D. Leslie, R.A., Mr. Hubert Herkomer, A.R.A., the President of the Society, Messrs. Jabez Hughes, Payne Jennings, W. Dillworth Howard, and Seymour Conway.

In conclusion, the President wished the Members success in their work during the recess.

The Meeting was then adjourned to—

Saturday, October 8th (Soirée).

THE FIRST TECHNICAL MEETING

OF THE

PHOTOGRAPHIC SOCIETY OF GREAT BRITAIN

Was held on Tuesday, May 24th, 1881, at the Gallery, 5a, Pall Mall East.

T. Sebastian Davis, F.C.S., Vice-President., in the Chair.

The following is a very brief abstract of the proceedings :—

The Chairman stated the reasons which had led to a trial of these informal meetings, and, after a few further remarks, a discussion was opened by

Mr. Peek, who inquired whether, when bichloride of mercury was used for intensifying negatives, some other body might not be found which would counteract those defects which attended its use?

Mr. Bedford and Mr. York both concurred that mercury should not be used for intensification, and the discussion gradually turned on other methods of intensification.

Mr. Warnerke stated that the bromide of copper intensification did not answer in his hands.

The Chairman expressed his view of the great desirability of maintaining a distinction between the intensification of correctly exposed and over-exposed plates, suggesting the possibility of the effect of light creating a chemical reduction of silver salts, of two distinct characters intermediate between silver bromide and metallic silver. It would be desirable to know whether the intensification of negatives by the methods under the consideration of the meeting was assumed to have received a proper exposure or to be over-exposed.

As regards intensification by means of silver, it was stated that the character of the emulsion seemed to influence the result in a great measure.

Messrs. Berkeley, Peek, and Cowan then introduced the subject of the cause of red fog on gelatine plates. It was stated that the quantity of developing solution had no bearing on its production.*

A statement was made by Mr. Cobb, which is worthy of attention—that a plate which had what is known as "green fog" gave the best results in printing.

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* Red fog is evidently entirely due to a dye formed by the pyro-gallic acid with the gelatine, since it is never met with when ferrous oxalate is employed.—Editor.

The discussion next turned upon the best mode of developing a gelatine plate.

Mr. SPILLER said that, in his view, it was better to begin with pyro-gallic acid alone, without washing the plate.

Mr. S. FRY stated that he had had the opportunity of asking 500 photographers as to which method of development was most in favour, and that without doubt Edward's glycerine developer was the favourite. This, he thought, virtually put ferrous oxalate out of the running.

Mr. COBB expressed his idea that the glycerine developer was unstable; and Mr. ENGLAND stated that he had given up using it, owing to having found it of no advantage. Mr. MAWDSLEY concurred in this.

Mr. YORK advocated the use of three times the quantity of bromide to the developer to that usually employed. The general consensus of opinion was that each make of plate required its own formula, and that the less pyro-gallic acid used the longer the development of a plate would take.

The question next arose as to the discoloration of the film, and whether it was advisable or necessary to wash a gelatine plate before it was immersed in the fixing bath. The meeting appeared divided in opinion as to this, and the Chairman proposed that the question should be discussed at the next technical meeting.

Mr. COBB showed a model of his apparatus for levelling plates during coating, and claimed for it the advantage, that the long stage so generally used was thus done away with. The apparatus met with general approval.

After a few remarks by Mr. WARNERKE on his new process, which will be found in the Transactions of the 14th instant, the Meeting was closed.

NOTES.

THE EXHIBITION.—The following are the names of the Judges of Awards for the forthcoming Exhibition:—G. D. Leslie, R.A.; Hubert Herkomer, A.R.A.; the President of the Society, James Glaisher, F.R.S.; Jabez Hughes; Payne Jennings; W. Dillworth Howard; and Seymour Conway.

ON THE WASHING OF GELATINE EMULSION; AND ON AN EFFECTIVE METHOD OF CONTROLLING THE SAME.—Dr. EDER writes as follows to the *Photographic News*:—1. The question whether the time necessary for washing a gelatine emulsion could not be shortened has often been raised, and while some experimenters maintain that a half-an-hour's washing is quite sufficient, there are others who prescribe one of at least twenty-four hours. In order to obtain an exact solution of this problem, I undertook a series of experiments to ascertain the nature of the washing process, in the course of which I arrived at several highly interesting results.

I took a gelatine emulsion containing 5 per cent. of gelatine and 2 per cent. of excess of soluble bromide, and squeezed it through coarsely woven canvas; in one set of experiments, the canvas employed had meshes of 4 mm. in diameter; in another it was more closely woven, and the meshes were of from 1 to 1½ mm. apart. Separate portions of this emulsion, each weighing 25 grams, were then washed, some by being laid in running water, others by suspension (enclosed in a small bag of fine stuff) on the upper surface of a large vessel full of water. The amount of soluble bromide still remaining in the emulsion was now quantitatively determined at occasional intervals, and the following results, which I have collected in the form of a table, were obtained:—

A.—*Gelatine after passing through canvas of 4 mm. mesh.*

	Percentage of Soluble Bromide remaining in the Emulsion.	
	Potassium Bromide.	Ammonium Bromide.
Original emulsion	2·00	1·65
After 35 minutes in standing water	0·42	0·32
Do. do. running do.	0·20	0·18
After $1\frac{1}{4}$ hours in standing do.	0·08	0·07
Do. do. running do.	0·04	0·04
After 12 do. standing do.	0·04	0·03
Do. do. running do.	0·02	0·01

B.—*Gelatine after passing through canvas of from 1 to $1\frac{1}{2}$ mm. mesh.*

	Potassium Bromide.	Ammonium Bromide.
Original emulsion	2·00	1·65
After 35 minutes in standing water	0·05	0·05
Do. do. running do.	0·03	0·04
After $1\frac{1}{4}$ hours in standing do.	0·02	—

It follows from this that 35 minutes' washing, either in standing or running water, is not sufficient for an emulsion passing through the larger mesh, since the amount of soluble bromide still remaining, which is as much as from 0·2 to 0·4 per cent., has a perceptibly injurious effect on its sensitiveness. The emulsion which had passed through the finer canvas was sufficiently washed in running water at the end of 35 minutes, and not quite so effectively in standing water; $1\frac{1}{4}$ hour's washing in running water was also sufficient when the larger mesh was used; the same time in standing water gave tolerably good, though not quite so satisfactory, results. The same percentage of bromide is washed out after $1\frac{1}{4}$ hours in running water, as after a much longer time in standing water. It is worthy of note that potassium bromide and ammonium bromide are got rid of with about equal rapidity.

Photographers may thus convince themselves that when the emulsion is finely subdivided (that is, squeezed through canvas with a 1 to $1\frac{1}{4}$ mm. mesh) a washing of from a half to three-quarters of an hour in running water, or even the same time in standing water, provided it is frequently changed, will suffice; further, that when more coarsely shredded (passed through a network of 4 mm. mesh), from one and a half to two hours' washing in running water is required in order to get rid of all injurious bromide, and that in standing water (which is often changed) a longer time—three hours and more—is necessary. Speaking generally, however, I consider that squeezing through a more loosely woven stuff is to be recommended, in spite of the longer time required for washing, because afterwards, when drained, less water adheres mechanically. It should also be observed that the length of time required for washing is affected by various secondary considerations; for instance, by the temperature of the water, by the hardness and toughness of the gelatine, &c.

2. Testing the emulsion for any remaining soluble bromide can, according to my own observations, be readily effected: 25 grams of the emulsion are weighed out and dissolved in an excess of hot water; this solution is then allowed to grow quite cold, a little neutral potassium bichromate is added, and then a sufficient quantity of a solution of silver nitrate of known strength is mixed with it, to turn the light yellow colour into red.

The above is the established method of testing; but I can recommend, in preference, the following convenient and simple means of ascertaining the amount of soluble bromide remaining in the emulsion. A solution is prepared containing accurately 4 grams of silver nitrate in a litre of water. Of the emulsion to be tested, 25 grams are weighed out in a fluid state, diluted with from four to five times its volume of water, then cooled, and mixed with neutral potassium chromate until the solution assumes a manifest yellow tint. Into this solution are stirred in 10 cub. cents. of the above-named silver solution, producing—when the emulsion has been properly washed to answer ordinary purposes—a reddish-yellow to deep red colour. Insufficiently washed emulsion shows no change of tint. This reaction will show a trace of soluble bromide as little as 0·1 per cent.

Emulsions that have been thoroughly washed show a change of colour with even 5 cub. cents. of the silver solution, and in that case they contain less than ·05 or ·06 per cent. of soluble bromide. Should the addition of even 20 cub. cents. of the silver solution not redden the mixture, the emulsion must have been very inefficiently washed. The test succeeds only in neutral aqueous solutions of gelatine: those which contain acetic acid should have the acid neutralised before being submitted to the test. The whole analysis should be carried out in daylight; ordinary candlelight or gas-light will, however, also serve the purpose, as the change of colour is readily discernible by artificial light.

GUM AND IRON PROCESS FOR REPRODUCING, IN BLACK, LINES ON A WHITE GROUND.—The following is an extract from a patent specification, which was taken out by M. Joltrain, on the 24th of March last, "for a photographic process for reproducing drawings, maps, and designs for lace, in black line on white ground:" this process is called, by the inventor, gommo-ferrique. M. Joltrain's sensitive liquid gives a rapidity requiring only a few seconds of exposure in the sun, and a few minutes in the shade or even in an intense fog. The photographic paper prepared with this liquid gives results which allow of the reproduction of drawings made only with an ordinary pencil. The reaction in the developing bath takes place instantaneously. The following is the composition of the liquid:—

Gum	25 grams.
Sodium chloride	3 "
Ferric chloride at 45° Beaumé	10 c.c.
Ferric sulphate	5 grams.
Tartaric acid	4 "
Water to make up to	100 c.c.

The developing bath may be a more or less concentrated solution of red or yellow prussiate of potash; it may be either neutral, acid, or alkaline. After exposure, the print is plunged into the developer, and the parts which have been preserved from the action of light by the lines, turn a deep green colour, while the rest of the sheet undergoes no change. It is washed in water to remove the prussiate and to stop the reaction, and it is then placed for some minutes in a bath acidulated with either acetic, hydrochloric, or sulphuric acid, where all the substances which may affect its whiteness are removed, and the lines assume a black colour, with an indigo reflection. It is once more washed in pure water, and then dried.—LEON VIDAL.

A NEW MERCURY INTENSIFIER.—Messrs. COSMO BURTON and A. P. LAURIE described before the Edinburgh Photographic Society a mercury intensifier which they consider excellent.

A saturated solution of mercuric bromide (solubility 1 in 250) is prepared and the negative plunged into it till it is thoroughly bleached. Ordinary development is then applied after well washing, and the silver bromide formed is reduced to the metallic state. Ferrous oxalate is preferred as the developer. This intensifier has many features in common with the cupric bromide intensifier often described, but it should be effective, provided that the washing is very completely done before the developer is applied.

DEVELOPMENT OF GELATINE NEGATIVES.—Colonel WORTLEY, writing to the *British Journal of Photography*, describes his mode of development and testing his plates, as follows:—In order thoroughly to get at the bottom of the capabilities of any class of plate I proceed as follows:—Having mixed the solution according to formula, I immerse the plate in it for two and a half minutes. I then add again half the original quantity of ammonia, bromide, and pyro., and immerse two-thirds of the plate for two and a half minutes more, and repeat again, immersing one-third, washing the plate between each. Thus, one-third has been two and a half minutes, one-third five, and one-third seven and a half minutes in the developer. From this I can deduce all I want.

To any one really caring about high-class work, and recognising the absurdity of trying to get such work by mechanically developing all kinds of plates by the same developer, the study of development is most interesting; and it is surprising how much can be done by those able to profit by the result of such study.

Some of your readers have written to me expressing surprise at my saying that some of my best negatives were developed with as much as ten minims of ammonia. A good plate will stand this easily, provided *when the pyro. is added a sufficiency of bromide goes with it.* I say that "when the pyro. is added," because I often develop negatives requiring such treatment by putting on the ten minims of ammonia alone in water first, and after forty or sixty seconds' immersion adding pyro. and bromide; but then a somewhat extra amount of bromide is required to counterbalance the ammonia having acted alone at first. It must be borne in mind that *the ammonia does not produce fog unless the pyro. is with it*, and therefore if, when the pyro. *is* added, sufficient bromide accompanies it you have gained the advantage of the energising action of the ammonia alone, and more detail can be brought out by this than by any other method of working. I know it will startle many to put ten minims of ammonia on to a plate; but, after all, not more than it startled Mr. R. M. Gordon and other friends the first time they saw me apply an eighty-grain solution of carbonate of ammonia to a plate. The gelatine-glycerine restrainer is useful here; it restrains action without stopping it.

By using this method of development, carefully graduating your bromide and pyro. to each class of subject and varied exposure, you will find work come much easier and *better.* By "class of subject" I mean this:—To-day I took two pictures—one, by the quickest possible shutter, of a lady with a cat on her shoulders and a dog jumping up and barking at it; the companion plate was exposed twenty-five seconds on a very dark bit of foliage. It would be impossible to get two *good* negatives of such subjects *unless by widely differing developments,* and I acted thus:—For the first one I began with soaking in water with ten minims of ammonia alone; and then, pouring off, added one and three-quarter grain of bromide and two grains of pyro. For the second one seven minims of ammonia, half-a-grain of bromide, five minims of gelatine-glycerine solution, and one grain of pyro., all mixed at once, was used.

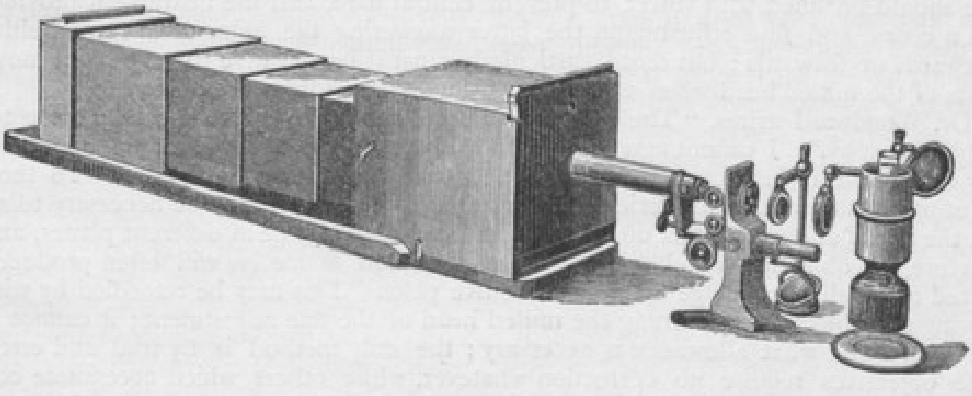
Now for my reason. The first would require *forcing;* therefore ample bromide must be used to keep all clear during the forcing, and two grains of pyro. are needed to give density. (N.B.—I never use plates giving much density. I prefer to bring my plates up to the density I want, rather than to have them taking upon themselves the density I do *not* want.) In the second case: less pyro. and less bromide in proportion to the ammonia are used, because, all the detail being given by the exposure, it was necessary to work so as to avoid any undue density of the sky behind the foliage, which would have ruined the picture.

Now, this brings me to mention two things which I think ought to be engraved on the side or tail-board of every camera:—1. Where the action of *light* on a plate has been *insufficient* no modification of development will give a good negative. 2. Where the action of *light* has been *excessive* a modified development *will* give a good negative. Therefore we see that an instantaneous picture exposed in a bad light *cannot* be developed into a good negative, but one over-exposed *can* be developed by a suitable developer. So we arrive at the suggestion—always expose thoroughly, and do not try instantaneous pictures when there is not light enough to impress them. I fear I have been tedious in entering thus into detail, but I doubt if all these points are sufficiently considered.

A word on intensifying. Personally I have no fear of the stability of mercury-intensified negatives, having by me negatives intensified thus in 1861-2, but these were *very thoroughly washed.* Many failures are due solely to the washing having been insufficient. In intensifying with silver it is always well to use a trace of *nitric* acid with the silver. It keeps the intensifier clear throughout. I do not like the citric acid and alum bath for clearing; its tendency to frill the plates is very strong. With hydrochloric acid no such tendency is found, and I strongly advise it to be used in preference.

One final word: gelatine plates require a thorough washing *after the developer*, and preferably in slightly acidulated water. Where the tendency to staining is strong, use half-a-grain per ounce of chrome alum in the developer.

PHOTO-MICROGRAPHY. BY GEORGE E. DAVIS, F.R.M.S.*—The camera employed is shown in the illustration, and is of the structure and form I specially recommend. You will, no doubt, hear some quite as strongly advising the use of a bellows camera, but there are serious objections to the use of such a one when extended to four or five



feet. This camera, you will observe, is made to detach from the base-board, but for portability only. The base-board should project for some distance in front of the camera, in order to carry the microscope and illuminating apparatus. The whole should be placed upon a firm table, quite free from vibration, in order to ensure perfect sharpness of the image upon the sensitive plate. My original apparatus was of French make and of large size; but it was soon discovered that a large camera was of very little use with the foreign lenses supplied with the apparatus. After this I made one of deal, or rather a portion of one to lengthen an ordinary $7\frac{1}{4}$ inches by $4\frac{1}{2}$ inches camera, with which I was enabled to take several very good pictures. Finally, I have settled down to the one as shown in the illustration, which was made for me by Mr. Furnival, of 5, Kay Street, Ardwick Green, Manchester, who treats miscroscopic cabinet-making as a speciality.

And now, perhaps, you can see the usefulness of a practical treatise on the subject; had I been able to see such a camera as this one at the outset, I should be some pounds richer, and instead of plodding along for a whole year, constantly making alterations, I might have spent the time much more profitably.

Perhaps you will be inclined to say that up to this point I have advanced nothing new, many having written on this subject; but what I wish to say most distinctly is, that it was the very existence of this literature which caused me to spend my money with such an unprofitable result.

The dark slide of the camera shown is made to take a large square plate, with carriers for any less size. The most handy dimensions, especially for the beginner, is the quarter-plate as it is termed, and measures $4\frac{1}{4}$ by $3\frac{1}{4}$ inches; it is only for special purposes that it need be larger. Of course the length of the base-board and of the camera body must depend upon the degree of amplification required, and the diameters to which an object is enlarged in my own camera when the sensitive plate is at a distance of 36 inches from the object may serve as a guide in this direction.

This is when using the microscope without the eye-piece, as is the general practice; but it is sometimes necessary to use this, and, therefore, the list of diameters so yielded

* Abstract of a paper read before the Birmingham Microscopical Society, January 18th, 1881, and also before the Manchester Microscopical Society, March 3rd, 1881.

is given, and compared with the ordinary amplification at ten inches when used as a microscope merely.

Designation	Diameters, Without Eye-piece.	With the A Eye-piece.	At 10 inches.
4 inch	12	36	18
2 ,,	21	63	32
1 ,,	37	110	56
$\frac{1}{2}$,,	80	240	120
$\frac{1}{4}$,,	173	520	260
$\frac{1}{8}$,,	360	1000	540
$\frac{1}{12}$,,	530	1600	800

The microscope employed may be of any ordinary kind, monocular preferably; and if provided with a means of shortening the tube or body, so much the better. The tube should be lined with velvet, to prevent central flare, and the instrument provided with a coarse and fine adjustment, the latter answering the helm when turned either backwards or forwards; but more particularly must it be sensitive to very slight movements of the milled head when *withdrawing* the objective from the object.

Dr. Woodward writes, "The objective selected should always be specially corrected for photography." I cannot agree with Dr. Woodward on this point: no doubt it is very convenient to have one so corrected, but it is by no means imperative. To those as yet uninitiated in the mysteries of the photographic art, it may be necessary to say that the *visual* and *actinic* foci often (though not necessarily) lie in different planes, and, therefore, an objective which gives perfect definition *to the eye* will often produce a blurred and indistinct image upon the sensitive plate. This may be remedied by withdrawing the objective, by turning the milled head of the fine adjustment; it cannot be told beforehand what allowance is necessary; the only method is by trial and error; some objectives require no correction whatever, while others, which necessitate considerable movement from the object, can be made to yield just as perfect results.

The best way to arrive at the necessary correction is to take a picture at the best *visual* focus, develop, fix, and dry it in the usual manner, and then withdraw the objective from the stage by means of the fine adjustment until the image appears on the ground glass about as indistinct as it does in the negative. Ordinary ground glass is too coarse to focus upon; two surfaces should be rubbed together with a little of the finest emery and water, or a focussing-glass may be prepared by coating an ordinary glass plate with spirit varnish, and allowing to dry without heat, or a coating of unboiled rice starch may be applied and allowed to dry on a levelling-stand. The correct focus may be obtained by the eye alone; nevertheless, it is always advisable to use a magnifier—an engraver's lens, or an ordinary eye-piece with the eye-lens removed.

It has been stated by several writers on this subject that monochromatic light brings both *chemical* and *visual* foci into the same plane, and therefore no correction is required. I cannot understand how any writer can have fallen into this error, for error it is, and which a single experiment would have set right. Of course the chromatic aberration is disposed of, but we have introduced a new element into our calculations; a lens which is spherically corrected for white light is not so for monochromatic, and therefore the picture is blurred with spherical aberration.

There now remains but to describe the method of illumination; well, there is the lamp, either an ordinary thirty-hole argand gas-burner, the rays being manipulated with the bull's-eye condenser, or the ordinary paraffin microscope lamp of no special pattern.

The sub-stage achromatic condenser is only used for powers of greater amplification than the half-inch, or where the camera is drawn out to a great extent. I have used several sources of illumination—sunlight, the electric light, the oxy-hydrogen lime-light, the magnesium light, as well as a paraffin lamp and gas; but for general work the two last are preferable, seeing that they give nearly all the light required for medium and low powers, and if a very intense illumination is requisite, it can easily be obtained from gas or oil in such an apparatus as the Sciopticon. Dr. Carl Seiler, in a communication to the *American Journal of Microscopy*, p. 159, 1879, upon this subject, seems to think that wet plates are preferable to dry, and adduces arguments in support of his views. I cannot agree with him, however; dry plates are so convenient, and with good manipulation are capable of producing excellent pictures. There are many kinds of dry plates in the market which are now largely prepared from a gelatine emulsion. Of those I have tried, the preference is given to Swan's, and to those made by Wratten and Wainwright. They are made up in packets of one dozen each, for which is charged three shillings if of quarter-plate size, or measuring $4\frac{1}{4}$ by $3\frac{1}{4}$ inches.

Do not over-expose these gelatine plates, for if you do you will get nothing but thin and misty images. From some cause those exposed to gas or lamp-light do not *commence* to develop so quickly as those exposed to sunlight; but if time is given and they are not over-exposed, perfect pictures may easily be obtained. The time of exposure in the camera depends upon the quality of the light, but more, perhaps, upon the nature of the object than anything else; for instance, the section of a kidney of a rat required twenty minutes' exposure, with the argand gas and bull's-eye condenser, while a crystal of glass under the same conditions of light and amplification required only three minutes.

There are two applications of this art I wish to mention which may be of great utility. I refer to the preparation of wood blocks for the purpose of illustrating microscopical work, and also for the production of lantern transparencies. The greatest expense in producing good illustrations is that of the artist, but if the picture is put upon the wood, the remainder of the work is cheaply and expeditiously performed. One way of doing this is to transfer an autotype carbon picture to the prepared wood block; but to produce good pictures upon wood, it is better to employ the wet-collodion process.

A slightly over-developed positive is produced upon glass in the ordinary way, being developed with an iron developer, and the film transferred to the wood block in the following manner:—The block is first to be coated with a gelatine solution made by soaking one ounce of Nelson's gelatine in nineteen ounces of water for twelve hours, dissolving with heat, and stirring in a solution of twenty grains of chrome alum in one ounce of water. When thoroughly incorporated, add sufficient lamp-black to form an even black coating upon the wood.

If the block be first coated, it will be dry and ready to receive the film by the time the operator has proceeded thus far. The positive having been taken, developed, and fixed, a piece of gum-paper is "squeegeed" to the wet film, which must then be stripped off the glass; the paper and film may now be trimmed with the scissors, and brought into contact with the prepared block under water, gently squeegeed to remove air and water, and set aside for awhile so that the two films may become amalgamated. After a short time the block is soaked in tepid water to remove the gummed paper, and it is then ready for the engraver.

CHANGING THE COLOUR OF GELATINE IMAGES.—Dr. Eder states that the colour of negatives, after fixing, may be changed, by changing the reduced metallic silver into chloride of silver, and then using any of the chloride-of-silver developers described in the April Number of this *Journal*. After immersion into chloride the negative should be exposed to light and be developed in the usual manner. Many varieties of colour may be produced, as indicated in the same *Journal*. To convert the metallic silver into chloride, Dr. Eder recommends a mixture of bichromate of potash and hydrochloric acid; great caution, however, must be used in using the acid, as the film is liable to dissolve. If the quantity of the acid be small, the film becomes toughened, and no dangerous result need be apprehended. The acid is decomposed by the bichromate, however, after a short contact, chlorine being liberated.

COMMITTEE ON SENSITOMETERS.—The Council of the Photographic Society have appointed the following gentlemen to consider the question of a standard sensitometer:—

Captain Abney, V.P.,
Mr. T. S. Davis, V.P.,
Mr. W. Bedford,
Mr. W. B. Bolton,
Mr. Cadett,
Mr. Cowan,
Mr. Warnerke;

with power to add to their number. Any one wishing to have a trial made of a sensitometer should communicate with the Assistant Secretary. This Committee is practically the same as the non-official Committee who have been considering this same question for several months past.

A COLLODION EMULSION PAPER PROCESS.—Mr. W. Brooks continues, in the *British Journal of Photography*, the above subject. He says:—

Collodion emulsion on paper makes a capital printing process, and I have no doubt will be found very useful for rapid printing. In business it very often occurs that a large number of prints are required from one negative by a given time (say for a bazaar, or something of the kind), and this process will step in and supply the long-wished-for want. It is very suitable for small *carte-de-visite* work and for larger portraits or views, as it gives prints of the utmost delicacy; and the photographer is no longer bound to do his printing by daylight, as it can now be done by the light of an ordinary gas jet or oil lamp.

I have produced the best results on the plain gelatinized paper, as described previously, and prepared either by transferring the film from the glass plate or by coating the paper on a flat board. The collodion emulsion requires to be diluted to about half the ordinary consistency with equal parts of ether 720° and alcohol 825°. If the collodion be used of same consistency as for the negative paper process there is a great tendency to bury the detail and give a sunken-in effect to the print.

After the paper is thoroughly dry it is cut up to the different sizes required and kept in a book (flat) for use. It is placed in contact with the negative in the ordinary way, the same as in printing on albuminized paper, and very great care must be taken as regards the exposure. With an ordinary negative to *dull* diffused daylight only an instantaneous exposure must be given; if more be given it will be found to be over-done. Slight under-exposure is better than the least over-exposure; for with the latter the image comes up too quickly, and with the former it comes up very slowly, and, perhaps a little black and white.

The best developer that I have found is the following:—

Saturated solution of oxalate of potash	6 parts.
" " sulphate of iron	1 part.
Sixty-grain solution of bromide of potassium	10 to 12 drops to the ounce.

The bromide solution requires to be added according to the strength of the original negative. If the negative be hard less is required, and if soft more is required to give contrast. I have no doubt that other and, perhaps, better restrainers will be found that may be more suitable. The paper can also be exposed to an ordinary gas jet, a few seconds being sufficient, according to the density of the negative, which can soon be gained by a little practice.

After the paper has been exposed, either to daylight or gaslight, it is placed face uppermost in a clean, white china dish. The developer can be easily applied at once, or the paper can be soaked in water for a few minutes, drained, and then the developer applied. The development takes from two to three minutes under ordinary circumstances. The operation had better be stopped just before the image appears strong enough, as in the subsequent washing the development appears to go on for a time. After it has been well washed the print is fixed in—

Hypo.	2 ounces.
Carbonate of ammonia...	2 drachms.
Water	1 pint.

Half-a-drachm of liquor ammonia can be substituted for the carbonate of ammonia. The print is then well washed in running water. Should the developer seem not to give sufficient density, one drop of the following to each ounce of developer will be found to increase it:—

Hyposulphite of soda	5 grains.
Water	1 ounce.

I do not recommend the use of this except in extreme cases. I find it also confers density in the negative paper process. Great care must be taken not to have any hypo. about the fingers when handling the prepared paper, for wherever it touches it developes into an intense black stain. After the prints have been thoroughly washed from the hypo., if the colour appear at all greenish, they can be toned by either of the following toning baths. I have been successful with the ordinary lime bath, namely :—

Chloride of gold	15 grains.
Chalk	2 drachms.
Chloride of lime	25 grains.
Water	2 quarts.

The older this bath is the better it seems to work. The other toning bath is—

Bichloride of platinum	4 grains.
Water	1 pint.

A few minutes' immersion in the gold bath will soon alter the colour. If toned in the platinum bath I believe the prints to be quite as permanent as prints by the platinotype process. At times I have used the gold and platinum baths together, but I prefer the platinum alone. After the prints are toned they can be washed for a few minutes in running water and dried, or, if time will allow, they can be placed in the following bath for a few minutes before the final washing :—

Saturated solution of alum	10 ounces.
Water	10 "
Alcohol (methylated)	1 ounce.

I tried using the alum bath previous to toning, but fancied it made the prints difficult to tone.

The prints produced by the above process are of a cool grey, and with a very pleasing matt enamel surface. After they are mounted and dry they can be polished with an old silk or cambric handkerchief. This gives more depth. They can be spotted out with either a fine black lead pencil or indian ink.

I have made some experiments with a view to get a warm tone similar to ordinary silver prints, and have succeeded, but it is at times difficult to keep the whites of the picture pure. The means consisted in using pyrogallic and ammonia as a developer, but sometimes the paper became stained in patches, and now and then I got pictures that were very good ; on fixing the hypo. bath the result was a chestnut-brown image, which could be toned to any colour by either of the above toning baths according to taste. I have not yet had time to try the addition of citric acid to the pyro. as recommended by Mr. A. Cowan, but I hope to do so shortly. I trust some will experiment in this direction and publish their experience for the benefit of others.

I have also tried to get the prints with an enamelled surface, and succeeded ; but to my mind the matt enamel looked the more artistic. The paper I used for the purpose was Dr. Monckhoven's enamelled double transfer paper, which I obtained in the market. It is rather horny to manipulate, and it does not seem to develop so readily as the plain gelatine paper. The manipulations are the same, and the resulting prints have far more gloss than albuminized paper. As the process is quite new in my hands, comparatively, I hope to continue my experiments and greatly improve it altogether.

I must not omit to mention that with pyrogallic development the exposures require to be longer. Another little bit of experience I found, namely, if a negative be too thin to be printed by contact the print can be made in the camera; and if a negative be over-exposed and a little foggy, a satisfactory print can be obtained in the same manner. Almost any kind of negative can be used, which is a great advantage.

I think this process might be used with advantage in this way :—Supposing a negative has been taken of a sitter : after the negative is varnished it is possible, with proper appliances at hand, to show a mounted proof within half-an-hour or one dozen *cartes* in one hour, and I think the process might be the means of securing customers who otherwise might be lost where time is an object. It might be possible for the paper to become an article of commerce, providing its keeping quality is established. I should think myself the transferred film from the glass would be more likely to keep than when the paper is coated direct. If it could not be made as an article of commerce, there is no more difficulty in coating the paper (about an hour or two before it is required) than there is to sensitise a sheet of albuminized paper. All that is required is to have a stock of gelatine paper and a bottle of suitable collodion emulsion; nothing is more simple. I am aware that gelatinized paper could at one time be obtained in the market, but of late I have not been able to obtain it. The dealers say they are seldom asked for it; but I have no doubt if a demand were created for it the supply would be forthcoming.

Magic Lantern and Stereoscopic Slides.—To use this process for making slides, either for the lantern or stereoscope, I find it best to have the paper heavily coated with gelatine, using about two ounces of gelatine to the pint of water. For this purpose it is best to use thin paper. It is exposed and developed according to the instructions given above, only the development must be carried a little further for lantern slides, and still further for stereoscopic slides. After the print has been washed and dried (I must not omit to mention the alum bath must on no account be used, or the gelatine becomes insoluble) the print is pinned by the four corners to a flat board, face upwards, and coated evenly with gold size thinned down a little with turpentine. It is best put on with a soft, large camel's-hair brush, and allowed to remain for a day or two in a dry, warm atmosphere, free from dust. If a lantern slide, after it is dry it is trimmed to its proper size, viz., $3\frac{1}{4} \times 3\frac{1}{4}$. A piece of glass the same size is taken, perfectly clean, and immersed both together in a dish of clean water, and after the paper has become perfectly saturated it is brought into contact with the glass under water, film side to the glass, and both brought out together and laid flat on a table, a soft squeegee being passed over the back. Allow it to remain for about a quarter of an hour, and then pour some hot water on the back of the paper, or immerse it in a dish of hot water, at about the temperature of 120°. The corner can then be raised with the point of a penknife and stripped off easily from the film. The film then requires to be washed carefully with a large camel's-hair brush, after which it is allowed to dry. When dry it will have a crystalline appearance, which disappears on varnishing with a clear varnish.

Stereoscopic slides can also be treated in the same manner; transparencies of still larger size can be used as window decorations. I have forgotten to mention that for transparencies the collodio-emulsion must not be diluted, but used the same as for negatives. Neither must the image be developed with pyrogallic acid, as the film refuses to leave the paper. The ferrous oxalate developer only must be used.

Another way which I like much better for transferring is a method I described some two or three years since, and that is, instead of coating the print with gold size, to flow over the glass plate intended to be used a solution of gelatine to which a trace of chrome alum has been added; the print in the meantime is to be wetted with warm water (not more than 80°). The print is then rubbed down in contact with the glass, and allowed to dry. It is then taken and wetted with water at the temperature of about 120°, and the paper is easily removed, leaving the film on the glass.

Many applications may suggest themselves to other workers. I have now given the whole system of my method of working. I do not give it as the best system, for I have no doubt many improvements can be made; but I hope that what I have written may be of service to the readers of *The British Journal of Photography.*

HYPOSULPHITE IN THE FERROUS OXALATE DEVELOPER.—In a paper read before the Society by Captain Abney, the advantage of the addition of sodium hyposulphite was shown. Dr. Eder has given a definite formula for this addition, which is as follows:—

Sodium hyposulphite	10 grains.
Water	4 ozs.

To every 2 oz. of developer used, 10 to 20 drops of the above solution are added. This addition causes rapid development, harmonious negatives, and a shortened exposure of the plate. In all probability one action of hyposulphite is to convert the rric salt into ferrous salt—an important point, as will be seen in another note.

REMOVAL OF STAINS FROM GELATINE PLATES.—We have at various times called attention to the removal of yellow stains from gelatine plates. Mr. J. Cowell's is one of the best, and has the form of citric acid 1 oz., alum 2 oz., water 10 oz. Mr. Hanson recommends chloratum, as purchased at the druggists, diluted with its bulk of water, for the same purpose. Chloratum reduces the intensity of the negative slightly, we are told.

A CAUSE OF LACK OF DENSITY IN THE IMAGES DEVELOPED ON GELATINE PLATES.—An unsuspected cause of want of density was lately given in the *Photographic News* by Captain Abney, which was the fact that ferric oxalate not only would destroy the photographic image, but also convert metallic silver into an oxalate—the latter fact being proved by treating a silvered plate with the solution, and then applying a developing solution to the parts altered by the ferric salt. A further cause of want of density can also be found. When using the ordinary alkaline developer, pyrogallic acid, when fully oxidized, appears to possess the power of destroying the photographic image in a marked degree. Ferrous oxalate should therefore be used when freshly made, or it should be hermetically sealed in bottles, and with the alkaline developer the pyrogallic acid should be mixed only when required for immediate use.

COMMUNICATION.

GELATINE EMULSION-MAKING IN HOT WEATHER.

I HAVE thought it might not be amiss to describe the plan I have been following in making emulsion during the last hot weather we had. In my plate-making room the temperature was often at 85°; but, nevertheless, several batches of plates were successfully prepared. The method adopted is very similar to that I described in my little book, "The Practical Working of the Gelatine Process;" but there are one or two alterations in procedure. In the first place, I use the formula which was described in last *Journal*, using potassium bromide instead of ammonium. It is as follows:—

Potassium bromide	250 grains
Nelson's No. 1 gelatine	45 "
Water	1 oz.
Strong hydrochloric acid	2 drops.

This is prepared in the usual way, and warmed up to 120° F., and the following added in the usual way:—

Silver nitrate	300 grains
Water	3½ oz.

Finally, this next solution is added—

Potassium iodide	12 grains
Water	1 drachm

This I mix in a long hock bottle, shaking between each addition. This is transferred to a glass flask and boiled for half-an-hour, shaking up the emulsion at intervals. This is allowed to cool in the flask for half-an-hour, when to it is added, with shaking—

Nelson's No. 1 gelatine	120 grains
Coignet's gelatine (new brand)	120 "
Water	3 oz.

After soaking, and very slightly heating to melt it, about three drops of strong ammonia in half a drachm of water is then stirred in, and the emulsion is poured out into a jam-pot, which is immediately placed in iced water, a few lumps of ice floating in it. In half-an-hour the gelatine will be firmly set. The jam-pot is then dipped for a few seconds into boiling water, which will loosen the gelatine from its sides, and the lump of emulsion is transferred into moist canvas, and squeezed through into a jar of iced water (the water having been run through filter-paper to get rid of all floating matter), in which a few small lumps of washed ice are floating. After ten minutes the water is changed, and after another ten minutes is changed again, when it is again collected in the canvas and squeezed through into water. One more change of water should be sufficient to free it from all except traces of soluble salts. It is then transferred to the canvas and allowed to drain over a jar hslf-an-hour to three-quarters.

It is again transferred to the jam-pot and melted, and a slight trace of carbolic acid (or other antiseptic) added, and then once more placed in iced water. In half-an-hour it is set, when it is covered with alcohol and allowed to ripen for a day; and if the jar is placed in water containing a lump of ice, so much the better. When plates have to be coated, the slab on which the plates have to be set is covered with small lumps of ice for half-an-hour, and if it be thick it is only very gradually cooled; but, on the other hand, it also only very gradually gets warmed again. During this time the emulsion is melted, six drachms of alcohol added, and filtered. When the plates are coated (after the slab has been dried from all water), it will be found that the film of emulsion will set in a couple of minutes, and that the slab remains cool enough to enable five or six batches of plates, filling the slab, to be prepared; that is, supposing your slab to hold eight plates, you can coat forty to forty-eight without re-cooling the slab. I find that the gas of the drying-box may be lighted immediately, and the drying of the plates will proceed rapidly, and not re-melt. If gelatine be once *well set*, it requires a high temperature to re-melt it; and the more the water is evaporated, the higher the temperature required. As the current of warmed air passes over the plates the moisture is rapidly absorbed, and hence the drying can be effected with safety.

My excuse for writing this must be the fact that I have had many letters, asking how I prepare emulsion in this weather, and I trust that this description may be taken as an answer to them. Doubtless there are other modes which will succeed as well; but, I think, for an amateur, this plan will be found to be everything that is required. I would just say, that supposing by any chance the emulsion appears too thin before coating the plates, 40 grains of Coignet's gelatine may be melted in three drachms of water and added to the emulsion, with much stirring, immediately before filtering.

W. DE W. ABNEY.

SPECIAL NOTICE.

The Exhibition of 1881 will open on *Monday, October 10th;* the Soirée being held on *Saturday, October 8th.*

The last day for receiving Pictures will be *Friday, September 30th,* for Pictures left by hand at "The Gallery," 5a, Pall Mall East, as also for Cases and Boxes by the Agent, J. Bourlet, 17, Nassau Street, Middlesex Hospital.

A Circular will be sent to each Member; but any further particulars can be obtained from the Assistant Secretary.

NOTICES.

The Committee on Sensitometers will be glad to consider the suitability of any Sensitometer which may be submitted to them for trial. Application should be made to the Assistant Secretary, M. E. Cocking, 57, Queen's Road, Peckham, S.E.

The next two "TECHNICAL MEETINGS" will be held at "The Gallery," 5a, Pall Mall East, on the Evenings of *Tuesday, June 28th,* and *Tuesday, July 26th,* when the Chair will be taken at 7.30.

Members unable to attend, or residing at a distance, are invited to send questions for the "Question Box," to be discussed at these Meetings.

Subscriptions for the current year should be paid to Mr. Arthur R. Cowdroy, Society of Arts, John Street, Adelphi, W.C.

Persons wishing to become Members of the Photographic Society of Great Britain, or to make a communication to it, are requested to apply to either of the Secretaries, or to the Treasurer.

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ON THE CONSTRUCTION OF ISOCHRONIC PASSAGE-CHARTS.

By Francis Galton, F.R.S.

From 'Proceedings of the Royal Geographical Society and Monthly Record of Geography,' November No., 1881.

ON THE CONSTRUCTION OF ISOCHRONIC PASSAGE-CHARTS.

By Francis Galton, F.R.S.*

Map, p. 704.

By "isochronic" passage-charts, I mean charts constructed to show the extreme distances that can be traversed in "equal times" from a common starting-point. Their principle is an extension of that by which the rise and fall of temperature as we proceed in different directions is shown by means of isothermic lines, or again as that of barometric pressure is shown by the isobaric lines which may be seen in the small weather chart published daily in the *Times*.

In the accompanying map the starting-point is London, and the travellers are supposed to use postal or other rapid or regular conveyances so far as they help, and thence to employ such private means of conveyance as the country may afford. I assume the seasons to be favourable, that immunity has been obtained from political obstructions, and that friends on the spot have made preparations to avoid delay in engaging travelling servants, beasts of burden, or boats. I have kept the idea in view of a special correspondent calculating how long it would take for his letters to reach his employers, under the best arrangements he could make without an extravagant cost. The sea isochrones are for the most part drawn through the points reached by the various passenger steamers in the corresponding number of days. In the Arctic regions the estimates are necessarily rude; they have been based on recent voyages. All places within ten days' journey of London are coloured green, those between ten and twenty are orange, between twenty and thirty they are red, between thirty and forty they are blue, and those beyond forty are brown.

The data upon which the map is based are (1) the time-tables of the principal ocean steam-packet companies. (2) A list which has lately appeared in the Postal Guide of the average time taken by the post to reach various places. (3) Private information furnished to me by friends, including some officials at the General Post Office. (4) Records of voyages. By these means I obtained data for a considerable number of important ports and other places distributed over the globe, and I

* Read at the Geographical Section of the British Association, York Meeting, September 1st, 1881.

procured as I best could the particulars concerning the chief local lines by sea and land proceeding from those places. In this way the skeleton of the chart was formed, which I filled up by means of measurements based on the average length of a day's journey in the country under consideration. I think there is no estimate in the chart that does not admit of defence, but I freely acknowledge that judgments may greatly differ in many cases as to whether a different estimate might not have been preferable. A common subject of doubt is whether to adopt the time occupied by the regular and roundabout communication, or that by an occasional direct one. Instances of this are found all along the West Coast of Africa, where the ports are regularly served by steamers that touch at every one of them in succession, and which consequently occupy more than forty days to reach even the mouth of the Congo, whereas steamers occasionally sail direct to one or other of those ports in considerably shorter time than these mail steamers. This particular difficulty is met and explained by the sea isochrones, which in this case do not conform to those of the land. It will be seen that the sea adjacent to the greater part of West Africa lies within the yellow band, and therefore that a ship going direct to any point within that band and steaming at the same rate as the mail steamer to the Cape, Pará, or St. Thomas, would arrive at its destination in from ten to twenty days.

In a map on a larger scale than this, many more details could and ought to be given, including at least the great through routes by sea and land, but the present map is too small for the purpose. It is offered merely as generalisation with the primary object of illustrating a new principle, which perhaps may hereafter be developed in more elaborate publications. The principle could be adapted in many ways for the convenience of tourists; thus isochronic maps might be easily constructed for Continental travel or for home excursions.

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Explanation of colours. Green within 10 days. Yellow 10-20 days. Pink 20-30 days. Blue 30-40 days. Brown more than 40 days journey.

Published for the Proceedings of the Royal Geographical Society, 1881.

43/

Physiognomy

& composite Portraiture

Feb/82

Reprinted from Vol. XXV of 'Guy's Hospital Reports.']

AN INQUIRY

INTO

THE PHYSIOGNOMY OF PHTHISIS

BY THE METHOD OF

"COMPOSITE PORTRAITURE."

By FRANCIS GALTON,[1] F.R.S.,

AND

F. A. MAHOMED, M.D.

The doctrine of diatheses, or what is often called "temperaments," in other words, the belief that certain physical conformations indicate predispositions to certain diseases, has always held so prominent a place in medicine from the earliest ages that it is unnecessary to dwell upon its history or its present position at any length. Of late years this doctrine has been repudiated by many of our most able teachers, though on the other hand it still receives the powerful support of some of the most distinguished and experienced of our physicians. So that what heretofore has been generally accepted has now become a much disputed question. The objections that have been raised against the doctrine by those of what may be called the new school are chiefly these: that it is founded on the utterly false and erroneous doctrine of "humours" held by physicians in the dark ages; that it is therefore only a relic of false traditions; and lastly, that it is not supported by any modern scientific observations, and that the statements of

[1] Though it would be difficult wholly to disentangle our respective shares in the inquiry, I must at least give the entire credit of the following memoir to Dr. Mahomed.—F. G.

"general impressions" made in support of it are those of impressions prejudiced by traditional beliefs.

In reply to these objections it may be said that the facts which the earlier physicians observed were probably correct enough, and that it was only their explanations and theories that were wrong; thus they may have observed certain facts in connection with the physical characteristics of individuals in association with certain diseases, and then sought to explain them by their false theories; the facts may nevertheless remain true.

The objection that this doctrine is only supported by personal impressions still holds good, and it is with a view to put it to the test of exact, and as far as possible unprejudiced investigation, that the following observations have been made.

Probably no diathetic types are more commonly recognised, either rightly or wrongly, than the so-called tubercular and strumous; both of these, but more especially the former are held, by those who believe in them, to play a prominent part in phthisis, inasmuch, that persons presenting the physical characteristics attributed to these diatheses are said to be especially predisposed towards this disease. It has appeared to us that this belief might be put to the test by means of the method of 'composite portraiture;' in short, that we might be able to ascertain whether there are any facial characteristics common to any large proportion of cases of phthisis.

In the first place, it was necessary that we should accumulate a large number of photographs of patients suffering from this disease, and with this view we obtained permission from the physicians of Guy's Hospital to photograph any patients coming under their care; the authorities of the hospital were also good enough to place the photographic studio at our disposal.

We began our work in January, 1881; by March we found that the progress was too slow, and that we must extend our field of operations in order to get a larger supply of patients. We therefore sought and obtained the permission of the physicians and the governing bodies of the Brompton and Victoria Park Hospitals for Diseases of the Chest, to photograph a large number of their phthisical patients. We would take this opportunity of expressing our gratitude to the authorities of these hospitals for the great facilities they so freely afforded us for carrying out our observations, and also our most sincere thanks to the physicians and resident medical officers (Dr. Hicks, of Brompton, and Dr. Humphry, of Victoria Park) for their very kind co-operation and assistance in our work. When all did so much, and so willingly, it would be invidious to mention those who were able to contribute most; but when we say that from the out-patient rooms and the wards of these hospitals we were supplied with about 400 cases, and that, in nearly all, the cards to be afterwards described were filled up by the physicians in the case of the outpatients, and by the resident medical officers for the inpatients, some idea may be formed of the labour entailed upon these gentlemen.

Our endeavour has been throughout to protect ourselves from any charge of a prejudiced selection of cases or distortion of facts; we therefore supplied those kind enough to help us with cards on which the chief details of the cases could be briefly recorded, by making a "tick" in the appropriate space; a copy of these cards is here inserted, the method having proved at once simple and convenient.

Hospital.		
PLEASE PHOTOGRAPH BEARER.		Initial of Physician.
Name.	Age.	Date. 1881

EXTENT OF DISEASE	ONSET OF DISEASE
Advanced	Insidious
Moderate	OR PRECEDED BY Severe hæmoptysis
Slight	Bronchitis
DURATION OF DISEASE	Pneumonia
Chronic (over 3 yrs.)	Pleurisy
Medium (1—3 yrs.)	Syphilis
Brief (under 1 yr.)	Gout
HEREDITARY TAINT	Alcoholism
Strong	
Some	
None	

Remarks

We asked the physicians to send us all cases of well ascertained phthisis occurring in either sex within the limits of fifteen and forty years of age. These limits of age were fixed, partly because the faces between these ages are more fairly comparable, and partly to exclude the more evidently acquired phthisis of advanced age.

Mr. Galton then engaged the services of Mr. Mackie as photographer, who has had large experience in rapid photography, having been employed professionally for some years in photographing, for the use of the authorities, the prisoners at the Pentonville Convict Prison. He was often able to secure for us twenty and thirty portraits from the out-patients in the course of an hour or two, and we were enabled to obtain the number we required during the months of April and May.

We now found at our disposal 442 portraits of patients suffering from phthisis, of whom 261 were males and 181 females. They were obtained as follows:

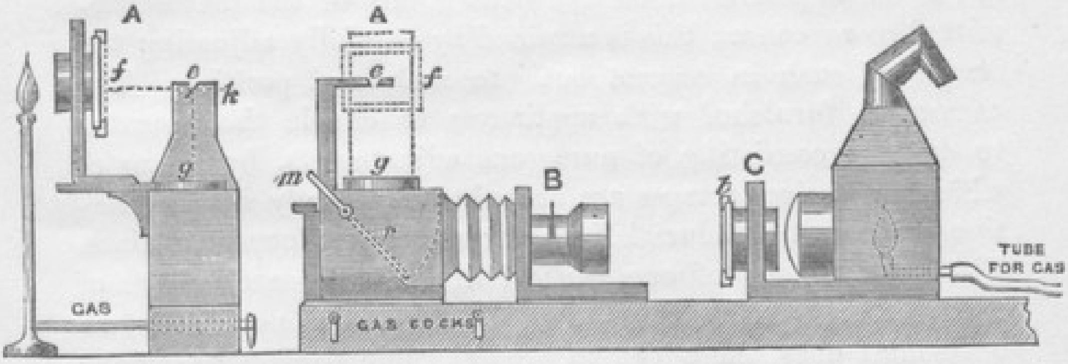
From Brompton—	Males .	140	
	Females .		116
„ Victoria Park—	Males .	36	
	Females .		42
„ Guy's	Males .	85	
	Females .		23
		261	181
	Total .	442	

For comparison with these we next proceeded to photograph 100 male and 100 female patients, taken without selection from the wards and out-patient rooms at Guy's, none of whom were suffering from phthisis; these we propose to use as a standard to represent the average of the population of the same class, but suffering from diseases other than phthisis.

During the process of making composites, we received some valuable assistance from Mr. George Turner, who came to our help when we were much pressed for time and made several composites for us, besides assisting in the selection of faces out of which they should be formed.

Before discussing the results obtained, it will be well to say a few words in explanation of the method followed. Composite portraits may be described as *pictorial averages*, they are independent of the fancy of the operator, just as numerical averages are, though like them, they may be vitiated by errors of calculation or of manipulation. Composite portraits, the method of producing them, and their adaptation to various purposes, have already been described by one of us on several occasions;[1] but it may be desirable for the benefit of our present readers to give an account of the process. They are

APPARATUS FOR MAKING COMPOSITE PORTRAITS.[2]



A The body of the camera, which is fixed.
B Lens on a carriage, which can be moved to and fro.
C Frame for the transparency, on a carriage that also supports the lantern; the whole can be moved to and fro.
r The reflector inside the camera.
m The arm outside the camera attached to the axis of the reflector; by moving it, the reflector can be moved up or down.
g A ground-glass screen on the roof, which receives the image when the reflector is turned down, as in the diagram.
e The eye-hole through which the image is viewed on *g*; a thin piece of glass immediately below *e*, reflects the illuminated fiducial lines in the transparency at *f*, and gives them the appearance of lying upon *g*,—the distances *f k* and *g k* being made equal, the angle *f k g* being made a right angle, and the plane of the thin piece of glass being made to bisect *f k g*.
f Framework, adjustable, holding the transparency with the fiducial lines on it.
t Framework, adjustable, holding the transparency of the portrait.

[1] "Composite Portraits," by Francis Galton, F.R.S., 'Journ. of Anthropological Inst.,' 1878 (reprinted in 'Nature' and in the 'Photographic News,' and translated in the 'Révue Scientifique.') "Composite Portraiture," by the same author, 'Photographic Journal,' June, 1881 (reprinted in the 'Photographic News,' July 8th and 15th). The process there described was the one adopted in the present inquiry. See also "Generic Images," by the same author, in the 'Proceedings of the Royal Institution,' to which some autotype reproductions of composites are appended.

[2] This woodcut is borrowed from the 'Photographic Journal,' June, 1881.

made from the individual negatives by throwing upon a photographic plate, disposed in a special form of camera, the images of several negatives in succession, an equal fraction of time being given to each, instead of the whole time to one. Thus, if a plate requires 200 seconds of exposure, and it is wished to make a composite of ten individuals, the image of each negative will be thrown upon the sensitised plate for twenty seconds. To obtain a clear result it is, of course, necessary that the chief features of each negative should fall as far as possible upon the same points of the plates submitted to exposure; this is attained by carefully adjusting the image of each in respect both to scale and position. The camera is furnished with appliances to enable the operator to do this, consisting of numerous adjustments, by means of which the various images are brought into exactly similar relations with certain fiducial lines thrown on the focussing glass. The latter consist of one vertical line for the median line of the full face, passing down the middle of the nose, and two horizontal ones, the upper to pass through the pupils of the eyes, the lower across the mouth. The image of each negative is enlarged or diminished as may be necessary, to secure that the distance from eyes to mouth may be the same in all cases. It is then rotated and shifted up or down and sideways, until the upper of the two horizontal lines intersects the pupils and the vertical one divides the face equally. The outlines of the face are entirely disregarded and left to take care of themselves. As it is necessary that the plate to be exposed, having been once put into position, should not be shifted until the close of the operation, and as each negative has to be focussed in succession, the composing camera is made with a horizontal focussing glass on its roof, as well as, or instead of, at the back. By means of a swinging reflector, let down at an angle of 45° with the top of the camera, the image produced by the lens is thrown upwards on to the horizontal focussing glass in the roof, where, by means of a camera lucida, the fiducial lines and certain tinted dots, illuminated by a standard illuminator (marked 'gas' in the diagram, but more properly a candle), are seen as if marked on the horizontal focussing plate, and to these lines the image is adjusted, as already described.

By the use of the standard tinted dots the gas illumination of the negative is controlled, so that the image of each portrait has equal intensity, and therefore contributes equally to the result; the errors that would be produced by the varying densities of the negatives are thus in great measure obviated. When the image has been exactly adjusted, and the proper illumination has been obtained, the reflector is raised and the image allowed to fall upon the sensitised plate; this process being repeated with each of the component negatives.

A single plate that has been exposed to several negatives yields what is called a *composite*. Several of these composites may in their turn be exposed to another plate under similar conditions, as if they were ordinary negatives; the result is called a *co-composite*. Several of these co-composites may be combined to produce a co-co-composite, and so on.[1]

With this brief account of the process, which is fully described in the papers already referred to, we may pass to the consideration of the photographs obtained. On looking over the individual portraits of the patients suffering from phthisis, one is first struck with the absence of those characteristic faces which we expected to find among them. With the exception of a few who were very severely ill, the faces did not seem to differ much from those of any group of ordinary patients, indeed, there seemed nothing characteristic about them. They were shown to many physicians, many of whom expressed their surprise at the absence of characteristic faces. We were inclined to accept this at first as a distinct answer in the negative to the question, Is there a tubercular diathesis? But after much sorting and arranging into groups, and after combining the individuals, so as to test the similarity of their features, certain results began to unfold themselves. Clinical facts were first taken as guides for grouping; thus the cases of "advanced disease" were grouped, but gave no result beyond well-marked emaciation (Pl. I, fig. 10, and Pl. II, fig. 22). The rapid cases of "brief duration and advanced disease" yielded no characteristic type, nor was anything very definite obtained at first from those in whom the "hereditary taint" was "strong." The one of us least likely to be prejudiced by preconceived notions, dealt with these latter cases single-handed and without consultation with the other.

[1] Composites and co-co-composites are positives and require to be reversed before printing from them.

Concerning them Mr. Galton writes as follows:—"Fifty-six cases (among the women) were recorded by the medical officers as having a strong hereditary taint of phthisis, and it is of these alone I now speak. On first examination of the collection of portraits, I was chiefly struck by their diversity, but after familiarising myself with them and sorting them tentatively in various ways, I began to perceive what seemed to be natural groups, leaving comparatively few that I could not classify. I made composites of each of these groups; there were eleven of them, containing on an average five components each, one only had as few as three, and one only as many as nine. I then sorted the composites and found that they fell into two main divisions, not, however, separated by any abrupt line of demarcation. In the one division there were six composites of, on the whole, thirty-six portraits, and in the other there were five composites of twenty portraits in all. The first division had blunted and thickened features, the second had thin and softened features. I then made a compound composite of each of the two divisions (Pl. I, figs. 4 and 6), and finally I threw both divisions into a doubly compound composite (co-co-composite, Pl. I, fig. 5) to form the general average. I need not stop here to speak of the precautions taken in doing this, further than to mention that the groups were always "weighted" in exact proportion to the number of their constituents, as by giving thirty-six seconds exposure to the co-composite of the first division against twenty seconds to that of the second division, when forming the general average.

"The trustworthiness of the final result must be estimated on the same principle as if we had been dealing with numerical averages. That is to say, we may rest content whenever the averages derived from two large subdivisions of any group resemble the general average as nearly as is needful in the case under consideration. I think this result has been fully reached in the present case, for notwithstanding that the divisions have been made so as to contrast as strongly as possible, their composites (Pl. I, figs. 4 and 6) resemble very nearly that of the general average (Pl. I, fig. 5). It is therefore obvious that if the eleven primary composites were divided into any other pair of groups, the co-composites of each of these two groups would have a yet more close resemblance to each other, and to the general average also. I have indeed made some trials which amply confirm this view. Therefore, as far as concerns the female patients between the ages of eighteen and forty in London Hospitals who have phthisis, with a strong hereditary tendency to the disease, I have no doubt that any future inquirer who deals as I have done with not less than fifty cases, will arrive at an ideal face almost identical with that which I have produced." The truth of this last remark as regards all cases of phthisis, has been strikingly corroborated by our further investigations, as will be seen by comparing with this result the three other co-composites of phthisis, containing about fifty cases in each (Pl. I, figs. 7, 8, and 9). The two co-composites (figs. 7 and 8), taken absolutely without selection, are almost identically the same face, while fig. 9 is composed of two opposite types of faces—the narrow ovoids and broad faces with coarse features; and this has produced a rather stronger face than either of the others, a nearer approach to the non-phthisical patients.

These observations were made while one of us was away from London; on his return it at once became evident that what have been described above as the two types, the one with blunted and thickened features, the other with thin and softened features, closely coincided with the two types constantly described by physicians as the "strumous" and "tubercular." Proceeding now to carefully sort our patients under these two heads, and to put our selections to the test of combination in composites, we soon obtained very striking and highly characteristic faces.

Reviewing the whole of our results, two important conclusions may be adduced, and these may be given as a preface to the consideration of the plates in detail. It appears that the method of composite portraiture may be employed to obtain two different and equally advantageous results. First, by throwing into one a large number, say fifty different faces, taken without any selection whatever, we can obtain an average of them all; but this presents no features or expressions characteristic of what may be called secondary types; such a result is an excellent method of obtaining the broad average as to the general proportions of the face, the average shape of the lower jaw, the average delicacy or coarseness of the features, and the average amount of emaciation, &c. Secondly, it is possible by taking very carefully selected faces to form a composite face having certain characteristic features; in making such a face the introduction of a few which are not strictly admissible into the group readily effaces the characters sought for in the composite, and as but few faces can be found which closely correspond, the larger the number employed the more does the result approximate to the general average face. This method of combining specially selected faces, is in itself an excellent test of the correctness of the selections made. If the result obtained has lost the special characters sought for, we may be sure that the faces selected were ill assorted; always, however, bearing in mind that the larger the number of faces introduced, the greater the probability of reverting to the general average. This method fails to obtain for us so typical a face as may often be seen in a single individual, yet it tests the accuracy of our opinions as to the general similarity of several selected faces.

Plate I is composed almost entirely of general average faces, both male and female, and the uniformity of the results is very noticeable. Figs. 1 and 3 each contain fifty patients, all suffering from diseases other than phthisis, taken without selection, and chiefly from among the out-patients attending Guy's Hospital. The results, over which the operator can have no voluntary control, are remarkably alike; yet they consist each of fifty entirely different people, no single person occurring in both. With these compare figs. 5, 7, 8, and 9 in the same plate; each of these contains about fifty cases of phthisis, and again the results are wonderfully alike, although another face is arrived at. A certain selection has been made in figs. 5 and 9; the former contains, as above mentioned, fifty-six cases, in all of whom a strongly-marked hereditary taint existed, and the resulting face has distinctly more delicate features, and is a narrower ovoid than figs. 7 and 8. Fig. 9, on the other hand, is composed of two opposite extremes; it was formed by combining a composite of selected narrow ovoids, and one of selected broad faces with coarse features, and contains most of the components of figs. 29, 30, 31, and 32, Pl. II. The result is a face standing midway between the very delicate fig. 5 and the broader faces and coarser features seen in figs. 1 and 3.

From a consideration of these, we are undoubtedly justified in saying that the average of phthisical faces gives more delicate features, an apparently lighter lower jaw, and an altogether narrower face than the average of other diseases. Probably in some measure this result is due to the greater average emaciation of these patients than that of those suffering from other diseases. But emaciation will not always alter the general outline of the face; this is well shown by Pl. I, fig. 10, a composite of eleven cases of phthisis in whom the disease was far advanced; in this face the results of emaciation are well shown in the deeply sunken eye, the hollow cheeks, and thinly-covered lower jaw, but the face nevertheless is not by any means a "narrow ovoid." A critical examination of fig. 1 will show, however, that emaciation in such a face would take away much of the "heavy-jowled" appearance of the lower part of the face, and would thin the nose and lips greatly, bringing it nearer to the phthisical type.

Pl. I, fig. 2, was obtained by selecting all the narrow ovoid faces among the hundred female patients not suffering from phthisis; it contains fifteen individuals. It may be compared with figs. 29 and 30, Pl. II, which contain nine and twelve individuals respectively, or twenty-one in all; these are the selected narrow ovoids occurring among our total number of 181 phthisical women. Pl. I, fig. 2, will be found to be very closely similar to Pl. II, fig. 29. We have, then, the unexpected result of 15 per cent. of the non-phthisical women giving this narrow ovoid face, and only 11·6 per cent. of patients with phthisis presenting it. It must be remembered, however, that many of these fifteen may hereafter develop phthisis, for several of them were young women suffering from those ill-defined functional disorders which often precede it. We may also find another explanation of this result in the fact that we are here dealing with phthisis among the lower classes, and that with them phthisis is probably much more often an *acquired* disease than what is called a *constitutional* one. Yet, allowing due weight to these considerations, the fact still remains well established that no larger proportion of peculiarly narrow ovoid or delicately-formed persons could be selected among those suffering from phthisis than among the ordinary female population; on the other hand, the general average of each class proves the phthisical women to have the more delicately-featured and narrow faces.

We may now examine the men, and we shall find very similar results. Pl. I, fig. 11, contains 100 patients suffering from diseases other than phthisis; it is a co-co-composite of figs. 13, 14, 15, and 16, which are co-composites containing twenty-five in each, being composed each of five composites having five individuals in each composite. This subdivision was only employed to check an error more readily and make repetition of a single group easier and shorter if necessary. On the other hand, fig. 17 is a co-co-composite of 206 cases of phthisis, it contains figs. 18, 19, 20, and 21, each of which contains fifty (except fig. 18, which contains fifty-six); these four are co-composites, each containing five composites of ten individuals suffering from phthisis, and taken without selection. Fig. 18 was made entirely from patients under treatment at Guy's, and the average severity of these cases is usually greater than those treated at the hospitals specially devoted to chest disease. A comparison of these groups gives much the same result as in the case of the females. The phthisical composites are evidently much narrower and more delicate faces than those suffering from other diseases. The same exact similarity is not at once apparent in the series of male phthisical faces as in the female, chiefly on account of the variable growth of hair, but still a strong resemblance is to be traced between them. It may be remarked that fig. 13, containing non-phthisical persons, presents a more delicate face than figs. 14, 15, and 16, and closely approaches to the phthisical type; an explanation of this is to be found in the fact that it chances to contain six out of the thirteen narrow ovoid faces contained in fig. 12, or, in other words, three times as many narrow ovoid faces as in either of the remaining three, supposing the rest are equally distributed.

Pl. I, fig. 12, contains thirteen narrow ovoid faces selected from the 100 cases 'other than phthisis,' and it may be compared with Pl. III, fig. 33, the co-composite of fifty-one narrow ovoid faces selected from the 262 males suffering from phthisis, and also with the two female narrow ovoid faces (Pl. I, fig. 3, and Pl. II, figs. 29 and 30). As in the case of the females, the two male narrow ovoids chiefly differ in the degree of emaciation visible, this being well marked in the phthisical cases and absent in the other diseases. The proportion of narrow ovoids in each class among the males is the reverse of that among the females, for we find only 13 per cent. among the cases 'other than phthisis,' and 19·46 per cent. among the phthisical patients if we include all the fifty-one cases contained in the plate; but at least six of these, we shall hereafter find, ought to have been excluded, for they do not belong to the "narrow ovoid" class. This would bring the number down to forty-five, which would give almost exactly 17 per cent. If we add together the percentages of the narrow ovoids in both sexes we find that the cases 'other than phthisis' give 14 per cent., while the phthisical cases have 14·3 per cent., in short, they are to be found in equal numbers both among the phthisical and non-phthisical patients. Let us here emphasise the fact that we are now comparing phthisis with *other diseases*, and not with the healthy population, and these observations would seem to show that a delicate person may fail in many ways besides becoming phthisical, and that a delicate narrow ovoid face, may mean liability to other diseases not necessarily tubercular.

Turning to Pl. II, fig. 22 is a co-composite of forty-two cases of advanced phthisis, all of whom showed in their faces the ravages of the disease; it contains the six composites, figs. 23, 24, 25, 26, 27, and 28, each containing seven individuals. The co-composite closely resembles fig. 33, the co-composite of narrow ovoids, and both of these in their turn closely approximate to the phthisical type seen in fig. 17 and its components; yet the components of fig. 22 are strongly dissimilar.

Fig 23, one of the components of fig. 22, is a most typical, perhaps the most typical phthisical face, yet the individuals contained in it were in no way selected, except for the severity of their disease; they were taken merely in the order in which they chanced to be photographed. It is interesting to compare this face with figs. 29 and 30, and some of the composites in Pl. III; in several of these instances the same face is very nearly arrived at. In this face the large projecting ears are very noteworthy; they are noticeable in several other composites, and in many of the component faces; these, taken together with a narrow mouth, often open, a short and small chin, a small and narrow lower jaw, make together an often-recurring face in phthisis. A very typical face of this nature is seen in Pl. III, fig. 34, No. 101. This face is one of the components of fig. 23, and lends to it much that is characteristic.

We have remarked that the method of composing faces is a good test of their real resemblance, that is, the more closely allied they are to each other the better composite will they produce; this is well exemplified in figs. 27 and 28, but especially in the former, in which the faces, having been taken without selection, have very imperfectly combined. While by adjustment the central features have been exceedingly well blended, yet the main outlines of the various faces remain very distinct, and at least five out of the seven it contains can be traced around the chin and ears, especially on the left side.

Figs. 29 and 30 are two composites of narrow ovoid faces, and have been already referred to. Their components are arranged adjacent to them; they are the nearest approaches we could find to the so-called "tubercular type," which seems singularly rare or much modified among the lower classes of the population. An attempt has been made to arrange them in two groups, containing a higher and a lower type of face, but the results are very similar.

The components of fig. 30 are chiefly characterised by the large ears, the narrow, open mouths, with prominent upper teeth, and short, small chins, which have been mentioned as forming a frequent type in phthisis. No. 608, the last portrait on this plate, was also included in this group. This face presents the narrow ovoid outline, but the coarse features and broken nose of fig. 32.

Figs. 31 and 32 are the direct converse of the narrow ovoid just described. In these we find the broad faces, heavy lower jaws, short upper lips, thick and rather up-turned noses, often with a depressed bridge, which are characteristic of what is called the "strumous diathesis." When we examine a group of the most degraded of this type, as seen in fig. 32, we cannot but recognise that we are dealing with such features as those which characterise syphilis. This view seems well borne out by the five faces at the bottom of this plate, and in the composite produced by them. If we compare fig. 31 and its components with them, we can readily trace a close similarity between these and the more degraded ones in fig. 32. Possibly one or two generations have sufficed to effect the change, so that the deformed and ill-formed faces, the direct products of disease, when sufficiently diluted, may give rise to the comely and attractive face seen in the composite, fig. 31, and in one or two of its components. In this way we may often observe the disappearance of eccentricities and deformities, and that return to the average type, by which alone the maintenance of the race is possible.

In Plates III and IV, the male patients having the same characteristics as the females in Plate II, have been selected and combined. They have required greater subdivision on account of their larger number and the growth of hair upon the faces. Among the narrow ovoids the selection has not been sufficiently critical, and several have been admitted which might with advantage have been excluded; the final co-composite, Pl. III, fig. 33, would then have been more typical. As it is, the outlines are somewhat ill-defined, and the face scarcely as narrow as it should be. In this group, moreover, numbers have increased the difficulty of arriving at a type; it contains fifty-one components and its tendency is to revert to the general average, as may be seen by comparing it with fig. 17, which it much resembles.

We would remark that the nine composites forming the components of fig. 33 are only stages in the production of the co-composite. The faces in each composite have not been selected for their resemblance to each other, but merely as belonging to the "narrow-ovoid" class. They have been chiefly taken in the order in which they were photographed, except that in the first three figures the hairless faces have been put together while in the next two those wearing hair have been combined; all contained in these first five composites were patients at Brompton, those in the next three at Guy's, and those in the last at Victoria Park.

The component composites of fig. 33 are more characteristic than the co-composite itself, though not so typical in many cases as they should be. Fig. 34 somewhat resembles fig. 30, and one of its components, No. 101, bears some resemblance to No. 578 in the female group. These are the two most typical faces in either, and partake more largely of the characteristic features of the groups than any other single faces; they therefore more closely resemble the composites. It may be argued that they have contributed too large a share to each, and possibly this may be true. They both exhibit in a striking degree the characteristics of the lower so-called "tubercular type" already described. Out of the components of fig. 34, No. 160 should have been excluded.

In fig. 35, No. 192 might have been omitted on account of the irregularity of his features, but his ears are highly characteristic of the semi-idiotic or degraded type.

Figs. 36 and 37 are striking faces and good results, but in fig. 38 none of the faces are very characteristic, though all belong to the narrow ovoid class.

Fig. 39 has been ruined by the admission of Nos. 7 and 11; No. 7 can only have been admitted by mistake, and No. 11 owes his admission to his emaciation and not to his original conformation; for the broad strong angles of his lower jaw are characters the reverse of what is noticeable in the other faces on this page. No. 6 also might have been excluded with advantage. Fig. 39 may be compared with fig. 18, and a strong resemblance traced between them and accounted for by the fact that among its fifty-six components fig. 18 includes all the components of fig. 39, as it does also those of fig. 40. From the latter figure Nos. 20 and 36 and perhaps No. 15 might have been omitted. Indeed, had the last two figures been merged into one and only allowed to include Nos. 9, 13, 28, 24, and perhaps 23, a far better result would have been obtained, both in it and in the final result (fig. 33). Time, however, did not allow us to make these alterations, when, on arranging these plates the errors of selection were discovered; in some measure it is perhaps desirable that the corrections should not have been made, for it will allow our readers to see both the strength and weakness of the process, that is, its mechanical accuracy and the check it makes on the selection of faces, and, on the other hand, the failures and misleading results obtainable by bad selection. This question of selection is still further emphasised by fig. 41 (Plate IV), which contains a very ill-assorted collection of faces; long and short faces, and the broad lower jaw of No. 212, being all mingled in terrible confusion, giving the many outlines to the face obtained in fig. 41, in which almost every one of its components can be traced.

Fig. 42, on the other hand, gives a very good result, except as regards its mouth.

Fig. 43 on Pl. IV, and the composites (figs. 44 to 48) out of which it has been formed, contain twenty-seven individuals selected as possessing broad faces with coarse features. In this group a difficulty arises from the fact that there is a mixed class, the representatives of which among the men are chiefly included in these composites; they have a narrow ovoid face, but with coarse and thick features, as in the case of No. 608, fig. 32, among the women, who was also included in the narrow ovoids of a lower type. If this work was being done again it would be well to put these in a class by themselves. Many of these faces are included in fig. 44, and the result has been to obtain a decidedly narrow ovoid face; the same is true of fig. 47, and these two faces having been admitted to the co-composite fig. 43, have done much to destroy its typical characters, though it still remains a well-marked contrast to fig. 33, and it much resembles the corresponding female composite fig. 31.

Fig. 44 contains eight individuals, namely, those on a line with it in the plate. We have already remarked that they present much in common, but that their faces are mostly narrow ovoids, though their features are coarse and the upper lips short. No. 193 and perhaps 336 might well have been introduced into fig. 46.

The components of fig. 45 make a good composite, but we are willing to allow that they do not possess typically "broad faces," nor the features generally called "strumous."

On the other hand, fig. 46 and its components have undoubtedly broad faces and powerful lower jaws, but they have not the broken noses nor short upper lips of the strumous face.

The features of the faces in fig. 47 are similar; all have the mouth open, a short upper lip, and a broad nose with more or less depressed bridge, yet the outlines of the faces are narrow rather than broad.

Fig. 48 and its components exhibit the broad face and characteristic features of struma when not sufficiently strong to be ill-favoured or deformed; they belong to much the same class of faces as fig. 31 among the women.

In closing this review and criticism of our own plates, we must express our great regret that some unforeseen pressure of time at the last did not allow us to revise our male composites before publishing them; if we had been able to do so, we should have obtained better results. We dealt with the women first, and by submitting them to revision, we have procured more characteristic faces than at first. We would also draw attention to the fact that this is the first attempt at applying the new process of composite portraiture on a large scale, and that many technical difficulties, mechanical and others, could only gradually be overcome.

There is one advantage, however, in submitting the photographs of men in a somewhat imperfect condition; it affords an opportunity of demonstrating errors in selection, and gives examples of one of the advantages of composites as tests of accuracy of selection and grouping.

Finally, we may say that our results appear to lend no countenance to the belief that any special type of face predominates among phthisical patients, nor to the generally entertained opinion that the narrow, ovoid, or "tubercular" face is more common in phthisis than *among other diseases*. Whether it is more common than among the rest of the *healthy* population we cannot at present say.

It is true that taking both sexes together we find 14·3 per cent. of faces that may be classed as "narrow ovoids" and 9·3 per cent. that come under the head of "broad faces with coarse features," making in all 23·6 per cent. of our cases which may be grouped under one or other extreme departure in either direction from the normal average; but we doubt if this is more than would be found among the general population. Our results are therefore negative, but it may be they are no less valuable; although we commenced our investigations with the expectation of establishing a "type" on a firm foundation, we shall be little less satisfied with them if they have succeeded in refuting an error.

Although these conclusions would seem to indicate that there is no foundation for the belief that persons possessing certain physical characteristics are especially liable to tubercular disease, yet it may hereafter be proved that some explanation of the doctrine may be found in the course of the disease when it attacks such persons. In suggesting this we are going beyond the facts recorded in our present inquiry, but the suggestion appears warranted by daily observation. Thus, the delicately organised individuals called "tubercular," and characterised by their "narrow ovoid" faces, have been compared with horses and cattle who have been what is called "over bred;" such animals are described as having too much nerve and too little bone and muscle; they have no "staying power" and readily "knock-up." In like manner these more delicately formed individuals, with highly susceptible nervous systems, well exemplified in the "precocious child," are little able to stand the strain and racket of disease, of whatsoever sort it may be, and more readily fall victims to its attacks than their more robustly built fellow-creatures.

Again, if it be true, as frequently asserted, that those having the features called "strumous" probably inherit a more or less diluted syphilitic taint; it is not surprising that they should be especially liable to inflammatory changes of a low type, and that disease in them should be readily amenable to treatment, especially by mercury, a result commonly seen in the so-called "strumous" diseases of children and often in those of adults.

These questions we hope to take up again hereafter, when possibly we may be able to demonstrate that though much error has been accumulated around the doctrine of "diatheses," it nevertheless contains a nucleus of valuable truth.

DESCRIPTION OF PLATES I, II, III, IV.

The *composites* are in medallions, the *original photographs* are in small squares.

The composites are numbered consecutively as Figs. 1, 2, 3, &c.

The small numbers attached to the composites and to the photographs of individuals are for the purpose of identification in our indices of composites and of cases.

The faces are classified under the following heads; the references in the columns being to a good composite specimen of each variety:—

	Phthisical.		Non-phthisical.	
	Male.	Female.	Male.	Female.
General average	FIG. 17	FIG. 7	FIG. 11	FIG. 1
Narrow ovoid ("tubercular" type):				
a. Delicate and regular	36	29	12	2
b. Coarse and thickened	34	30		
Broad faces, thick features ("strumous" type):				
a. Somewhat comely	48	31		
b. Coarse and deformed	47	32		
Emaciated	22 or 18	10		

PLATE I

Contains *general averages* of phthisical and non-phthisical patients, both males and females (except Figs. 2, 10, and 12).

FIGS. 1 and 3.—Female non-phthisical patients (each contains 50).

FIG. 2.—Do., with narrow ovoid faces (contains 15). Compare also Figs. 29 and 30, Plate II.

FIGS. 7, 8, and 9.—Female phthisical patients, taken without selection (each contains 50).

FIG. 5.—Do., with strong hereditary taint (contains 56).

FIGS. 4 and 6.—Do., components of Fig. 5.

FIG. 10.—Do., with advanced disease (contains 11).

FIG. 11.—Male non-phthisical patients (contains 100).

FIGS. 13, 14, 15, and 16.—Do., components of Fig. 11 (each contains 25).

FIG. 12.—Do., with narrow ovoid faces (contains 13).

FIG. 17.—Male phthisical patients, taken without selection (contains 200).

FIGS. 18, 19, 20, and 21.—Do., components of Fig. 17 (50 in each).

Plate II.

Fig. 22.—Male phthisical patients, with advanced disease (contains 42).

Figs. 23, 24, 25, 26, 27, 28.—Do., do., components of Fig. 22 (each contains 7).

Fig. 29.—Female phthisical patients, narrow ovoids, high type (contains 9).

Fig. 30.—Do., narrow ovoids, low type (contains 12).

Fig. 31.—Do., broad faces, with comely features (contains 10).

Fig. 32.—Do., do., with deformed features (contains 5).

Plate III.

Fig. 33.—Male phthisical patients, co-composite of all narrow ovoids (contains 51).

Figs. 34, 35, 36, 37, 38, 39, 40.—Do., components of Fig. 33, with the individual portraits.

Plate IV.

Figs. 41, 42.—Male phthisical patients, components of Fig. 33 (continued from Plate III).

Fig. 43.—Do., co-composite of broad faces with thick features (contains 27).

Figs. 44, 45, 46, 47, 48.—Do., components of Fig. 43, with the individual portraits.

Plate No 1

Fig. 1. Co-composite of 50.
Fig. 2. co composite (narrow ovoids)
Fig. 3 co composite of 50
Fig. 4 (No 94)
Fig. 5 (No 96) of 56
Fig. 6 (No 95)
Fig. 7. (No 151)
Fig. 8. (No 150)
Fig. 9. No (159)
Fig. 10. (No 196)
Fig. 11. Co-co-composite
Fig. 12. Co composite (Narrow ovoids)
Fig. 13. (Nos 150-154)
Fig. 14. (Nos 155-159)
Fig. 15. (Nos 160-164)
Fig. 16. (Nos 165 ...)
Fig. 17. Co-co-composite
Fig. 18. (No 7)
Fig. 19. (No. 201-205.
Fig. 20. (Nos 206-210)
Fig. 21. (Nos 211-215)

Plate II.


Fig 22 Co-composite.
Fig 23 (No 176)
Fig 24. (No 177)
Fig 25 (No 178)
Fig 26. (No 185)
Fig 27 (No 186)
Fig 28. (No 187)
Narrow ovoid faces with Phthisis
Fig 29 (No 196)
Narrow ovoids (lower type)
Fig. 30. (No 122)
Also 605.
see Fig 32
Broad faces with thick features (Struma)
Fig. 31. (No 199)
Broad faces (lower type)
Fig. 32 (No 120 (Struma, Syphilis?))

Plate. III.


Fig 33.
Co-composites of males having narrow ovoid faces
Fig. 34.
Fig 35
Fig. 36.
Fig. 37
Fig. 38.
Fig. 39.
Fig. 40.
Components of "Narrow ovoid" Composite (Fig 33)

Plate. IV.


Fig 41
Fig 42
"Narrow ovoids"
Fig 43.
Fig 44.
Co-compn Broad faces
Fig. 45.
Fig 46.
Fig. 47
Fig. 48.
Components of "Broad and Thick" Composite. (Fig 43.)

OUTFIT FOR AN ANTHROPOMETRIC LABORATORY.

WITH the help of friends, I am endeavouring to compile a list of instruments suitable for the outfit of an Anthropometric Laboratory, especially those for testing and measuring the efficiency of the various mental and bodily powers.

The simplest instruments and methods for adequately determining the delicacy of the several Senses are now under discussion. After these shall have been disposed of, the next step will be to consider the methods of measuring the quickness and the accuracy of the Higher Mental Processes.

Any information you can give, or suggestions that you can make, will be thankfully accepted.

FRANCIS GALTON.

42 RUTLAND GATE, LONDON:
March, 1883.

MEASUREMENT OF SENSITIVITY.

THE first desideratum is a correct analysis of the facts of Sensation, arranged in separate paragraphs, so as to methodically cover the ground of inquiry without overlapping, and to serve as a programme to the work. On this being accomplished, we may proceed to consider the most suitable apparatus to afford the measurements (or other tests) suggested by the several paragraphs.

Professor G. CROOM ROBERTSON *has drafted the following scheme, for the special purpose in view :—*

I.—SKIN-SENSATION.

Temperature and Touch (including Contact and Pressure) being distinguished as the two main kinds or qualities of skin-sensation, differently stimulated (whether otherwise differently conditioned or not),—

Determine, first, what parts of the skin are more sensitive to the one kind of stimulus than to the other; afterwards, proceed to test under each head separately.

a. Temperature :—

1. Most and least sensitive parts of the skin (including lips and tongue).
2. Degree (at any part) :—Least perceptible impression. Least perceptible increment. Highest impression (not painful).

b. Touch :—

1. Most and least sensitive parts of skin, (*a*) by least perceptible single impression, (*b*) by distinction of simultaneous impressions (compass-points).
2. Distinguishable qualities or kinds of Touch at any part.
3. Degree (at any part) :—Least perceptible impression. Least perceptible increment (passive appreciation of Weight). Strongest impression (before sensibility is blunted or passes into pain).

f.2r

4. Active Touch or Pressure (involving '*Muscular Sense*'):—Discrimination of Weights (by handling); of Hardness (by pushing); of Roughness (by surface movement); of Textures generally (by combined or opposed movements of thumb and fingers).
5. Power of localising impressions in different regions. Measure of Extension, with different parts moving or at rest.
6. Comparative sensibility (passive or active) of corresponding lateral parts, right and left.

II.—SIGHT.

1. Qualitative distinction of Colours and Tints:—Lowest Red and highest Violet visible. If colour-blind, in what way?
2. Degree:—Least perceptible light-impression. Least perceptible increment of light. Brightest light (short of dazzling).
3. Varying sensitiveness of parts of Retina. To be tested between yellow spot and lateral parts:—(*a*) by least perceptible single impression (dots, &c.); (*b*) by power of distinguishing simultaneous impressions (dots, lines, &c.); (*c*) distinction of colours or tints.

   In connexion with (*a*) and (*b*), maximum sensibility and power of distinction with yellow spot (direct vision) to be specially noted.
4. After-images.
5. Power of following swiftly-moving objects.
6. Range of accommodation by ciliary muscle.
7. Varying measure of Extension, with horizontal and vertical movements.
8. Comparison of the Two Eyes in all respects.
9. Æsthetic (emotional) aspects of Vision.—Interest in Light, in Colour, or in Form? Preference of what Colours? of what Forms? Perception of Harmony and Discord of Colours.

III.—HEARING.

1. Qualitative distinction of Sounds (musical and unmusical).
2. Degree:—Just perceptible sound. Just perceptible increment. Loudest sound (distinguishable).

3. Pitch :—Lowest continuous note. Least perceptible interval. Highest audible note.
4. Sense of Direction (by mere sound)?
5. Comparison of the Two Ears in general efficiency.
6. Æsthetic (emotional) aspects :—Interest in Noise or Musical Sound? in Melody or Harmony? Perception of Harmonies and Discords.

IV.—SMELL.

1. Qualitative distinction of Odours.
2. Degree :—Just perceptible impression. Just perceptible increment. Strongest distinguishable impression.

V.—TASTE.

1. Qualitative distinction of Tastes.
2. Degree :—Just perceptible impression. Just perceptible increment. Strongest distinguishable impression.
3. Varying sensibility of organ ;—between tip and back of tongue ; between edges and middle of tongue.

VI.—'MUSCULAR SENSE.'

The so-called 'Muscular Sense,' implicated as it is with other modes of sense (chiefly Touch and Sight), is hardly to be tested separately.

[Query, whether 'Muscular Sense' is not most properly regarded as a general coefficient transforming *passive* into *active* Sense. The distinction between passive and active Touch is commonly allowed—the difference between 'being touched or pressed' (passively) and 'touching or pressing' (actively). A like distinction runs through all the senses :—'seeing' and 'looking'; 'hearing' and 'listening'; 'smelling' and 'sniffing,' &c. ; though it is of chief account in Touch and Sight, where the sensitive organ (or seat) is at the same time, by its attachments, a muscular organ, or has what may be called a *proper* motion. This view of the function of 'Muscular Sense' holds equally, whether the special conscious experience so designated depends immediately on the innervation of muscle from the brain (Bain, Wundt), or arises only upon backward stimulus (by afferent fibres) from the muscle in contraction, or, as is most probable, involves always both kinds of nerve-process.]

ISSUED BY AUTHORITY.

Anthropometric Laboratory;

ARRANGED BY

FRANCIS GALTON, F.R.S.,

FOR

THE DETERMINATION OF HEIGHT, WEIGHT, SPAN, BREATHING POWER, STRENGTH OF PULL AND SQUEEZE, QUICKNESS OF BLOW, HEARING, SEEING, COLOUR-SENSE, AND OTHER PERSONAL DATA.

THE LABORATORY IS SITUATED IN THE EAST CORRIDOR ANNEXE,
ENTRANCE FROM THE SOUTH GALLERY.

Admission to the Laboratory 3d., for which a Schedule filled up with the above details will be furnished.

LONDON:
WILLIAM CLOWES AND SONS, LIMITED,
INTERNATIONAL HEALTH EXHIBITION,
AND 13, CHARING CROSS, S.W.
1884.

PRICE ONE PENNY.

p.la r

Females

1.3 ~~healthy~~ non phthisis patients 50 . 50

7 8 9 phthisis 50 each - all like sisters

10 Emaciated.

Males.

health sabbers

14. 15 non phthisical (~~100~~) 50

18 19 20 21. phthisis 50 each like brother

Females

1. 3 not. 50 each

7. 8. 9. yes just like sisters 50 each

10. Emaciated 11 cases
advanced disease

Tamiz Wkenels

Female

50 50

1 – 3

100

? – [illegible]

50 50 50

9 8 ?

?

Male

p. 1a v

25 25 25 25

13 – 14 – 15 – 16

100

11

25 25 25 25

18 19 20 21

200

17

non phthisical
Hospital patients

Phthisical

Emaciated
" 10

"Tubercular"
~~0001 5~~
0001
fine coarse
29 – 30

"Strumous" Emaciated
thick features
fine coarse

Emaciated

Female

	Special subdivisions					
	Average	Emaciated	Good fine	Good coarse	Strumous fine	Strumous coarse
Phthisis M F	O	O	O	O	O	O
non Phthisis M	O	O	O	O	O	O
Health	O	O	O	O	O	O

Male

p. 1c

✓ Nature
✓ Athenaeum
✓ Spectator
✓ Academy
✓ Pall Mall
✓ Royal Society
✓ Mac alister
✓ L. Darwin
✓ Capt Abney

Allen Thomson
✓ Bowman
Simon
✓ Busk
✓ Sir J. Paget
✓ H. Spencer
✓ Huggins
✓ Carpenter
14

14 carried over
Self
Emma

ANTHROPOMETRIC LABORATORY;

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International Health Exhibition,

LONDON, 1884.

THE

ANTHROPOMETRIC LABORATORY;

ARRANGED BY

FRANCIS GALTON, F.R.S.

THE object of the Anthropometric Laboratory is to show to the public the great simplicity of the instruments and methods 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 these respects, and a card containing the results furnished to him, and a duplicate 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 shews the progress of the individual; the other, that of portions of the nation, or of the nation as a whole. We will consider these two uses separately.

Personal use.—Periodical measurements afford a sure test whether the physical development of the child or youth is proceeding normally. They draw attention to faults in rearing to be diligently sought for and remedied, lest the future efficiency of the child, when it grows to manhood or womanhood, be compromised. There are hundreds of thousands of cases in which eye-sight has been heedlessly injured beyond repair by pure neglect; of lop-sided growth, and of stunted chest capacity, which measurement would have manifested in their earlier stages, and which could have been checked if attended to in time. The necessity of periodical measurement is thoroughly recognised by those who have studied the subject of health, but it has not yet obtained that hold on popular opinion which it deserves, and which it will hereafter undoubtedly exercise.

Statistical use. — Anthropometric records are treated statistically to discover the efficiency of the nation as a whole and in its several parts, and the direction in which it is changing, whether for better or worse. They enable us to compare schools, occupations, residences, races, &c. The Anthropometric Committee of the British Association took great pains to collect available data for inquiries of this kind, but their returns were by no means adequate to solve even the more important national questions, although many interesting facts were derived from them. There is great need for a more systematic registration of physical measurements. Their value is indisputable, the cost of making them is trifling, and the facility of registration in any permanent institution is obvious. It seems strange that they should be neglected at any school or university.

To show the use of preserving even the minor personal data, it will be well to dwell for a moment on the colour of the Eyes and Hair, which might be thought at first sight to have no obvious bearing on the general efficiency of the nation. This is far from being the case. The British nation is partly a blend and partly a mosaic of very distinct types. The short black-haired ancient British race unites imperfectly with the tall fair-haired Danish or Scandinavian. Their union resembles what druggists call an emulsion, that is, a mixture of oil and water, so well shaken together that they form an apparently homogeneous substance; but the combination is not durable. Leave the emulsion alone, and after a longer or shorter time it will separate into its component elements. Types are stable, but the forms of their mongrel offspring are not; and whenever the external features of the old types are found in something of their original purity, it is reasonable to suppose that their inward characteristics are present also.

Whether it be as a race peculiarity or not, the colour of the hair is related, at least in America, to certain forms of immunity from disease or susceptibility to it. This is shown by the statistics published by the American War Office in 1875, under the direction of Dr. Baxter. At the time of the war of their rebellion all male citizens of the United States between the ages of 20 and 45 years were medically inspected with great minuteness, to learn how many were fit for service. About one quarter of those examined were rejected, and the diseases that incapacitated them are specified in Dr. Baxter's book, together with various particulars, including the colour of the hair and eyes. It appears from an analysis of between 330,000 and 340,000 of the best reported cases of invalidism, that the proportion of the light complexioned men who were unfit for service was larger than that of the dark. The light haired men in America were more affected than the dark haired by every form of disease except chronic rheumatism. A diagram in which these proportions are shown is hung up in the laboratory.

It follows that even the colour of the hair is a proper subject for anthropometric record; much more may we feel assured that obviously important personal data deserve measurement and registration.

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 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 colour for 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 list of 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 is made regarding the colour of the hair, for the reason that what with the darkening effect 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 is a long and light frame with a single eye hole. Blocks of wood, each with a sentence in diamond print pasted upon its end, are fastened square to the line of sight at measured distances along the frame. 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 note is made to that effect.

b. Colour-sense.—A series of bars are packed closely side by side in a frame, looking something like the keys of a pianoforte. 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 a flap, and he is required to insert a peg opposite each of the bars that has any shade of green wool wound upon 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 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 are moveable pointers attached to the lower of each pair of strips. In the one pair, it 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 hundredth part of the total length of the rod.

As regards Squareness.—A board including a sector of a circle, has an arm movable about the centre of the circle, while a broad flap hides its free end. A black line 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 indiarubber 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 that 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.

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 through a tube the vessel rises, and the scale at its side shews 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.

SWIFTNESS OF BLOW.—A flat bar with a pad 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, and its swiftness of motion is measured 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, a stud upon its face holds the spring forcibly to one side, but as soon as the bar begins to move, the stud leaves 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 cardboard used for white flexible slates. The pencil leaves a sinuous trace on the strip, 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 instrument 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 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. Mr. Gammage, of Messrs. Tisley & Co., 172, Brompton Road, assisted me in putting the instruments in working order. The larger of the small whistles are made by them; the smaller and more delicate ones are made by Mr. Hawkesley, 357, Oxford-Street.

FRANCIS GALTON.

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.	
Error per cent. in dividing a line of 15 inches	in three parts / in two parts
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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Twins — History of 1

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The HISTORY *of* TWINS, *as a Criterion of the* RELATIVE POWERS *of* NATURE *and* NURTURE. By FRANCIS GALTON, F.R.S.*

THE exceedingly close resemblance attributed to twins has been the subject of many novels and plays, and most persons have felt a desire to know upon what basis of truth those works of fiction may rest. But twins have many other claims to attention, one of which will be discussed in the present memoir. It is, that their history affords means of distinguishing between the effects of tendencies received at birth, and of those that were imposed by the circumstances of their after lives; in other words, between the effects of nature and of nurture.† This is a subject of especial importance in its bearings on investigations into mental heredity, and I, for my part, have keenly felt the difficulty of drawing the necessary distinction whenever I tried to estimate the degree in which mental ability was, on the average, inherited. The objection to statistical evidence in proof of its inheritance has always been: "The persons whom you compare may have lived under similar social conditions and have had similar advantages of education, but such prominent conditions are only a small part of those that determine the future of each man's life. It is to trifling accidental circumstances that the bent of his disposition and his success are mainly due, and these you leave wholly out of account—in fact, they do not admit of being tabulated, and therefore your statistics, however plausible at first sight, are really of very little use." No method of inquiry which I have been able to carry out—and I have tried many methods—is wholly free from this objection. I have therefore attacked the problem from the opposite side, seeking for some new method by which it would be possible to weigh in just scales the respective effects of nature and nurture, and to ascertain their several shares in framing the disposition and intellectual ability of men. The life history of twins supplies what I wanted. We might begin by inquiring about twins who were closely alike in boyhood and

* Reprinted, with revision and additions, from *Fraser's Magazine*, Nov. 1875.

† In my "English Men of Science," 1874, p. 12, I treated this subject in a cursory way. It subsequently occurred to me that it deserved a more elaborate inquiry, which I made, and of which this paper is a result.

youth, and who were educated together for many years, and learn whether they subsequently grew unlike, and, if so, what the main causes were which, in the opinion of the family, produced the dissimilarity. In this way we may obtain direct evidence of the kind we want. Again, we may obtain yet more valuable evidence by a converse method. We might inquire into the history of twins who were exceedingly unlike in childhood, and learn how far their characters became assimilated under the influence of identical nurtures, inasmuch as they had the same home, the same teachers, the same associates, and in every other respect the same surroundings.

My materials were obtained by sending circulars of inquiry to persons who were either twins themselves or the near relations of twins. The printed questions were in thirteen groups; the last of them asked for the addresses of other twins known to the recipient, who might be likely to respond if I wrote to them. This happily led to a continually widening circle of correspondence, which I pursued until enough material was accumulated for a general reconnaissance of the subject.

There is a large literature relating to twins in their purely surgical and physiological aspect. The reader interested in this should consult *Die Lehre von den Zwillingen*, von L. Kleinwächter, Prag. 1871. It is full of references, but it is also unhappily disfigured by a number of numerical misprints, especially in page 26. I have not found any book that treats of twins from my present point of view.

The reader will easily understand that the word "twins" is a vague expression, which covers two very dissimilar events—the one corresponding to the progeny of animals that have usually more than one young one at a birth, each of which is derived from a separate ovum, while the other is due to the development of two germinal spots in the same ovum. In the latter case, they are enveloped in the same membrane, and all such twins are found invariably to be of the same sex. The consequence of this is, that I find a curious discontinuity in my results. One would have expected that twins would commonly be found to possess a certain average likeness to one another; that a few would greatly exceed that degree of likeness, and a few would greatly fall short of it. But this is not at all the case. Extreme similarity and extreme dissimilarity between twins of the same sex, are nearly as common as moderate resemblance. When the twins are a boy and a girl, they are never closely alike; in fact, their origin is never due to the development of two germinal spots in the same ovum.

I have received about eighty returns of cases of close similarity, thirty-five of which entered into many instructive details. In a few of these not a single point of difference could be specified. In the remainder, the colour of the hair and eyes were almost always identical; the height, weight, and strength were generally nearly so. Nevertheless, I have a few cases of a notable difference in these, although the resemblance was otherwise very near. The manner and address of the thirty-five pairs of twins is usually described as very similar, though there often exists a difference of expression, familiar to near relatives, but unperceived by strangers. The intonation of the voice when speaking is commonly the same, but it frequently happens that the twins sing in different keys. Most singularly the one point in which similarity is rare, is the handwriting. I can with difficulty account for this, considering how strongly handwriting runs in families, but I am sure of the fact. I have only one case in which nobody, not even the twins themselves, could distinguish their own notes of lectures, &c.; barely two or three in which the handwriting was undistinguishable by others, and only a few in which it was described as closely alike. On the other hand, I have many in which it is stated to be unlike, and some in which it is alluded to as the only point of difference. It would appear that the handwriting is a very delicate test of difference in organisation—a conclusion which I commend to the notice of enthusiasts in the art of discovering character by the handwriting.

One of my inquiries was for anecdotes as regards the mistakes made by near relatives, between the twins. The replies are numerous, but not very varied in character. When the twins are children, they have commonly to be distinguished by ribbons tied round their wrist or neck; nevertheless the one is sometimes fed, physicked, and whipped by mistake for the other, and the description of these little domestic catastrophes is usually given to me by the mother, in a phraseology that is somewhat touching by reason of its seriousness. I have one case in which a doubt remains whether the children were not changed in their bath, and the presumed A is not really B, and *vice versâ*. In another case, an artist was engaged on the portraits of twins who were between three and four years of age; he had to lay aside his work for three weeks, and, on resuming it, could not tell to which child the respective likenesses he had in hand belonged. The mistakes are less numerous on the part of the mother during the boyhood and girlhood of the twins, but almost as frequent on the part of strangers. I have many instances of tutors being unable to distinguish their twin pupils. Two girls used regularly to impose on their music teacher when one of them wanted a whole holiday; they had their lessons at separate hours, and the one girl sacrificed herself to receive two lessons on the same day, while the other one enjoyed herself. Here is a brief and comprehensive account:—"Exactly alike in all, their schoolmasters never could tell them apart; at dancing parties they constantly changed partners without discovery; their close resemblance is scarcely diminished by age." The following is a typical schoolboy anecdote:—"Two twins were fond of playing tricks, and complaints were frequently made; but the boys would never own which was the guilty one, and the complainants were never certain which of the two he was. One head master used to say he would never flog the innocent for the guilty, and another used to flog both." No less than nine anecdotes have reached me of a twin seeing his or her reflection in a looking-glass, and addressing it, in the belief it was the other twin in person. I have many anecdotes of mistakes when the twins were nearly grown up. Thus:—"Amusing scenes occurred at college when one twin came to visit the other; the porter on one occasion refusing to let the visitor out of the college gates, for, though they stood side by side, he professed ignorance as to which he ought to allow to depart."

Children are usually quick in distinguishing between their parent and his or her twin; but I have two cases to the contrary. Thus, the daughter of a twin says:—"Such was the marvellous similarity of their features, voice, manner, &c., that I remember, as a child, being very much puzzled, and I think, had my aunt lived much with us, I should have ended by thinking I had two mothers." In the other case, a father who was a twin, remarks of himself and his brother:—"We were extremely alike, and are so at this moment, so much so that our children up to five and six years old did not know us apart."

I have four or five instances of doubt during an engagement of marriage. Thus:—"A married first, but both twins met the lady together for the first time, and fell in love with her there and then. A managed to see her home and to gain her affection, though B went sometimes courting in his place, and neither the lady nor her parents could tell which was which." I have also a German letter, written in quaint terms, about twin brothers who married sisters, but could not easily be distinguished by them.* In the well-known novel by Mr. Wilkie Collins of "Poor Miss Finch," the blind girl distinguishes the twin

* I take this opportunity of withdrawing an anecdote, happily of no great importance, published in "Men of Science," p. 14, about a man personating his twin brother for a joke at supper, and not being discovered by his wife. It was told me on good authority; but I have reason to doubt the fact, as the story is not known to the son of one of the twins. However, the twins in questions were extraordinarily alike, and I have many anecdotes about them sent me by the latter gentleman.

she loves by the touch of his hand, which gives her a thrill that the touch of the other brother does not. Philosophers have not, I believe, as yet investigated the conditions of such thrills; but I have a case in which Miss Finch's test would have failed. Two persons, both friends of a certain twin lady, told me that she had frequently remarked to them that "kissing her twin sister was not like kissing her other sisters, but like kissing herself—her own hand for example."

It would be an interesting experiment for twins who were closely alike, to try how far dogs could distinguish between them by scent.

I have a few anecdotes of strange mistakes made between twins in adult life. Thus, an officer writes:—"On one occasion when I returned from foreign service my father turned to me and said, 'I thought you were in London,' thinking I was my brother—yet he had not seen me for nearly four years—our resemblance was so great."

The next and last anecdote I shall give is, perhaps, the most remarkable of those I have; it was sent me by the brother of the twins, who were in middle life at the time of its occurrence: "A was again coming home from India, on leave; the ship did not arrive for some days after it was due; the twin brother B had come up from his quarters to receive A, and their old mother was very nervous. One morning A rushed in, saying, 'Oh, mother, how are you?' Her answer was, 'No, B, it's a bad joke; you know how anxious I am!' and it was a little time before A could persuade her that he was the real man."

Enough has been said to prove that an extremely close personal resemblance frequently exists between twins of the same sex; and that, although the resemblance usually diminishes as they grow into manhood and womanhood, some cases occur in which the resemblance is lessened in a hardly perceptible degree. It must be borne in mind that the divergence of development, when it occurs, need not be ascribed to the effect of different nurtures, but it is quite possible that it may be due to the appearance of qualities inherited at birth, though dormant, like gout, in early life. To this I shall recur.

There is a curious feature in the character of the resemblance between twins, which has been alluded to by a few correspondents; it is well illustrated by the following quotations. A mother of twins says:—"There seemed to be a sort of interchangeable likeness in expression, that often gave to each the effect of being more like his brother than himself." Again, two twin brothers, writing to me, after analysing their points of resemblance, which are close and numerous, and pointing out certain shades of difference, add—"These seem to have marked us through life, though for a while, when we were first separated, the one to go to business, and the other to college, our respective characters were inverted; we both think that at that time we each ran into the character of the other. The proof of this consists in our own recollections, in our correspondence by letter, and in the views which we then took of matters in which we were interested." In explanation of this apparent interchangeableness, we must recollect that no character is simple, and that in twins who strongly resemble each other, every expression in the one may be matched by a corresponding expression in the other, but it does not follow that the same expression should be the dominant one in both cases. Now it is by their dominant expressions that we should distinguish between the twins; consequently when one twin has temporarily the expression which is the dominant one in his brother, he is apt to be mistaken for him. There are also cases where the development of the two twins is not strictly *pari passu;* they reach the same goal at the same time, but not by identical stages. Thus:—A is born the larger, then B overtakes and surpasses A, and is in his turn overtaken by A, the end being that the twins become closely alike. This process would aid in giving an interchangeable likeness at certain periods of their growth, and is undoubtedly due to nature more frequently than to nurture.

Among my thirty-five detailed cases of close similarity, there are no less than seven in which both twins suffered from some special ailment or had some exceptional peculiarity. One twin writes that she and her sister "have both the defect of not being able to come downstairs quickly, which, however, was not born with them, but came on at the age of twenty." Three pairs of twins have peculiarities in their fingers; in one case it consists in a slight congenital flexure of one of the joints of the little finger; it was inherited from a grandmother, but neither parents, nor brothers, nor sisters show the least trace of it. In another case the twins have a peculiar way of bending the fingers, and there was a faint tendency to the same peculiarity in the mother, but in her alone of all the family. In a third case, about which I made a few inquiries, which is given by Mr. Darwin, but is not included in my returns, there was no known family tendency to the peculiarity in the twins of a crooked little finger. In another pair of twins, one was born ruptured, and the other became so at six months old. Two twins at the age of twenty-three were attacked by toothache, and the same tooth had to be extracted in each case. There are curious and close correspondences mentioned in the falling off of the hair. Two cases are mentioned of death from the same disease; one of which is very affecting. The outline of the story was that the twins were closely alike and singularly attached, and had identical tastes; they both obtained Government clerkships, and kept house together, when one sickened and died of Bright's disease, and the other also sickened of the same disease and died seven months later.

In no less than nine out of the thirty-five cases does it appear that both twins are apt to sicken at the same time. This implies so intimate a constitutional resemblance, that it is proper to give some quotations in evidence. Either the illnesses were non-contagious in the instances to which I refer, or if contagious, they caught them simultaneously; they did not catch them the one from the other. Thus, the father of two twins says: "Their general health is closely alike; whenever one of them has an illness, the other invariably has the same within a day or two, and they usually recover in the same order. Such has been the case with whooping-cough, chicken-pox, and measles; also with slight bilious attacks, which they have successively. Latterly, they had a feverish attack at the same time." Another parent of twins says:—"If anything ails one of them, identical symptoms *nearly always* appear in the other; this has been singularly visible in two instances during the last two months. Thus, when in London, one fell ill with a violent attack of dysentery, and within twenty-four hours the other had precisely the same symptoms." A medical man writes of twins with whom he is well acquainted:—"Whilst I knew them, for a period of two years, there was not the slightest tendency towards a difference in body or mind; external influences seemed powerless to produce any dissimilarity." The mother of two other twins, after describing how they were ill simultaneously up to the age of fifteen, adds, that they shed their first milk teeth within a few hours of each other.

Trousseau has a very remarkable case (in the chapter on Asthma) in his important work "Clinique Médicale." (In the edition of 1873, it is in vol. ii. p. 473.) It was quoted at length in the original French, in Mr. Darwin's "Variation under Domestication," vol. ii. p. 252. The following is a translation:—

"I attended twin brothers so extraordinarily alike, that it was impossible for me to tell which was which, without seeing them side by side. But their physical likeness extended still deeper, for they had, so to speak, a yet more remarkable pathological resemblance. Thus, one of them, whom I saw at the Néothermes at Paris, suffering from rheumatic ophthalmia, said to me, 'At this instant my brother must be having an ophthalmia like mine;' and, as I had exclaimed against such an assertion, he showed me a few days afterwards a letter just received by him from his brother, who was at that time at Vienna, and who expressed himself in these words—'I have my ophthalmia; you must be having yours.' However singular this story may appear, the fact is none the less exact; it has not been told to me by others, but I have seen it myself; and I have seen other analogous cases in my practice. These twins were also asthmatic, and asthmatic to a frightful degree. Though born in Marseilles, they were never able to stay in that town, where their business affairs required them to go, without having an attack. Still more strange, it was sufficient for them to get away only as far as Toulon in order to be cured of the attack caught at Marseilles. They travelled continually, and in all countries, on business affairs, and they remarked that certain localities were extremely hurtful to them, and that in others they were free from all asthmatic symptoms."

I do not like to pass over here a most dramatic tale in the "Psychologie Morbide" of Dr. J. Moreau (de Tours), Médecin de l'Hospice de Bicêtre. Paris, 1859, p. 172. He speaks "of two twin brothers who had been confined, on account of monomania, at Bicêtre. . . . Physically the two young men are so nearly alike that the one is easily mistaken for the other. Morally, their resemblance is no less complete, and is most remarkable in its details. Thus, their dominant ideas are absolutely the same. They both consider themselves subject to imaginary persecutions; the same enemies have sworn their destruction, and employ the same means to effect it. Both have hallucinations of hearing. They are both of them melancholy and morose; they never address a word to anybody, and will hardly answer the questions that others address to them. They always keep apart, and never communicate with one another. An extremely curious fact which has been frequently noted by the superintendents of their section of the hospital, and by myself, is this: From time to time, at very irregular intervals of two, three, and many months, without appreciable cause, and by the purely spontaneous effect of their illness, a very marked change takes place in the condition of the two brothers. Both of them, at the same time, and often on the same day, rouse themselves from their habitual stupor and prostration; they make the same complaints, and they come of their own accord to the physician, with an urgent request to be liberated. I have seen this strange thing occur, even when they were some miles apart, the one being at Bicêtre, and the other living at Saint-Anne." Dr. Moreau ranked as a very considerable medical authority, but I cannot wholly accept this strange story without fuller information. Dr. Moreau writes it in too off-hand a way to carry the conviction that he had investigated the circumstances with the sceptic spirit and scrupulous exactness which so strange a phenomenon would have required. If full and precise notes of the case exist, they certainly ought to be published at length. I sent a copy of this passage to the principal authorities among the physicians to the insane in England, asking if they had ever witnessed any similar case. In reply, I have received three noteworthy instances, but none to be compared in their exact parallelism with that just given. The details of these three cases are painful, and it is not necessary to my general purpose that I should further allude to them.

There is another curious French case of insanity in twins, which was pointed out to me by Professor Paget, described by Dr. Baume in the "Annales Médico-Psychologiques," 4 série, vol. i. 1863, p. 312, of which the following is an abstract. The original contains a few more details, but is too long to quote: François and Martin, fifty years of age, worked as railroad contractors between Quimper and Châteaulin. Martin had twice had slight attacks of insanity. On January 15 a box in which the twins deposited their savings was robbed. On the night of January 23-4 both François (who lodged at Quimper) and Martin (who lived with his wife and children at St. Lorette, two leagues from Quimper) had the same dream at the same hour, three a.m., and both awoke with a violent start, calling out, "I have caught the thief! I have caught the thief! they are doing mischief to my brother!" They were both of them extremely agitated, and gave way to similar extravagances, dancing and leaping. Martin sprang on his grandchild, declaring that he was the thief, and would have strangled him if he had not been prevented; he then became steadily worse, complained of violent pains in his head, went out of doors on some excuse, and tried to drown himself in the River Steir, but was forcibly stopped by his son, who had watched and followed him. He was then taken to an asylum by gendarmes, where he died in three hours. François, on his part, calmed down on the morning of the 24th, and employed the day in inquiring about the robbery. By a strange chance, he crossed his brother's path at the moment when the latter was struggling with the gendarmes; then he himself became maddened, giving way to extravagant gestures and using incoherent language (similar to that of his brother). He then asked to be bled, which was done, and afterwards, declaring himself to be better, went out on the pretext of executing some commission, but really to drown himself in the River Steir, which he actually did, at the very spot where Martin had attempted to do the same thing a few hours previously.

The next point which I shall mention, in illustration of the extremely close resemblance between certain twins, is the similarity in the association of their ideas. No less than eleven out of the thirty-five cases testify to this. They make the same remarks on the same occasion, begin singing the same song at the same moment, and so on; or one would commence a sentence, and the other would finish it. An observant friend graphically described to me the effect produced on her by two such twins whom she had met casually. She said: "Their teeth grew alike, they spoke alike and together, and said the same things, and seemed just like one person." One of the most curious anecdotes that I have received concerning this similarity of ideas was that one twin, A, who happened to be at a town in Scotland, bought a set of champagne glasses which caught his attention, as a surprise for his brother B; while, at the same time, B, being in England, bought a similar set of precisely the same pattern as a surprise for A. Other anecdotes of a like kind have reached me about these twins.

The last point to which I shall allude regards the tastes and dispositions of the thirty-five pairs of twins. In sixteen cases—that is, in nearly one-half of them—these were described as closely similar; in the remaining nineteen they were much alike, but subject to certain named differences. These differences belonged almost wholly to such groups of qualities as these: The one was the more vigorous, fearless, energetic; the other was gentle, clinging, and timid: or again, the one was more ardent, the other more calm and gentle: or again, the one was the more independent, original, and self-contained; the other the more generous, hasty, and vivacious. In short, the difference was that of intensity or energy in one or other of its protean forms; it did not extend more deeply into the structure of the characters. The more vivacious might be subdued by ill health, until he assumed the character of the other; or the latter might be raised by excellent health to that of the former. The difference was in the key-note, not in the melody.

It follows from what has been said concerning the similar dispositions of the twins, the similarity in the associations of their ideas, of their special ailments, and of their illnesses generally, that the resemblances are not superficial, but extremely intimate. I have only two cases altogether of a strong bodily resemblance being accompanied by mental diversity, and one case only of the converse kind. It must be remembered that the conditions which govern extreme likeness between twins are not the same as those between ordinary brothers and sisters (I have spoken of this in my memoir on the 'Theory of Heredity,' "Journal Anthropological Institute," December, 1875, p. 329); and that it would be wholly incorrect to generalise from what has just been said about the twins, that mental and bodily likeness are invariably co-ordinate, such being by no means the case.

We are now in a position to understand that the phrase "close similarity" is no exaggeration, and to realise the value of the evidence about to be adduced. Here are thirty-five cases of twins who were "closely alike" in body and mind when they were young, and who have been reared exactly alike up to their early manhood and womanhood. Since then the conditions of their lives have changed; what change of conditions has produced the most variation?

It was with no little interest that I searched the records of the thirty-five cases for an answer; and they gave an answer that was not altogether direct, but it was very distinct, and not at all what I had expected. They showed me that in some cases the resemblance of body and mind had continued unaltered up to old age, notwithstanding very different conditions of life; and they showed in the other cases that the parents ascribed such dissimilarity as there was wholly, or almost wholly to some form of illness. In four cases it was scarlet fever; in one case, typhus; in one, a slight effect was ascribed to a nervous fever; then I find effects from an Indian climate; from an illness (unnamed) of nine months' duration; from varicose veins; from a bad fracture of the leg, which prevented all active exercise afterwards; and there were three other cases of ill health. It will be sufficient to quote one of the returns; in this the father writes: "At birth they were *exactly* alike, except that one was born with a bad varicose affection, the effect of which had been to prevent any violent exercise, such as dancing or running, and, as she has grown older, to make her more serious and thoughtful. Had it not been for this infirmity, I think the two would have been as exactly alike as it is possible for two women to be, both mentally and physically; even now they are constantly mistaken for one another."

In only a very few cases is there some allusion to the dissimilarity being partly due to the combined action of many small influences, and in none of the 35 cases is it largely, much less wholly, ascribed to that cause. In not a single instance have I met with a word about the growing dissimilarity being due to the action of the firm freewill of one or both of the twins, which had triumphed over natural tendencies; and yet a large proportion of my correspondents happen to be clergymen whose bent of mind is opposed, as I feel assured from the tone of their letters, to a necessitarian view of life.

It has been remarked that a growing diversity between twins may be ascribed to the tardy development of naturally diverse qualities; but we have a right, upon the evidence I have received, to go further than this. We have seen that a few twins retain their close resemblance through life; in other words, instances do exist of an apparently thorough similarity of nature, in which external circumstances do not create dissimilarity. Positive evidence, such as this, cannot be outweighed by any amount of negative evidence. Therefore, in those cases where there is a growing diversity, and where no external cause can be assigned either by the twins themselves or by their family for it, we may feel sure that it must be chiefly or altogether due to a want of thorough similarity in their nature. Nay, further, in some cases it is distinctly affirmed that the growing dissimilarity can be accounted for in no other way. We may therefore broadly conclude that the only circumstance, within the range of those by which persons of similar conditions of life are affected, capable of producing a marked effect on the character of adults, is illness or some accident which causes physical infirmity. The twins who closely resembled each other in childhood and early youth, and were reared under not very dissimilar conditions, either grow unlike through the development of natural characteristics which had lain dormant at first, or else they continue their lives, keeping time like two watches, hardly to be thrown out of accord except by some physical jar. Nature is far stronger than nurture within the limited range that I have been careful to assign to the latter.

The effect of illness, as shown by these replies, is great, and well deserves further consideration. It appears that the constitution of youth is not so elastic as we are apt to think, but that an attack, say of scarlet fever, leaves a permanent mark, easily to be measured by the present method of comparison. This recalls an impression made strongly on my mind several years ago, by the sight of some curves drawn by a mathematical friend. He took monthly measurements of the circumference of his children's heads during the first few years of their lives, and he laid down the successive measurements on the successive lines of a piece of ruled paper, by taking the edge of the paper as a base. He then joined the free ends of the lines, and so obtained a curve of growth. These curves had, on the whole, that regularity of sweep that might have been expected, but each of them showed occasional halts, like the landing places on a long flight of stairs. The development had been arrested by something, and was not made up for by after growth. Now, on the same piece of paper my friend had also registered the various infantine illnesses of the children, and corresponding to each illness was one of these halts. There remained no doubt in my mind that, if these illnesses had been warded off, the development of the children would have been increased by almost the precise amount lost in these halts. In other words, the disease had drawn largely upon the capital, and not only on the income, of their constitutions. I hope these remarks may induce some men of science to repeat similar experiments on their children of the future. They may compress two years of a child's history on one side of a ruled half-sheet of foolscap paper, if they cause each successive line to stand for a successive month, beginning from the birth of the child; and if they mark off the measurements by laying, not the 0-inch division of the tape against the edge of the pages, but, say, the 10-inch division—in order to economise space.

The steady and pitiless march of the hidden weaknesses in our constitutions, through illness to death, is painfully revealed by these histories of twins. We are too apt to look upon illness and death as capricious events, and there are some who ascribe them to the direct effect of supernatural interference, whereas the fact of the maladies of two twins being continually alike, shows that illness and death are necessary incidents in a regular sequence of constitutional changes, beginning at birth, upon which external circumstances have, on the whole, very small effect. In cases where the maladies of the twins are continually alike, the clocks of their two lives move regularly on, and at the same rate, governed by their internal mechanism. When the hands approach the hour mark, there are sudden clicks, followed by a whirring of wheels; the moment that they touch it, the strokes fall. Necessitarians may derive new arguments from the life histories of twins.

We will now consider the converse side of our subject, which appears to me even the more important of the two, though I had little suspected it would be so, when I first began the inquiry. Hitherto we have investigated cases where the similarity at first was close, but afterwards became less; now we will examine those in which there was great dissimilarity at first, and will see how far an identity of nurture in childhood and youth tended to assimilate them. As has been already mentioned, there is a large proportion of cases of sharply contrasted characteristics, both of body and mind, among twins. I have twenty such cases, given with much detail. It is a fact that extreme dissimilarity, such as existed between Esau and Jacob, is a no less marked peculiarity in twins of the same sex, than extreme similarity. On this curious point, and on much else in the history of twins, I have many remarks to make, but this is not the place to make them.

The evidence given by the twenty cases above mentioned is absolutely accordant, so that the character of the whole may be exactly conveyed by two or three quotations. One parent says: "They have had *exactly the same nurture* from their birth up to the present time; they are both perfectly healthy and strong, yet they are otherwise as dissimilar as two boys could be, physically, mentally, and in their emotional nature." Here is another case: "I can answer most decidedly that the twins have been perfectly dissimilar in character, habits, and likeness from the moment of their birth to the present time, though they were nursed by the same woman, went to school together, and were never separated till the age of fifteen." Here again is one more, in which the father remarks: "They were curiously different in body and mind from their birth." The surviving twin (a senior wrangler of Cambridge) adds: "A fact struck all our school contemporaries, that my brother and I were complementary, so to speak, in point of ability and disposition. He was contemplative, poetical, and literary to a remarkable degree, showing great power in that line. I was practical, mathematical, and linguistic. Between us we should have made a very decent sort of a man." I could quote others just as strong as these, in some of which the word "complementary" again appears, while I have not a single case in which my correspondents speak of originally dissimilar characters having become assimilated through identity of nurture. The impression that all this evidence leaves on the mind is one of some wonder whether nurture can do anything at all, beyond giving instruction and professional training. It emphatically corroborates and goes far beyond the conclusions to which we had already been driven by the cases of similarity. In these, the causes of divergence began to act about the period of adult life, when the characters had become somewhat fixed; but here the causes conducive to assimilation began to act from the earliest moment of the existence of the twins, when the disposition was most pliant, and they were continuous until the period of adult life. There is no escape from the conclusion that nature prevails enormously over nurture when the differences of nurture do not exceed what is commonly to be found among persons of the same rank of society and in the same country. My only fear is, that my evidence seems to prove too much, and may be discredited on that account, as it seems contrary to all experience that nurture should go for so little. But experience is often fallacious in ascribing great effects to trifling circumstances. Many a person has amused himself with throwing bits of stick into a tiny brook and watching their progress; how they are arrested, first by one chance obstacle, then by another; and again, how their onward course is facilitated by a combination of circumstances. He might ascribe much importance to each of these events, and think how largely the destiny of the stick had been governed by a series of trifling accidents. Nevertheless all the sticks succeed in passing down the current, and they travel, in the long run, at nearly the same rate. So it is with life, in respect to the several accidents which seem to have had a great effect upon our careers. The one element, which varies in different individuals, but is constant in each of them, is the natural tendency; it corresponds to the current in the stream, and inevitably asserts itself.

Much stress is laid on the persistence of moral impressions made in childhood, and the conclusion is drawn, that the effects of early teaching generally, must be important in a corresponding degree. I acknowledge the fact, but doubt the deduction. The child is usually taught by its parents, and their teachings are of an exceptional character, for the following reason. There is commonly a strong resemblance, owing to inheritance, between the dispositions of the child and its parents. They are able to understand the ways of one another more intimately than is possible to persons not of the same blood, and the child instinctively assimilates the habits and ways of thought of its parents. Its disposition is "educated" by them, in the true sense of the word; that is to say, it is evoked earlier than it would otherwise have been. On these grounds, I ascribe the persistence of habits that date from the early periods of home education, to the peculiarities of the instructors, rather than to the period when the instruction was given. The marks left on the memory by the instructions of a foster-mother are soon spunged clean away. Consider the history of the cuckoo, which is reared exclusively by foster-mothers. It is probable that nearly every young cuckoo, during a series of many hundred generations, has been brought up in a family whose language is a chirp and a twitter. But the cuckoo cannot or will not adopt that language, or any other of the habits of its foster-parents. It leaves its birthplace as soon as it is able, and finds out its own kith and kin, and identifies itself henceforth with them. So completely is its change of life carried out, and so utterly are its earliest instructions in an alien bird-language neglected, that the note of the cuckoo tribe is singularly correct. Mr. Romanes tells me that he has compared the cuckoo's note with a tuning-fork, at home and abroad, and has found it to be identically the same in both cases.

Much might finally be said in qualification of the broad conclusions to which we have arrived, as to certain points in which education appears to create a permanent effect, partly by training the intellect, and partly by subjecting the boy to a higher or lower tone of public opinion; but this is foreign to my immediate object. The latter has been to show broadly, and, I trust, convincingly, that statistical estimation of natural gifts by a comparison of successes in life, is not open to the objection stated at the beginning of this memoir. We have only to take reasonable care in selecting our statistics, and then we may safely ignore the many small differences in nurture which are sure to have characterised each individual case.

Twins (2)

2

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Short Notes on HEREDITY, *&c., in* TWINS. By FRANCIS GALTON, F.R.S. [With Woodcut.]

SOME subsidiary results that I obtained in an inquiry into the resemblances between twins, are perhaps worth recording as a separate memoir. My primary results were published in *Fraser's Magazine*, November, 1875, and are reprinted, with revision, among the miscellanies in this volume.

What I am now going to give, concerns the hereditary tendency towards twin-bearing, the largeness of the families in which twins are born, the degree in which they themselves contribute to the population, and the conditions of their sex.

First, in respect to heredity. It was impracticable to judge of this from my returns by any direct method. Twins do not marry so frequently as other people, and I think they are less fertile; hence the parents of twins, who are themselves one of a pair of twins, are relatively few, and the numerical ratio between such parents and the parents of twins generally, would be a fallacious test. Neither could I institute a direct comparison between two groups of children, one of whom were the offspring of fathers or mothers who themselves were of twin birth, and the other were not, because my material was insufficient. I therefore have confined myself to data derived from uncles and aunts.

I find with regard to 94 cases of twins, of whom I have sufficiently full returns, that they had a total of 1,065 uncles and aunts, and that among these there were 27 sets of twins.* In other words, there were twice 27, or 54 persons, who were severally one of a pair of twins among the 1,065 uncles and aunts—say 1 in every 20.

In the population generally the proportion is not nearly so great, but it varies largely under different conditions; and I therefore prefer to compare my returns with those derived from parallel returns supplied by precisely the same classes, which have been drawn up by Mr. C. Ansell, junr., in his most valuable "Statistics of Families of the Upper and Professional Classes of England." It was compiled at the cost and under the direction of the National Life Assurance Society, and leaves nothing to be desired in its completeness, terseness, and adequacy. From these we learn that there is 1 twin birth to

* To save complexity, I include among these, three cases in which the parent was one of the twins.

about every 100 ordinary births; in other words, there are 2 persons, each severally a twin, among every 101 persons—say 1 in every 50.

Hence the chance of an uncle or aunt of a twin being himself or herself a twin, is as 50 to 20, or $2\frac{1}{2}$ times as great as that of people generally. It may perhaps be thought simpler to state the result in this form:—Among the uncles and aunts of twins, there is an excess per cent. of three individuals of twin birth, due to hereditary causes. The average influence of heredity in fathers and in sons may be taken as fully five times as great as that in uncles and aunts; we should therefore expect, on general grounds, that the former would yield an excess of at least 15 per cent., or an absolute number of $15+2=17$ per cent. of individuals who were twins; but this, I feel sure, is in excess of the truth. (I have discussed these hereditary ratios, so far as ability was concerned, in my "Hereditary Genius," p. 321.)

Next, as regards the relative power of the male and female in transmitting an hereditary tendency to bear twins. I find that the 94 sets of twins above mentioned had—

On the father's side	538 uncles and aunts,
Among whom were ..	14 sets of twins.
On the mother's side	527 uncles and aunts,
Among whom were ..	13 sets of twins.

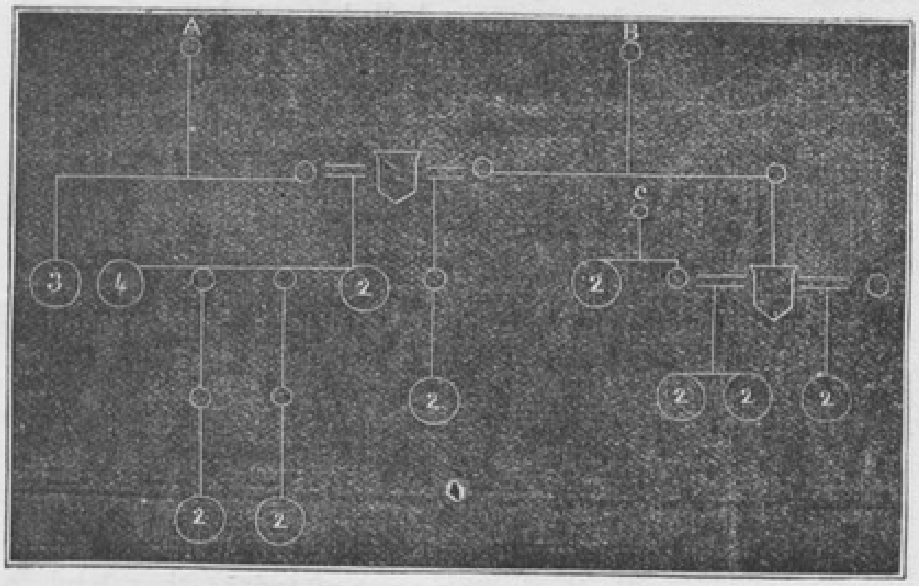
These numbers may be considered identical, in a statistical sense; hence the hereditary tendency is the same in the male and female lines.

The largeness of the families in which twins are born is sufficiently manifest from these returns, which happen to be the only ones I possess that can be adduced in proof of it. We see that 94 sets of twins had, on the father's side, a total of 538 uncles and aunts, which, added to the 94 fathers, makes 632 individuals in 94 families; this is at the rate of $6\frac{3}{4}$ in each of the families of which the father of a twin was a member. Almost the same occurs (the precise figure is $6\frac{1}{2}$) in each family of which the mother of a twin was a member.

I annex a curious instance of the intermarriage of three twin-bearing families, A, B, and C. They all consist of many individuals; but my information is a little imperfect, and even if it were not, it would hardly be necessary to give more details than the number of multiple births and the connecting links between them. I may add that the three families are well known socially, and have each of them distinguished members. The circles in the diagram indicate the cases of multiple births, and the figures inside the circles show the number of children born on each of these occasions. It will be observed that in three generations there were produced one quadruplet, one triplet, and eight pairs of twins.

A very instructive fact has been related to me concerning a family remarkable for its twins; in which it appears, according to the mother's account, that whenever single children were born to her, they always had six fingers and six toes, but the sets of twins never had. This shows a strong constitutional tendency to multiple propagation.

The vigour of body and mind of those twins who survive infancy, who strongly resemble one another, and who have sent me returns, is certainly not below the average. On the contrary, I find, from the returns that I have received, that nearly one-half of them are decidedly above par; and thence I infer that more than one-half are somewhat above par. It is easy to adduce instances of vigorous twins. One of my own cor-



respondents, a twin, was a senior wrangler; Lords Eldon and Stowell had each a twin sister; and among others who have successfully fought the battles of life may be mentioned Bendigo, the ex-champion pugilist, who was one of a triplet birth.

Notwithstanding Sir J. Simpson's statistical results,* I still think the popular belief to be a true one, that twins contribute less to the population than other people. My returns were not framed to afford a direct answer to the question of their fertility; but I can incidentally gather enough from them to be sure of the fact; also that there is not so strong a tendency among twins to marry, as among other people (however this may be accounted

* Obstetrical Memories, i. p. 327.

for); and lastly, that the popular belief that both twins, whether of the same or opposite sexes, *never* have children, is erroneous, for I have many instances to the contrary.

There is nothing known in the human race, except as a great rarity, corresponding to the "free-martin" * in cattle; and where known, it has never yet been found, so far as I am aware, in connection with twin births. Neither is this peculiarity of neutral sex found in such domestic animals as dogs or cats, except in the rarest instances; but in the horse, ass, and sheep, and especially in cattle, it is comparatively common.

John Hunter's "Memoir on the Free-Martin" (vol. iv. p. 34, edition of 1837) is extremely curious. It appears that when a cow (he says he can only speak of black cattle, but I understand it is a more general fact) brings forth two calves, one of which is a bull calf and the other, to external appearance, a cow calf, the former grows up into a proper bull, but the latter does not commonly grow into a proper cow. The animal is unfit for propagation, and is kept for labour and fattening, like an ox; and it is as well known as a specific form of cattle as is the bull or cow, and is called a "free-martin" by farmers. Close examination and dissection show that the animal is neither a complete female nor male, but combines the anatomical characteristics of both in a very undeveloped and imperfect manner; and those of the male rather predominate over those of the female. This, at least, is the modern view. Hunter's three dissections of free-martins still exist in the Museum of the Royal College of Surgeons, in the teratological division. (See the catalogue of it, pp. 97—101.) Sir J. Simpson subsequently investigated the subject. His principal memoir, alluded to above, is most interesting; and there are several other allusions to free-martins, and to writers upon them, to be found elsewhere in the two volumes of his memoirs.

There is a peculiarity in the sexes of twins; they tend to accord. The word "twin" covers different classes of events—those in which each twin is derived from a separate ovum, and those in which they come from two germinal spots in the same ovum. In the former case they are enveloped, previously to their birth, in separate membranes; and in the latter in the same membrane. Now it appears that twins enveloped in the same membrane are invariably of the same sex, and these,

* Marten seems originally to have meant an animal intended to be killed at Martinmas, which was the period in former years, before the introduction of root-crops, when cattle were slaughtered and salted down for the winter's food of the population. As barren cows were slaughtered preferably to others, the name of marten became especially applied to them. Why the animals about which I have been speaking were called free-martens, it is not altogether clear. Free might mean "naturally admitted to the privilege" of being slaughtered at Martinmas.

according to the cases of Späeth, who has evidently taken great pains to secure reliable data,* are 24 per cent. of the whole number. (This is, however, greatly in excess of other estimates, which usually give about 6 per cent.) In the remainder they have either one placenta between them, and two membranes, or else they are quite independent, and have separate placentas and membranes. The statistics as to members and sexes under these conditions, vary so astonishingly that I can conclude nothing concerning them. The general upshot is, that about twice as many twins are born of the same sex as of opposite sexes; whereas if there were no influences to produce accord, and on the supposition of an equality of male and female births generally, the numbers ought to be equal.†

I have explained in my memoir above alluded to (reprinted in the miscellanies of this volume from *Fraser's Magazine*), that it is only among twins of the same sex, and therefore presumably only among twins derived from the same ovum, that we find an extremely close likeness, or else an extremely marked dissimilarity. On the other hand, in twins of the opposite sex, we find only an ordinary family likeness or dissimilarity. To this I shall recur in my Memoir on the Theory of Heredity that is about to be read, and I will now conclude the present one.

* "Studien uber Zwillingen." Zeitschrift der Wiener Gesellschaft der Aerzte, 1860. Nos. 15 and 16.

† This is clear, as was pointed out by Mr. C. Ansell, from the following considerations. Supposing absolute independence of sex, the births may be: (1) boy and boy; (2) boy and girl; (3) girl and boy; (4) girl and girl. All these events would in the supposition be equally likely, and they give two cases of the same, and two of opposite sexes.

K.4

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HINTS TO TRAVELLERS.

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1854

HINTS TO TRAVELLERS;

CONTAINING

1. Report of Sub-Committee of the Royal Geographical Society, consisting of Capt. R. FitzRoy, R.N., and Henry Raper, Esq., R.N.; also
2. Papers by Rear-Admiral W. H. Smyth;
3. Rear-Admiral F. W. Beechey;
4. Lieut.-Col. W. H. Sykes;
5. Francis Galton, Esq.;
6. Hints, etc.

Preface.

[Applications are frequently made to the Council of the Royal Geographical Society by travellers about to set out for imperfectly known countries for instructions by which they may make their labours useful to geography. When a traveller addresses a specific question relating to a local matter, or some particular point of equipment as regards instruments of observation, it will generally be easy to refer him to some member of the Society whose experience may enable him to afford a satisfactory answer; but when he puts his question in a general form, it is extremely difficult for the Council to return a suitable reply.

It seems a natural solution of the difficulty that the Council should themselves draw up a body of instructions to meet such occasions. But some insuperable objections very speedily suggest themselves. A complete system of instructions adapted to general application would embrace every point which could present itself to the notice of the accomplished traveller, and such a work would be an encyclopædia. On the other hand, a few general remarks of an elementary nature would be superfluous to an individual of moderate attainments, while it could not possibly impart the necessary qualifications to one who had no other knowledge or experience of the subject. Again, the nature of the observations which a traveller may make must depend on the character and quality of the instruments he carries—that is, on the quantity of baggage which it may be convenient or safe to carry with him, and therefore on his personal resources. It is evidently as impossible to treat all such particulars to any useful purpose, as it would be to prescribe the equipment suited to the various unexplored regions of the world. But this is not all; differences prevail amongst experienced travellers themselves, not merely as to details of observations, the degree of accuracy which it is advisable to aim at, and other matters, but as to whether particular instruments should be carried or not.

On these grounds the Committee do not think it advisable to undertake the formal publication of instructions for travellers; nevertheless, as many valuable suggestions have been made by scientific men on the occasion of these applications, which they are desirous should be made accessible to travellers in quest of information on particular points, they have recommended the publication of certain papers which follow, with the names of the authors.]

Hints to Travellers.

WITH respect to such applications for instructions, it may be advisable to reply in the first instance with reference to special enterprises, and afterwards to compile more extended and detailed information for the use of travellers generally—if, indeed, so wide and comprehensive a range should be deemed within the province of the Royal Geographical Society.

Some inquiries refer more particularly to the case of "a traveller who proposes to visit really wild countries," and "to lay down a useful map of his journey."

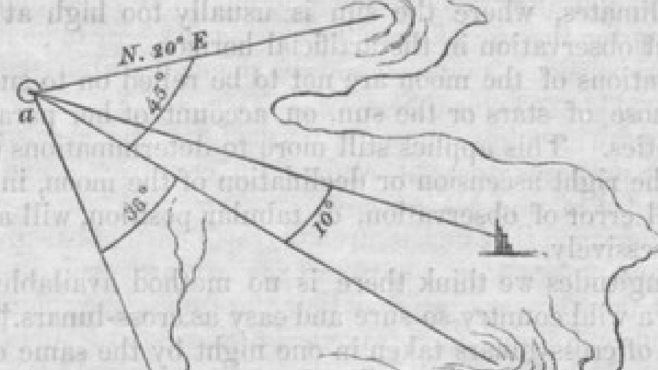
It is understood that he has already travelled, and has given proofs of his acquaintance with the use of several necessary instruments. Were not this the case, we fear that the fullest instructions would hardly suffice to give any traveller possession of such practical skill as should be acquired experimentally.

We think that the following list of instruments will be found sufficient for the traveller's purposes, and not too cumbersome or difficult to carry:—

A sextant, horizon, pocket-sextant, Kater's compass, Rochon's micrometer, and a sympiesometer, two pocket-chronometers, two thermometers, two portable barometers, two aneroids, and two boiling thermometers. It would be very desirable to carry a second sextant or circle, an additional horizon, and another prismatic compass, in case of accidents. Writing and drawing materials, stationery, scales, tapes, and register-books, should be carried in convenient cases—water-tight, if possible. With these, or even a part of these materials, a complete map may be laid down.

We consider the sextant (or circle) and horizon to be an efficient and reliable observatory for travellers, when accompanied by two or three chronometers. With such simple means there is far less risk of error than in using instruments of higher pretensions and more complicated structure.*

To lay down a useful map is an easier task than usually supposed, if correct principles be adopted and carefully followed in practice. A field-book (angle or bearing-book) should be always at hand, in which every particular relating to the direction travelled (or course), the distances, times, angles, bearings, and observations, should be noted on the spot and as they occur, as far as may be practicable: the less left to memory the better. Descriptions should be written with the objects *in view.* Times of occurrences, changes of course, and other data, should be noted as often as possible; but that which is subsequently found invaluable when laying down the work permanently, is a collection of sketches of the country passed over, in *plan*, with a partial mixture of profile views, on which the angles observed or lines of bearing are traced by hand, with their corresponding figures written along them or across the angles, thus:—



By noting the angles and bearings on the *plan*, as well as in regular columns, in the field (or bearing, or angle) book, and inserting as many profile views, half-plan half-profile sketches, and horizontal plans, in the book as time will allow, an immense amount of perplexity will be prevented, and increase of accuracy will be ensured.

* Raper's Navigation is a storehouse of information, not only on the practical use of instruments, but on the various methods of computing or reducing the observations by easy compendious calculations (suitable for the traveller on a journey, or the seaman in a gale); as well as by the longer and exactly accurate computations. Neither seaman nor traveller ought to leave England without the *latest* edition of this valuable work.—R. F.

Such plans as these are so many sketch maps made on the spot, from which accurate compilations may afterwards be made with comparative ease. In laying down or connecting points trigonometrically between the stations that are determined astronomically, true bearings and angles by reflecting instruments should be preferred to any use of the compass, which, however valuable as an auxiliary to fill in minor details, is not to be relied on in all places, and is apt to get out of order in consequence of its centre wearing by friction, as well as from other causes not always self-evident.

By fixing principal points astronomically, using trigonometrical connection between them, and filling in minor details by angles, bearings, and eye-sketches, it is surprising how much work may be done in a short time by a practised traveller.

For latitudes, besides the ordinary meridian or circum-meridional observations, single, double, or equal altitudes will be useful, the TIME being always obtained as accurately as may be.

Cross-altitudes* of stars are excellent, and easily obtained in tropical climates, where the sun is usually too high at noon for convenient observation in the artificial horizon.

Observations of the moon are not to be relied on to such accuracy as those of stars or the sun, on account of her parallax and irregularities. This applies still more to determinations of longitude by the right ascension or declination of the moon, in which a very small error of observation, or tabular position, will affect the result excessively.

For longitudes we think there is no method available for travellers in a wild country so sure and easy as cross-lunars.† A few good sets of cross-lunars taken in one night by the same observer, with the same instrument, will give the longitude within three miles of the truth. This is stated *confidently* as the result of experience. The altitudes must be calculated, for which the *time* must, of course, be known *nearly*. Differences of time by chronometers suffice for intermediate distances, and are, within such limits, most satisfactory. But in a country where there are good marks well defined, accurate meridian distances may be obtained by good differences of latitude and true bearings between points connected trigonometrically.

When equal altitudes are not obtained for time (with a view to ascertaining the error and rate of chronometers, or difference of longitude), absolute (single or independent) altitudes may

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* By cross-altitudes we mean observations of stars on opposite sides of the zenith, and nearly equidistant from it.

† Observations of the moon's distance from stars, nearly equidistant, east and west.—R. F.

be observed; and it should be remembered that those taken before noon should be compared with other forenoon sights, and those taken afternoon with other afternoon sights, in deducing the difference of time between places from their results.

For base lines, the more rapid methods of attainment are alone suitable to the present object. No measure is more accurate and speedy than that obtained by meridian altitudes of the same heavenly body (sun or star, *not* the moon) at different stations, by the same observer, with the same instruments. If the stations are in the true meridian, or nearly so, their difference of latitude is their distance, near enough for our traveller's purpose; and if they are otherwise situated, their true bearing, obtained by an azimuthal observation of the sun or a star, and their difference of latitude, give the true meridian distance or difference of longitude between them, as well as their direct distance, the required base line.

If for particular purposes, such as a local plan, a short base line be required, an accurately measured board or pole, and a Rochon micrometer, or a sextant, will suffice. A short base may be measured first, two poles erected, and then a longer space ascertained by angular measurement. Field-work should be laid down on a moderately large scale, from a quarter of an inch to an inch, and for particular plans even several inches to a mile.

Magnetic observations, geological researches, mineralogy, zoology, botany, and ethnology, require the assistance of competent authorities on those subjects.

Your Sub-Committee will now add a few brief remarks on the instruments they have recommended.

Beautiful as instruments of a higher class than sextants and reflecting circles seem to be in theory, and are, when fixed securely and properly, in practice;—the great difficulty of finding a secure foundation for them to stand on, of adjusting them accurately (under the ordinary circumstances of a traveller), and of maintaining their exact adjustment while the observer is moving round them on, perhaps, unsound ground, are reasons which have induced us to think it unadvisable to recommend them to hurried travellers, who are not such adepts in their use as practised astronomers, and who might easily overlook an error of adjustment that would vitiate a whole set of observations.

Unless the results that are carefully recorded be those of observations made on correct principles, with instruments sufficiently accurate, practically as well as theoretically, all the time and pains they have cost are thrown away; and these consequences have been witnessed too frequently where transit, or azimuth and altitude, or other instruments *on stands* were employed.

It should be borne in mind that travellers cannot attempt to attain perfect accuracy in their observations made during a first exploration. It is not the fraction of a second of time, nor even the fraction of a mile of latitude that is required, but the degree and nearest minute.

All instruments should be in leather or canvas cases, painted *white*, slung by straps, and "becketed" * sufficiently.

Barometers and other glass instruments are better carried on men's backs (with the *upper* ends down) than in any other manner, but their bearers must of course be very careful.

If possible, chronometers should be worn *night and day*, to ensure uniform temperature and care. Motion affects chronometers far less than change of temperature.

The reflecting circle is an instrument highly esteemed (especially by the French), but it cannot measure an arc larger than 140° satisfactorily, on account of the extreme inclination of the index to the horizon glass. It is heavier and more cumbersome than a sextant of equal radius, and is more difficult to handle.†

A sextant with a doubly graduated arc and an additional horizon glass, will measure 160° satisfactorily.

When the sun is more than 80° high, his altitude cannot be taken in an artificial horizon, because the head of the observer obstructs the sun's rays while endeavouring to observe the altitude.

The roof of the artificial horizon should be portable, fitted to fold together, and, whenever used, should be reversed from time to time, so that half the altitudes should be taken with one end of the roof towards the observer, and the rest with the other. The trough should be smaller than usual, and raised in the roof by a thick bottom, so that an angle may be taken near the horizon. The mercury should be level with the edges of the trough, so that any dross may be scraped off by a piece of paper, or a *thin strip of wood*, kept in the box. Having the quicksilver level with the edges of the trough facilitates observing a low altitude. The mercury may be kept in a wooden, or in an iron bottle, with a screw-stopper and cap-funnel. To prevent spilling, the trough should have a notch at one corner *inside*, and be cut away *underneath*. The bottle should hold more mercury than will fill the trough, and a spare bottle should be carried. If all the mercury should be lost, coloured water will do nearly as well (dark or inky water). The horizon trough and roof should be placed on a flat plate of metal standing on three knobs. This will at once give a place for the horizon where the ground is uneven, wet,

* Having loops or eyes of leather or cord at the sides or corners for slinging or fastening them, when travelling.

† The *repeating* reflecting circle is much praised by Col. Sabine. *See* also article upon it in the Penny Cyclopædia, under "*Sextant*."—Ed.

grassy, or soft. A leaden plate, about the size of a thin octavo book, with three knobs an inch long underneath, is suitable, because its weight steadies it among grass or in soft places.

The observer should endeavour to sit down on the ground, as near the artificial horizon as he can, in order to steady his arms and body, and avoid being disturbed by any wind. Cross-legged, with the elbows steadied on the knees, is a position as firm as can be maintained.

Rochon's micrometer is frequently useful as a telescope, carried in a case on the back.

Barometers might be made, for the special use of exploring travellers, in a simple manner. The tube should be unconnected permanently with the cistern, open at both ends, but capable of being hermetically closed temporarily at one end; it should be large, strong, and graduated on the glass. The cistern should be capable of admitting the tube when required, which should then be supported by a rod of iron screwed to the cistern, and steadying the tube by an arm with a clamp ring. There should be a float in the cistern, and, when not in use, the apertures for the float and tube should be closed by screws. The cistern should be of iron. The mercury should be clean, and as pure as possible, but not boiled.

Such a barometer might not give results *strictly* accurate, as independent measurements of pressure, but if filled carefully so as to exclude visible air-bubbles, duly compared with good standard instruments, and the temperature of the mercury, as well as that of the atmosphere, properly noted at each observation, it is believed that it would give valuable comparative results.

Several tubes might be carried in one strong case, *with the baggage*, and a spare one in a light metal case, to the place of observation.

HENRY RAPER,
ROBERT FITZROY, } *Sub-Committee.*

Extracts from a Letter by Rear-Admiral Smyth, addressed to the Secretary.

THE first duty of a geographical traveller is the accurate determination of the route, stations, and topography of his journey; and the fewer instruments he is encumbered with the better will they be worked. Skilful mapping of regions little known is an actual boon to science, especially when accompanied by the observations of an intelligent mind. The general elevation of the countries passed over is ever a co-ordinate of importance, since much physical information may be derived therefrom as to the nature of production, modified by geographical position and consequent climate. Such are the appropriate ends of scientific travellers: but it should ever be borne in mind that quality, and not quantity, is the true end.

For he who explores Africa I may, from experience, advise that no accurate part of his instruments—as for instance the barometer-case—be made of wood; and everything should be exposed to a high heat before leaving London, perhaps equal to 120° in the shade, and 160° in the sun. No levels nor thermometers that will not stand this ordeal should be taken. The instruments should be few and simple, light and well-packed in non-conducting cases; and a portmanteau full of clothing is the very best packing-case for levels and thermometers, since the extremes of temperature will not be experienced therein. Thus, in a tent with the temperature of the day at 95°, a thermometer in a portmanteau was at 60° only.

An African traveller needs a pocket prismatic azimuth compass, with which he must take rounds of angles—including the sun—at all his stopping places, and on the road also if opportunity offers; but his best instruments should only be unpacked at crucial stations. He should have the most improved measuring tapes, and every opportunity must be taken of multiplying measurements of length for base-lines for filling in between standard positions by what may be deemed *dead-reckoning*. Still the most important object is an efficient angular instrument for fixing the latitude and longitude, without which the other work is comparatively useless. I alluded to this subject in my last Address to the Society, and have little to add unless a person were under drill. Latitudes, of course, will be measured by altitudes of the sun or stars in the meridian; but, for the determinations of longitude, more practice and attention will be required. Small differences may be measured by a chronometer, yet cannot be trusted, as the going-rate in African travelling is mostly useless: the capital measures made by Admiral Beechey, with two pocket chronometers, when a Lieutenant of the Adventure under my command, round the shores of the greater Syrtis, form a solitary exception. Absolute determinations by the moon, as I have said, must be looked to, not lunar distances—so often mischievous under unpractised hands—but altitudes of the moon and star, getting the right ascension when out of the meridian, or the declination when in the meridian. As to the favourite plan of observing the eclipses of the satellites of Jupiter, besides the uncertainty of instant, the method involves the necessity of carrying a telescope of power, and obtaining corresponding observations.

This is what is to be done; now how should we do it? A sextant is the most portable of instruments; but you must add thereto an artificial horizon, a heavy, lumbering thing, if good, and if of the best kind, namely, quicksilver, the fluid will assuredly be spilt and lost in the sand, besides some getting out and damaging the instrument. Then again, unpractised persons require a stand for the horizon, or the instrument, or for both; and at best you can only thus measure altitudes above and below a certain height, and so lose a great part of the sky, and that very part in which the sun is placed in Central Africa. A portable altitude-and-azimuth instrument, with its stand, would therefore be necessary to good work.

Now such a complication is adverse to the ends of the mere explorer, and therefore it is with equal pleasure and conviction that I recommend the Universal Instrument proposed by my son the Astronomer Royal of Scotland, who has had great experience of these matters in South Africa, during the late mensuration of an arc of the meridian in that country. It is a sextant when used for hand-work; or, if fastened on its stand, will measure any altitude and azimuth angles, give meridian transits on five wires, and the like, more conveniently and quickly than by the usual theodolites and other graduated instruments in use. It is, moreover, simple in construction, inexpensive, and easy to use; and a three-legged stand, for general purposes, is always readily carried, as it requires no care.

It remains to notice the determination of heights, independent of the troubles of levelling or trigonometry. The most accurate of the secondary methods in practice is, of course, by means of the barometer; but the difficulty of transporting this instrument (in my own opinion rather exaggerated) has been so greatly complained of by travellers, that mechanicians have turned their attention to the subject; and miniature barometers, sympiesometers, aneroids, and other substitutes have been the consequence. Yet such are the discrepancies of zero and scale, that, except the improved Adie's sympiesometer, the boiling thermometer is better than either of them, as keeping its zero constantly. But that method, though affording comparative ranges for a traveller in a new and wild region, is liable to very serious errors, particularly from the boiling taking place under unequal pressure when the steam is confined; nor is it easy to boil properly. The apparatus which I used was made expressly for me by M. Drescler at Palermo, in the year 1813, and consisted of a cylindrical pot, with a lamp appendage for boiling the water; and the thermometers were so mounted that half the scale could be turned up the back by a hinge, leaving only the bulb and part of the stem immersed in the hot water. This is the instrument mentioned in my account of Mount Etna (*Sicily and its Islands*, p. 145); and it is also described in Baron de Zach's *Correspondance Astronomique et Géographique*. Various experiments were made with it during my operations in the Mediterranean, merely, however, in comparison with the trigonometrical or barometric points.

But even in this boiling-point process, though on the whole so eligible, there is weight to carry, and trouble to take; and as, in addition to the importance of very frequent observations to get at the section of a country, readings of the barometer and thermometer will be absolutely necessary as an appendage to the astronomical work — it is my own opinion, that a small Adie's sympiesometer is the best instrument for an exploring traveller; and I may further say, that though a chronometer may undeniably be useful, still it is not importantly so, because absolute longitudes by the moon should always be aimed at: a mere seconds' watch will therefore do for intermediate work.

Extracts from a Letter by Rear-Admiral Beechey, addressed to the Secretary.

I BELIEVE that arcs by chronometer may be accurately measured, but with great care, and you may remember that some were done with excellent results, on my plan, by Dr. Colthurst, who went to the Colorado through Sonora. But a track by a pocket compass, well timed and filled into a form of this description, would tell a very good tale if checked by meridian altitudes and observations at the beginning and the end of the day:—

Date.	Place.	Time.	Bearing forward Track.	Bearing back Track.	Bearing of remarkable Objects.	Estimated Distance.

I do not think a transit instrument calculated for an exploring expedition. If the traveller has a caravan, and is going to remain a fortnight or more at a place, and then proceeds on again with the same means of transporting his baggage and instruments, a transit might be taken, as it could be fixed on a stand sufficiently stable for the purpose. But I should have no faith in moon culminations with a transit on a tripod: I would by far rather trust to lunars with a six-inch sextant. You may perhaps recollect that I have had a good deal of experience both with portable transits, and with lunars: a transit involves some kind of an observatory, and a meridian line; to have any results which are satisfactory with a transit, there must be convenience for observation, without letting in the wind or sand, and there must be stability in the instrument. Poor Ritchie took a transit, but I found it at Tripoli after his death, with many valuable scientific instruments, which he procured in this country and at Paris, and probably never used.

I should strongly recommend the sextant being fitted with a telescope with a horizontal wire in the focus of the field-glass, adjusted to a level on the top, after the manner of levelling instruments; for when so fitted and screwed to a stand, altitudes of hills and low altitudes of stars or other objects may be very accurately observed, and, with a cross level, altitudes of any elevation may be approximately determined; or a small collimator, such as I supplied Sir E. Belcher with, might be hooked on opposite the horizon-glass, which would answer the same purpose; or if loose, and placed on a wall or large stone, it would answer all the purpose of an artificial horizon.

Principal Instruments for the Track.

A six-inch sextant on a plan which will measure accurately *any angle.*

A double pocket sextant.

A good achromatic telescope fit for observing satellites of Jupiter and an occultation.

A prismatic azimuth compass and spare card and pin.

An artificial horizon.

Two pocket chronometers.

For meteorological purposes—Thermometers (small), barometer aneroid.

Pocket compass, for the track, to be hung round the neck and consulted at every turn of the road.

I do not think a Rochon of much use on such a journey; a sextant will measure with considerable accuracy small bases. I tried both in my survey of the Severn, and had the means of detecting every measurement; and I do not think Rochon so superior as to recommend it to a traveller about to explore a country. The longitudes of the principal places may always be determined with sufficient accuracy by lunars, E. and W., with a sextant. Jupiter's satellites, occultations and eclipses, should always be observed when practicable, as being valuable adjuncts.

The track between these places may be kept by D. R., by observing the course and the time of starting and of arrival at each place and at each turn in the road, and especially of final arrival at resting-place at night. The rate of travelling with camels or horses may be ascertained with considerable accuracy,* and any error in the track may be corrected by observations made before starting and after arrival at night.

The traveller should rigidly adhere to the rule of getting morning sights for chronometer before he starts from any place, and evening sights when he alights for the night; as the chronometer will thus be checked in its rate, which is not the same when travelling during the day as when resting during the night. He will therefore have to observe at every place, both evening and morning, for his chronometer, and as many more altitudes of stars as he thinks necessary for his latitude. This is tedious after a hot, fagging day's journey, but I always did it, and found the greatest satisfaction from it. He should, as before said, take his course and time throughout the day, and at noon alight and get the meridian altitude—connecting it with this track and with bearings of distant objects.

He should have a square book, ruled in inch or half-inch squares, to mark down the track and to delineate the topography as he proceeds. One set of lines should always be considered the magnetic meridian, and the track should be laid down upon it as the traveller proceeds, and as nearly in the right direction with respect to the meridian as possible for the eye to draw it. The squares should be considered as miles of distance, each square representing one mile, and all places be put down according to their estimated distances in their proper positions. A little practice will familiarize the traveller with this species of plotting, and the more careful he is in marking down objects the greater will be his satisfaction at the end of the day.

At night before he goes to rest he should look over his D. R. to run it out fair, and enter it at once in its proper column in his day's-work book. Bearings of remarkable points should be carried on from time to time as long as the objects can be seen, as they afford an excellent check to the track and a correcting point for the surrounding topography. These bearings should be particularly observed at the great turnings of the track-path, and astronomical bearing should be constantly resorted to. The azimuth may be computed from the time by chronometer, which will save the double observation.

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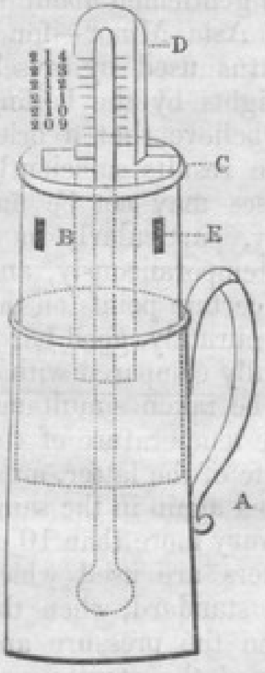
* I found the average rate of laden camels to be 2¼ miles per hour; they will walk considerably faster than this, but the difference is lost in the many little bends in the road which cannot be taken into account in plotting the track. The nature of the road will also vary their speed; a little experience will enable the traveller to estimate this correctly.—*See* my African journey from Tripoli to Egypt through Cyrene.

On the Use of common Thermometers to determine Heights.

[Extracted from the Paper by Lieut.-Col. W. H. Sykes, in Vol. VIII. of the Journal of the Royal Geographical Society.]

HAVING been recently applied to by two gentlemen about to travel—the one in Africa, and the other in Asia Minor—for a description of the thermometers and apparatus used by myself for some years in India for determining heights by the boiling temperature of water, I have ventured to believe that a brief account of a process which I found to produce results sufficiently near to the truth for most practical purposes may not be unacceptable to some members of the Society, particularly as I carried on my barometrical observations contemporaneously, and thereby obtained data for fixing the value of certain points on the thermometric scale. To determine heights accurately, good barometers are necessary, which have been carefully compared with a standard barometer; the observations must be taken simultaneously at the upper and lower stations, and the temperature of the mercury and the air, and the hygrometric state of the latter, must be noted. Heights so determined, when tested again in the same or succeeding years, I have rarely found to vary more than 10 or 20 feet in 4000 or 5000. When barometers are used which have not been previously compared with a standard, when the observations are not simultaneous, and when the pressure and temperature at the level of the sea are *assumed*, the results may by accident be near to the truth, but they will usually be from 100 to 300 feet wrong—at least such is the result of my experience within the tropics. But good barometers are very costly; they are troublesome to carry, are particularly exposed to accident on a journey, and get out of order by the escape of the mercury, which being frequently unobserved, the barometer continues to be used as if it were correct. The late Archdeacon Wollaston, aware of these facts, invented the thermometric barometer to supply the place of the ordinary barometer. This instrument is very sensible, but it is very fragile from the great weight of the bulb compared with the slenderness of the stem: moreover, there are some complex accompaniments, and the instrument is also expensive. In short, I found it not fit for *rough work* out-of-doors, having had three destroyed at the outset of my labours; and the same opinion is expressed by Mr. James Prinsep, of Calcutta, who is well known for the practical application of his scientific knowledge. I had then recourse to common thermometers, and, with certain precautions in their use, found them answer my purpose sufficiently well. A tin shaving-pot was my boiler; dry sticks and pure water were usually to be had, and by the time my barometers were settled I was ready to take the boiling temperature. The following is a sketch of the apparatus.

It will be seen that the chief part of the scale usually attached to the thermometer is removed, only so much of it being left as may be desirable: I, however, permitted the brass scale of one of my thermometers to remain, and I did not discover that it was the cause of error. Previously to taking the thermometers inland, it is necessary to ascertain their boiling points at the level of the sea; for in many instances the scales are so carelessly applied that a thermometer may indicate a boiling temperature of 213°, 214°, or 215°, at the level of the sea: one of mine stood at 214·2 when water boiled. Nevertheless, by making a deduction of 2° 2′ in all observations, the indications rarely differed five-hundredths of a degree from the other thermometer, of which the boiling point was 212°: the temperature of the air and the height of the barometer at the time the *verification* of the thermometers is made must be noted. The following is the manner in which my observations were taken:—from 4 to 5 inches of *pure* water were put into the tin pot; the thermometer was fitted into the aperture in the lid of the sliding-tube by means of a collar of cork; the tin tube was then pushed up or down to admit of the bulb of the thermometer being about *two inches* above the bottom of the pot. Violent ebullition was continued for ten minutes or a quarter of an hour, and the height of the mercury was repeatedly ascertained during that time, and the temperature of the air was noticed. Similar operations were repeated with a *second* thermometer; for it is never safe to rely upon *one* instrument. Having obtained the boiling points, it remains to determine the value of the indication of diminished pressure when the observations are taken above the level of the sea. The elastic tension of steam at different points on the thermometric scale has been determined by experiment, but not at regular intervals on the scale, nor with similar results, by different persons; tables,



A, A common tin pot, 9 inches high by 4 in diameter.

B, A sliding tube of tin moving up and down in the pot; the head of the tube is closed, but has a slit in it, C, to admit of the thermometer passing through a collar of cork which shuts up the slit where the thermometer is placed.

D, Thermometer, with so much of the scale left only as may be desirable.

E, Holes for the escape of steam.

therefore, computed from the formulæ of the various experimenters, do not accord; but, in three tables (by Mr. Prinsep, Lieut. Robinson, and one by an anonymous author in the Madras Gazetteer for 1824) which I have in my possession, the neights computed by them, when compared with heights determined by corresponding barometrical observations with previously compared barometers (the only satisfactory way to ascertain heights not taken trigonometrically), approximate sufficiently near for all practical purposes where great accuracy is not desired. These tables, however, differ slightly from each other.

TABLE I.—To find the Barometric Pressure and Elevation corresponding to any observed Temperature of Boiling Water between 214° and 180°.

Boiling Point of Water.	Barometer Modified from Tredgold's Formula.	Logarithmic Differences or Fathoms.	Total Altitude in Feet from 30·00 in. or the Level of the Sea.	Value of each Degree in Feet of Altitude.	Proportional Part for One-tenth of a Degree.
°			Feet.	Feet.	Feet.
214	31·19		- 1013		..
213	30·59	00·84:3	507	- 504	..
212	30·00	84·5	0	- 507	..
211	29·42	84·9	+ 509	+ 509	51
210	28·85	85·2	1021	511	..
209	28·29	85·5	1534	513	..
208	27·73	85·8	2049	515	..
207	27·18	86·2	2566	517	52
206	26·64	86·6	3085	519	..
205	26·11	87·1	3607	522	..
204	25·59	87·5	4131	524	..
203	25·08	87·8	4657	526	..
202	24·58	88·1	5185	528	53
201	24·08	88·5	5716	531	..
200	23·59	88·9	6250	533	..
199	23·11	89·3	6786	536	..
198	22·64	89·7	7324	538	54
197	22·17	90·1	7864	541	..
196	21·71	90·5	8407	543	..
195	21·26	91·0	8953	546	..
194	20·82	91·4	9502	548	55
193	20·39	91·8	10053	551	..
192	19·96	92·2	10606	553	..
191	19·54	92·6	11161	556	..
190	19·13	93·0	11719	558	56
189	18·72	93·4	12280	560	..
188	18·32	93·8	12843	563	..
187	17·93	94·2	13408	565	57
186	17·54	94·8	13977	569	..
185	17·16	95·3	14548	572	58
184	16·79	95·9	15124	575	..
183	16·42	96·4	15702	578	..
182	16·06	96·9	16284	581	..
181	15·70	97·4	16868	584	..
180	15·35	97·9	17455	587	59

TABLE II.—Table of Multipliers to correct the Approximate Height for the Temperature of the Air.

Temperature of the Air.	Multiplier.	Temperature of the Air.	Multiplier.	Temperature of the Air.	Multiplier.
°		°		°	
32	1·000	52	1·042	72	1·083
33	1·002	53	1·044	73	1·085
34	1·004	54	1·046	74	1·087
35	1·006	55	1·048	75	1·089
36	1·008	56	1·050	76	1·091
37	1·010	57	1·052	77	1·094
38	1·012	58	1·054	78	1·096
39	1·015	59	1·056	79	1·098
40	1·017	60	1·058	80	1·100
41	1·019	61	1·060	81	1·102
42	1·021	62	1·062	82	1·104
43	1·023	63	1·064	83	1·106
44	1·025	64	1·066	84	1·108
45	1·027	65	1·069	85	1·110
46	1·029	66	1·071	86	1·112
47	1·031	67	1·073	87	1·114
48	1·033	68	1·075	88	1·116
49	1·035	69	1·077	89	1·118
50	1·037	70	1·079	90	1·121
51	1·039	71	1·081	91	1·123

Enter with the mean temperature of the stratum of air traversed, and multiply the approximate height by the number opposite, for the true Altitude.

My thermometers were not graduated to less than half-degrees, and long practice enabled me to determine the height of the mercury in the stem to one-twentieth of a degree; but I would recommend thermometers being used in which the degrees are graduated to fifths or tenths of a degree. On the whole, I think the results of six years' experience justify me in saying that common thermometers may be satisfactorily used to supply the place of barometers in measuring heights where great accuracy is not required; and it will be recollected that what is usually looked upon as a difficult and troublesome operation with barometers, will be attainable by any person who carries with him a couple of thermometers, the requisite tin pot, and the tables, and who is master of the simplest rules of arithmetic.

Of the three above-mentioned tables I have chosen Mr. Prinsep's to submit to the Society, from their perspicuity and the facilities they offer for the conversion of boiling temperatures into heights with very little trouble; but a glance over the figures in my tables of altitudes, published in the eighth volume of the Journal of the Royal Geographical Society, will show that the tables are susceptible of considerable improvement; for, with two exceptions, all the heights deduced from Mr. Prinsep's and Lieut.

Robinson's are much below those determined by simultaneous observations with good barometers; and I join with Mr. Prinsep in expressing a hope that every traveller boiling his thermometers will at the same time, if he possess a barometer, make a record of its indications, and thus render essential service to physics by fixing so many points on the scale of the elastic tension of steam at different temperatures.

When the thermometer has been boiled at the foot and at the summit of a mountain, nothing more is necessary than to deduct the number in the column of feet opposite the boiling point below from the same of the boiling point above: this gives an approximate height, to be multiplied by the number opposite the *mean* temperature of the air in Table II., for the correct altitude:—

	°	Feet.
Boiling point at summit of Hill Fort at Purúndhur, near Púna	204·2 =	4027
Boiling point at Hay Cottage, Púna	208·7 =	1690
Approximate height		2337
Temperature of the air above	75°	
Ditto ditto below	83	
Mean	79=Multiplier.	1·098
Correct altitude		2·566 ft.

When the boiling point at the upper station alone is observed, and for the lower the level of the sea, or the register of a distinct barometer is taken, then the barometric reading had better be converted into feet, by the usual method of subtracting its logarithm from 1·47712 (log. of 30 inches) and multiplying by ·0006, as the differences in the column of "*barometer*" vary more rapidly than those in the "*feet*" column.

	Feet.	Feet.
Example.—Boiling point at upper station	185° =	14,548
Barometer at Calcutta (at 32°) 29 in. 75°		
Logar. diff. = 1·47712 — 1·43749 = 00363 × 0006 =		218
Approximate height		14,330
Temperature, upper station 76°, Ditto lower 84°	80 = multiplier	1·100
Correct altitude		15,763

Assuming 30·00 inches as the average height of the barometer at the level of the sea (which is, however, too much), the altitude of the upper station is at once obtained by inspection of Table I., correcting for temperature of the stratum of air traversed by Table II.

[*Note.*—Dr. Hooker finds that the index error of boiling point thermometers is often more than 1° even in instruments supplied by the best makers.—(1854).

According to Mr. Adie, optician, Edinburgh, the index error is liable to change from some hidden cause amounting to nearly a degree in a few months.

See Dr. Buist's paper on the Aneroid, page 43, vol. xxi. Royal Geographical Journal.]

Ed.

Letter addressed by Francis Galton, Esq., to the Secretary.

Sir,—When a man, for the first time in his life, proposes to explore a wild country, he is sure to ask, "What astronomical and mapping instruments ought I to take with me, and how should I pack and carry them?" It therefore seems to be a very proper undertaking for persons who have already had to do with these things, to record their experience in answer to the above question. And, further, I am sure it would be of infinite service to young travellers if different lists of instruments, books and stationery were drawn up; each complete in itself, down to the minutest detail, so that a tyro having selected any one of them might straightway take it down to the different shops and order off hand his complete outfit. He would then be satisfied that he had omitted to provide himself with no object of real importance, that he had bought nothing superfluous, and that the different items of his store corresponded together in size, in power, and in their several uses.

Half-a-dozen or more different lists might be drawn up; they would vary according to the accuracy of the results aimed at, to the character of the observations intended to be made, and also, to a great extent, according to the fancy of the person who might draw up the list.

But a young traveller would never go far wrong who followed to the letter any one of these lists. His danger lies in following the advice of observers who have little experience of the bush, or else in adopting scattered hints from many sources, and starting with instruments which, though individually good, are, when considered as a set, incongruous and incomplete.

A rough estimate of prices might be added to these lists, and hints on packing and carrying them would be of great importance. It is a desideratum as yet unsupplied, to arrange one or more light strong cases fit for strapping on an animal's back, or on men's shoulders, readily to be opened and unpacked, which shall contain all the books and instruments that a traveller requires for his daily use.

Guided by these views I will proceed to describe an outfit based upon that which I used, which would suit an explorer in any part of the world, who desired the means of bringing back as good geographical determinations, generally speaking, as explorers over large tracts of land have ever yet succeeded in obtaining. And in the list that I am now about to draw up professedly for an inexperienced observer, simple and well known instruments shall only find a place. I am very far indeed from thinking that instrument makers have yet learned to meet the wants of land travellers, but as we *know* that good results may be obtained from such sextants, &c., as are to be bought at any optician's shop, I would urge a young explorer to make *these* his mainstay; and if he takes other instruments, to do so more for the purpose of testing and reporting on their performances, than of relying in entire confidence upon their suiting him.

Again, it is hazardous for a man hurriedly preparing himself for a journey, to order new apparatus from a maker; he can never be sure that it will be made nicely or punctually, and he may have to set sail in possession of a strangely shaped instrument—very delicate, difficult to pack—whose adjustments he has not had opportunity of mastering, and on which nobody out of England can give information; whilst if he determines on buying a sextant, he may make his selection out of great numbers that are always ready made to hand, and practise himself in its use, under the tuition of the officers of his ship, during the whole of his voyage from England. It will, therefore, be my present object to give a list of instruments which, though confessedly improvable in numerous points, will, all things considered, be what I should advise a traveller of but little experience to provide himself with, and which, beyond all doubt, are thoroughly adequate to do his work.

Outfit for an Explorer.

A Sextant—

A sextant of five-inch radius, light in weight, by a first-rate maker, divided clearly, and on platinum, to quarter degrees. It must have a ground-glass screen fixed in front of the reading-off lens to tone down a glaring light, and a coloured glass to screw on to the telescope for index error purposes, in addition to the coloured shades.

The handle must be adapted for fixing on the telescope stand.

A Sextant—

A sextant of three-inch radius, graduated boldly to half degrees, in a leather case, like that of an azimuth compass, suitable for slipping on to a leather belt and being worn round the waist, if required.—Reserve, a second five-inch sextant, or other angular instrument of whatever kind the traveller may wish to take.

Artificial Horizon—

The trough must not be less than 3½ inches, inside length; it may be of the usual construction for filtering the mercury when it is poured in. The glass screen must be a folding one, and by a first-rate maker.—

Reserve, one spare glass and a strong two-ounce glass bottle full of mercury, wrapped up loosely in a roll of clothes and well tied up and labelled.

Watch—

A common, strong, silver watch, not too heavy, with an open face and a second hand; it must wind up at the back. The hands should be black steel, not gilt, and they and all the divisions should be very clear and distinct. The performance of the watch is really a very secondary matter. 4*l.* is quite enough to give for it.—Reserve, at least two other watches of the same character; these should be rolled up separately, each in a loosely-wrapped parcel of dry clothes, say of old stockings, and they will never come to harm: they should be labelled, and rarely opened. Half a dozen spare watch glasses, fitting easily; two to each watch. Three spare watch-keys; one might be tied to the sextant-case, one wrapped up with each watch.

Compass—

An azimuth compass, graduated from 0° to 360°, and if the maker understands how to do it, have a shield of brass cut out here and there, to admit light, fixed over the glass.—Reserve, two spare glasses and a second azimuth compass.

Three common pocket compasses, from an inch to an inch and a half in diameter. Their needles must carry cards graduated, like those of the azimuth compass, from 0° to 360°, in addition to the points. These compasses should be very light in weight, have plenty of depth, and be furnished with catches. The needles should work steadily and quickly. Avoid one that makes long, slow oscillations.

Telescope—

One of 2¾-inch object-glass, for observing occultations of small stars and eclipses of Jupiter's satellites. The buyer should test it on the satellites, and be himself satisfied of its power, before concluding the bargain.

Stands—

A clamp to screw into a tree or a block of wood for the purpose of holding the telescope or sextant; one with three legs is perfectly useless to a traveller, for he has no table or anything else to put it on.

The ordinary telescope clamp makes a very good rest for a sextant by clamping a rod of wood, one end of which is weighted as a counterpoise, and the other, ending in a neck, is pushed through an auger-hole in the sextant handle, with a linch-pin stuck through its projecting end. Smooth action is not at all wanted for a sextant rest.

Thermometers—

Two boiling-point thermometers. (Try them yourself against a good barometer to learn their index errors, at least; and recollect that for all purposes of determining heights, common water does just as well as distilled water.)

Two or three common thermometers, graduated to 160° at least, if for hot climates.

A pot arranged to boil the thermometer in.

Lantern—

I can only suggest a "bull's-eye," which was what I used: I wish I knew of a better. A small ball of spare wick. Oil.

Mapping Instruments—

Protractors—1 large circular brass one, 4 or 5 inches in diameter; 2 semicircular brass ones of 3½; all graduated, like your compasses, from 0° to 360°, and not twice over to 180°.

A station pointer for protracting sextant angles.

Two or three rulers of one foot each in ivory; a small square; a set of scales; small parallel rulers; compasses with pencil and pen; small pair of reserve compasses; fine ruling pen; a dozen artists' pins. Medium size measuring tape, say 12 yards; pocket ditto, 2 yards.

Additional Instruments not necessary, but convenient—

A pedometer of the best construction.

An Adie's sympiesometer.

I cannot recommend an explorer to have anything to do with either a chronometer or a mountain barometer.

Stationery—

A light board of the very best mahogany to rule and draw upon, as large as the writing-case will hold, say 11 inches by 7.

Plenty of metallic note-books, with spare pencils, all of one size, say 5 inches by 3½, or larger, with a leather pouch, having a flap buttoning easily over, to hold the one in use.

Two (or three) ledgers of strong ruled paper, 11 inches by 7, each with a leather binding; the pages should be numbered, and journal observations, agreements, sketches, and every single thing that is written, written in them.

Plenty of spare paper; it should be smooth, sufficiently thick, and fold up into 11 inches by 7.

A sheet of blotting-paper cut up and put here and there in the ledgers.

Tracing-paper, both black and transparent.

Blank map ruled for latitude and longitude.

Two dozen steel pens and holders; half a dozen fine drawing and holder; half a dozen FH pencils; half a dozen HB ditto.

Two penknives; India-rubber cut up in 5 or 6 bits.

Ink in abundance (ink-powders require vinegar). Red ink.

Paints, one cake or half a cake of each, viz. Indian ink, lake, cobalt, gamboge, oxgall, in a small tin case.

Half a dozen common paint brushes, one or two of which are kept in the case.

Books—

Raper's Navigation.

Nautical Almanack for current and future years, well bound.

Tables of Logarithms of Society of Useful Knowledge, well bound.

Tables for boiling-point thermometers.

Celestial Maps (uncoloured) pasted on canvas (and learn how to use them).

Three or four small 6*d.* or 1*s.* almanacks of any kind (the Nautical is far too cumbrous and on too bad paper for daily use; Hannay and Dietrichsen give a vast deal of information; the Seaman's Almanack, White's Ephemeris, &c.: they are useful to select and cut tables out of).

The best maps of the country you are going to visit that are to be obtained.

Notes on the above Outfit.

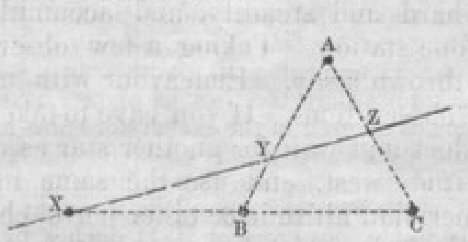
With these instruments, latitudes can easily be found to 300 yards; the sextant, mounted as it may be on a stand, will give nearly as accurate longitude as a sextant can be expected to give. When observing lunars with the larger one, the small sextant will take time at the end of each set as a check upon the watch. The telescope will give the traveller an opportunity of observing occultations of small stars, (not only those given in the Nautical Almanack, but also of others,) which is the most accurate way of finding longitude and the eclipses of Jupiter's satellites, which is the readiest way, and by no means so inaccurate a one as to be altogether worthless. For rapid reconnoitering expeditions on horseback the little sextant would be carried by a belt round the waist, and would give latitudes easily to 500 yards.

It may save trouble to others if I mention here the way which, after many trials, I adopted of observing with a sextant. During the day time I made out a list of the stars that culminated at convenient hours, and their expected altitudes. I set my watch by sunset, if it was very wrong, and spread my rug north and south in an open spot of ground, trampling down the bushes and long grass round it. Then, when the time of observing approached, I lighted my lantern and set it on the ground in front of my rug; to this I brought all my instruments, and first spreading a small cloth to the right of the lantern, I set my horizon on it, filled it with mercury, and covered it with a glass. The cloth was to catch any mercury that might be spilled. I then propped up my watch to the left of the lantern, laid down my note-book, with the leaves tied open, and taking out my sextant, adjusted it to the expected altitude, and screwing on the telescope, which always was kept at my focus, I laid myself flat down on the rug, and taking off the roof from the horizon if there happened to be no wind, and turning the glare of the lantern away from my eyes, and upon the watch, I made accurate contact of the star; then looking quickly round, I observed the watch. I now turned the lantern towards me, changed hands with the sextant, read off and wrote down, turned the lantern back on the watch and recommenced. For a meridian altitude I read off and wrote down about ten observations, both time and altitude, beginning a little before the star reached the meridian, and continuing after it had perceptibly sunk; it was thus easy to tell with accuracy what the meridian altitude really was. For lunars my sextant was always on a stand. I took time with another sextant before beginning, also two or three times during the progress of the lunar, and finally at the close of all. I was thus very independent of the good going of my watch, for by observing every half hour, no watch that went at all could be many seconds wrong. It is of very little consequence that the movements of a stand should be smooth and steady; its object is to so hold the sextant as to retain the moon and star in its field, while the observer is reading off and writing down. Neither of my instruments were fitted to a stand, but by very rough carpentering I made two which proved of infinite service and comfort to me, and which I have alluded to under the head telescope stand. The figures upon the face of a common watch are inconvenient in reading off minutes: therefore, in the first place open the glass, and with a pen and ink make a good conspicuous dot upon each five-minute division, and should you happen to make a blot, it does not in the least matter, for a wet finger entirely removes it. Next stitch together a watch-pocket, to be used when observing; it should be padded soft behind, and expose the face of the watch after the usual way, through a round hole cut out of the middle of a cardboard front. Now draw radiating lines on the outside of this cardboard opposite to each five-minute division, never mind if they are not very accurate, and write legibly on them in italics, the numbers 5, 10, 15, &c., up to 60. Many a mistake will be avoided by doing this, for after long observing the eye becomes sorely puzzled and all kinds of misreadings are put down. If the figures on the watch dial be faint, the numbers may be written over them, or the hour hand may be shortened and a paper ring pasted on to receive the new numbers; the observer can suit his fancy in this.

It will be most in place here for me to add what remarks I would make about sextant and other observations. If you commence to observe for longitude at all, make a regular night of it, working hard and steadily, and accumulating masses of observations at one station. Taking a few observations at many stations is time thrown away. Endeavour with much forethought to balance your observations. If you have to take a star's altitude for time east, select and wait for another star as nearly as may be of the same altitude west, and use the same telescope, horizon roof, &c. If a meridian altitude be taken north, choose another star and take it south, and so with lunars. In this way your observations will be in pairs, and the mean of each pair will be independent of all instrumental and refraction errors; and by comparing the means of these pairs, one with another, you will know your skill as an observer, and estimate with great certainty the accuracy that your results have reached. Never rest satisfied with your observations, unless you feel sure that you have gained means of ascertaining the limit beyond which you certainly are not wrong. Weight all your observations; that is, when you write them down, put good, very good, doubtful, &c., by their sides. When taking occultations, if the star be not down in the Nautical

Almanack list, do all you can to identify it by drawing diagrams of adjacent stars, and indicating the point of the moon's limb that occults it. Before observing, see that the minute hand and second hand of your watch go together, that is to say, that the minute hand is truly over a division when the second hand points 0 seconds; if it be not, move it till it is.

The azimuth compass is one of a traveller's most useful instruments. To use it, it is best to make a pile of stones and lay the cover of the compass on the top, with its bottom upwards; this makes a smooth table for the azimuth compass itself to be moved about on. Be on guard against magnetic rocks; it may happen that the bare peaks of high hills, which are the best of places for observing from, and which a traveller often makes great sacrifices to reach, are found so magnetic as to make compass observations worthless. The little sextant should always be taken up on these excursions. It is of little use in a wild country to devote much time to getting accurate bearings, as the landmarks themselves are rarely well defined: the main endeavour should be not to mistake one hill for another, to judiciously select good angles, and to carry on more than one independent scheme of triangulations at the same time, by comparison of which the accuracy of the whole may be tested. It is surprising how much work may be thrown away by want of judgment; and also how much may be done, with very little trouble, by a person who has acquired a good eye and memory of country. The daily difficulty of an explorer is to triangulate without leaving his caravan: to do this he must note the bearings of hills when they are in conjunction, and also when he happens to be in a straight line between them.



Thus, in travelling along X Y Z, the hills A B C can be mapped; for at X, or thereabouts, the bearing of B from C can be determined; at Y that of A from B; and at Z that of A from C; and so on for any number of hills. And it is very important to recollect that it is not necessary to catch these lines of sight precisely; for by taking bearings twice, and the intermediate course approximately, there are sufficient data for protracting out upon paper the required bearing. Thus, as soon as the peak of a distant hill is about to be occulted by the shoulder of a nearer one, a bearing should be taken; and again another one as soon as it has reappeared on the other side, and the intermediate course noted.

Suggestions for carrying the above Instruments.

Wear a leather belt $1\frac{3}{4}$ inches broad round the loins, to the outside of which, besides any other pouches that you may wish to carry, the leather case of the azimuth compass and the leather pouch of the note-book are sewn. The place for the compass is against the small of the back; for the note-book, behind the right hip. The other instruments must be carried in cases. I have tried many ways myself, and if I were to start again on a journey I should adopt the following. First, I should divide the instruments into three groups, A. B. and C., of which I give the average weights :—

A.

	Weight in lbs.
Five-inch sextant	2
Three-inch sextant	$1\frac{1}{4}$
Horizon trough, bottle, and roof	$2\frac{1}{2}$
Thermometers and watch	$\frac{3}{4}$
Loose sheets of tables, a ruler, protractor, compass, and pencil, spare watch glass	$\frac{1}{2}$
	7

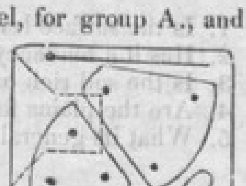
B.

	Weight in lbs.
Telescope (about)	4
Lantern	$1\frac{1}{2}$
Light stand and counterpoise	2
Spare oil	$\frac{3}{4}$
Pot for boiling thermometers	$\frac{1}{2}$
	$8\frac{3}{4}$

C.

Almanacks, maps, tables, mapping instruments, &c.

I should next fit up a common deal box, as a model, for group A., and would follow the general arrangement of the sketch, differing from it only so far as peculiarities, in the particular instruments bought, might render necessary. The horizon apparatus would slip into a separate compartment which did not communicate with the interior of the box, for fear of any mercury getting loose upon the instruments; its position is dotted out. The dark lines show the sizes of sextant boxes as usually made, so that plenty of room is allowed in the sketch for horse-hair stuffing. The small sextant I should pack in its leather case; for the larger one I should take no case at all. The thermometers, thrust into a thin tin case, would go along the upper part of the box, and a watch in the right hand lower corner: the size of the whole inside measurement is 11 inches by $7\frac{1}{2}$, and about 4 deep. Having satisfied myself that the fittings of the box were secure and convenient, I would have a light one made of painted tin after its model; it should lock, and also have hooks to secure the lid, even when it was not locked. Places should be made for leather handles, one to go at each end of the box, and two, crossways, on the top. Its weight would be in all 4 lbs.—the box a little less than 3 lbs. and the stuffing rather more than 1 lb. This accomplished, I would order two more boxes of the same length and breadth, one of them of 4 inches deep to hold group B, which might be arranged with the telescope along one side, the lantern and thermometer pot end to end along the other, and the stand and counterpoise between them, while a flat oil-can an inch deep extending the whole breadth and depth of the box, or 7 inches by 4, would fit in a narrow compartment at one end. The third box should be a kind of writing-desk, and of a depth sufficient to hold group C, say 6 inches. The fitting up of this would be entirely a matter of fancy. Lastly, I would have



a couple of thickly-quilted canvass bags, in which the boxes A and B might be slipped endways. Each bag should have a flap to button down, be painted some light colour, and have strong loops sewn at the four corners of its back. The weight of each bag would be 1½ lbs. Thus, A, box, bag, and all, would weigh 12½ lbs.; B, 14 lbs. These weights are certainly heavy, but they are practicable, and each package is very convenient for carrying as a knapsack or in any other way. For ship, boat, or waggon travelling, a light wooden chest should be procured which would just hold the three boxes, and then putting the quilted bags at the bottom, the boxes would lie one above another as the trays in a canteen. As a protection against the fearful jolting of a waggon, it would be advisable to inclose the chest in a large pannier and loosely to stuff up the interval between them with sacking, clothes, or anything else at hand. The outside measurement of the chest would be 13 inches long, 9 broad, and 15 inches deep; those of the pannier 5 inches larger every way.

Hints for Collecting Geographical Information.

1. *Aspect.*

1. What is the general aspect of the country?
2. Mountainous or hilly?—Sharp peaks or rounded outlines?
3. Of the coast? Abrupt or shelving? Rocky or in cliffs?
4. Downs of Sand? Low or flat?
5. Any active volcanoes? or traces of extinct ones? or their probable forms in the outline of the mountains?

2. *Surface.*

1. Is the surface level or undulating?
2. Has it a tendency to table lands, or steppes?
3. Is the soil rich or poor? loamy—sandy—boggy?
4. Are the plains fertile or barren? wooded or cultivated?
5. What its general capabilities?

3. *Physical Divisions.*

1. Note the chief divisions of the country.
2. Mark especially the line of separation of waters.
3. Trace the outlines of the principal basins of the chief rivers.
4. Group the country into basins as far as practicable; it will be found the simplest mode of describing it.
5. Trace also the limits of the secondary valleys comprising the tributaries to the main stream.
6. May they from position be called upper and lower basins?
7. Do distinct traces of mountainous—hilly—flat—wooded jungle—cultivated—sandy—marshy, or barren, country exist? if so, note their limits generally.

4. *Mountains.*

1. What the direction of the chief range, or ranges?
2. What the general form of outline? (Describe while in sight, *not* from memory.)
3. What is the *estimated* height (if no measurement can be had) of the chief points; and also of the general range?
4. Are any of them snow-capped? (State the season.)
5. How far down does the snow extend? (Note north or south side.)
6. Are they wooded?—At what height does the wood finish?

7. At what height does vegetation cease?
8. Are the mountains in groups or masses? or detached?
9. Obtain bearings, by compass, of the limits of the range, and of all remarkable points, masses, gaps, &c.
10. Mark the chief mountain-passes, and note if they might be easily defended against an enemy.
11. What their general structure?

5. *Rivers.*

1. What are the native names of the chief rivers?
2. Trace the general course of each; with its windings, if possible.
3. Does it receive many tributaries? note their names in order, from its sources, distinguishing on which side they join.
4. Is it navigable for large or small craft? and to what extent?
5. How far up does the tide reach? Is the current rapid? What its rate?
6. Does it flow by several outlets, or by one grand mouth to the sea?
7. Does it form a bar, or banks, or islands at its mouth?
8. What the width of the river at its outlet? and at various points?
9. Is the river ever fordable? Name the chief fords.
10. Does it form cascades, or rapids, or occasionally inundations?
11. Does it at any season lose itself in sand, or otherwise not reach the sea?
12. Does it flow from a lake, or from other sources or springs?
13. What may be the probable elevation of its source above the sea? measured or estimated?
14. Is the bed of the river gravel, sand, or mud? Does it bring down much detritus?
15. What the colour of the water? Does it retain it at any distance from land?
16. Is the river obstructed by islands, shoals, rocks, snags, or any obstacle to steam navigation?
17. Are its banks wooded? Is fuel easily procured?
18. Does it abound in fish? and in what species?
19. Is it navigated by native boats? and how far up?
20. Describe each affluent as a main stream, with its tributaries, marking the position of junction, and the angle at which it joins its recipient.

6. *Lakes.*

1. What the native names of the lakes?
2. What is the situation and extent of each?
3. What its level, above and below the sea? How ascertained?
4. Is it formed by rivers or springs? or does it feed any river?
5. Is it of salt or fresh water? Is it said to rise periodically?
6. What its general depth of water?
7. Are there any vessels or boats upon it? and of what size?
8. Are its banks rocky or steep, or low? Are they wooded or barren?
9. Could fuel be readily procured? Does it offer facilities, or the contrary, to steam navigation?
10. Are its shores thickly inhabited? Are birds, fish, shell-fish, &c., plentiful, and of what sort?
11. Are any marshes or ponds known, and where? Are they constant or periodical?

7. *Sea Coasts and Ports.*

1. Does the coast form gulfs, bays?
2. Promontories, peninsulas, capes, low points, &c.?
3. Is it abrupt, bold, rocky? or low, flat, and shoal?
4. Are there currents along the coast? Note their force and direction.

5. Name the chief *ports*. Are they secure harbours, or only open bays, or roadsteads for anchoring?
6. What the depth of water, and what bottom for anchoring?
7. Is the port capable of containing many vessels? Does it offer facilities for repairs?
8. Can water, provisions, and fuel be easily procured?
9. Note the time of high-water at full and change of moon, and rise and fall of tide; and direction and velocity of stream.

8. *Volcanoes and Mineral Springs, &c.*

1. Are any now active? or, are there traces of extinct volcanoes?
2. Give their position—height above the sea—and native names.
3. Does tradition or history record any eruption? at what date?
4. Was the eruption of fire, lava, scoriæ, water, or mud?
5. Are earthquakes frequent? Are there records of any having occurred?
6. What were their effects? how far did they extend? any up-heaving or depression of land recorded?
7. Are many *mineral springs* known? Hot, tepid, or cold? (Note the temperature if possible.) Are their waters used medicinally?
8. Do they form deposits? Siliceous or calcareous?

9. *Maps, Charts, &c.*

1. Do any charts of the coast, or maps of the country, or partial surveys exist? Native or otherwise? What their respective dates?
2. Are they believed to be accurate? Upon what scale?
3. Endeavour to map the country, starting, if possible, from a fixed point; if exact observations cannot be obtained, give compass bearings, and estimated heights and distances. (N.B. Heights may often be obtained by length of shadow, &c.; distance by velocity of sound, &c.) The scale of one inch to a geographical mile is recommended.
4. Take bearings of all remarkable objects in sight from any known station, as mountain-peaks, masses, gaps, towns, villages, forests, &c. &c., and transfer all to paper immediately; trust nothing to memory.
5. Preserve all original observations and documents relating to surveys; and make two or three copies of observations.
6. Obtain correct *native* names if possible, and keep to one standard of orthography. Mark all hearsay information with the initials of the informant. If a journey is made by night, or in foggy weather, trace it with coloured ink.

10. *Astronomical Observations.*

1. Are any positions astronomically determined? What reliance may be placed on them?
2. It is very important to obtain observations for the position of all capes, headlands, points, towns, villages, &c.; mountain-peaks, passes, limits of range, &c.; lakes, sources, confluence, and outlets of rivers; in short, of every remarkable object.
3. Endeavour to obtain the *latitude* by meridian altitude of the sun, or of a planet, or of a star, or of the moon.
4. *Longitude*—by eclipses of Jupiter's satellites, especially by eclipses of the third and fourth satellite, when both immersion and emersion can be observed,—or by any other eclipse; by moon culminating stars; by occultations of fixed stars by the moon; by lunar distances from the sun, or a planet, or a star, always East and West when possible; by an altitude of the moon in the prime vertical; or by chronometers:

state always by which method obtained, and what reliance may be placed on it.

5. Observations on the variation of the compass, and dip of the needle, are very important.

Instruments.

A repeating circle, or sextant of five-inch radius; a pocket sextant; an artificial horizon; Kater's compass; two mountain barometers; and two thermometers, with a good pocket chronometer; are sufficient for all common purposes.

Meteorology.

1. Keep an exact register of the barometer and thermometer.
2. What are the prevalent winds? What the periodical?
3. What the average fall of rain?
4. What the amount of evaporation, &c.

Natural History.

1. Note the geographical distribution of man, animals, birds, fishes, insects, plants, &c.
2. Obtain information on all branches of Natural History, bearing in mind that the useful and practical is of more importance than the merely curious.

For detailed instructions, under each head, recourse should be had to the respective sources.

Ethnography.

1. Obtain vocabularies of the native language—phrases rather than single words. Keep to a fixed standard of orthography in writing them down: the sounds of the vowels in *father—there—ravine—mole—lunar*, are recommended as the most simple, and as being both English and European.
2. Note the habits, manners, customs, and amusement of the natives.
3. What notion have they of a supreme being? what of a future life? what, if any, their religious ceremonies?
4. What their treatment of the aged, of the sick, and of children?
5. What seems to be the form of government? Is division of property recognised? Do they buy and sell land?
6. Do they trade or barter with each other, or with strangers?
7. Note the number of natives seen from day to day, distinguishing the sex, and children.
8. Are there many lunatics or idiots?
9. What the usual form of feature? the shape of the skull? hair? colour? stature? bodily constitution?
10. Is plurality of wives common? are women without husbands frequent?
11. Have they any marriage ceremonies? how do they treat their wives?
12. Do they give proof of capacity for civilization?
13. May the natives be trusted as guides—as messengers—or to procure food?
14. What presents please them best?
15. What words or signs do they use when hostile? or when friendly?
16. What are their dwellings? What their chief articles of food?
17. What their disposition—savage or gentle; rash, hasty, or inoffensive? Are they disposed to receive instruction?
18. Are any cases of cannibalism reported? N.B. To investigate strictly under what circumstances they occurred.
19. Are the people said to be increasing or decreasing?
20. Does slavery exist? What is the condition of a slave?

21. What are their diseases? What their medical treatment?
22. Can the traveller point out the most probable mode of civilizing and benefiting the natives?
23. What traditions are current respecting the origin of the people?
24. Collect all information that can throw light on the migration of nations.

N.B. The greatest forbearance and discretion are strongly recommended in all intercourse with the natives—never to allow an imaginary insult to provoke retaliation which may lead to bloodshed. It must be borne in mind their's is the right of soil—we are the aggressors.

DESCRIPTIVE GEOGRAPHY.

Names of Country.

Boundaries.

Configuration of Surface—

General Character; Mountains; Plains; Valleys.

Names, Passes, Culminating Points, Snow-level, Altitudes, Peculiar Phenomena, Outline of Base and on given Contours, Sections, Latitudes and Longitudes, Bearings and Distances.

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Topography and Itineraries.

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Zoology—

Species found in Country; Distribution of Animals; Useful Animal Products, Wild and Domesticated; Directions for Collecting, &c.

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E. S. Galton
from Francis Galton
March 18/94

IDENTIFICATION OF HABITUAL CRIMINALS.

REPORT

OF A

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COMMITTEE APPOINTED BY THE SECRETARY OF STATE

TO INQUIRE INTO THE

BEST MEANS AVAILABLE FOR IDENTIFYING

HABITUAL CRIMINALS;

WITH

MINUTES OF EVIDENCE AND APPENDICES.

Presented to both Houses of Parliament by Command of Her Majesty.

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PRINTED FOR HER MAJESTY'S STATIONERY OFFICE,
BY EYRE AND SPOTTISWOODE,
PRINTERS TO THE QUEEN'S MOST EXCELLENT MAJESTY.

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HODGES, FIGGIS, & Co., LIMITED, 104, GRAFTON STREET, DUBLIN.

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[C.—7263.] *Price* 10*d*. +

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[C.—7263.] Price 10d.

CONTENTS.

well to tell him first. I enclose a slip I have
printed here as an experiment. It contains all and much
more than is wanted to ferret out who the unknown man
is whom it refers to out of some hundreds of thousands of
names. Ever affectly Francis Galton
(Don't want it back, having a lot of them.)

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Lefevre.		1894.		105287	

WARRANT APPOINTING THE COMMITTEE.

I HEREBY nominate and appoint—

CHARLES EDWARD TROUP, Esquire, of the Home Office;

MAJOR ARTHUR GRIFFITHS, Inspector of Prisons;

and

MELVILLE LESLIE MACNAGHTEN, Esquire, Chief Constable in the Metropolitan Police Force;

as a Committee to inquire (*a*) into the method of registering and identifying habitual criminals now in use in England; (*b*) into the "Anthropometric" system of classified registration and identification in use in France and other countries; (*c*) into the suggested system of identification by means of a record of finger marks: to report to me whether the anthropometric system or the finger-mark system can with advantage be adopted in England either in substitution for or to supplement the existing methods, and if so, what arrangements should be adopted for putting them into practice, and what rules should be made under section 8 of the Penal Servitude Act, 1891, for the photographing and measuring of prisoners.

I further appoint the said Charles Edward Troup, Esquire, to be Chairman, and Harry Butler Simpson, Esquire, of the Home Office, to be Secretary of the said Committee.

Given at Whitehall, this twenty-first day of October, 1893.

H. H. ASQUITH.

REPORT

TO THE

RIGHT HONOURABLE H. H. ASQUITH, Q.C., M.P., PRINCIPAL SECRETARY OF STATE, HOME DEPARTMENT.

SIR,

IN this Report, which we have the honour of laying before you in accordance with the Commission issued to us on 21st October last, we think it will be convenient to follow the order of the several heads given in the terms of reference. We shall, in the first instance, state the results of our inquiries into the methods of identifying Habitual Criminals now in use in England, into the Bertillon system as practised in France and into the method of identification by finger-prints suggested by Mr. Francis Galton. We shall then proceed to make a recommendation as to the system which may, we think, most advantageously be adopted in England—the system we propose to recommend being one which borrows M. Bertillon's admirable method of classification, and at the same time embodies the practical results of Mr. Galton's investigations—and we shall conclude by suggesting in detail the arrangements to be followed and the rules to be made in the event of our recommendation being adopted by you.

THE METHODS OF IDENTIFYING HABITUAL CRIMINALS NOW IN USE IN ENGLAND.

For the purpose of making ourselves thoroughly acquainted with the manner in which the different Police Forces of this country put in practice the means at present available for the identification of Habitual Criminals, we have issued two circulars, copies of which, with an abstract of the replies, are given in Appendix A., and we have conferred personally with the Chief Constables and the heads of the detective forces of various counties and boroughs, some of whom attended to give evidence at the Home Office, while others we met on our visits to Wakefield, Leeds, Bradford and Birmingham. We have also visited the Convict Supervision Branch at New Scotland Yard and the Habitual Criminals Registry in the Home Office, and have minutely examined the registers, records and volumes of photographs there in use. From the officers of these two departments, from the Prison Commissioners and from the Governors and officers of the Prisons of Holloway, Pentonville, Wakefield, Leeds and Birmingham, we have from time to time obtained much useful information. Most of the evidence we have obtained was taken informally, and a large part had immediate reference to books, forms, documents and photographs. Only a small portion, therefore, of the oral evidence has been put in writing, and will be found in the Minutes.

It may at the outset be stated in general terms that the practice of the English police, though the details differ widely in different forces, is always dependent on personal recognition by police or prison officers. This is the means by which identity is *proved* in criminal courts; and, though its scope is extended by photography, and it is in some cases aided by such devices as the registers of distinctive marks, it also remains universally the basis of the methods by which identity is *discovered*.

A personal knowledge of habitual offenders is obtained primarily in connexion with their arrest and trial for their earlier offences; but further facilities for acquiring it are supplied by the conditions which require licensed convicts and persons under sentence of police supervision to report to the police once every month, and also on every change of residence. The personal interviews thus rendered necessary afford the police excellent opportunities of acquiring a good knowledge of the more dangerous of the criminals with whom they have to deal; and the faculty of recognising and remembering faces, which is so useful in all departments of a police constable's work, is by this means encouraged and developed to a very high degree.

Generally speaking, therefore, when an habitual criminal resident in a particular district is arrested in that district on a criminal charge, his antecedent history will be within the knowledge of the local police; and it is rare for such a man, when once he finds himself known, to dispute the evidence of his identity or attempt to repudiate the record of his previous misdeeds. The greater part of the crime in this country is committed by criminals of this class—by men who are well known to the police of their district, whether it be a borough or a division of a county, and who have little chance, unless the circumstances be exceptional, of disguising their identity.

On the other hand, the offences committed by men travelling from county to county or conducting operations in one police district while habitually residing in another are not inconsiderable in number, and are often of a peculiarly serious and dangerous character. If a distinction be made between a "professional" criminal—the man who has deliberately adopted a career of dishonesty or violence as a means of obtaining a livelihood—and the man who only lapses into crime occasionally and, as it may be said, under stress of circumstance, it is clear that the travelling thief or burglar belongs almost always to the former rather than the latter category. To the former class also belong, as a rule, those criminals who, without being regular travellers, move from a district where they have become well known to the police to another in which they hope to be rid of their past history and to begin with a clean record a new career of crime. The answers received to our circular of 13th November (p. 70) illustrate the proportion between local crime and crime committed by "foreigners" in the different parts of the country; and, though no exact statistics can be given, we think it may be taken as certain that while in most districts the bulk of crime is local, a considerable proportion of the most serious offences are committed by those "foreign" criminals, whose antecedents it is at once most desirable and most difficult to trace.

Further, even with regard to local criminals, the difficulty of personal recognition becomes very great in the large centres of population. The number of criminals seen by each officer is so great that it is impossible after any considerable interval for any but a man endowed with a singularly good memory to remember more than a few of them; and unless the memory is aided by photographs and registers, mere personal recollection is insufficient to secure the identification of those persons who repeatedly come before the Courts. This is especially the case in London, where not only the criminal but the ordinary population is constantly moving from one district to another, and where an offender might be arrested in a dozen police divisions and convicted in a dozen different courts, without being seen twice by the same officer. Were it not for the assistance of the prison warders—all remand cases from London being concentrated in one large prison—and the special police arrangements to be described later, it would be impossible in London to secure the identification of more than a very small proportion of the local criminals.

The question, then, is what are the agencies by which "travelling" criminals throughout the country, and the local criminals of the Metropolis and of other large centres of population, can be so traced that, when arrested for a fresh crime, they may be confronted with the police officer or the prison warder, who is able to prove their identity and to establish their past record.

The Habitual Criminals Registry.

The Habitual Criminals Register is the only agency specially established by Parliament and intended for general use throughout the country in the work of identifying old offenders. It was first established under the Habitual Criminals Act, 1869, which was repealed and partially re-enacted by the Prevention of Crimes Act, 1871. The latter Act now provides (section 6) that "Registers of all persons convicted of "crime . . . shall be kept in such form and containing such particulars as may "from time to time be prescribed . . . by the Secretary of State . . . The "register for England shall be kept in London, under the management of the Com-"missioner of Police of the Metropolis, or such other person as the Secretary of State "shall appoint." At first the register included all persons *convicted of crime:* crime being defined by section 20 as including any felony and the following misdemeanours:—the uttering of false or counterfeit coin, the possession of counterfeit gold or silver coin, the obtaining of goods or money by false pretences, conspiracy to defraud and the offence of being found by night in possession of housebreaking instruments. It was soon found, however, that the registration of criminals was being carried out on far too large a scale, and that the results attained were altogether disproportionate to the labour involved—a large part of the persons registered not being habitual criminals in any ordinary sense of the term—many of them first offenders, and some children convicted of trivial thefts. The Prevention of Crimes Amendment Act, 1876, was therefore passed, and power given to the Secretary of State to determine what classes of prisoners should be registered; and by Lord Cross's regulations of 15th March 1877, the register was confined to "every person convicted on indictment of a *crime*, a pre-"vious conviction of a crime being proved against him." This is the class to which sections 7 and 8 of the Prevention of Crimes Act, 1871, are applicable; and it appears to us to be the best legal definition of "Habitual Criminal" which it is possible to obtain. The register, which had at first been established at Scotland Yard under the charge of the Commissioner of Metropolitan Police, was now transferred to the Home

Office, and Sir Edmund Du Cane, the Chairman of the Directors of Convict Prisons, was appointed Registrar.

At the present time the work of this Register is carried on, under the direction of Sir Edmund Du Cane, by Mr. Grace, who is also keeper of the register of convicts—a register maintained chiefly for the purposes of prison administration—and who has two assistants, one for each branch of his work. It is still controlled by the regulations of 15th March, 1877, except that, in addition to the habitual criminals, as above defined, the names of all convicts released under sentences of penal servitude are included. The register is prepared from a return which is made from convict and local prisons a few days before the discharge of every convict and of every habitual criminal. This return (commonly known as "Form R") contains a statement of the prisoner's convictions, his personal description, the full particulars of his distinctive bodily marks and his photograph. The total number of these returns received last year was 3,851; it has been annually decreasing since 1884, when the number was 5,210. From these returns two volumes are annually compiled:—

(I.) *The Habitual Criminals Register*, containing all the names in alphabetical order, and giving, in columns opposite to each name, the prisoner's full description at the time of his discharge including his distinctive marks, the particulars of his last conviction, his destination on discharge and the number of his previous convictions, with references to entries in previous registers. The register is published annually and contains all habitual criminals and convicts who have been liberated between 1st January and 31st December of the previous year.

(II.) *The Register of Distinctive Marks*, in which cases are classified by the position of the marks on the body. It falls into nine main divisions according as the marks are on (1) the head and face, (2) the throat and neck, (3), the chest, (4) the belly and groin, (5) the back and loins, (6) the arms, (7), the hands and fingers, (8) the thighs and legs, (9) the feet and ankles. These are again sub-divided: *e.g.*, under the head "arms" we have "loss of arms," "tattoo marks," "distortion from fracture or dislocation," "loss of power," "scars from wounds or burns." The purpose of this register is to enable a criminal to be traced by means of his distinctive marks. If, for example, a prisoner is found with a burn on the right upper arm, the register is searched in the division "arm, right," under the head "scars from burns." Under this head will be found a list of all convicts and habitual criminals discharged during the year having a burn on the upper arm, with further descriptions, such as "inside" or "large," or indications of other marks such as "burn on side," "burn above knee"; and by means of these indications it is supposed that the particular case can be identified, reference being made, in cases where more than one person is entered as having the same distinctive marks, to the fuller descriptions contained in the Alphabetical Register. The Register of Distinctive Marks is published annually at the same time with, or soon after, the alphabetical register, and the Registers of Distinctive Marks for each period of five preceding years are also tabulated in one volume and published.

Copies of both registers are distributed to all police forces. The evidence we have received as to their use conveys an impression that even those police forces who frequently consult them do not by this means make a large number of identifications. An abstract of the opinions we have obtained will be found in the Appendix, p. 68. The statement of the Chief Constable of Newcastle-on-Tyne that they are frequently examined, and occasionally with success, may be taken as typical of the verdict of the majority of the police authorities. In some districts, such as the North Riding of Yorkshire, the borough of Leeds and the Metropolitan Police District, they are very rarely or never referred to, and on the other hand Liverpool is the only force of those we have consulted that reports many identifications actually effected thereby.

The information we have obtained as to the number of inquiries received in the Registry from the police seems likewise to show that these volumes are not very extensively used. During the months of August, September and October last sixty-one inquiries were thus made; in twenty instances no information could be given; in forty-one the original returns from the prisons, with the photographs attached, were forwarded to aid in the identification of the suspected persons. In twenty-three of these cases the identification was correct, in eight incorrect, while in the remainder no intelligence had reached the Registrar of the result of the information supplied.

We think it a matter for regret that this Register has not been more generally used by the police. It was the first attempt made in England or elsewhere to form a classified register of criminals in which they could be traced by their personal description and independently of the names they might assume; and it has the advantage of being prepared in a form which can be circulated and used by each police force for itself. The work of preparing the register has, we are satisfied, been carried out with great care and accuracy.

The drawbacks which have led to its comparative failure appear to us to be:—

(i.) That many persons have no bodily marks that are really *distinctive*. Some have no marks sufficiently permanent and definite for the purpose in view, as in a case mentioned by Mr. Grace where a minute red spot under the eye was the only "distinctive mark" given. A much larger number have marks which, though definite in themselves, are shared by a great number of other convicts. For instance, in the Register for 1892, in the division "Left Hand," under the head "tattoo marks," no fewer than twenty-eight persons are entered as having a ring on the second finger, and in only three of these cases is a second distinctive mark added; if therefore a person is arrested having a tattoo ring on the second finger of the left hand at least twenty-five cases in that volume alone would have to be examined, besides a much larger number of other cases with the same distinctive mark in previous volumes. It is true that this difficulty could to a large extent be overcome by more minute descriptions and by measuring the position and size of the marks, but this would involve a complexity of entries which would make an Index Register almost unmanageable.

(ii.) That the published registers are for one year only, and that no volume, therefore, contains a complete list of habitual criminals. The same case reappears in successive volumes only in the case of repeated convictions. If an habitual criminal has escaped detection for several years, his name will only be found in the year on which he was last released from custody. In the case of the Register of Distinctive Marks this difficulty is partly met by publishing quinquennially a volume covering a period of five years.

(iii.) That the registers for each year are not published until late in the following year. The Alphabetical Register of criminals discharged from prison in 1890 was not issued till 7th October 1891; that for 1891 not till 25th July 1892; and that for 1892 not till 9th of September of this year. The Distinctive Marks Register for 1890 was not issued till 9th December 1891; that for 1891 not till 23rd November 1892; and that for 1892 not till 9th September 1893.* An habitual criminal's name is, therefore, not available for the police in the registers until a period of from 9 to 20 months after his release, though this is precisely the time during which he is most likely to be wanted. A large number of the persons whose names are contained in the register must, before the date of its publication, be again convicted, and either escape identification or be identified by some other means.

The fact that scarcely any of the police authorities whom we have consulted have complained of the lateness of publication is of some significance. If the police generally found the registers of essential value in their work, they would be anxious to have copies for use much earlier than at present.

It appears to us, therefore, that the comparative failure of these registers is due, not to any want of care in the way in which the work has been done, nor to the mode of classification, but rather to the inherent difficulty of devising any exhaustive classification of criminals on the basis of bodily marks alone, and also to the difficulty of using a register of criminals that is published at intervals and in a printed form. We do not, however, think that the police have made all the use of this register which they might have done; and had no other means of classifying criminals been available, we should have been ready to recommend that this register should be improved by (1) a reduction of the number of cases included, (2) by a more definite description of the bodily marks, and (3) by publication at shorter intervals, and possibly also by the introduction of a card index, which could always be kept up to date. As will appear, however, from the later part of our report, we consider that a better principle of classification may now be adopted, and in view of its introduction any extensive alteration in the Register of Habitual Criminals is, it appears to us, unnecessary. Even in its present form, however, it is of some use to the police, and, probably, of more use in prisons,—we had evidence, for instance, of its frequent use in Pentonville; and we are satisfied that it ought not to be discontinued until a better system is not merely decided on, but is in full working order.

Identification by the Metropolitan Police.

While the Habitual Criminals Registry is the only means provided by the central government for the identification of criminals, greater importance in practice attaches to the agencies which have been organised for this purpose by the police themselves. In an account of these agencies, the first place is to be given to the Metropolitan Police,

* The issue of the registers in 1891 was delayed beyond the usual time by the transfer of the Prison printing establishment from Chatham to Parkhurst.

partly because they are an imperial as well as a local Police Force, having duties beyond the area under their immediate control, but chiefly because within that area the problem of identification is far more difficult and complex, and the means of solving it more highly organised than elsewhere. The local jurisdiction of the Metropolitan Police extends over an area that is more than twenty times as large, and over a population that is more than ten times as large, as that of any other urban police district. Moreover, as the seat of Government, the commercial and financial capital of the empire, and the centre of wealth and luxury, London presents peculiar attractions for the professional criminal. Not only is the local knowledge, on which all police forces must so largely rely, less easy to accumulate than either in a county where the population is comparatively sparse, or in a borough whose area is comparatively small; but the character of the criminal population is somewhat different from what is found elsewhere, and the police have to contend with difficulties which are unknown, or known only on exceptional occasions, to county and borough forces. As, for one week in the year, Doncaster Races, Hull Fair, and Birmingham Cattle Show afford to swindlers and pickpockets from all parts of the country a special opportunity for reaping a harvest, so at all times London offers to the able and energetic criminal an exceptionally favourable field for his operations. It is probable that all the most dangerous criminals in the country, who are not themselves Londoners, visit London for purposes of crime at some stage or other in their career, and London is the chief if not the only resort in England for the most bold and cunning criminals of foreign countries. It is also the residence and place of refuge of most of the travelling burglars and pickpockets who make excursions to the counties and country towns, and of some who extend their operations to the Continent. Its importance as a centre for the criminal classes is illustrated by the fact that the majority of convicts take their discharge in London: in 1891 out of 1,024 convicts 696 were discharged into the Metropolitan Police District, and in 1892 871 out of a total of 1353, although these figures must be taken with the qualification that the number of convicts who remove from London during the currency of their licences is greater than the number who report themselves to the police as coming from other districts to reside there.

The work of identifying criminals in the Metropolitan Police District is managed by the Convict Supervision Office, a department originally formed to deal with convicts and others under sentence of police supervision, but which has now far outgrown its original functions, and, under the direction of Chief Inspector Neame, deals with the whole of the records of crime and all habitual criminals. This Office receives from the Divisions a report of every conviction of crime in the Metropolis with a personal description of the offender, and in the case of convicts and of persons under sentence of police supervision ("8th section men") about to be discharged from prison, a much fuller description, accompanied by photographs, is forwarded to them by the Governor. Photographs are also obtained from the prisons of a large number of other criminals who are not under supervision—most, but not all of them, "7th section men." From these materials, which are carefully numbered and filed, various registers are formed:—

1. There are Alphabetical Registers of the names of convicted criminals.

2. There are albums of photographs of all convicts, and since 1887 of a large number of other habitual criminals. In the earlier volumes the photographs are arranged chronologically as they were received; but those of later date are sub-divided according to the age and stature of the persons, and according to the class of crime that each criminal affects.

3. A very elaborate Register of Distinctive Marks is now kept, and this being in manuscript can be kept constantly up to date, and is in a more convenient form for use than the printed Habitual Criminals Register, the use of which it entirely supersedes. The general division of the book is by the parts of the body, and is similar to that of the Habitual Criminals Register; but the sub-divisions are by parallel columns on the same page as in the example given in the footnote below.* In this way

* SPECIMEN of the FORM of the DISTINCTIVE MARKS REGISTER kept in the CONVICT SUPERVISION OFFICE.

RIGHT ARM.

Name.	No.	Limb deficient, malformed, injured, or diseased.	Tattoo Marks.					Moles or Warts.	Other Marks.
			Anchor or Cross.	Man or Woman.	Ship or Flag.	Heart or Star.	Other Marks.		

entries of several marks can be made simultaneously, and in consulting the register, the eye running down the columns can very quickly pick out cases having the particular marks sought for. It was, however, strongly represented to us by Chief Inspector Neame and his officers that there should be greater precision in the taking of descriptive marks, and that their distance from fixed points in the body should be measured and recorded.

Supplementary to this volume there is an Alphabetical Register of Tattoo Initials and Names, and it is extraordinary how large a number of habitual criminals provide the police with an easy means of identifying them by names or initials tattooed on their bodies. There are also classified lists of criminals addicted to particular varieties of crimes, such as coining, stealing bicycles, larceny from lodgings, or swindling by means of an employment agency.

When any person is arrested in the Metropolis and charged with crime, whose antecedents are unknown, but who is suspected of being an old offender, an inquiry on what is called a "Search Form," with a description of the accused, is at once sent to the Convict Supervision Branch, and the officers of that branch endeavour, if possible, to trace the case by their registers. If the accused is brought before a magistrate and remanded, the officer in the case himself attends at the office to aid in the search and particularly to examine the volumes of photographs. When the previous convictions of a prisoner are known, the particulars are sent, with the search form, to the Convict Office, but the officer himself does not attend.

A large number of inquiries are received from County and Borough police forces, and some from abroad, in pursuance of which search is always made, not unfrequently with success. Details as to the mode in which the search is made will be found in Chief Inspector Neame's evidence, page 38, and a paper handed in by him, and printed on page 39, gives statistics of the amount of work and of the results.

In other ways the Convict Supervision Office aids in the general work of identifying criminals. The descriptions of persons in custody given in the daily "informations" issued to all Metropolitan Police Stations sometimes lead to recognitions, and the "Police Gazette," which is published twice a week, contains descriptions and occasionally portraits of persons in custody who are supposed to be concealing their identity or to be wanted, as well as of convicts on licence or under supervision who fail to report and whose arrest is required, and in the country as well as in London identifications are sometimes made by this means.

In the case of a considerable number of habitual criminals, photographs are not merely kept at Scotland Yard, but are also sent to the divisions, and albums of photographs are kept at the divisional stations. Three times a year the Office also issues to other police forces as well as to the divisions a circular with photographs of the more eminent criminals known by the Metropolitan Police to be at large. This circular appears to be of very great service to forces in the South of England. In the North, however, few of the forces subscribe to it, and it is said that the criminals included in it are not often to be found extending their operations to the northern counties. Several police forces have strongly urged that this circular should be issued more frequently.

The work of the Convict Supervision Office does not, however, exhaust the means by which the Metropolitan Police secure the identification of criminals. The most characteristic and, as it would appear, the most effective method is the inspection of remand prisoners in Holloway. To this prison are sent all persons committed for trial or remanded by magistrates within the Metropolitan Police District, and here, three times a week, come warders from the gaols at Wormwood Scrubs, Pentonville, Wandsworth and Chelmsford, and detective officers from the twenty-two Metropolitan Police Divisions, an inspector from New Scotland Yard, and six officers from the City of London Police, to view the unconvicted prisoners at the hour of exercise. In this way a prisoner, whose identity is unknown to the constable by whom he has been arrested, will often be recognised either by a warder who has known him in prison, or by a police constable who has had him in custody on some previous charge. The following figures will show of what value this practice is for the purpose of identifying old offenders.

PRISONERS IDENTIFIED in HOLLOWAY by CRIMINAL INVESTIGATION OFFICERS and WARDERS as having been previously CONVICTED.

Year.	Identifications.	By Warders.	By Police.
1883	1,826	1,427	399
1884	1,986	1,730	256
1885	2,081	1,834	247
1886	1,913	1,727	186
1887	1,594	1,367	227
1888	1,711	1,495	216
1889	1,462	1,188	274
1890	1,797	1,553	244
1891	1,671	1,485	186
1892	1,964	1,765	199
1893	1,949	1,759	190

It should be explained with reference to these figures that the warders see the prisoners before the police officers and that all the identifications which are made both by warders and by police officers are put down to the credit of the former.

It will be clear that this method is merely a specially organized form of the personal recognition which is the basis of the whole of the English system, but so much importance is attached to it by the Metropolitan Police that it seems to deserve very special consideration.

Identification by County and Borough Police.

Turning now to the local police forces, we find that most, if not all of them, endeavour to aid the work of personal recognition by keeping registers and photographs of the prisoners who pass through their hands, and in some of the larger and better organized forces special registers of other kinds are also kept. At Birmingham there is a register which shows, by means of coloured drawings, the tattoo marks with which so many criminals ornament their bodies, and some very remarkable though isolated instances have occurred of recognition by this means. Again, at Liverpool special registers are kept of the maiden names of the wives and mothers of criminals, as it is found that in a large proportion of cases an offender, when he changes his name, takes either his wife's or his mother's. In no case, however, of which we are aware, does any force attempt to maintain such an elaborately classified register of descriptions as that kept at Scotland Yard, and where offenders are traced by an index of personal descriptions, it is, as at Liverpool, the Habitual Criminals Register that is used for the purpose.

Nor is there among provincial police anything corresponding to the regular inspection of remand prisoners at Holloway. In some cases where two or three prisons are near one another (like Wakefield and Leeds), warders from one prison will visit the others, for the purpose of making identifications; and it is stated that at Liverpool prisoners from the county and from neighbouring boroughs are brought to the city Bridewell if it is thought that the Liverpool Police might be able to recognise them. We think, however, that great advantage would ensue if the prison at every large centre of population were regularly visited by detectives from the neighbouring police forces, so that there might be an opportunity of prisoners arrested in one district being identified by officers from the adjacent districts. Thus at Birmingham it would probably lead to a good many identifications not otherwise obtainable, if detective officers from Birmingham, Staffordshire and Warwickshire were to make periodical visits to the prison, and we understand that the Prison Commissioners have always been anxious to afford facilities for this purpose.

There seems, however, to be an almost complete agreement in the opinion that the method most generally used by the county and borough police, and used with the best results, is the "route form." The route form (of which a specimen will be found in the Appendix, page 73) gives the photograph, if it can be obtained, and a written description of a prisoner charged with some criminal offence, concerning whose antecedents it is desired to obtain information; while below the description are noted the names of five or six police or prison authorities that are considered most likely to have had the man in custody. This is the "route," and the paper passes from one authority to another, being examined at each place by as many as possible of the detective officers or warders, and each authority notes on the paper any information they possess, as to the previous convictions of the accused, or the words "not known" if they possess no information. The last authority named in the route then returns it to the place whence it was issued, the route being as a rule so arranged as to secure its return within a week, the usual time for a remand.

These forms are very commonly issued by the police during the interval of a remand, or, if the prisoner be committed for trial, during the time he is awaiting trial, and are addressed to the police of districts from which he may be suspected to come, and more especially to the police of the Metropolis, to the governors of prisons and to the Habitual Criminals Registry. Sometimes only one route is issued, sometimes several are sent out simultaneously, occasionally in an important case route forms are spread broadcast to almost every police force in the country. Routes are also issued by prison governors in the case of prisoners committed to their custody to await trial, and even sometimes in the case of convicted persons whom there is reason to suspect of being old offenders unknown to the police by whom they were charged. They are, of course, used only for the purpose of *tracing*, never for the purpose of *proving* identity, and to verify an identification suggested by this means, the attendance of a constable or warder is always necessary. But they afford the most remarkable instance of the way in which the facilities for personal recognition are increased by the use of photography.

The improvement in the present system which has been most urged on us by all the police witnesses whose evidence we have taken, has been that greater facilities should be given for obtaining photographs of untried prisoners with a view to issuing more route forms. At one time such photographs were regularly taken in many county prisons, but the practice was discontinued in consequence of doubts as to the power to compel remand prisoners to be photographed; they are now taken only in exceptional cases, and a prisoner who refuses to be photographed is not subjected to restraint or punishment. Many police forces, however, are able, either with their consent, or without their knowledge, to obtain photographs of all prisoners strongly suspected of being old offenders; and it is these photographs circulated in the route forms that in the great majority of instances lead to the identifications, the route form without a photograph being rarely or never of any use. Under the Penal Servitude Act, 1891, the Secretary of State has power to provide by rule for the photographing of untried prisoners, and we are strongly of opinion that a rule for this purpose should be made under safeguards which we shall suggest hereafter. It will, indeed, be necessary for the carrying out of the system of identification which we propose to recommend, and apart from that it would be of immense use in making the existing methods of identification more efficient.

In recommending an increase of the number of photographs taken, we must not be understood as recommending an indiscriminate increase in the number of route forms, and indeed we are confident that the system we are about to recommend will, when fully established, lead to a great reduction. It is to be remembered that each route issued involves a large total amount of labour though it is spread over many persons, and their indefinite multiplication would be a great evil. They are hardly used at all by the Metropolitan Police; only in one or two special instances, where a judge has ordered a case to stand over to an ensuing sessions for the purpose, have untried prisoners been photographed and "routed." If the Metropolitan Police were to issue them as some borough forces do, police officers and prisons would be overwhelmed with the number which they would receive. Even as it is we have received complaints, particularly from Liverpool, of the great waste of time involved in examining route forms indiscriminately sent them by certain forces.

Inadequacy of the Existing Methods of Identification.

We are next to consider whether any necessity exists for a radical change in the methods of identification of which we have given a brief account above.

The necessity for such a change, if the necessity exists, might be assignable to three causes: that is to say, good ground for change would be established if it were shown either (i.) that under the present system it is possible for a prisoner to suffer undeservedly through a mistake of identification; or (ii.) that a considerable proportion of old offenders are successful in concealing their identity when arrested for a fresh breach of the law; or (iii.) that, even if they are identified, the process of identification is more slow and cumbersome than necessary.

(i.) *Mistakes in Identification.*

If the first contingency were found to be probable, it would, as Sir Richard Webster has urged in his evidence, supply by far the strongest argument for a change of system. On this point it is not easy to obtain positive evidence. In any system which depends, as the English system does, on personal recognition, there must be some possibility of error arising from defective or confused memory on the part of the warder or constable who undertakes to say that a prisoner is the same man whom he had in custody for another offence some months or some years before; but, on a careful consideration of the evidence available, the safeguards in the existing system appear to us to have been sufficient either to prevent such errors or to secure their correction in time. With one possible exception (Callan's case mentioned below), no instance of any prisoner having actually undergone additional imprisonment through a previous conviction being erroneously imputed to him has been brought to our notice. In the Home Office records which we have carefully examined, we have found a certain number of cases in which prisoners have, in the first instance, been credited with offences of which they had never been convicted. These mistakes, however, seem to be rare; they are due to a faulty memory on the part of some constable or warder or to some want of proper care, and in every case they have been corrected before the prisoner suffered actual prejudice. In none of them has there been any serious difficulty in ascertaining the truth on inquiry through the means afforded by the existing system, and we do not think there is any ground for fearing that there are at present any persons suffering in consequence of a mistake of this kind. If there were, it is certain that they would complain both in petitions to the Secretary of State and by word of mouth to the Inspectors in their visits to the prisons, but it is the fact that among the thousands of petitions which are received from persons who profess to consider themselves unjustly convicted or too severely punished, it is rare to receive any representation that a previous conviction has been wrongly attributed to the prisoner, and equally rare for an Inspector to have such complaint made to him. When any such complaint is made, conclusive evidence to settle the question of identity can usually be obtained without much difficulty. On this point Mr. Murdoch's evidence (page 63) only confirms the opinion to which our own inquiries have led us.

It may make this matter clearer, however, if we give particulars of the most striking cases of mistaken identity that we have been able to dicover; but to prevent misapprehension we should first point out that nine-tenths of the cases of "mistaken identity," commonly so-called, fall altogether outside the scope of our inquiry. We are merely concerned with the machinery by which habitual criminals, when charged with a fresh offence, may be recognized. If a witness is mistaken in believing that a prisoner is the individual who was seen on a certain day to do a certain act, such a mistake is not one that can be prevented by the French method or by any other imaginable method of identification. The cases we are about to mention are all of recent date: older ones could be found but they are of less importance for the present purpose because they occurred before the existing safeguards against mis-identification were established. We also omit certain cases where a prisoner has acquiesced in an erroneous identification, knowing his own previous record to be worse than that of the man for whom he was mistaken.

The first case we have noted as bearing on our inquiry is that of a man named James Coyle, who was convicted at the Clerkenwell Sessions on the 4th June 1889, of larceny from the person. A warder from Millbank swore to his having been previously convicted under the name of Hart in 1879. The prisoner denied this in court; and, though the jury after having seen Hart's photograph and description gave a verdict in accordance with the warder's evidence, sentence was postponed and further inquiry made. It was then found that the distinctive marks ascribed to Hart did not tally in all respects with the prisoner's, and that Coyle had been, as he alleged, serving a short sentence at Lewes at the time when Hart was in penal servitude.

Coyle, when brought before the police magistrate as Hart, had not denied the identification, reserving his defence till the trial. There can be no doubt that if it had not been supposed that on being charged he admitted the identification a sufficient number of warders and others who were well acquainted with Hart would have been confronted with the prisoner before the trial, and there is no reason to think that they would have made the mistake that was made by the one warder called as a witness.

The second case is that of a woman giving the name of Eliza Witchurch, who was convicted at Gloucester on the 23rd November 1891, of burglary. She had been arrested by the County Police six days before, and as she was not known, descriptions of her had been sent to the Police Gazette, the Metropolitan Police, and other forces. From information received from the police at Ilminster it was thought that she was a woman who had been convicted of stealing at Southampton in December 1889, under the name of Elizabeth Smith, and of burglary at Poole in October 1890, under the name of Elizabeth Clode. She repudiated neither of these convictions, and the latter was formally proved in court by a police officer from Poole. But from other information received from Scotland Yard, Gloucester and Newport, she was thought to be also one Eliza Rolfe, against whom no less than eight previous convictions for larceny and other similar offences were recorded. The photographs and descriptions of the two women corresponded pretty closely, especially in the particular that both had lost the left breast; and, though the shortness of the interval between arrest and trial left no time for the identification to be verified by witnesses personally acquainted with Rolfe, the prisoner was credited with Rolfe's convictions and they were entered in the list which the judge had before him when he passed sentence. After she was lodged in prison, while admitting the convictions in the names of Smith and Clode, she protested in a petition against the identification with Rolfe, and inquiry being made, it became clear that in fact she was not Rolfe. As the list of previous convictions had affected the amount of punishment awarded, the sentence was reduced at the request of the judge from seven years' penal servitude to six months' imprisonment.

In February 1892, a man giving the name of Henry Hodgson was charged before the Liverpool magistrates with "frequenting." The clothes that he was wearing showed that he had recently come out of a convict prison, and he was believed to have come from London. The police on searching the list of discharged convicts identified him with one Thomas Franklin, a licence-holder, whose photograph and description were found to correspond with the prisoner's. After his conviction and committal to Liverpool Prison, when the question of revoking his licence arose, the prisoner strongly denied being Franklin. The police, being so informed by the Governor, made further search, and seeing in the Police Gazette notice of one Peter Connor a licence-holder, obtained Connor's descriptive form from the Salford Police. On being confronted with this evidence, Hodgson admitted being Connor, who is an experienced and dangerous criminal, and who had been discharged on licence on the 19th January into the Metropolitan Police district and had notified his intention of going to Manchester. The prisoner therefore gained nothing by the correction of the mistake; in either case he was an habitual criminal and in either case a licence-holder whose licence was liable to revocation on account of his recent conviction.

Another case of mistaken identification has been referred to by Mr. Spearman in his evidence—that of David Callan or Callaghan, charged at Westminster Police Court on 6th May 1889, with begging from house to house in Wilton Crescent. A police constable positively identified him as one William Minson, who was convicted two years before of being drunk and disorderly and who had 11 convictions of begging and similar offences. At the police court an officer of the Mendicity Society proved these convictions, and a warder from Chelmsford Prison is also said to have attended to prove his identity but was not called. The prisoner was convicted under the Vagrancy Act as an incorrigible rogue—a conviction which depends on evidence of previous convictions—and was sent to the North London Sessions where he was sentenced to six months hard labour. In a petition to the Home Office he protested that at the time of the convictions attributed to him he was an inmate of St. George's Workhouse, Westminster. Inquiry at the workhouse showed the truth of his statement, and he was at once discharged from prison. In this case the mistaken identification led to a conviction which could not otherwise have taken place; but it is proper to add that Callaghan was subsequently convicted as an incorrigible rogue, and there is good reason for believing that in May 1889 the mistake was not the conviction itself, but the conviction under a wrong name.

The last case we shall cite is also in our opinion the one that might most naturally excite doubt as to the sufficiency of the existing safeguards against an erroneous identification. In the early morning of 4th May 1893, one Percy Albert Blake was found in a pawnbroker's shop in the Strand in circumstances which pointed strongly to a deliberately planned burglary; the prisoner gave a rambling and incoherent account of himself, and it became evident that he was either a lunatic or a clever and dangerous burglar. The police constable who arrested him, on searching the collection of photographs at the Convict Supervision Office, believed that he could be identified with one Henry Steed, alias John Blake, who had been convicted of attempted burglary in July 1881, had failed to comply with the conditions of a sentence of police supervision then passed on him and had not since been heard of. Inquiry was made of persons who had known Steed, and they agreed that the present prisoner was the same man; and evidence to this effect was given at Bow Street by the police constable who had charge of the case against Steed in 1881, by another constable who was present at Steed's conviction, by a retired police constable who had also had to do with the case, and by a retired prison-warder who had seen Steed daily at Pentonville while he was serving the sentence of 18 months' imprisonment passed on him in consequence of the conviction in 1881. On the other hand, the description in the Convict Office of Steed stated that Steed's right leg showed signs of a fracture. At Bow Street a surgeon gave evidence that there were no signs of a fracture on either of P. A. Blake's legs, expressing the opinion however that all signs of a fracture may disappear in the course of time. There was also evidence given on the prisoner's behalf showing that he could not be the man who was convicted under the name of Steed in 1881. Blake was committed for trial on 25th May: the prosecution was undertaken by the Treasury and careful inquiries were at once made in various directions to test the truth of the statements made in his defence. As the result of these inquiries, evidence was obtained of Blake's having been at liberty at the time when Steed was undergoing the sentence passed on him in 1881; and at the trial on 29th June no evidence being offered for the prosecution, Blake was acquitted and he was afterwards sent to a lunatic asylum. Subsequent investigations left no doubt that Blake was entirely innocent of the previous convictions at first ascribed to him.

In this case the personal description of Steed was too vague to establish an identification even had there been no discrepancies, and too uncertain to prevent a mis-identification when discrepancies occurred. The mistake arose either from a very strong resemblance between P. A. Blake and Steed, or from a very defective recollection on the part of no less than four persons, and it was only corrected by what may be described as evidence of an alibi—evidence which might not be available if a similar mistake were to occur in another case.

All these cases, and especially the last one, although in each of them the mistake was corrected in time, do undoubtedly suggest the possibility that another case of mistaken recognition might occur which the record of distinctive marks or the evidence of an alibi might be insufficient to correct. We cannot say that we have found any case in which the existing safeguards have failed to prevent a convicted prisoner suffering unmerited punishment; but we think it must be admitted that if a new method can be devised which would afford an absolute safeguard against such mistakes in the future, it would be a great gain to the administration of justice.

(ii.) *Failures to identify old Offenders.*

The second ground on which the existing system may be deemed insufficient is its failure to secure in every case, or in the majority of cases, the identification of old offenders; in other words, a new system may be required to afford a more perfect means of acquiring knowledge of each offender's antecedents.

And here we feel bound to mention an objection which we have heard several times in the course of our inquiry:—"Of what use is it," we have been asked by chief constables and others, "to elaborate a system whereby the previous convictions of a "prisoner charged by the police may be accurately recorded and brought to the notice "of his judge, if they are to be altogether overlooked by him in passing sentence? It "is vain for us to exert ourselves to discover the history of offenders, if no difference "is to be made between a hardened criminal and a first offender, or at any rate none "between the man who has deliberately set himself to gain a living by crime, and "one whose lapses are not the outcome of a settled design, but are merely occasional "and due perhaps to momentary impulse." We have heard this objection repeatedly, but we confess it has not influenced our judgment as to the importance of securing accurate identifications. We are satisfied that most judges, recorders and chairmen of quarter sessions not only distinguish between first offenders and old criminals but are ready to make allowance even for a man who has more than once relapsed into crime, if his record does not show that he has adopted crime as a regular means of livelihood. If no record, or a defective record, were kept of a prisoner's previous convictions it would become impossible to make these distinctions—the average sentence might be less, but the distinction in favour of the first offender or the occasional criminal could not be maintained. We are certain that the majority of judges and chairmen of quarter sessions would object, and rightly object, if the police made the occasional leniency shown to old offenders an excuse for relaxing their efforts to supply the court before sentence is passed with an accurate history of each prisoner's judicial antecedents, and that a failure on their part to carry out this duty would be detrimental to the fair administration of justice. On the other hand if any improvement can be made by which the antecedents of prisoners can be more easily and more accurately ascertained, it will be easier and safer for judges to discriminate in favour of the less criminal portion of the offenders on whom they have to pass sentence.

Indeed, we ourselves would venture to go further than this, and to look forward to a time when an even more marked distinction may be made between different classes of criminals. When experience has at last shown that on a certain class of criminals long sentences and short sentences fail equally to produce any reformatory or deterrent effect, we believe that the country and Parliament will be ready to make provision by which the incurably criminal may be treated in the same way as the incurably insane, and subjected, alike in their own interest and in that of the public, to some form of more or less permanent detention. As there are some criminals who ought never to be sent to prison, there are others who ought never to be released; and when this distinction is established, and provided for by legislation, it will be of even greater importance than at present to have an exact record of each criminal's offences.

Returning to the question of the sufficiency for this purpose of the present system, it appears to us that all the evidence which we have collected leads directly to the conclusion that, though it gives good results in a large number of cases, there remains a considerable number in which it fails. From the nature of the case positive evidence as to the number of habitual criminals who escape identification cannot be obtained, but the different police authorities whom we have consulted, though their bias would naturally be to minimise the proportion who escape, are fairly unanimous in thinking that an appreciable proportion of habitual criminals when arrested for new offences are not recognized.

At Liverpool it is thought that not more than 15 per cent. escape; at Nottingham, 10 or 15 per cent.; at Bristol, not more than 30 in a year; in the North Riding there are said to be "very few" prisoners whose antecedents are not made out. On the other hand the chief constable of Doncaster does not consider that the existing agencies are sufficient to ensure the majority of old offenders being identified, and believes that a large number of pickpockets arrested and dealt with summarily during race meetings are really old offenders who escape identification. The chief constables of Worcestershire, Newcastle-on-Tyne, Herts, Portsmouth, and Manchester are also agreed in thinking the existing means insufficient. These may be taken as fairly typical examples of such opinions as we have elicited on this subject, but we have little doubt that in small boroughs the proportion of habitual criminals whom the police fail to identify is considerably larger than in the more important forces from which we have mostly derived our information.

More interesting, perhaps, are the figures collected in Appendix A. 2, page 70, with regard to route-forms, and the results obtained by them. Thus at Doncaster during the year 1892 out of 407 persons who were arrested for all classes of offences, and of whom 272 are said to have been strangers to the borough, 46 were "routed"—28 before conviction and 18 after—30 were recognised as having been previously convicted, and 16 were not recognised. In Manchester out of 41 route-forms issued with photographs attached, during the twelve months ended 31st October 1893, 22 led to the suspected persons being identified as old offenders. In Newcastle-on-Tyne, during the same period, of 500 adults proceeded against for serious offences, 395 resided in the city, 80 belonged to the adjoining counties, and 25 came from further off; 23 prisoners were "routed," and of these 12 were traced. At Nottingham, during 1892, out of 58 prisoners "routed" 43 were identified by other police forces. Of 474 persons arrested at Scarborough during the last five years, 361 belonged to the district; and of 70 prisoners "routed," 34 were identified.

In Somerset, during the year ended 30th September last, of 515 persons arrested for offences involving dishonesty, 303 were natives of the county or permanently resident in it. Of 12 persons "routed," 6 were identified. The remark of the chief constable that these figures showed a higher per-centage of "foreign" crime than he had anticipated, is, we think, of some significance. It appears to us probable that police authorities are somewhat inclined to underrate rather than to overrate the number of "travelling thieves" who are to be found in different parts of the country; and, though this is only a matter of conjecture, we should not be surprised to find a perfected system of identification demonstrating that the number of the criminal classes is smaller than is commonly supposed, but showing at the same time that increased facilities of communication have led to an increased activity in the operation of the "travellers."

In forming any inference from these figures it must be remembered that, in general, route-forms would only be sent out in the case of prisoners whom, from their general demeanour or their mode of procedure, there is good reason to suspect of being old offenders. Though, no doubt, some of the prisoners "routed," but not identified, had not in fact been previously convicted, it can scarcely be questioned that a certain proportion of the prisoners not "routed" were old offenders who were either not suspected of previous convictions, or were, in consequence of the triviality of their offence, dealt with summarily without any investigation of their antecedents.

A further indication of the inadequacy of the present system is afforded by a list supplied to us by the Prison Department in answer to our inquiry as to how many person were recognised only after reception in convict prisons as being old penal servitude men. The list gives five instances of this occurring in the year ended 31st October last, and, in addition, two instances of persons summarily convicted being recognised after conviction as licence-holders. Though too much importance must not be given to individual cases, we think that, in view of the plain impossibility of ascertaining, even approximately, the total number that escape, it may be of interest to state briefly the particulars of these seven cases.

The first convict in the list was sentenced at Derby in April 1892, to seven years penal servitude for larceny from a dwelling-house. He was then known to have been twice previously convicted of larceny in Ireland in 1887, but of his history previous to that date nothing appears to have been known. In January 1893, on removal to Portland Convict Prison, he was recognised as a man who had been convicted of theft at Durham in 1879, and Glasgow in 1880, when he had received a sentence of seven years penal servitude. From the statements he has since made, he appears to be an old and experienced housebreaker.

No. 2 was convicted summarily, and had a sentence of three months at Southwark Police Court in December, 1892, for attempting to pick pockets. He was not at that time known to the Metropolitan Police, but in the January following, from information received, he was discovered to be an old offender several times convicted of theft at Birmingham, Norwich and elsewhere and given a life sentence at Leicester in 1877 for a murderous attack on a policeman. He had been released on licence in November 1892, and had gone to Manchester, but he left that place the same month, and coming to London failed to report himself to the police in pursuance of the conditions of his licence.

No. 3 was sentenced to three years penal servitude at Stafford for several burglaries. No previous convictions were recorded against him, but at Lewes Prison the Governor found some reason to suspect him of being an old offender. By means of a route-form he was ascertained to be a man who had previously received as many as three sentences of penal servitude—at Knutsford, Stafford and Wakefield—besides other minor sentences for burglary and similar crimes.

No. 4, after being 13 times convicted summarily during 1890, 1891 and 1892, was sentenced at Kendal Quarter Sessions, in October, 1892, to three years penal servitude. In August 1893, on his removal to Parkhurst Prison, he was recognised as a thief who had been sentenced to 12 months imprisonment at Northallerton in 1882, and five years penal servitude at Newcastle-on-Tyne in 1885.

No. 5 was a London thief who had been discharged on licence in July 1893 from his second sentence of penal servitude. The same month he was found attempting to pick pockets at Goodwood Races and was sentenced to six weeks imprisonment. It was only after his conviction that he was identified.

No. 6 was known to have been convicted at different times during 1888–92 at Maidstone, the Central Criminal Court, Leicester and Newington Sessions of housebreaking and kindred offences. When at Wormwood Scrubs, in September 1893, he admitted being a man who was sentenced at Exeter in 1880 to five years penal servitude for burglary.

No. 7 was given five years penal servitude at the Central Criminal Court in January 1893. It was known he had been twice previously convicted there in 1891 and 1892, but it was only after he was removed to a convict prison that, in order to repudiate another conviction of a much less serious character which had been attributed to him, he disclosed his identity with a burglar who had been frequently convicted in London prior to 1891. At the time of his conviction at the Central Criminal Court in 1891 he was wanted by the London police for having failed to conform to the conditions of the ticket-of-leave granted under a sentence passed on him in 1886.

The two cases in this list (2 and 5) of men convicted summarily, and afterwards found to be convicts under sentence of penal servitude, and at the time of their conviction at large on licence, are typical instances of a class of cases constantly coming to the notice of the Criminal Department of the Home Office, as in every such case the question of the revocation of the convict's licence is dependent on the decision of the Secretary of State. Several cases of this sort are quoted in Mr. Murdoch's evidence (page 64), and one of them is so remarkable that it seems to deserve special mention here. A convict under sentence of five years penal servitude, passed at the Middlesex Sessions, was released on licence in London in August 1892; in October he was brought up at the Mansion House Police Court for stealing a watch, and not being recognized was convicted summarily and sentenced to six weeks imprisonment. On reception into Pentonville his identity was discovered, and the Secretary of State thereupon revoked his licence. In ordinary course he was released on a new licence in August last. In

November he was again charged at the Mansion House Police Court, again treated as a first offender, and sentenced on this occasion to a term of three months imprisonment. Only on reception into Pentonville were his real name and antecedents again brought to light.

This case seems strikingly to confirm the evidence given by Mr. Douglas, Justices' Clerk at the Mansion House, as to the difficulty of obtaining identifications in London. It also supports an opinion expressed by Mr. Grace that it is in London that the largest number of failures to identify old convicts occur. In order to elucidate this point further, Mr. Farrant, of the Statistical Department of the Home Office, has at our request prepared a comparative table of the number of cases in which persons tried on indictment in London and in one or two groups of counties and boroughs, had previous convictions recorded against them in the prison calendars. For comparison with London we selected three populous counties, three large industrial towns, and two groups of counties where the population is mainly agricultural. The table, printed on page 72, shows that during the first three months of the present year in Lancashire, the West Riding of Yorkshire and Staffordshire about 70 per cent. of the prisoners tried were known to have been previously convicted; in Liverpool, Birmingham and Bradford, 79 per cent.; and in Norfolk and Suffolk, 61 per cent.; while in London the proportion was only 47 per cent.* It is impossible to suppose that the proportion of habitual criminals in London is smaller than in the other districts just mentioned; and the figures appear therefore to point irresistibly to the conclusion that in London the proportion who either escape arrest or when arrested escape identification is larger than in other districts. But, if this is the case, it cannot be stated too clearly that the reason is not any want of energy or ability on the part of the London police, but solely the incomparably more difficult problem with which they have to deal. In other places there is, speaking generally, no difficulty in dealing with the local criminal; it is only "travelling thieves" and the immigrants from other districts that are likely to escape identification, and these are not very numerous: in London local criminals have the same advantages for concealment that "foreigners" have in other places, while the proportion of "foreigners" is reasonably believed to be much larger. On the causes of this, the immense population within the area, its shifting character and the impossibility of any officer acquiring personal knowledge of more than a few criminals, we have already dwelt sufficiently; and it is only to be expected that in a system which depends so much on personal recognition the results obtained in London should be less complete than elsewhere.

(iii.) *Labour involved in the present System of Identification.*

But if the existing system leaves something to be desired in the completeness of the results obtained, there is, it appears to us, not less room for improvement in the working of its machinery. No one, however favourable the view he may take of the present system on the somewhat uncertain question of the proportion of old offenders recognized, would venture to say that the recognitions are obtained easily and without the expenditure of much labour. Thus, in using the Habitual Criminals' Register, when a prisoner has some special and unique mark, his identity may perhaps be discovered easily; but according to all the evidence we have received, the use of this register in ordinary cases is extremely laborious and it appears to be mainly on account of the time and labour which the searches involve, that it is generally so little used by the police. In Scotland Yard the process of identification is somewhat more easy as regards the searching of the Mark Registers, but an enormous amount of time is spent in examining the books of photographs. It will be seen from the figures furnished by Chief Inspector Neame that on the 1st March last 21 officers searched for 27 prisoners—the total time spent being 57½ hours—and made 7 identifications. This was on average of more than two hours for each prisoner sought for, and more than eight hours for each identification.

The inspection of unconvicted prisoners at Holloway no doubt serves other uses and will have to be continued whatever new system may be introduced; but, viewed merely as a process of identification, it involves a large amount of labour for each recognition effected by the police. Thirty police officers attend there three days in each week, and in each week they obtain on the average about four identifications, not including those that are also made by warders; allowing four hours for the time spent by each officer at Holloway and in going and returning to his division, this means that the amount of detectives' time spent on each identification is about 90 hours.

* In the group of counties consisting of Dorset, Devon, and Cornwall, the percentage, as might be expected, is smaller than elsewhere.

In the case of route forms, which are looked on with so much favour by the country police, the work is distributed among a great number of officers of different forces, but it appears to us that if the whole amount of labour involved in each case could be summed up, this method would not show any economy of time and trouble as compared with the other methods and with the results achieved. It is plain that if a "travelling thief" contrives to conceal all indications of his place of origin, and does not carry on his person any very distinctive and indelible marks, the only way to trace his identity is to disseminate route forms broadcast among police and prison authorities. The shorter the interval between a criminal's arrest and his trial, the greater the difficulty of identification becomes, and the greater the necessity for multiplying the forms sent out. Inspector Reeve, of the Doncaster Police, mentioned to us a case in which route forms were issued by him to more than eighty places. One of these was recognised as far away as Swansea; the remainder merely served to give fruitless trouble at the places where they were received. This, perhaps, was an exceptional instance; but in any case where a zealous and industrious police force have a prisoner in custody, whom there is good reason to suspect of being a practised criminal, and whom there are no other means of identifying, the labour expended on the issue of route forms may be increased to an almost indefinite extent. Zeal and industry turned in this direction call for a corresponding amount of zeal and industry on the part of the people to whom the inquiries are addressed, and as we have already mentioned complaints on this score have reached us especially from Liverpool. When it is considered that in order to identify what he considers a "good" criminal, a police officer will often spend hours and sometimes days in searching the photographic albums at Scotland Yard, the labour involved by the route form method of procedure becomes apparent.

Nor do the suggestions made to us by different police forces tend for the most part towards the simplification of the machinery or the saving of labour. The point most generally urged is that regulations should be made for the photographing of unconvicted prisoners. This, as we have already intimated, we believe to be a very important amendment in the prison rules; but we cannot disguise from ourselves that its object is to increase the issue of route forms, and consequently to multiply the amount of time and labour devoted to their examination and circulation; and, as we have already said, we can recommend the increased use of photography only in connexion with a much more extensive change of method. Again, it has been urged repeatedly that it is highly desirable that distinctive marks should be registered more precisely, and in more detail, with measurements showing their size and position. Clearly this will give extra trouble to the warders to whom this duty falls, and, as will be seen hereafter, we can only recommend it if it be limited to such marks as are really clear and distinctive, and if the labour-saving devices adopted in France in the way of arrangement and abbreviations be adopted. We think an official list of abbreviations to be used in this service should be made out, such as *r* for "right," *l* for "left," *sc* for "scar," *f* for "finger" and so on. Each finger and each joint should be indicated by a number; conventional signs might be used to represent the forms and directions of scars or other marks, and a small rule should be employed to ascertain readily, for the purpose of registration, the distance of such marks as are recorded, from well-defined points of the body. With clear instructions no difficulty in reading the personal description of a prisoner should be experienced by warders, police officers and others occupied in the work of identification.

But even with more photographs and more exact descriptions we are agreed that the present system will leave much to be desired. What is wanted is a means of classifying the records of habitual criminals such that, as soon as the particulars of the personality of any prisoner (whether description, measurements, marks or photographs) are received, it may be possible to ascertain readily and with certainty whether his case is already in the register, and, if so, who he is. Such a system is not, we believe, attainable merely as a development of the existing English methods; if it is to be found at all, it must be found in the application of some such scientific method as those on which we have next to report.

The Bertillon System of Identification.

We deal first with the system of identification invented and carried into practice by M. Alphonse Bertillon. In order to study this system we visited Paris and saw the process in operation at the Prefecture of Police. We wish to take this opportunity of tendering our thanks to M. Lépine, the Prefect of Police, and M. Goron, the head of the

Service de Sûreté, for the courtesy with which they received us, and especially to M. Bertillon for the care with which he explained to us the details of the practical working of his system. Mr. E. R. Spearman, J.P., who has studied the Bertillon system during several years' residence in Paris, and who by his writings has done more than anyone to make it known in England, has, while we were in Paris and during all our inquiry, rendered us every assistance in his power, and we feel that to him also our thanks are due. Besides studying the system in Paris, we visited the prison at Pontoise in order to see the manner in which the measurements are taken in provincial prisons. We have further consulted eminent experts in anthropology in England on the scientific aspects of the system, and on this point would refer particularly to the evidence of Dr. Garson and Professor Thomson.

The principles and arrangements of the Bertillon system, or "Bertillonage" as it is conveniently called, are so well known that it will be sufficient for us to deal with them very briefly. They are most fully stated in the introduction to M. Bertillon's "Instructions Signalétiques" (Melun, Imprimerie Administrative, 1893); and an interesting account of the organisation of the Service d'Identification will be found in a pamphlet published as one of the Bibliothèque d'Anthropologie Criminelle et des Sciences Pénales bearing the title "L'Anthropométrie Judiciaire à Paris en 1889." (Paris: G. Steinheil, 1890.) In England the most complete accounts of the system of which we are aware are articles by Mr. Spearman in the "Fortnightly Review" of March 1890, and in the "New Review" of July 1893.

The principle of the system may be stated in a very few words. A record of each prisoner has first to be taken consisting of certain measurements which depend mainly on the length of bony structures in the body and may therefore be treated for practical purposes as invariable in adults; the cards on which these particulars are recorded are so classified that each can be found by means of the measurements and without the name of the person; and then by taking the measurements of any person who is arrested, it is possible to ascertain his identity if he is already included among the records under any name whatever. The special features of this system are the choice of the measurements to be taken and the mode of classification.

The measurements taken by M. Bertillon are the following:—

Height,
Span of arms,
Height of trunk (sitting height),

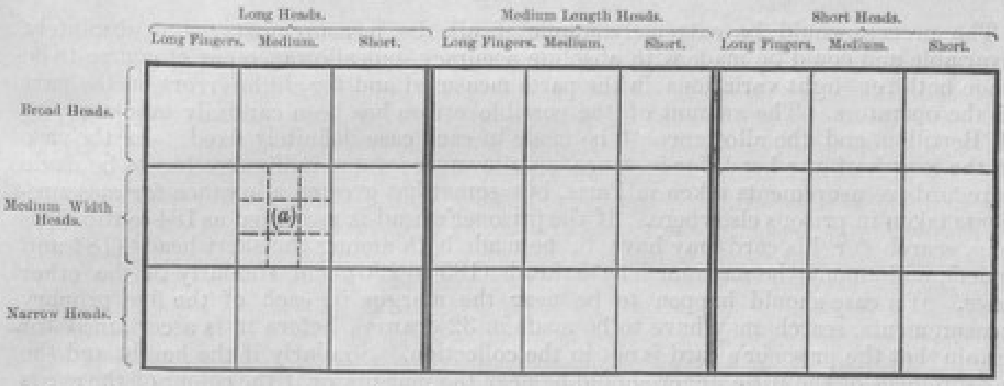
Length of head,
Width of head,
Length of right ear,
Width of right ear,

Length of left foot,
Length of left middle finger,
Length of left little finger, and
Length of left forearm.

In addition to these measurements the colour of the eye is noted. The measurements of each person on the first occasion when he passes through M. Bertillon's office are noted in duplicate on two cards, one of which is placed in an alphabetic register and the other in the "Anthropometric" or classified register.

The five most important measurements which form the basis of the classification in the Anthropometric register are the length and width of the head, the length of the left middle finger, the length of the left foot, and the length of the left forearm: these being selected as the most constant in each individual, the most varied in different persons, the least correlated to one another and the easiest to take accurately. The mode of classification, which is the most admirable part of M. Bertillon's system, is described by him as follows. He assumes that he has to classify in one cabinet the cards on which are recorded the measurements of 90,000 adult male prisoners. These he first divides into three classes according as the heads are "long," "of medium length," or "short." The measurements distinguishing long from medium and medium from short are selected so as to give approximately equal divisions of 30,000 cards each; the medium length is from 185 to 190 millimetres, heads of 191 or upwards are long, those of 184 or under are short. Each class of 30,000 is then subdivided into three classes of 10,000, according to the width of the head, the numbers distinguishing broad, medium and narrow being again so selected as to give equal classes.* Proceeding in the same way, by the length of the middle finger each class of 10,000 is subdivided into three classes of 3,300; by the length of the left foot each class of 3,300 is subdivided into three classes of 1,100; and by means of the cubit or forearm we reach a subdivision in which each class is less than 400. This is a primary classification; we start with the cards of 90,000 criminals, and using each of the five measurements for the purpose of tripartite division we obtain 243 classes approximately equal and each containing on an average less than 400 individuals.

This classification is carried out in M. Bertillon's office in a cabinet which is first divided vertically into three divisions for long, medium and short heads; each of these divisions being divided horizontally into three sections for broad, medium and narrow heads, and again vertically for long, medium and short fingers. We thus have a cabinet divided into 27 sections as shown below:—



Each of the twenty-seven sections is then—as shown by dotted lines at (a)—divided into three shelves, in which the cards are arranged according to the length of the foot, the upper shelf containing the "longs," the middle the "mediums" and the lower the "shorts." Finally, in each shelf there are three drawers containing respectively the cards in which the forearm is long, medium and short. There are thus 243 drawers, each containing one class of cards numbering about 400; and, when the five measurements of any prisoner are ascertained, the drawer in which his card will be found can at once be determined.

A further subdivision of the cards in each drawer is next required, and this is given by the height, the length of the little finger, and the colour of the eye. By the height the cards in each drawer are divided into three divisions of about 140 each; by the measurement of the little finger each division of 140 is divided into three groups, which are not quite equal—the length of the little finger being too closely related to that of the middle finger to allow of the three divisions being made equal. Finally each of these groups, the largest of which contains about 60 cards, is divided by the colour of the eye—seven different colours being recognised—into parcels, containing on the average seven cards each, but varying in actual number from 3 or 4 to 15 or 20.

Let us see how this classified cabinet is used. All the prisoners arrested in Paris and the Department of the Seine on any criminal charge are brought every morning to the depôt of the Prefecture of Police, and are there passed to M. Bertillon's bureau for measurement. A prisoner is first asked whether he has previously been measured, and if he admits that he has and gives his name, the card containing his measurements is obtained from the alphabetical register, and a few of the measurements taken in order to check the identification and prevent the exchange of names which is sometimes attempted. If he denies having been previously measured, his measurements and description are taken in full, that is to say, the 11 measurements given above are taken, the colour of the eye is noted, the shape of the ear and nose are also noted

* Owing to a slight correlation between the measurements, the numbers selected are different in different divisions. Thus in the division for long heads, the limits of the medium width are 155 and 159 millimetres; in the division for heads of medium length they are 154 and 158, and in the division for short heads, 153 and 157. The same is true of the other measurements and the difference is more marked, but the figures arrived at by M. Bertillon are too complicated to be given here. They will be found in Mr. Spearman's article in the "New Review" of July 1893, and show that M. Bertillon's measurements, though not closely correlated, are by no means altogether independent of one another.

(unless a photograph is taken), and the distinctive marks in different parts of the body are recorded with great exactness, the precise position and direction being stated, and their distance from certain fixed points measured. Search is then made in the classified cabinet to ascertain if there is a card containing his measurements taken on the occasion of a previous arrest. By means of the five primary measurements mentioned above the drawer in which his card will be found, if he has been previously measured, is first fixed; then by the height, length of the little finger and colour of the eye, the particular group of ten or a dozen cards in which his card will be placed, is determined. This group of cards is searched one by one, and by comparing the measurements just taken with those on each card in succession, his former card, if it exists, is picked out without difficulty. If it be found, the identification is verified, first, by means of the distinctive marks noted in both cases, and, secondly, by means of the description of the features or by the photograph. In this way an identification can be established which leaves no room for doubt, the correspondences being too close and too minute to be the result of chance.

The process would be extremely simple if all the measurements were absolutely invariable and could be made with absolute accuracy, but allowance has of course to be made both for slight variations in the parts measured and for slight errors on the part of the operators. The amount of the possible errors has been carefully tabulated by M. Bertillon, and the allowance to be made in each case definitely fixed. In the case of the length of the head, for instance, an allowance of one millimetre has to be made as regards measurements taken in Paris, but somewhat greater allowance for measurements taken in prisons elsewhere. If the prisoner's head is measured as 184 centimetres long, search for his card may have to be made both among the short heads (184 and under), and among the medium length heads (185 to 190), and similarly in the other cases. If a case should happen to be near the margin in each of the five primary measurements, search may have to be made in 32 drawers before it is ascertained for certain that the prisoner's card is not in the collection. Similarly if the height and the measurement of the little finger should be near the margin, or if the colour of the eye is transitional between two classes, duplicate searches may have to be made in each drawer. These duplicate searches necessarily occur in a large proportion of cases and add materially to the labour of using the register, as it is of course impossible to say that the card is not in the register until all the drawers and groups of cards in which the card sought for may possibly be found, have been exhausted. When the searches are numerous they are made in fixed order which secures that they are complete, and though they add to the labour of using the bureau, they are essential to secure accuracy in the results.*

There is a separate cabinet for women prisoners, arranged in the same way, but as the number is much smaller the classification is not carried quite so far, and distinctive marks on the face and hands only are noted.

There is also a separate cabinet for lads under 20, in whom the parts that are measured are still subject to growth, and the measurements therefore not final. The lads included in this cabinet are all photographed, and more reliance has to be placed on the photograph and marks than on the measurements in making identifications. It has also to be noted that, in dealing with adult male prisoners, M. Bertillon has now adopted a preliminary classification according to age, persons born in successive periods of 15 years being placed in separate cabinets. This of course often makes a double, sometimes a triple search necessary, as prisoners who give false names are ready to falsify their ages and often do so by many years; on the other hand it will in future greatly facilitate the weeding out of the cards of dead criminals and prevent such an accumulation of useless records as would become in time inevitable if all the cases were placed in the same collection.

It seems clear that in theory this system is perfect. If all the measurements were absolutely invariable and were taken with absolute accuracy, then, after measuring a prisoner, the card containing his previous record if it existed would be found at once and with certainty, and if not found in a particular set of cards in a

* "Ces explorations *limites* allongent considérablement les recherches lorsqu'elles portent à la fois sur plusieurs mesures. Elles demandent alors à être dirigées dans un ordre constant et en suivant une loi mécanique de combinaisons qui a reçu des employés spéciaux qui en sont chargés le nom caractéristique de *recherches doubles*. C'est en elles que réside la seule difficulté de l'identification anthropométrique. Elles incombent d'ailleurs uniquement au personnel du service central. Les résultats obtenus en dix ans de pratique ont démontré que l'obstacle était aisément surmontable."—(*Instructions Signalétiques*, ed. 1893, p. xxiv.)

particular drawer it would be known that the case was not in the register. Further, for a certain margin of error arising either from variation in the parts measured or from a failure on the part of the operator, provision can be made by process of double or multiple searches. There still remains, however, the practical question whether the efficacy of the system is vitiated either by not allowing sufficient margin for necessary errors and variations, or by carelessness on the part of employees in making gross errors of measurement, in misplacing cards in the register or in making insufficient searches. We satisfied ourselves that in all these particulars precautions had been taken by M. Bertillon, the margins of variation which he gives are the results of many years' experience,* and checks are provided to secure the accurate putting away of cards and the thoroughness of the searches. And so far as we can form an opinion of the statistical results of his system, we find reason to think that the precautions are sufficient.

We take first the figures of the number of measurements and of identifications made in Paris :—

(1.) Year.	(2.) Total Measurements taken.	(3.) Identifications of Persons giving false Names.	(4.) Re-measurements of Persons admitting their Identity with Persons on Register.	(5.) New Measurements.
1885	14,965	424	4,040	10,501
1886	15,708	352	4,694	10,657
1887	19,150	472	6,347	12,331
1888	31,289	527	14,465	16,297
1889	34,515	622	17,585	16,308
1890	34,328	614	19,517	14,197
1891	36,204	600	21,167	14,437
1892	40,312	674	25,448	14,184

At first sight the number of identifications seems small, but on reference to Column (4) it will be seen how large a proportion of persons previously measured admit their identity, and can be traced by their names. This is said to be due in a great measure to the efficiency of the Bertillon system—old offenders knowing that it is useless to attempt concealment—and this view is borne out by the fact that a considerable number, about 500 a year on the average of five years, have given false names on arrest but have confessed their identity on being brought to be measured in the Bertillon Bureau. However this may be, these figures give no basis of comparison with the numbers of identifications said to be made by the English police, as it is clear that for whatever reason a much larger proportion of re-arrested criminals give their true names in France than in England. In France only one "recidivist" in 15 gives a false name; in England the exact proportion cannot be given, but it is certainly much larger than this, probably a majority. Further, in England a prisoner cannot be questioned as to his criminal antecedents; so that even if he has given his true name, it still remains to identify him,—to show that he is the same person who was convicted under that name on some previous occasion. This is often a matter of considerable difficulty, and such identifications are included in the figures furnished by the English police. Clearly then there is in England a larger proportion of prisoners requiring to be identified; and if the English system makes any approach to efficiency, it must show a much larger proportion of identifications than in France.

The true test, however, of the efficiency of a system of identification is not the number of identifications made, but the number of mis-identifications or of failures to identify.

* With regard to the amount of the possible variation in the Bertillon measurements, reference should be made to the evidence of Professor Thomson (pp. 64–67), and to that of Mr. Galton (Q. 240–245, p. 59). It seems desirable that account should be taken, in dealing with the head measurements, of a somewhat greater range of variations than is admitted by M. Bertillon. This point is not, however, discussed in detail in the report, because the Committee are satisfied that it does not affect the principle of the system, nor interfere with its practical efficiency. The number of cases for which the margins allowed by M. Bertillon would not provide, must in any case be small, and if the system should be adopted in England, it will be easy with scientific advice to adjust the margins so as to provide even for exceptional cases.

As regards mis-identification, in England the most that can be claimed is that mistakes are very few, and that those which have occurred have been corrected either when the evidence of identification came to be examined in court or when special inquiries were made on the appeal of the convicted prisoner to the Home Secretary. In France, on the other hand, M. Bertillon claims that of all the identifications made in his office during the past 11 years not one has proved wrong. It is certain that though the opportunities of appeal which a prisoner enjoys in France are at least as ample as in England, every one of M. Bertillon's identifications has been confirmed by the final judicial decisions; and looking at the enormous safeguards which his system affords in the numerous measurements each of which must correspond within certain limits in order that an identification may be established, in the photographs or descriptions substituted therefor, and in the precision with which all distinctive marks are catalogued, we feel no difficulty in believing that under his system a mis-identification is practically impossible.

The other test is the number of cases in which failure to identify occurs. Of this we can only judge by the number not identified *before* conviction whose identity is *afterwards* discovered. In England we have no exact statistics on this subject; but some figures and examples have been given in an earlier part of our report which show, we think, conclusively that the number is considerable. In France the matter is put to a very severe test. In 1887, when it was decided to abolish the granting of rewards for identification, a "gratification" of 4,800 francs a year was granted to the assistants in M. Bertillon's bureau subject to a deduction of 10 francs to be paid as a reward to any prison warder who might identify an old offender who had escaped recognition in M. Bertillon's office. The result of this exacting test was that in 1889 of 31,000 persons measured in the bureau only 14 were afterwards recognised as old offenders, and of these 10 had never before been measured, and could not therefore have been identified by M. Bertillon's staff. There remained, therefore, only *four* failures to be set against M. Bertillon. In 1890 the number of failures was *four*, in 1891 *six*, and in 1892 *none*. These results, which we see no reason to question, appear to be eminently satisfactory.

So far we have dealt only with the results of the system as applied in Paris where it has been in operation for 11 years. Its application to France as a whole is yet in too early a stage to allow of our speaking of it in the same way. M. Bertillon freely admitted that in some of the prisons there were not yet any warders trained to perform the measurements with the requisite accuracy; and that a much greater margin for error is allowed for measurements taken outside Paris than for those taken at his own office. The country magistrates do not appear to have yet recognised fully the value of the system; and though he receives from the prisons the measurements of nearly 70,000 prisoners in each year, the number of cases in which M. Bertillon is asked to make identifications is comparatively small. It seems to us, however, that the results obtained in Paris, where the system has so long been in operation, are amply sufficient to show its practical efficiency, and the chief point of interest to us in its application to this country was to ascertain whether men of the class of prison warders can be trained to take the measurements with sufficient accuracy. M. Bertillon's reply to this question is that while he finds a certain proportion of those sent to his office for training to have no aptitude for the work, a large number can in a comparatively short time be trained to take the measurements with the necessary accuracy and that in time he would be able to provide an efficient staff at all the prisons. When we visited Pontoise, we formed a very favourable opinion of the work done by the warder employed in taking measurements in that prison.

It remains only to add that we find the opinions of all persons qualified to judge of the efficiency of the system by practical acquaintance with its working unanimous in recommending it. M. Goron, the head of the Paris detective police, and himself an officer of the greatest skill and experience, spoke to us in the strongest terms of its practical utility. Colonel Talbot, the military attaché to the British Embassy in Paris, reported in 1890 that he had studied M. Bertillon's system, and arrived at the conclusion that its adoption by military authorities in this country would "put an end to "fraudulent enlistment." We may refer also to the evidence of Sir Richard Webster (p. 48), who has made several visits to the office of M. Bertillon and who recommends the adoption of his method mainly because it would, in his opinion, afford a complete protection to an innocent man against any possibility of a wrong identification.

In Appendix D. will be found an account of the introduction of the Bertillon system in France and other countries.

The Finger Print System.

The second system on which we are specially directed to report is that now associated with the name of Mr. Francis Galton, F.R.S., though first suggested and to some extent applied practically by Sir William Herschel. In Mr. Galton's "Finger Prints," published by Messrs. Macmillan & Co. in 1892, a very full account of this system is given; but, as the author investigated the subject originally from the anthropological point of view, and was chiefly interested in its bearings on questions of heredity and racial distinctions, the book is likely to give a somewhat exaggerated impression of the complexity and difficulty of the method as applied to purposes of criminal investigation. A visit to Mr. Galton's laboratory is indispensable in order to appreciate the accuracy and clearness with which the finger prints can be taken and the real simplicity of the method. We have during this inquiry paid several visits to Mr. Galton's laboratory; he has given us every possible assistance in discussing the details of the method and in further investigating certain points which seemed to us to require elucidation. He also accompanied us with his assistant to Pentonville Prison, and superintended the taking of the finger prints of more than a hundred prisoners.

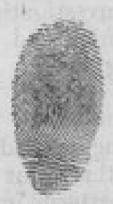
The materials on which Mr. Galton works are impressions taken from the bulbs immediately below the tips of the fingers and thumbs. The papillary ridges which cover the palms of the hands form at this point patterns of well-marked form and of a curious variety and shape; of these patterns impressions or "imprints" can be taken on paper or cardboard by means of printers' ink, so as to show the directions, terminations, and junctions of the ridges with much greater clearness than can be seen on the hand itself; and these imprints can be examined through a lens or microscope, or can be enlarged to any size by means of photography. The patterns and the ridges of which they are composed possess two qualities which adapt them in a singular way for use in deciding questions of identity. In each individual they retain their peculiarities, as it would appear, absolutely unchangeable throughout life, and in different individuals they show an infinite variety of forms and peculiarities.

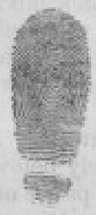
Both these qualities have formed the subject of special investigation by Mr. Galton; and having carefully examined his data, we think his conclusions may be entirely accepted. The persistence of the ridges and patterns has been proved by the examination of imprints taken from the fingers of various persons after intervals of years and minutely compared in every detail. The cases taken extend over the whole of life, from infancy to extreme old age, not of course in one individual, as no records are available of older date than 30 years, but the different cases taken together cover the whole period. In all the cases examined there was only one instance in which a minute detail was found to vary—a case where a ridge which bifurcated in an impression taken at the age of 2½ was found to have united at the age of 15. In all the cases where the finger prints of adults taken at different ages have been compared the correspondence has been found to be exact.

In studying the variety in the finger prints of different individuals, account has to be taken not only of the general form of the pattern and of the number of ridges between fixed points, but also of all the *minutiæ* appearing in each finger print—breaks, junctions, bifurcations, &c.—which are equally persistent with the general form of the pattern. We cannot here set out the details of Mr. Galton's reasoning as to the number of possible variations in a single finger print, but it is sufficient to state that the conclusion at which he arrives is that the chance of two finger prints being identical is less than one in sixty-four thousand millions, that is to say, if the number of the human race is reckoned at sixteen hundred millions, there is a smaller chance than one to four that the print of one finger of any person should be exactly like that of any finger of any other person. If, therefore, two finger prints are compared and are found to coincide exactly, it is practically certain that they are prints of the same finger of the same person; if they differ, the inference is equally certain that they are made by different fingers. The prints of one finger, if clearly taken, are therefore enough to decide the question of identity or non-identity, and if the prints of three or more fingers be taken and compared, all possibility of error is absolutely eliminated. We are clearly of opinion that for the purpose of *proving* identity the finger prints examined and compared by an expert furnish a method far more certain than any other. They are incomparably more certain than personal recognition or identification by photograph. Under the Bertillon system it is conceivable, though most improbable, that two persons might have measurements coinciding within the limits which have to be allowed for error, and that they might also have the same distinctive marks; but it is wholly inconceivable that two persons should show an exact coincidence in the prints of two or three, not to speak of ten, fingers.

There is, however, the further question how far the finger prints can be used for the purpose of *tracing* identity, that is to say, how far they can be classified. Mr. Galton founds his classification on three forms or types of pattern, to one or other of which every finger print may be assigned, viz., "arches," "loops" and "whorls." In all digits the ridges immediately adjoining the third joint run across the finger, while those towards the tip follow the form of the nail in a rounded arch, but in the space left at the centre of the bulb the ridges have various curvatures forming the pattern of the finger print. The pattern is an "arch" when the ridges in the centre run from one side to the other of the bulb without making any backward turn or twist; a loop, when there is a single backward turn but no twist; and a whorl, when there is a turn through


ARCH.


LOOP.


WHORL.

at least one circle, or a double turn in the form of a duplex spiral. (Galton's "Finger prints," page 78.) Typical specimens of these three forms of patterns are given in the illustration above. The general form of the patterns only is shown, as the precision and delicacy with which the minutiæ appear in imprints taken direct from the finger, cannot adequately be reproduced here.

In reading off the patterns and translating them into symbols Mr. Galton takes the prints of the ten fingers in the following order: the first, second and third finger of the right hand, the first, second and third finger of the left hand, the thumb and little finger of the right hand, the thumb and little finger of the left hand; and marking an arch as "*a*," a loop as "*l*," and a whorl as "*w*," he obtains a formula for each person in some such form as *alw, all; wl, ll.*

In order, however, to give greater variety in the formulæ, he distinguishes on the forefingers between loops coming from the radial or thumb-side of the hand, and loops from the ulnar or little finger side, the former being marked "r," and the latter "u." In the other fingers so large a proportion of the loops come from the ulnar side, that nothing would be gained by carrying this distinction further. As examples of the formulæ thus obtained, we give those of Mr. Galton himself, and of the members and Secretary of the Committee.

(1.) *wll, wll; wl, wl.*
(2.) *wll,* ull_w*; ll, ll.*
(3.) *rll, ull; wl, ll.*
(4.) *rwl, rll; wl, wl.*
(5.) *rlw, ulw; ll, ll.*

Each person thus possesses a formula which is, as it were, a personal name, that may be read from his finger prints, and for the purpose of an index these formulæ are arranged in alphabetical order, like the names in the alphabetical list in a directory.

A difficulty is caused in some of the formulæ by transitional forms of patterns, but this Mr. Galton meets by adding to the letter that best represents the pattern a second letter representing the alternative interpretation. Thus, in the second formula given above, l_w represents a pattern which he considers to be a loop, but which might possibly be read as a whorl. With this precaution a form transitional between one pattern and another presents no more difficulty than a name which is spelt different ways; and just as in an alphabetical list of names we should look under "Thomson" for a name we had failed to find under "Thompson," so the formula in question would be treated as though there were some doubt as to the right way of spelling it.*

The number of possible formulæ, if the arches, whorls and loops occurred quite indiscriminately, would be 104,976, and if that were so there would be no difficulty in

* For the further use of subordinate symbols to mark special features in the form of patterns, *see* Appendix G.

classifying in this way 100,000 imprints, or even a much greater number. Unfortunately for the purpose in view the different patterns do not occur indiscriminately. The arches are much less frequent than the other patterns; there is a tendency for particular patterns to occur more frequently in particular fingers; and there is also apparently a tendency in certain hands to repeat the same pattern on all the fingers. The result is that in the collection of 2,645 cards examined by Mr. Galton, while a considerable number of formulæ occurred only once, there were no less than 12 particular formulæ which occurred oftener than 26 times, that is in more than 1 per cent. of the cases, while one formula (*ull, ull; ll, ll,*) occurred 164 times or in 6 per cent. of the imprints. It is therefore clear that while this mode of classification is useful for a small collection it would be insufficient to index a large collection consisting of many thousand cards. To carry further the comparison with the alphabetical list of names already suggested, it is as if, in a list of proper names, the name "Smith" made 6 per cent., and "Jones" and "Thomson" 3 per cent. of the whole, and it was therefore necessary to find further names for indexing the persons bearing the same surname. At our suggestion, Mr. Galton carried further an investigation which he had already begun as to how far a sub-classification of the commoner formulæ is possible. He has devised for this purpose an ingenious system, depending partly on the number of ridges in each loop and partly on minutiæ in the core of the pattern. Some account of this is given in Appendix G.*; here it must suffice to say that, on testing him with duplicates of finger-prints of the *ull, ull; ll, ll* type, we found that he was able without difficulty to select the proper card; that is to say, he readily picked out by means of one set of imprints the card containing the imprints of the same person from among the 164 cards of the *ull, ull; ll, ll* type. He showed himself able, in fact, by finger-prints alone, to discover at once the identity of any one of the 164 persons whose formulæ were of that type which presents by far the greatest difficulty in classification.

The conclusion at which we have arrived is that for a small collection of cards, say, under 1,000, Mr. Galton's system is admirable. Even if no sub-classification be adopted, it is always found that on some one or other of the fingers the pattern contains some well-marked peculiarity, and there is no difficulty in running through 50 or 60 cards (to take the most numerous type) and ascertaining at a glance whether on any of them this particular feature presents itself. If however Mr. Galton's system of classification is to be applied to a larger collection than 1,000 cards, it becomes necessary to introduce the sub-classification. This could certainly only be carried out by a thoroughly trained expert, and, though the results of our trials in Mr. Galton's collection of 2,500 cards were eminently satisfactory, it is still a question how far the same method could deal effectively with a much larger collection.

It remains to mention one or two practical points on which we had to satisfy ourselves before deciding that Mr. Galton's system could be used for the purpose of proving or of tracing identity.

(i.) It has been suggested that the finger-prints could easily be altered or removed, and if this were so it would be a fatal objection to their use. We thoroughly satisfied ourselves that they could not be altered so as to cause any possibility of mis-identification; they can of course be altogether destroyed, but this would be a difficult and painful operation and would at once afford a new personal mark of a most distinctive character. Cuts and ulcers destroy a portion of the ridges, but generally leave the pattern perfectly distinguishable; in any case they could not possibly cause such a change as might lead to a mis-identification. In the classification of imprints a finger in which the pattern is destroyed assists the classification; it is represented by the symbol *x*, and gives a further set of formulæ in which the constantly recurring *u's* and *l's* are varied by an occasional *x*.

(ii.) It may also be objected that this mode of identification would be rendered futile by the liability of the ridges to become obscured in the hands of persons engaged in manual labour. It is true that this is in some degree the case as regards persons employed in hard manual labour, but it does not affect the majority of habitual criminals, who when at liberty are not distinguished for their application to manual labour and who are not employed in prison in forms of labour which produce this result. We took at Pentonville the finger-prints of 100 prisoners, most of whom were engaged in oakum picking, some were stokers, and some bakers and tailors. In every case we obtained perfectly clear and complete finger-prints, the only two exceptions being a prisoner who had lost a hand and another who had lost one of his fingers.

* Reference may also be made to Mr. Galton's evidence, Q. 222–232, p. 58.

(iii.) It has further been suggested that the finger-prints are too complicated and difficult to be understood and used by warders or policemen. This is true as regards identification and classification, and would be a serious objection if this part of the work had to be done in prisons or police stations; but it is obvious that the classification and comparison of the imprints would be carried out entirely at head-quarters and by an expert. All that the warders would have to do would be to take the finger-prints —a simple mechanical process which any warder could learn without difficulty. At Pentonville a warder with no previous practice whatever took in an hour 35 sets of impressions of three fingers, each in duplicate, and every one of these was easily decipherable.

(iv.) One more objection which has been made to the use of finger-prints is that they could not be used for purposes of proof in courts of law. We are not by any means sure that this is the case. If enlarged photographs of finger-prints were produced, and were explained by counsel to a jury, we believe that at the cost of some time and trouble proof of identity could be established; but for the purpose now in view this is not necessary. What is required is in the first place assistance in tracing the criminal, and secondly a check to prevent the occurrence of mistakes in the ordinary process of identification by means of personal recognition. In tracing a criminal the finger-prints would be of much assistance. For verifying identifications they would give a test, which in the hands of a skilled person would be unimpeachable.

It seems impossible to insist too strongly on the absolute certainty of the criterion of identity afforded by the finger-prints. Considered merely as a test of identity and not as a detective agency—there being no longer any question of classification—their use becomes at once extremely simple, and in the hands of an expert free from any danger of error. Apart altogether, therefore, from their use in tracing habitual criminals, it would be a very easy matter to use them much more extensively as a check to all identifications. If the prints of three fingers only of every criminal prisoner were taken before his discharge, and kept with his papers in the prison, it would be impossible afterwards wrongly to attribute the conviction to any other person. This would cover, for instance, the case of Callaghan mentioned on page 14, a case which would not come within the scope of the system we propose to recommend for the identification of habitual criminals. So if the finger-prints of pensioners were taken and kept with their papers, an absolute test would be available if any question of fraudulent drawing of the pension (*e.g.*, after the death of the pensioner) should ever arise.

These last suggestions, however, go somewhat beyond the main point which we have still to deal with in our report. We have completed our account of the three systems of identification mentioned in the first part of the reference, and we shall now proceed to answer in explicit terms the questions put in the latter part of your Commission.

I. Whether the Anthropometric System or the Finger Print System can with Advantage be adopted in England.

The conclusion at which we have arrived with regard to the English methods is that they are on the whole fairly effective; that the majority of old offenders who are arrested for new offences are in the long run identified, and that cases of misidentification are extremely rare. On the other hand, some of the methods in use involve the expenditure of much labour and time, and in spite of the best that can be done, it is clear that a certain proportion of old offenders, small in some districts, considerable in others, escape identification altogether. If a system can be adopted which will secure the prompt and easy recognition of every old offender the ends of justice will be furthered, a great administrative improvement will be effected, and much expense will ultimately be saved.

Before considering the question further, it may be well to say at once that in no circumstances can the system of M. Bertillon be adopted in its entirety on account of the fundamental differences between French and English judicial procedure. In Paris every person arrested for any offence is at once subjected to the process of measurement and is sometimes photographed before being brought before any magistrate. It would not be consistent with English ideas to entrust to the police an arbitrary power of measuring or photographing every person arrested without authority from a magistrate and without regard to the necessity for the purposes of justice of discovering his antecedents and character. Nor do we think that, if the Bertillon system is adopted in principle, its indiscriminate application will be necessary for the purpose in view. The enormous number of measurements taken appears to us to be likely even in France to cause ultimate difficulty, and in England so extensive an application of the system would certainly hamper its first introduction.

In deciding what system should be adopted, three main conditions may be laid down:—

1. The descriptions, measurements or marks, which are the basis of the system, must be such as can be taken readily and with sufficient accuracy by prison warders or police officers of ordinary intelligence.
2. The classification of the descriptions must be such that on the arrest of an old offender who gives a false name his record may be found readily and with certainty.
3. When the case has been found among the classified descriptions, it is desirable that convincing evidence of identity should be afforded.

The 1st and 3rd of these conditions are met completely by Mr. Galton's finger print method. The taking of finger prints is an easy mechanical process which with very short instruction could be performed by any prison warder. While in M. Bertillon's system a margin greater or less has always to be allowed for errors on the part of the operator, no such allowance has to be made in Mr. Galton's. Finger prints are an absolute impression taken direct from the body itself; if a print be taken at all it must necessarily be correct. While the working of this system would require a person of special skill and training at headquarters, it would have the enormous advantage of requiring no special skill or knowledge on the part of the operators in the prison, who would merely forward to headquarters an actual impression taken mechanically from the hand of the prisoner. With regard to the third condition again, as we have already pointed out, Mr. Galton's system affords ample materials for conclusive proof of identity: the imprints of the ten finger tips give such enormous scope for variation that if two sets are found to correspond exactly within the portions common to the two impressions, it becomes impossible to doubt the identity of the persons. It is true that this evidence can only be deciphered in detail by an expert, and that it could not at present be substituted in legal procedure for the ordinary evidence of identity from personal recognition; but this would not affect its value as a complete check on the accuracy of the ordinary evidence.

The Committee were so much impressed by the excellence of Mr. Galton's system in completely answering these conditions that they would have been glad if, going beyond Mr. Galton's own suggestion, they could have adopted his system as the sole basis of identification.

When, however, the second condition is approached, serious difficulties arise. The method of classifying finger marks proposed by Mr. Galton affords, as we have seen, an admirable means of indexing a comparatively small collection, and the difficulty which arises from the transitional forms is not insuperable; but when the method is applied to a large collection amounting to many thousands, as would be the case in a Criminal Register, the difficulty arising from the inequality of the classes becomes serious. One class alone includes 6 per cent. of the whole number of imprints, and several other classes include 2 or 3 per cent. each. In a collection of, say, 25,000 imprints (and it is probable that the number will be greater than this) it would be found that 1,500 imprints would fall into one class, while there would be several other classes each containing between 500 and 1,000 imprints. The sub-classification of the largest class, which Mr. Galton at our suggestion carefully worked out, is very elaborate, and in the matter of the counting of the number of the ridges in the loops, it seems to us open to some uncertainty; and we believe we are only following Mr. Galton's own opinion in saying that it would not be desirable to adopt it for a very large collection if any better system is available.

On the other hand, the strongest point in favour of M. Bertillon's system is the method of classification. If absolutely invariable and accurate measurements could be obtained, then from the measurements of any person the card giving his name and antecedents could be found in M. Bertillon's cabinet as certainly and almost as quickly as an accurately spelt word could be found in the dictionary. Absolute perfection is of course not obtainable, all measurements being subject to error arising from actual variations in the body and from want of skill in the operator; but these causes affect some measurements in a much slighter degree than others, and by selecting five measurements which are least subject to variation in adults and which can be taken with the greatest accuracy by ordinary operators, M. Bertillon has obtained a primary basis of classification as nearly perfect as possible. By means of these five measurements, each divided into "long," "medium" and "short," M. Bertillon obtains 243 classes, represented by the 243 drawers in his cabinet, and these classes are approximately equal. Where a measurement lies near the margin of two classes it may be necessary to search for the case in two of the drawers; if two measurements be on the margin it may be necessary to search in four of the drawers, but even in the extreme case where each of the five measurements lies on the doubtful margin between two classes it would be necessary to search in only 32 out of the 243 drawers. It seems impossible to us to improve on M. Bertillon's system so far as this primary classification is concerned. Other measurements were suggested to us by Mr. Galton and Dr. Garson, which have special points of superiority to those of the middle finger and the foot,* but on the whole the balance of advantage appears to be in favour of the five chosen by M. Bertillon, even apart from the fact that it is desirable for international purposes to have the same basis of classification in England as in France. The taking of measurements, though it requires some training, does not require any high degree of skill, and we are thoroughly satisfied after seeing the process in operation in France that there would be no difficulty in training English warders of ordinary intelligence to take them with the required accuracy. On this point we would refer also to the evidence of Dr. Garson, who has practical experience in training assistants in anthropometry.

The case is different, however, when we come to the further sub-divisions of the Bertillon classification, those by the height, the length of the little finger and the colour of the eye. The length of the little finger is closely correlated with the length of the middle finger; in most cases where the one is long, the other is long also. The height again is a very unsatisfactory measurement; it is subject to variations in the same person, and it may be altered by trickery on the part of the person measured. By the Metropolitan Police a margin for error of two inches in each direction is allowed in classifying cases by height. Even with the greater accuracy of the French measurement a considerable margin has to be given. The accurate description of the colour of the eye is still more difficult. The seven colours taken by M. Bertillon can be discriminated only by persons having much practical experience, and even then many doubtful and transitional cases must occur.

In adapting M. Bertillon's system to English use we think it would be desirable to abandon these criteria and make the final classification dependent on the finger-prints.

Our recommendation, therefore, is that the prisoners who are to be included in the register should be measured as regards the length and breadth of the head, the length of the left middle finger, the length of the left forearm and the length of the left foot; that these should form the primary classification, giving 243 nearly equal classes; that the finger-prints of each prisoner should be taken and that the sub-divisions should be by means of Mr. Galton's method of classifying the finger-prints. The measurements and finger-prints should be taken in prison by prison warders, and should be afterwards classified and used for identification in a central registry for the whole of England.

We think that this system should not in the first instance be applied to all persons convicted of crime, but only to all convicts and to habitual criminals, that is, persons coming within section 7 of the Prevention of Crimes Act, 1871. The Registrar might also have a discretion, on application by the police, to add to the Register a limited number of other prisoners who, though only once convicted, are reasonably believed to belong to the class of travelling thieves.

We further recommend that in all cases photographs should be taken; they are of much use in making the search in the Register, and, when the case is found, they afford in most cases a ready and obvious evidence of identity. The finger-prints will, however, afford in most cases the scientific proof of identity, and, wherever the system is applied, will render a wrong identification practically impossible.

II. Whether the Proposed New Method should be in Substitution for or Supplementary to the existing Methods.

Our answer to this question is that, in the first instance, it should be supplementary to the existing methods and not in substitution for them. It must take several years before the new system is in full operation, and until then it will be necessary to continue all the existing devices for identifying old offenders. Even when the new system is introduced, as it is proposed to confine it almost entirely to convicts and habitual criminals, it will be necessary that, except in the case of convicts, at least one

* If it were found desirable to add a sixth measurement, the measurement of the breadth of the face, suggested by Dr. Garson, has great advantages. (*See* the evidence given by him, Q. 38–43, p. 50, by Mr. Galton, Q. 247, and by Professor Thomson, Q. 422–424, p. 66.)

previous conviction should be proved against the prisoner before he is placed on the Register.

We think, therefore, that the practice of metropolitan police officers visiting prisoners at Holloway must be permanently continued and that a similar system might with great advantage be introduced in other prisons out of London.

The Habitual Criminals Register, which is used with some success by a large number of police forces, should certainly be continued in its present form with any improvements that may be possible, until experience shows that the need for it no longer exists. The same may be said of the various registers kept in Scotland Yard and by the chief provincial police forces; they should be continued until gradually and naturally superseded by the new system.

The issuing of route forms should also be continued in the same way, and in a limited number of cases the practice will have to be continued permanently unless a wider range should hereafter be given to the system of measurements and of finger-prints which we have now recommended.

III. What Means should be adopted for putting into Practice the Method of Identification recommended.

We now propose to describe in detail the arrangements which we think should be followed if the preceding recommendation is adopted. No doubt some of our proposals will have to be modified as practical experience suggests improvements, and it will be important that the possibility of improvement should be kept steadily in view.

In the first place, it will be necessary before their discharge to measure, &c. those convicted prisoners whom it is proposed to put on the Register. The process will be—

(1.) *To photograph them as at present.*—It has been strongly represented that the photograph of the side face should not be taken, as now, by means of a mirror, but should be, as in France, a second distinct photograph on the same plate. This has the advantage, first, of giving a clearer portrait and showing very distinctly the forms of the ear and nose, which are the most important features for purposes of identification, and, secondly, of not reversing the sides of the face, a change which sometimes causes confusion. It would, however, involve some additional expense; and the photographs which have been sent us by the Austrian Government as well as many taken in English prisons, show that very good results are obtainable by means of the mirror The important point is that there should be a perfectly clear side photograph showing distinctly the profile and the form of the ear.

(2.) *To take the five measurements required for purposes of classification*, viz., the length of the head, the width of the head, the length of the left middle finger, the length of the left forearm, the length of the left foot.

This should be done in accordance with the instructions printed in Appendix E., which have been adapted from those issued by M. Bertillon.

The measurements should be taken with the same instruments as in France, and should be stated in millimetres. The millimetre gives exactly the degree of accuracy that is required, and its use will much facilitate identification in international cases. It would of course be possible to take the measurements in inches, and in twentieth parts of an inch, but this would give awkward and complex figures; while if millimetres are taken, a single number represents each result. It requires no knowledge of the metric system on the part of the operator, who has merely to read off the figures from the instrument. The evidence of Dr. Garson, who has had large practical experience in training Englishmen to take measurements in millimetres, is convincing on this point.

(3.) *To take the finger prints* by Mr. Galton's method. This should be done in accordance with the instructions in Appendix F.

(4.) *A description* should also be taken as at present, but somewhat briefer, including the height in feet and inches, colour of hair, eye and complexion, and the distinctive marks. This is not required for the purpose of classification; but it is necessary (*a*) in case the arrest of the criminal should be required while he is at large, and his description has to be published for this purpose; (*b*) in case his identity should be disputed, when the distinctive marks often supply the evidence which can most easily and most satisfactorily be put before a jury.

The marks noted should, however, be those only which are definite and distinctive their position, size, and direction should be given accurately and abbreviations should be used according to the suggestions we have made above, page 19. They should be arranged in a fixed order, beginning with the head, then the hands and arms, then the body, and last the legs and feet. Instructions on this point are contained in Appendix E.

These measurements, &c. should be recorded on a card of the size now used by Mr. Galton (12 inches by 5). On the back of this card will be the finger prints, on the face the other particulars arranged as follows :—*

Head, length ,, width Left middle finger Left forearm Left foot Height Eyes Hair Complexion Distinctive marks— I. II. III. IV. V. VI.	Name No. Date of birth Place of ,, Particulars of convictions—	Finger Print Formula (Photograph.)

This card will be prepared in duplicate and forwarded to the Central Registry. One card will be placed in an alphabetical register for use when the prisoner's name and antecedents are known. The other will be arranged in the classified Index Register.

The arrangement of this Index Register will be the same as M. Bertillon's, a cabinet of drawers first divided vertically into three divisions according to length of head, and horizontally according to width of head. The nine sections thus formed will be divided vertically according to length of finger and horizontally according to length of forearm, and again vertically according to length of foot. There will thus be 243 drawers each containing one class of cards. The figures which are to determine the "long," "medium" and "short" of the several classes might be borrowed in the first instance from M. Bertillon, but in that case on account of racial differences they would have ultimately to be altered in order to keep the classes equal in size. It would be best therefore that the measurements taken in this country by Mr. Galton and by the Anthropological Institute should be utilized, and correct figures for England fixed from the outset. *See* the evidence given by Dr. Garson, page 53.

At the outset, while the number of cards is few, it may suffice to use only four measurements for classification, omitting the foot, and thus making only 81 classes. In any case, however, the measurement of the foot should be recorded, so that it may be available afterwards if required for further classification.

Before each card is put away in its proper drawer the finger print formula will be determined according to Mr. Galton's method. This will be noted conspicuously on the face of the card in the right-hand top corner above the photograph. The cards in each drawer will then be arranged in accordance with Mr. Galton's method, that is, following the alphabetical order of the formulæ. Instructions for determining the formulæ and for arranging the cards will be found in Appendix G.

At first there will be only one cabinet for all adult male criminals, but it will soon become necessary to form a separate one for older men, say for persons born before the year 1830. The age of criminals is often wrongly given, and it would be necessary at first to search this cabinet in all cases of persons apparently above 50 who may not be found in the ordinary cabinet, but such searches would become gradually rarer as the older convicts die out, and ultimately all the older cases would be eliminated.

The separate cabinet for older criminals will be required, even if the deaths of all habitual criminals, so far as known to the police and prison authorities, are reported from time to time to the registrar, and their cards withdrawn from the registers. We strongly recommend that the police should be called on to report all such deaths known to them, and that in other ways efforts should be made to reduce the bulk of

* The margin at the bottom of the card is part of a contrivance designed by Mr. Galton to check mechanically the sorting of the cards and described in Appendix H.

the records; but, even when the utmost has been done in this direction, there will remain so many cases where old offenders disappear or die unrecognised, that unless there is a separate classification for the older cases, the registers would, in the course of years, become seriously encumbered with a mass of obsolete and useless records.

There will be a separate cabinet for women, but as the numbers are smaller the arrangement would be simpler and the fifth measurement may be omitted so far as the classification is concerned.

As regards boys and lads whose bones have not attained their full growth, it may be best to measure and classify them separately as is done by M. Bertillon, and to allow for growth in the search for the card. We are disposed, however, to recommend as an experiment that for this class, which is small compared with the number of adults, a separate index based entirely on Mr. Galton's method might be formed.

The Register having thus been constituted, it will be necessary, before it can be used to discover the antecedents of unknown offenders charged with crime, that rules to authorise the measuring and photographing of untried prisoners should be made by the Secretary of State under section 8 of the Penal Servitude Act, 1891. On this subject a recommendation is made below in accordance with the reference to the Committee. Assuming the rules to be made, the steps in each case will be as follows: When the antecedents of a prisoner charged with crime are unknown and it is suspected that he is an old offender, the police will apply to the magistrate at the first hearing of the case to make an order for the accused to be measured and photographed, and the magistrate, if satisfied that it is a proper case, will, on remanding the prisoner or committing him for trial, make an order for that purpose. On reception in prison the prison authorities, acting on the magistrate's order, will take the measurements, finger prints, description and photograph of the accused in the same way as in the case of habitual criminals about to be discharged, and they will be noted on an inquiry card similar to that already described but distinguished from it by a difference of colour. This will be forwarded to the registrar. On its receipt search will first be made in the alphabetical register under the name given by the prisoner, and if he should have given the name of a person previously convicted, the identity can at once be proved or disproved by the measurements, finger prints and photograph. If he is not found by means of his name, search will be made in the Index Register. If the case is found, information will be given to the police who have charge of the case, of the prisoner's previous convictions, and of the means by which his identity can be established. If the case is not found after adequate search—a sufficient margin for errors in the measurements being allowed—it will be practically certain that he is not an habitual criminal within the class included in the register, and information to this effect may be given to the police.

We do not anticipate that the adoption of this system will increase materially the number of prisoners detained in prison on remand. It is already the practice to remand prisoners suspected of being old offenders for at least one week, often for several successive weeks, for purposes of inquiry. Under the new system the number remanded for one week might be somewhat increased, but this would probably be more than counter-balanced by the smaller number who would be remanded more than once, as the search in the register would in every case be completed within the first week.

Nor do we anticipate that any serious difficulty would arise from resistance on the part of prisoners to measurement or any other process which may lead to identification. It has been stated in evidence that prisoners have in some cases resisted being photographed, but the number who do so is not large, and they usually base their resistance on the ground that they cannot legally be required to submit. If it is once made clear to them that the rules are enforced under statutory powers, the cases of resistance would, we believe, become extremely rare. This is the view of all the Governors and other persons experienced in the management of prisoners whom we have consulted. In the last resort the measurements and finger prints could, we are satisfied, be taken even when active resistance is offered by the prisoner. Resistance to measurement would interfere less with the process and could be more easily overcome than resistance to photography; and, as regards the finger prints, an ingenious mechanical contrivance has been suggested by Mr. Galton for taking the finger-prints of a recalcitrant prisoner, though we do not think it will be necessary to have recourse to this.

A point of some importance is whether the proposed Anthropometric Registry is to be connected with the Habitual Criminals Register in the Home Office or with the Convict Office at New Scotland Yard. In many respects it connects itself naturally with the Habitual Criminals Register; but it appears to us that there are great practical advantages in favour of the work being done at Scotland Yard. If placed in Scotland Yard, it would be in the hands of officers actually engaged in the work of detecting offenders, experienced in all branches of police work and having a keen interest in securing the utmost possible efficiency in the working of the Register. At Scotland Yard the assistants to be employed in the work would be selected from a force of 15,000 men, and any assistant found on trial not to have the special aptitude for this particular work could be withdrawn to other duties. The identification of offenders is primarily police work, and though the measurements and finger marks should be taken in prison and by prison officers, their use in the work of identification will be best in the hands of the police. The peculiar character of the Metropolitan Police, who are at once a local police force for London, and an imperial police under the control of the Government, particularly fits them for undertaking a work which will be specially useful in London, but whose main feature is that it will enable travelling criminals to be traced and identified in whatever part of the country they may attempt to begin a new course of crime. Ultimately it may be found convenient to transfer the existing Habitual Criminals Register to New Scotland Yard, but we do not think this should be done until the new Register is in full working order.

A word must also be said as to the cost of the new system. At its first introduction it must inevitably involve some expense, but this, as we shall show, will be much smaller than might be supposed, and we confidently anticipate that it will gradually supersede the existing method of identification and in the long run effect a considerable economy. In the meantime the additional cost will fall under two heads, the cost of taking the measurements in the prisons and the cost of keeping the Register at New Scotland Yard.

As regards the former, the number of convicts and habitual criminals to be measured, &c. before discharge will, judging from the numbers entered in the Habitual Criminals Register in recent years, be about 4,000, or say for safety, 4,500. It is impossible to say how many unconvicted prisoners will have to be measured, &c. while on remand or waiting trial, but if we take the same number, 4,500, it will, we think, be a liberal allowance. We may assume therefore 9,000 prisoners, convicted or unconvicted, to be measured and to have their finger prints taken in the year. In M. Bertillon's office, the measuring, taking of marks, &c., occupies two clerks on the average rather less than 10 minutes. As it is proposed not to take so many measurements in England, we may perhaps safely assume that the time spent will not be more, and if we may add five minutes for the finger prints—a skilled operator can take the complete finger prints in duplicate in two minutes—we have then 9,000 measurements to take, each occupying two warders for 15 minutes,—that is, occupying two warders for 2,250 hours, or for 281 days of eight hours in the year. If therefore all the prisoners were concentrated in one prison the whole work could be done by two additional warders. The work will, of course, be distributed over 63 prisons, and the arrangements to be made for its performance will be a matter of prison administration not more difficult than those involved in any slight increase of prison work. It will be for the prison authorities to decide whether it can all be done by the existing staff,—it is proposed that the measurements, &c. should be taken in the morning when the reception officers have usually comparatively little to do—or whether in one or two central prisons an addition to the staff will be necessary. In the end, it may fairly be anticipated that the work of measurement and of taking finger prints will occupy even less time than the present laborious method of taking distinctive marks which occupies from seven to ten minutes and sometimes longer.

There will, it should be added, be one or two minor items of expenditure in introducing the new system in the prisons, particularly the cost of instruments for use in each prison, that of taking some additional photographs of untried prisoners, and the expense of bringing warders for a few weeks to London to learn the methods of measurement, &c. For the latter purpose it will probably be well to establish for a time at Pentonville, where the larger number of convicts and habitual criminals are discharged, a sort of school where selected warders from country prisons may be given the necessary training.

As regards the Central Registry, we can best estimate the cost by a comparison with M. Bertillon's office. He employs eight assistants, who in the morning take measurements and in the afternoon attend to the registers and make searches. The work of the registry therefore (as distinguished from the measuring) only occupies the time of four men, or of five if M. Bertillon himself be included. But M. Bertillon now receives and classifies in each year about 15,000 measurements from Paris and about 70,000 from the departments. In the English registry it is proposed to limit the cases to be entered on the registry to about 4,000 or 4,500 in the year. It seems, therefore, safe to say that for some time at least one or two men will be able to do all the work of the Central Registry. This increase will appear very small when it is borne in mind that, since the Convict Office undertook in 1889 the extra work of registering the habitual criminals discharged in the Metropolis, the staff has been increased by six officers, four men having been added for this purpose in 1889 and two in 1892; and it is understood that in order to maintain this work, a further increase of staff is now considered necessary.*

In addition to this we are strongly of opinion that it is essential to the complete success of the registry to secure, at all events at the outset, the services of an expert practised in the methods of scientific anthropometry, and if possible one who has had practice in training other persons in making scientific measurements. We have the utmost confidence in the skill and ability of the officers in the Convict Supervision Office, and we think it might be possible, by sending two or three intelligent officers to learn the system at Paris in M. Bertillon's office, to secure the necessary knowledge and training to start the new system, if we are content to follow strictly on his lines. But we feel sure that it would be better that the Convict Office should have from the first the assistance and guidance of a scientific adviser in England. He would be able from the outset to settle such questions as the limits to be adopted in England for the classes of large, medium and small (as already mentioned, the Anthropological Institute have data available for this purpose), he would be able to superintend the training of warders in taking measurements, and he would instruct the officer in charge of the registry in the decipherment and classification of finger prints.† Moreover, when practical experience had been obtained of the use of the finger prints, he would be able to revise the suggestions which we have made as to the respective place of the Bertillon and the Galton methods in the system, and might possibly find it advantageous to extend the Galton method of classification further than, with the limited experience we possess of its practical application, we have ventured to propose. On every ground therefore we think it desirable that the English Anthropometric Office should from the first have the advantage of scientific guidance not inferior to that which the French Service d'Identification enjoys in having M. Bertillon at its head.

IV. What Rules should be made under Section 8 of the Penal Servitude Act, 1891, for Photographing and Measuring Prisoners.

Our recommendations under this head have to some extent been anticipated. The section in question was introduced into the Act of 1891 to remove doubts as to the power to photograph and measure *untried* prisoners; but a promise was given that the rules, which have to be laid before Parliament, would contain safeguards against the indiscriminate photographing of untried prisoners whose antecedents might be well known. The safeguards we suggest are (1) that an untried prisoner should be measured and photographed only on the order of a magistrate; (2) that this order should be made on reasonable ground being shown for suspecting that the prisoner is an old offender. We therefore make the following recommendations:—

1. *As regards convicted prisoners.*—We recommend that the Rules should provide that—

(1.) all convicts and all persons coming within section 7 of the Prevention of Crime Act, 1871, should before release be *photographed*, the photograph to show both the full face and the side face, but not the hands;

(2.) That in each case the following measurements should be taken:—Length of head, width of head, length of left middle finger, length of left forearm, length of left foot and the height; the first five measurements to be taken in millimetres, the height in feet and inches;

(3.) That in each case impressions should be taken of the tips of the ten fingers giving the measurements of the papillary ridges;

(4.) That the position and size of distinctive marks on the head, limbs, and body should be ascertained by measurement in the following order:—I. Left arm and hand; II. Right arm and hand; III. Face and throat; IV. Chest; V. Back; VI. The rest of the body (prominent features only).

* In a memorandum on the work of the Convict Supervision Branch, recently submitted to the Secretary of State, Mr. Anderson says:—"The scheme imposes a large amount of extra work in my Department, and the "time has now arrived when we must go forward with a stronger staff, or else draw back."

† Another question on which scientific advice as well as practical experience is required is that raised by Professor Thomson and alluded to in the note on page 23, namely, the amount of variation to be allowed for in dealing with each measurement.

Only clearly distinctive marks should be taken. If sufficient marks are found in I. II. and III., the others should not be taken.

2. *As regards prisoners received on remand or for trial.*—The Rules would be similar but restricted to persons accused of crime or charged under the Vagrancy Act, for whose measurement an order is given either by the magistrate by whom they are remanded or committed, by a member of the Visiting Committee of the Prison, or by a Prison Commissioner, such order to be made on an application by the prosecutor or by the police, on the ground that prisoner's history is unknown, or that he has no permanent abode, and that from the character of the offence with which he is charged or from any other cause there is reasonable ground for suspecting that he may have been previously convicted or may be habitually engaged in crime.

In the case of untried prisoners the photograph, measurement, &c., should be taken not later than noon on the day after reception; and the card with the measurements and photograph should be forwarded the same or at latest the following day to the Central Registry.

The rules should also provide (in accordance with a pledge given in Committee on the Penal Servitude Bill) that photographs taken before trial should be destroyed if the prisoner is afterwards acquitted.

In conclusion, we have only to say that the method of identification which we have recommended, or any other scientific method that may be adopted, must not be expected to produce its full results until after a considerable time. When it has been several years in operation, when the warders employed to make the measurements have acquired experience and skill, and when a large mass of records has accumulated, then, and not till then, is it likely to work as smoothly and to produce results at least as satisfactory as those obtained by M. Bertillon in France. Even in France, though Bertillonage is now in full operation in Paris, its application to the country as a whole is still, as we have said, incomplete. The success of a similar system in England can come only with time, and by means of the hearty co-operation of all concerned in its working. We may confidently anticipate that, if fairly tried, it will show very satisfactory results within a few years in the Metropolis, but the success of its application to the country generally will depend on the voluntary co-operation of the independent county and borough police forces. This we feel sure will not be withheld; when the principles of the system are understood and its usefulness appreciated, we believe it will not only save much time and labour to the police in the performance of an important duty, but will give them material assistance in tracing and detecting the antecedents of the guilty, and will afford, so far as its scope extends, an absolute safeguard to the innocent.

We trust that when the system is to some extent established in England, it may speedily be extended to Scotland and to Ireland.

We have the honour to be,

Sir,

Your obedient Servants,

C. E. TROUP.

ARTHUR GRIFFITHS.

MELVILLE L. MACNAGHTEN.

H. B. SIMPSON,

12th February 1894.

LIST OF WITNESSES.

MINUTES OF EVIDENCE.

THE following Minutes represent only a very small part of the Evidence taken by the Committee. They consist of brief notes taken by the Chairman and Secretary of statements made during the visits of the Committee to police offices and prisons outside London, and of a shorthand report of the evidence heard at the Home Office. The report of the evidence heard on the 11th, 17th, and 18th November is much condensed. In the greater part of the inquiry, consisting of informal conversation, examination of books and photographs, and inspection of the process of measurement, photography, &c. in actual operation, it was impossible to take any formal record of the evidence.

Home Office, 11th November 1893.

Chief Inspector NEAME, Metropolitan Police.

(*Chairman.*) The Committee wish to ask you some questions to supplement the information you gave us when we visited your office last Thursday. You have looked up the cases mentioned in Mr. Simpson's memorandum?—Yes.

Take first the case of George Twohigg, who was sentenced to imprisonment at the Middlesex Sessions in 1882; he was afterwards convicted summarily at Worship Street Police Court in the name of George Smith. Was his previous conviction then known?—On reference to the papers I find that his previous conviction was reported to the magistrate, who preferred, however, to deal with the case summarily. The previous convictions did not come before him in evidence.

Then there is the case of Henry Austin who was discharged as a licence-holder, in October 1891, under a sentence of penal servitude. Shortly afterwards he was convicted summarily at Clerkenwell Police Court for stealing a watch. Was his previous conviction known?—No; he appears to have escaped identification.

Will you now state, in order, the steps taken to trace an unknown offender in your office. Assume that you have under arrest a person strongly suspected to be an habitual criminal, but not identified. What steps would you take?—After receiving his description and photograph, the first step would be to search for his name in the alphabetical registers.

That search would not take long?—Not very long, perhaps 10 minutes.

It is not likely that you would identify him by the name; what, then, would be the next search?—The next search would be in the register of distinctive marks. That register is classified according to the different parts of the body on which the marks occur, the marks of each part being arranged in columns. If the person searched for has distinctive marks on six parts of his body, it might be necessary to make searches in six different divisions of the volume.

That would occupy some time?—Yes; it might be a long search. We have also a register of tattoo initials and names; that is indexed alphabetically, and if one of the marks was a tattoo name, the search would not occupy long.

Supposing this failed, where would you search next?—The third step would be to search the register of names classified according to crime. Certain special crimes are selected, and persons who habitually commit them are classified accordingly.

And if this failed what would be the next step?—Then we should try the albums of photographs.

How many of them have you?—14 albums containing on the average 5,000 photographs each and a library of 90 volumes, dating from 1864, each containing 500 photographs and complete descriptions.

You would first search the more recent volumes which are classified, I understand, according to the height of the persons?—Yes.

Suppose your man was 5 ft. 6 in. high. You would first search for him in that division?—A margin would be allowed for error.

You would search also, I suppose, among 5 ft. 7 in. and 5 ft. 5 in.?—Yes, and we should search also among those 5 ft. 8 in. and 5 ft. 4 in.; there might be an error in either direction of 2 inches.

If he were not found in the classified albums, you would next have recourse to the older volumes where the photographs are arranged merely in chronological order?—Yes. We should have to search right through these.

And would you use Sir Edmund Du Cane's Registers of Habitual Criminals?—We might do so, but they are seldom used. They are somewhat too complicated for Metropolitan police officers.

So that to find one man, you might have to make five or six distinct searches?—Yes.

And that would occupy some time?—Yes.

Would an hour be too long for a search?—No. An hour would not be an unusual time to spend in searching for a "good" criminal; we have much longer searches that that. Our men would search for days rather than lose a "good" man.

So that if some other system were added to those you now use, either the Bertillon method or the finger-point method or some other method, even if it did not lessen the process, it would not imply a very great addition to your labour. It would only be a seventh search added to the six others?—Yes. That is so, but there would be the labour of collecting and classifying the cards.

That is not what we are at now. We are speaking only of the searches. A seventh search added to the sixth now made would not be a disproportionate increase?—No, I think not; but I do not know the Bertillon system sufficiently to be able to offer an opinion.

Now, I want to ask you about some improvements in the existing system that have been suggested. First, about the photographs. Do you have all the photographs you want?—When convicts are discharged we receive three or four photographs, but we can always obtain as many more as we want by paying at the rate of 2*d*. each.

When prisoners are discharged from local prison?—Then we have to pay for the photographs at a higher rate, varying at different prisons. When we wish to have a large number of these, we have them copied by the London Stereoscopic Company; they do it better, and more cheaply than it is done at the local prisons.

Are the prison photographs not good ones?—There has been a great improvement in the photographs of convict prisons, those from Portland and Dartmoor being especially good.

Do you require more photographs than you now have for route-inquiry forms while prisoners are under remand?—We very rarely issue route-inquiry forms, only perhaps four or five times in a year. When we do so, we usually can obtain photographs without difficulty.

Then what improvements would you suggest?—I would suggest that the photographs should be printed without the hands. I think it is the experience of all officers much engaged in searching the albums that the hands distract the attention, and make the recognition of faces slower and more liable to error.

But are not the hands sometimes useful?—If a finger or thumb has been lost or injured, it may be the means of identifying him, but that is always included among the distinctive marks. The photographs of the hands are not required.

Then as to the forms supplied to the police when convicts are discharged. There are several different forms which seem to contain the same particulars and photographs but differently arranged. Do you require these?—No. We do not know the reason for it. So far as the police are concerned, it would be better that the arrangement should be the same in all cases. It would be best to have the photograph always at the top. Placed below on the right-hand side it is often folded, and then it is more difficult to use.

Then with regard to distinctive marks, have you any improvement to suggest in the mode of registration?—I think it would be better if the position were more definitely fixed. Such an entry as "Scars on back and shoulders" is of no use; but it would help to establish the identity if the distance of the marks from particular points were definitely fixed. The body should be divided into sections, and the marks in the different sections measured from particular points.

But would not these increase very much the size of the list?—It would be better to have fewer marks and these marks more definitely fixed. A few distinct marks are better than a large number of vague ones—the marks on the face, hands, and arms are the most useful and the most easily seen.

But are not some of the marks on the hands common to many persons. Look at Sir E. Du Cane's Distinctive Marks Register, "Left hand, tattoo marks, ring on second finger." In the volume for 1892 there are 28 persons who have this mark, and only three of them have any other noted. If you had a prisoner with this mark you would have to search the records of these 25 and probably as many others in previous volumes?—Yes. In our register we should probably have other marks arranged in parallel columns.

You don't use this register much?—No. We do not use the Register of Distinctive Marks often.

NOTE.—Mr. Neame, since giving oral evidence, has supplied the Committee with the following statement:—

The number of persons registered in this office who have been charged with fresh offences in the Metropolitan Police District during the year 1893 is 2,149, made up as follows:—

License holders, supervisees, and expirees - - - -	860
Other registered criminals - -	1,289
	2,149

The "other registered criminals" are mostly habitual criminals within the 7th section, but not all. A good many other cases are registered of persons believed to be habitual criminals but who have not yet come technically within the section.

It may be useful to state what is done by my officers to obtain personal knowledge of convicts and others. As regards convicts who are about to be liberated from prison on license or supervision or on expiration of sentence, a certain number of trained officers (three or four) from this office visit Pentonville Prison every Wednesday to inspect them prior to release for the purpose of getting a knowledge of their features and marks, &c., so as to be able to recognise them again if they should be taken into custody for fresh offences.

The convicts' marks are compared with the descriptive forms furnished by Prison Governors, and corrected where necessary, and the men are told to report at the Convict Supervision Office on liberation.

When they come to the Convict Supervision Office they are required to give an address and directed where to report in future.

The same officers, if possible who saw them at Pentonville see them at the Convict Supervision Office and again visit them very quietly at their registered addresses for the purpose of verification.

Thus these officers have three good opportunities of observing the features of the men, and this system has proved most useful as regards identifications.

It has operated so beneficially that I have suggested its application to the persons who have been sentenced to police supervision under the 8th section and who are subject to the same regulations as to reporting as license holders, so that the officers might be allowed to see these supervisees at a local prison prior to their discharge in the same manner as the convicts at Pentonville.

One or more of these officers attend the remand prison three times a week with divisional officers.

MEMORANDUM OF WORK done at the CONVICT SUPERVISION OFFICE, NEW SCOTLAND YARD.

Search forms received from divisions during March 1893.	1,551	Daily average (27 days), 59.
Identified - - -	175	Daily average (27 days), 6.
Route-inquiry forms received from provincial police during March 1893.	55	Daily average (27 days), 2.
Identified - - -	12	(Proportion 1 in 5.)

On the 1st day of March, 1893, 21 officers attended to search for 27 prisoners, taking in all 57¼ hours to search: resulting in 7 identifications.

Number of photographic albums in use, 14.

P. NEAME,
Chief Inspector.

COMPARATIVE TABLE, showing work performed in the 7th Section Branch of the Convict Supervision Office for the years 1890-1-2-3.

Nature of Work.	Year.			
	1893.	1892.	1891.	1890.
No. of license holders and supervisees registered.	1,495	1,840	1,056	1,838
No. of other criminals registered.	1,286	1,308	2,144	1,226
No. of persons photographed	*1,066	1,221	†1,800	1,532
No. of attendances at C.S.O. to search records.	4,853	4,001	2,637	1,014
No. of search forms received from divisions.	13,140	5,582	2,065	—
No. of identifications from records.	‡2,134	‡1,765	533	176
No. of route-forms received and dealt with.	722	339	462	—

P. NEAME,
Chief Inspector.

* This number is now believed to be normal, and it is not anticipated that there will be any considerable annual increase in future.

† As it was thought that too many photographs were being applied for in 1892 the number was limited to "7th Sec." cases, and to any other cases considered of special or extraordinary interest.—M. L. M. 13/11/93.

‡ Includes identifications for Provincial Police Forces from route-inquiry forms; for 1893, 99; for 1892, 61.

Wakefield, 14th November 1893.

Mr. Emmett, Commitment Clerk, Wakefield Prison.

Remand prisoners are rarely photographed. If this could be done it would be a great help in identification. They would submit quietly if they knew it to be the rule.

Most prisoners admit previous convictions.

Route-forms with photographs are sent out by county police. They ceased to be sent from prison when the order forbidding the use of force in photographing was issued.

Lists of previous convictions are sent in by police along with prisoners if they know them.

The prisoners for trial are mostly local. There are a few "foreigners," travelling pickpockets, and burglars. Of trial prisoners I should say about 10 or 15 per cent. are travellers, and, perhaps, 2 or 3 per cent., or 20 in the year escape identification. If route-forms could be sent out, these would probably be traced. Route-forms are only sent out to ascertain whether suspected previous convictions can be verified. They are mostly north-country men; there are few Londoners, except at the races.

Some are traced by means of their letters.

Measurements are not of much use in tracing old offenders; marks are very useful *if prominent*. If you bring forward clear evidence of who a man is, he will almost always admit it.

The Register of Habitual Criminals is used if there are prominent marks.

Mr. Benjamin Johnson, Warder, Wakefield Prison.

I am reception warder at Wakefield Prison. I have held that office for four years. I am employed until 6 p.m. three nights in the week, until 8 p.m. one night, and until 10 p.m. two nights.

I see, practically, all the prisoners received. I know many by sight. There are some strangers; more this year than before.

There are more colliers in prison this year owing to the strike.

The average time occupied with each prisoner is eight or ten minutes; it usually requires about five minutes to take the marks; but sometimes if they are numerous it takes much longer.

The "foreigners" are not very numerous; they are brought chiefly from Doncaster. I can sometimes distinguish them as "foreigners" by accent.

In this prison we have many cripples—men injured in mines or by machinery; these are easily known.

Tattoo marks are sometimes defaced; usually they are scorched in order to obliterate them.

Paper handed in by the Chief Warder in Charge, Wakefield Prison.

Return of Prisoners committed to Her Majesty's Prison, Wakefield, for Trial at the Leeds Summer Assizes, 1893, and the West Riding, Sheffield City, and Doncaster Borough Michaelmas General Quarter Sessions, 1893.

1. Local prisoners and fully known as such - - - - - - - - -	55
2. Number of prisoners sentenced as first offenders - - - - - - -	18
3. Number of prisoners who are strangers and travelling criminals - - - - - -	16
Total number of prisoners committed -	89
4. Number of class (3) against whom nothing had been found at the time of trial - - - -	1

Jno. Harrison,
Chief Warder in Charge.

Her Majesty's Prison, Wakefield,
15th November 1893.

Dr. Clarke, Medical Officer, Wakefield Prison.

Some years ago I made a study of cranial measurements.

Head measurements increase up to 50 years of age.

In a paper on cranial measurements contributed in 1876 to the West Riding Lunatic Asylum Medical Reports I gave the following figures as a result of the measurement of 500 prisoners:—

Ages.	Diameters of Head.		Circumferences.	
	Anterior. Posterior.	Transverse.	Whole.	Frontal.
Under 20 -	7·397	6·127	21·700	10·477
20 to 30 -	7·589	6·316	21·992	10·791
30 to 40 -	7·651	6·150	22·132	10·764
40 to 50 -	7·674	6·268	22·172	10·797
50 to 60 -	7·541	6·105	21·822	10·661
60 to 70 -	7·531	6·237	22·137	10·787

The number of prisoners of the later ages was small compared with those of middle life, and it would, therefore, not be safe to draw a conclusion from the figures relating to prisoners above 50.

The measurement of height is very uncertain. I have just compared the heights of 32 convicts transferred from other prisons as they were recorded in the prison they came from and as taken here. In 15 cases the heights were the same. In 17 they differed.

The difference was:—

¼ inch in 10 cases.
½ " 4 "
¾ " 1 case.
1 " 1 "
1½ " 1 "

Prisoners could alter their finger-marks only by making a scar, which would itself be a distinguishing mark.

Inspector Harry Reeve, Doncaster Borough Police.

We have in Doncaster a considerable number of criminals who come from other districts. These we endeavour to identify by issuing route-forms.

In 1892 we had 46 foreigners, including 14 in the race week. All these we "routed." There were other trivial cases not routed. Of the 46 the majority were identified; mostly by officers of other police forces.

In one case (January 1893) routes were sent to between 80 and 100 towns. In one only was the criminal known, viz., in Swansea, where he had had convictions of obtaining by false pretences. He was not in the Register of Habitual Criminals.

Photographs are taken at Doncaster in the Police Office yard.

Old thieves sometimes object to be photographed.

They would not, in my opinion, object if a rule were made requiring untried prisoners to be photographed. We have 300 or 400 prisoners in custody in the year.

Magistrates are always ready to remand for inquiry. If it were in their discretion to order photographs to be taken, they would make no difficulty.

This year, in the race week 16 persons were summarily convicted no previous convictions being known, but are now believed to be old offenders. The Governor of Wakefield Prison has been asked to supply photographs. One of the 16 has been already traced.

Distinctive marks are not of use for identification unless prominent. In one case an old offender traced by a scar on each hip. He was traced in Habitual Criminals Register. It required several hours' search before he was found.

The Habitual Criminals Register is useful.

The last quinquennial volume is most used, and after that the annual volumes.

I suggest that there should be district registries because the information supplied by the Habitual Criminals Registry is often defective. In a recent case while prisoner was awaiting trial we obtained form R. from Habitual Criminals Registry, but several more recent convictions were not given. This was the case of Thomas Howard or Holt, sentenced to 15 months at last Doncaster Borough Sessions.

The omitted convictions were summary cases.

I could, if required, report to Habitual Criminals Registry deaths of local habitual criminals out of gaol, as the police would usually know of them through inquests or otherwise.

Habitual Criminals Register arrives very late; it would be much more useful if received in March. I think monthly or quarterly publication would be useful.

A year or two ago a man (Pearman, *alias* Thomas) was arrested at the races; he could not be identified.

After conviction a photograph was sent to Scotland Yard but was not recognised. It was afterwards recognised in London by a warder. The man was a London thief, and was at the time on licence and wanted at Scotland Yard for failing to report himself.

If prisoners were photographed in prison on remand, we should wish photographs taken in prisoners' own clothing. It is easier to recognise them in their own clothes than in prison dress.

If suggested district registries were established, recognition would be from one photograph to another. We often recognise photographs sent us from photographs in our books.

At Doncaster a register is kept of men arrested by police, with their marks, &c.; but after an interval of five years a man would probably escape recognition unless well known.

Leeds, 15th November 1893.

Mr. F. T. Webb, Chief Constable, Leeds Borough Police.

The Criminal Register is seldom referred to. I frequently send descriptions to Criminal Registry Office, but they rarely find the man.

In one case, where the chief mark was a scar on the forehead they sent us two photographs to compare, but neither was right. He was afterwards recognised by a detective from Manchester. His name was in Distinctive Marks Register 1884. Several pages are covered by names of persons with scar on forehead; it is not really a distinctive mark. Photographs are of immense use. We photograph every one charged with serious crime, both local persons and strangers. Descriptions without photographs are useless.

Major Lane, Governor of Leeds Prison.

The bulk of old offenders are identified by route-forms, but route-forms are useless without photographs.

Photographing of all remand prisoners, more particularly those committed for trial, would be most useful.

In a case last week, a route-form came from Wakefield Prison giving description of young fellow for trial who stated he had been in Leeds Gaol lately, but gave a false name. It was returned marked "not known *in that name*." Photograph was sent next day and was recognised at once.

Habitual Criminals' Register is used; three copies are in use in prison. Not many identifications are got by means of it.

Leeds and Bradford prisoners usually photographed by the police. County cases are not often. Prisoners under remand sometimes object to be photographed, but when they do, snap shots can often be taken.

All habitual criminals are photographed before discharge, some in their own clothes, others in prison dress. The photographs are taken by an outside photographer. They are very good photographs.

Mr. W. Poppleton, Assistant Warder, Leeds Prison.

I was reception warder at Wakefield for several years. I took distinctive marks of prisoners on reception. In *felony* cases the marks are taken on reception on remand; the marks revised before Form R. is sent to Habitual Criminal Registry at expiration of sentence.

Of our prisoners, nine out of ten are local men and known to me.

The taking of particulars, marks, &c., occupies about seven minutes. I can take about eight an hour. In some cases the time is much longer, when there are many tattoo marks.

Since circular of distinctive marks not taken in minor cases, such as persons committed for drunkenness or begging, &c., or on bastardy orders.

Tattoo marks are sometimes defaced. I know one case where a prisoner had letter D on left breast; it is now made into mermaid. This is sometimes done to prevent recognition in prison. Sometimes the tattoo is removed, but a flesh mark of same shape left.

I suggest that an experienced warder should go round other prisons in the district to detect old offenders on remand. A warder goes to Wakefield, but not elsewhere.

Mr. Taylor, Chief Warder, Leeds Prison.

I have been warder at Wakefield, Scarborough, Southwell, Northallerton, and Leeds. Good many old offenders escape. More since the photographing of remand prisoners in prison was discontinued. I think about one-third of trial prisoners are strangers—pickpockets, burglars, travelling thieves. They are routed both by prison and police, but the prison routes are without photographs, and we do not get many results. The distinctive marks are not sufficient.

The Habitual Criminals' Register used, but rarely gives results. There are too many names. The marks are not sufficiently distinctive. "Scar on forehead" would be no guide whatever.

Warders from Wakefield come here before assizes and sessions, and two warders from here similarly visit Wakefield. Visits from warders from Manchester, &c., would be useful.

Bradford, 15th November 1893.

Mr. WITHERS, Chief Constable, Bradford Borough Police.

We take photographs in all cases. The Convict Prisons' photographs very bad. They should be better, and there should be two of each convict, one with beard and one without. The head ought to be bigger. Magistrates would order photographs to be taken if they had the power.

About one-half of our trial prisoners are strangers—travelling thieves, &c. There are very few from London. They are mostly north-country men.

Birmingham, 16th November 1893.

HON. CAPTAIN ANSON, Chief Constable of Staffordshire.

Photographs of untried prisoners would be of much use. It would be better to take them in prison than in police cells. They would be much better done in prison than by the police of county districts.

Generally speaking, Scotland Yard not so successful in giving help as might be expected. They sometimes fail to recognise London men.

Warders do not recognise so many prisoners as formerly. They are less interested in the work.

More identifications of strangers are required in cases of obtaining by false pretences than in any other class of crime in proportion to the number of cases of each class.

Thomas Tattersall was three times sentenced to five years' penal servitude for larceny and for obtaining by false pretences, the last of the three convictions being in October 1889. Within four years of his last sentence, viz., in October 1893, he was again convicted of the same offence for which his unexpired sentence of five years was awarded, and got only 12 months from the same chairman who passed last sentence of five years. There was no distinction as to the offence in each case—several charges of obtaining by false pretences.

[*Two other similar cases specially mentioned to show the comparative uselessness of proving previous convictions.*]

Short sentences are much more frequent. It is now necessary to convict several times in a year where formerly if convicted once the man was sent to prison for a long period.

The Habitual Criminals' Register is useful only in Head Office, not in divisions. It is too complicated.

We receive very few route-forms.

Criminals are either wanderers over all the country or live in settled home in the place where their crimes are committed. Cases of men who have fixed place of abode, but make excursions for purposes of crime into neighbouring districts, are rare except in the neighbourhood of Birmingham. There are very few travelling burglars of the first class.

The death of habitual criminals could be reported in some cases by the police, but many must die where they are not known, and therefore deaths could not be reported with sufficient certainty to be of much use.

The "Police Gazette" would be much more useful for apprehension and identification purposes if published oftener. At present many days are often lost in publishing particulars of cases in "Police Gazette."

Mr. G. VAN HELDEN, Chief Inspector, Birmingham City Police.

The criminals we have to deal with are mostly local, but there is a considerable number of "foreign" thieves. We have pickpockets from London and elsewhere. In one recent case of a notorious burglar released to Bristol on licence under a 10 years' sentence was arrested in Birmingham for failure to report. Thieves on the way from London to Manchester, Liverpool, &c., frequently make a stop at Birmingham. On the other hand, we have Birmingham thieves who carry on their work in the adjoining counties, and only bring their goods for disposal in Birmingham.

So far as Birmingham criminals are concerned, we have little difficulty. They are known personally and we have carefully-kept records. The special feature of our books are the carefully-made drawings of tattoo marks. When a person is arrested for crime who is unknown, but who is suspected to be an old offender from some other district, our first step is to search the groups of photographs from Scotland Yard. These are most useful. I should strongly urge that more should be issued, and at more frequent intervals. Failing them, we have recourse to the Habitual Criminals Register, with the Register of Distinctive Marks. These are often useful. The average time of a search would be about three-quarters of an hour. If this fails, we would issue a route-form. These should have photographs attached. We get photographs as often as we can, but some prisoners refuse to sit. We cannot, as a rule, get photographs from the prison—the consent of the Commissioners has to be obtained and that takes too long. A prisoner was recently remanded for seven days, on the fifth the consent of the Commissioners was received and the photograph was then taken, but too late to be of any use. In that case the magistrates would not give more than one remand. I would strongly urge that prisoners on remand should be photographed on a magistrate's order. We do not get so much help from the prisons as we should have. If they do anything, they do it as a favour to us. In some cases the marks are useful; in one case, where a photograph had not been recognised by anyone, a copy was circulated of a tattoo mark which prisoner had on his arm. This was at once recognised by a Liverpool officer who had had the man in custody more than 10 years before. But as a rule, photographs are the main thing. My strongest recommendation is for greater facilities in photographing.

Home Office, 17th November 1893.

Mr. J. G. GRACE.

(*Chairman.*) You are the officer in charge of the Habitual Criminals Registry?—Yes.

How long have you held that position? — Since February 1884.

You also keep the General Register of Convicts, and are Home Office agent for discharged convicts?—Yes.

The Committee wish to ask you some questions to supplement the information as to the working of the Habitual Criminals Registry which you gave us when we visited your office last week. In the first place, we asked you, I think, to find from the Convict Register for 1892 how many cases occurred in that year of convicts who had escaped identification at their trial being recognised after registration as men who had already been in penal servitude. Have you looked this out?—Yes. I have sent the list through the Prison Registry to Sir E. Ducane, who will send it to the Committee. The number of cases in the year ended 31st October last was seven, five of them cases of persons recognised when received under sentence of penal servitude, and two cases of persons summarily convicted and found to be license-holders.

Then have you ascertained how many inquiries you had from the police during the last three months, and what were the results?—Yes, I hand in a paper with the figures. In the three months there were 61 inquiries, and in 41 cases Form R was sent. In 23 of these cases identifications were reported.

Does the identification mean identifications by the distinctive marks?—Not in all cases. Sometimes the police send the name and ask for the photograph in order to see if they have the right man. Cases traced in this office by distinctive marks are not numerous. It is to be remembered that usually the police apply to us only when other sources of information fail. When they have searched the register themselves unsuccessfully, or have issued route-forms unsuccessfully, then they apply to us.

Now, as to the dates when the registers are issued, do you issue the copies to the police and to prisons?—No, that is done from Parkhurst Prison where they are printed. I cannot give the dates.

We have got them from the Prison Department; they are, 1891, Alphabetical Register, October 7th; Distinctive Marks, December 9th; 1892, July 25th and November 23rd; 1893, both Registers, September 9th to 15th. Are not these dates very late? If a prisoner was released in January 1892, his name did not appear in the published Register till September 1893?—Yes, after one year and eight months; that is the longest.

And if he was released in December 1892, there would still be an interval of about nine months?—Yes.

So the interval varies from 20 months to nine months?—Yes.

And in this interval a great many of them will be re-convicted?—A large number will be re-convicted before the register comes out.

Some will be identified by other means, some not identified at all; but the register gives no help in these cases?—That is so.

Can you suggest any way to meet this?—I think the registers should be published monthly.

But if that were done you could no longer have the alphabetical arrangement?—Yes, the monthly issue could be alphabetical, but I would suggest that there should also be an annual issue to supersede the monthly.

(*Major Griffiths.*) It could no longer be printed in the prison. It would take too much time—Yes, but I think, if issued monthly, it would be more useful to the police.

In the interval, before publication, some of the habitual criminals die, are their names omitted?—If they die in prison, their names are omitted. If out of prison, we do not know of the death. It would be an advantage if the police would report deaths to us. We cannot know of them.

(*Chairman.*) Now can you suggest any improvement in the arrangement of the Register of Distinctive Marks? Some of the marks are very indefinite—"scar "on forehead," for instance? That is not much of a distinctive mark?—No, but the register divides the forehead into sub-sections—"above right eyebrow," "right temple," &c., and it distinguishes cuts from burns, &c., and sometimes adds a side note as an additional guide.

Even so it is still very vague At Leeds they told us of a case where the only mark was one scar on the forehead. They sent the description to you, and you sent down two photographs, but neither was right. Can you say how you selected these cases?—There are many pages in the Quinquennial Register under the heading "Head and face." "Scar—forehead"?—I cannot tell. As there was little to help us in the way of marks, we might perhaps have been guided by similarity of name.

(*Major Griffiths.*) Would it be better, if instead of dividing the Register by Head, Hands, &c., you took all the scars together, then all the birthmarks, and so on?—I think not so; it would only be one classification instead of another, and the present is better because the body is divided into sections and sub-sections, and you would only have to look at these whatever the marks might be. Under each sub-section the marks themselves are also classified.

Are all scars entered in the Distinctive Marks Register?—No, the book would be useless if we did that. I would not enter a scar unless further defined—"large scar," "abscess," "burn," &c.

(*Chairman.*) But would it not be better to limit the number further and make them more distinctive?—Yes, I do this as far as I can. If it were a scar on the thigh, I would add "back" or "front," "inside" or "outside," as the case might be, and if this were not shown in Form R. when received from the prison, I would send it back with a query.

Do you think there should be more marks?—I believe not; they have the full record in the Alphabetical Register.

But would you not give greater detail— the direction of the scar or mark, and its distance from a fixed point?—It would be of greater assistance to have the direction and size or extent of the mark given.

How often are you successful in tracing cases by means of scars?—Not often; not one case in ten.

In the table you have given us, what does "no "report" mean?—It means that we could send no information.

That is when you cannot identify the man?—Yes, cannot identify nor send a case for inspection.

Do you know when the information you supply leads to the identification of the man?—We are usually informed.

When route-forms are sent you, are they usually accompanied by photograph?—Yes, usually, but not always. Sometimes the prisoner refuses to be photographed, but that is quite exceptional.

Does the photograph help much?—It does not help us much in tracing the case as our photographs are not arranged in albums but affixed to the Forms R. When the case is traced, it confirms the identification. But the case has to be found by the marks.

Have you any other suggestions to make?—I would suggest that there should be two additional columns in Distinctive Marks Register, one giving date of birth, the other the height. It would be easier to find the right case, and would save looking up each case in the Alphabetical Register.

But can you get the age and height accurately 2—Not accurately, but approximately. The additional column would eliminate cases obviously wrong and save much labour. If your man's age was given as 60, you would not look up cases under 45.

I may mention a case which occurred some time ago where I believe the prisoner could have been identified if his mark, a scar on the back of the left hand, had been closely defined. It was described on his discharge from Woking merely as a scar on back of left hand, and so was not entered in Distinctive Marks Register. If it had been closely described, in size and extent, it would have been used as a distinctive mark. When this man was afterwards in Wandsworth Prison his photograph was sent all round London, but not recognised, and this happened again 18 months later when he was in prison at Lewes, yet he had had a career of crime in London from his boyhood. He was at last recognised by a warder in Woking, and all his convictions were brought together. This was the case of Joseph Smith, *alias* Edmund Robinson. When in Wandsworth he was a ticket-of-leave man, and was no doubt on the books at Scotland Yard, and in the "Police Gazette," as wanted for failing to report.

(*Mr. Macnaghten.*) Was that case referred to the Convict Office?—Yes, it must have been; it went everywhere all round London.

(*Chairman.*) What do you wish to show by bringing forward this case?—That it would be better to define the marks very closely where permanent scars are concerned. If the exact size and exact location had been given, the scar in this case would have been a good distinctive mark, and probably have led to identification.

Do many escapes occur in London?—I think more old convicts escape recognition till after trial in London than elsewhere.

MEMORANDA as to POLICE ENQUIRIES made of the HABITUAL CRIMINAL REGISTRY during the months of August, September, and October, 1893.

Month.	Number of Enquiries.	Number of cases in which Forms R. were forwarded.	Number of cases in which Forms R. were NOT forwarded.	Number of identifications reported.	Number of cases in which Prisoners were not identified by the Forms R.	Number of cases not yet reported as settled.	Number of cases in which no report as to identification was made.
1893.							
August - - -	18	10	8	4	2	—	4
September - - -	23	16	7	9	5	2	—
October - - -	20	15	5	10	1	—	4

Mr. J. B. MANNING, Governor of Pentonville Prison.

(*Chairman.*) What is your experience as a Governor?—I have been Governor and Deputy Governor since 1859. I was Governor at Chester Castle in 1865, at Wakefield in 1882, and at Pentonville since 1890. I have also been connected with the police, and had charge of a section of the Cheshire Constabulary. Previously I was in the army.

What is your idea of the number of old criminals who escape identification?—I do not think that a large number escape recognition. Of course in Pentonville I have only convicted prisoners, not prisoners awaiting trial.

Do you think that any considerable number of persons who are convicted as new offenders are really old criminals?—Only a few. They sometimes escape, seldom more than once, but criminals generally hover round a certain district and are well known to the police.

But do you notice cases of London thieves going down to the country?—Generally to race meetings and large gatherings. The local police generally get assistance from the London police, and men who are caught are, as a rule, identified. They are remanded for inquiry, and information is obtained.

Do you receive many route-forms from the country?—A good many. Seldom a day passes but we get two or three. They are put in the Central Hall, and every officer looks at the photographs.

And are a considerable number identified?—A considerable number. We have a large collection of photographs. The men look at those and refresh their memories, and the confirmed criminals are likely to be found.

The route-forms come from country and not from the Metropolitan Police?—No, not from the Metropolitan Police. The Metropolitan Police attend at Holloway to make identifications. An officer from Pentonville also attends at Holloway three days a week.

Do you sometimes receive route-forms without photographs?—Frequently. These lead to identification very rarely; we look, if we have time, at marks given in Criminal Register, but we are not often successful.

Speaking generally, the photograph is the chief thing?—Yes, the chief thing; all the officers look at photographs.

Do not you find the Habitual Marks Register of very much use?—Yes, but it is a large volume, and it is published much in arrear.

Many are convicted before you get it?—Yes; the sooner you get it out, the better it would be.

Do you make much use of the "Police Gazette"?—Yes.

Do not you at Pentonville receive "lodgers" for discharge?—Yes; that is, convicts from other prisons to be discharged in London.

Are their distinctive marks taken?—No; not by us. The police come and see them and serve them with certain notices.

You take photographs of convicted men?—Yes, of people whose photographs are required by the police, and those against whom a previous conviction has been proved.

Are they taken by a clerk?—By a clerk who also photographs for Wormwood Scrubbs.

Do they ever resist?—Not often. Refusal to have a photograph taken would be considered a suspicious sign and would be likely to lead to more inquiries. Some men when turned out into the yard are recognised by the others and this puts the officers on the track.

Do you think there would be much difficulty in photographing remand prisoners?—Not if there is an authority for it. In making rules for Chester Castle while it was a county prison a rule was inserted that any person convicted of, or charged with a crime might be photographed. We had a remand man in who wholly refused to be photographed; he was punished and the case was reported to the Secretary of State, who decided that the rule was *ultra vires* and should not have been made. This was the same Secretary of State who had previously approved of it.

If a prisoner resisted it would be possible to take a photograph by stratagem. Have you ever done this?—I have never done it. They are not very successful.

If it were known to be the rule, do you think the remand prisoner would object to be taken?—No, if they knew the rule and could be punished for disobedience. One man who was convicted of frequenting objected, but he was punished and he then consented.

(*Major Griffiths.*) In your experience how do men with previous convictions fare at the quarter sessions. Do the chairmen trouble about previous convictions?—Formerly if a man had two or three previous sentences he would be likely to get a long sentence, but that is not so now.

It is the custom of judges to ignore previous convictions to a very large extent?—To a very large extent.

(*Chairman.*) Would there be any difficulty in getting the finger-prints of prisoners by Mr. Galton's method?—Not much difficulty, I think.

That would be an advantage of his suggested system as opposed to measurement?—Measurements are very difficult to take. I have tried the employing two men to take the measurements, but they were found not to agree.

But this was on first trial?—Yes, on first trial. The men had no training or instructions.

If persons resist photography, would not they resist measurements?—Yes, they would do so.

Finger-prints would be much easier?—Yes, and measurements would be difficult.

Is the Distinctive Marks Register used in London?—I did not use it much in Wakefield, but find it now a great help in answering some of the questions that come to us. It is useful for prison purposes.

Mr. EDWIN COATHUPE, Chief Constable, Bristol City Police.

(*Chairman.*) You are the Chief Constable of the city of Bristol ?—Yes.

For how long have you held that office ?—For 18 years.

And had you any previous police experience ?—I had been eight years Deputy Chief Constable at Manchester, and for three years before that I was attached to Scotland Yard.

Can you give us some general idea. (1st.) As to the proportion of local crime in Bristol; and (2nd.) Of crime by "foreign" criminals, or criminals coming from other districts ?—What proportion of offenders would be local men, and what proportion foreigners ?—Fully three fourths would be our own people—would be local.

Have you any difficulty in identifying them ?—The difficulty is with regard to the other fourth.

What are the means you usually adopt ?—The police strip the man and examine his body for marks.

And having got the marks ?—They then search the register, and endeavour by this means to identify the man.

Do you identify many ?—Yes; the method is extremely useful. We trace them constantly.

Can you suggest any improvements that should be made in the form of register ?—I do not think so; I have not given special attention to that point.

Nothing occurs to you ?—No.

Failing the Habitual Criminal Register, do you get some cases from the Police Gazette and Illustrated Circular ?—Yes; from the circulars.

The police would like to have the circulars more often, or larger photographs.

Failing to get at a prisoner's identity in any other way, would you use route-forms ?—Very seldom; we have no time for that. Our magistrates would not often remand for purposes of inquiry. We have no stipendiary magistrate in Bristol. It is the largest town in England which has no stipendiary, and no solicitor to prosecute on behalf of the police.

Then you have a difficulty in getting photographs for use in the routes ?—Yes.

Do you photograph prisoners yourselves ?—No; we want to be able to do so.

Would it not serve your purpose if you could have a photograph from the prison after the prisoner is remanded ?—Yes; that would be the better plan if magistrates would remand for this purpose.

But as a matter of fact you do not photograph yourselves ?—No, we have no power to do so; we cannot hold prisoners over for a number of hours for that purpose.

Do you ever send out forms without photographs ?—Very seldom; that is of no use.

Have you any idea of the proportion of persons arrested who have come from other districts, and who have previous convictions ?—I think there are many old travelling thieves; a good number come to Bristol. It is a convenient place for Wales, and the West of England, and for Ireland. They can get away at short notice. It thus becomes a sort of centre for travelling thieves.

What character. Mostly pickpockets ?—Mostly pickpockets and housebreakers. We are infested with pickpockets. Many come from London. The quarterly circular issued by the Metropolitan Police comes in useful, but we have the greatest trouble to get men to take an interest in the cases. They take less interest in detective work altogether than they used to do.

You do not sent out many routes, but if you got photographs you would send out a large number ?—Yes.

When your offenders have been convicted, do you get photographs and keep books ?—Yes.

Do you have many inquiries from London and other counties and boroughs ?—Yes. Our only difficulty has been with Liverpool. They would not render any assistance to enable us to prove the identity of a man charged with burglary; they refused to give the name of the officer who could identify him. The case occurred at the assizes. It was brought before the notice of the judge, and I believe it is all to be reported to the Home Secretary. We got information from the warder and it made a difference in the sentence. Some of the judges do not care about previous convictions and are extremely lenient to old offenders. We have got a good Recorder.

Have you many thieves or burglars in Bristol who carry on business in neighbouring counties ?—No, very few, I think.

Do the police visit prisons in order to see if there are old offenders among the remands ?—No. We would like to inspect remands. It would be a great help to go to the prison to see offenders. It would also be a good thing for county officers to inspect city prisoners. They are all in the same prison.

Do you keep any register of local criminals ?—Yes. We keep a book with descriptions of all prisoners we have dealt with. It is indexed well, so that cases are easy to find.

Then do you think many previous convictions escape notice ?—A good many do, I fear. Our men often know a man to be an old offender but cannot remember his name and previous history. Such cases are often dealt with summarily by magistrates as first offenders.

Do you send route-form when you issue them to the adjoining police forces first, before you send to London ?—We send forms to local forces and to London in duplicate. In the case of an important prisoner we send to other places simultaneously. I do not recollect a single case of identification by route-form without photograph.

Have you any further suggestions to make ?—No, all the points I had in mind have been touched on. Bristol, with a population of a quarter of a million inhabitants, has no stipendiary magistrate and no solicitor to assist in police prosecutions.

Mr. CECIL DOUGLAS, Chief Clerk to the Lord Mayor.

(*Chairman.*) What office do you hold ?—Chief Clerk to the Lord Mayor.

Have you held that office long ? I have been there six years, and a magistrate's clerk for 18 years.

I suppose a good many of the criminals who come before your court are habitual offenders ?—Certainly, a large number.

Do you consider it the duty of the police to find out previous convictions ?—I do not know whether it is the duty of the police; we remand prisoners for that purpose. It is, I think, partly the duty of the police, and partly of prison warders.

Is there, in your opinion, any considerable number of the prisoners you remand for that purpose who are not identified ?—I could not give an accurate per centage; a good number whom I believe to be old offenders are not reported as known.

You remand to Holloway for eight days ?—Yes, for a week, sometimes for eight days.

And the police, or the police and the prison warders, are supposed to make enquiries ?—Yes.

And in a considerable number of cases they fail to make recognition ?—Yes; I should like to enlarge that statement. I know a very large proportion of the habitually criminal class. I know some of them by face, I can tell others by the way they stand in the dock, and the part they take in the proceedings. I am surprised in a great number of instances that men whom I feel sure are old offenders are not recognised.

If they are not recognised are they usually dealt with summarily ?—That depends on the nature of the case. In many cases they are dealt with summarily, where, if known to be previously convicted of felony, they must be sent for trial. For other offences we must of course commit for trial, even when there are no previous convictions.

Of those committed for trial, is any considerable number recognised after committal ? –Unquestionably.

Would you hear of this officially?—Not officially, but I should see it from the prison calendar.

Does that occur in a considerable number of cases?—In about 20 per cent. I think.

And you think a good many prisoners of that class are not recognised?—Yes, I think so. I am often surprised that old offenders are not recognised.

What steps do the police take in the city besides inspection at Holloway?—I do not know exactly. An illustrated circular is issued by the Metropolitan Police, which is used.

If a prisoner is recognised in Holloway, how are you informed?—In the City if a prisoner is remanded and recognised by a warder, or by a constable, we get a form from Holloway telling us of his previous convictions, and the name of the warder who can prove them; where the magistrates will require to know of the previous conviction, we give notice to the warder to be in attendance on the day of trial. In other cases we do not require his attendance.

How do you do, at Mansion House, in the case of prisoners charged with larceny who have been previously convicted?—We are told that in some courts such cases are often dealt with summarily. In such cases we invariably send for trial. We have no option. If a prisoner has been previously convicted of felony on indictment we cannot legally deal with the case summarily under the Summary Jurisdiction Act, 1879. It is prohibited by section 14.

Have you anything to suggest in the way of improvement of system?—I believe your Committee has had a report from Mr. Williams. With regard to paragraph 4 of that letter, I would like to say this to the Committee. I have been in communication with Detective Inspector Downes, who is in charge of the arrangements for the identification of prisoners, and he states that under the old system, before the issue of the circular preventing the warders and police consulting together, they conferred together before they decided whether A or B had been previously convicted. This is now stopped, and that makes identification more difficult. Another thing, Inspector Downes tells me, and I can easily understand it, that the wearing of prison clothing by the prisoners makes the work of identification much more difficult. An experienced old thief will get into a suit much too large for him and will attempt to disguise his figure. Another reason why I think old offenders escape recognition is this: that a great number of old warders have retired.

It has been suggested to us by a good many police officers that more photography of prisoners on remand would be a useful thing for identification purposes?—Yes, certainly.

The Secretary of State has power to make rules for this purpose?—Yes.

Supposing a rule were made in this shape—that photographs be taken by order of the magistrate who remands prisoners, would that work well?—I think my own bench would make the order in all proper cases. My bench would give all necessary facilities for identification if the Home Office asked for it.

As a matter of fact you now always remand if a man's previous character is suspected to be bad, and inquiry is desired?—Yes, I have no reason to believe that any man charged with picking pockets who has been convicted before is dealt with without the remand necessary for inquiries. In a case of false pretences we should be more likely to commit for trial at once, and let them find out his previous convictions between committal and trial. We do not make a hard-and-fast rule. The great percentage of prisoners charged with crime are remanded.

A good many you think still escape in remand cases?—Yes, I have not worked out any calculation. My impression is that under the old system before 1888 more were recognized than now.

(*Major Griffiths.*) Have you a large amount of local crime?—The City is a happy hunting ground for pickpockets from all parts of London. We see a great number of old friends, but few of them are resident in the City.

Are there any "foreigners" from the country?—Yes, a fairly large number. I believe also, a good number of foreigners from abroad—German and Polish Jews, and a few Russians.

Have you anything to suggest?—I think that all prisoners on remand should be photographed. I have had in my experience many instances which shows its value. A prisoner has been remanded and no report received from the Governor of the gaol; but an officer from Scotland Yard has sent word to the police officer in charge of the case, that he had been recognised from photographs in their books. In a case where the magistrate would deal summarily with the man, I would say to the prisoner, "You have been convicted at Wolverhampton, do you admit it or will you be remanded?" Prisoner says, "I deny it." I then say, "Is that your photograph?" The man says, "It is no use denying." Further than that, I do not think I have anything to add to the report Colonel Smith has sent in on the subject.

Home Office, 18th November 1893.

Captain Showers, Chief Constable of Essex.

(*Chairman.*) You are Chief Constable of Essex?—Yes.

How long have you been Chief Constable?—Six years.

Had you any police experience previous to your appointment as Chief Constable of Essex?—Yes, at Exeter, and Superintendent in the Devon County Constabulary.

How many years in all?—Since 1883—10 years.

The first question on which we wish information is as to the methods used by your police for the identification of old offenders?—We depend mainly on photographs. When a person brought before the magistrates is supposed to be an old offender, we apply for a remand. Then he is photographed, and route-forms with the photograph are circulated. In this way we obtain identification in most cases.

You get prisoners remanded for the purpose of being photographed?—Yes. In most cases we get photographs without difficulty, but power should be given to the police to compel suspected persons to be photographed.

Do many refuse?—A good many; particularly old offenders.

And in that case do you get them by stratagem or by force?—By stratagem usually. We do not use force.

Do you get good photographs?—Very good. They are taken by local photographers. Some are taken in gaol, but this is exceptional.

Do you use the Distinctive Marks Register and Habitual Criminals Register?—Yes, very much indeed; we find them very useful.

Do you have a good many London prisoners?—Yes. All our worst criminals come from London, the burglars and horse stealers. They go back to London to dispose of their booty. The crimes of local people are usually poaching and larceny.

Does each of your superintendents use the Habitual Criminals Register?—Yes.

Does it require a very long search to find a prisoner in the Distinctive Marks Register?—Not, I think, very long.

Something would depend on the nature of the marks?—Yes. Some are easily found. In other cases the marks are not very distinctive. I think it is a pity that old offenders are not branded. That would be the most effective way of securing recognition.

What about the Police Gazette?—It is not of much use in tracing the antecedents of prisoners. The route-form is the best when there is a photograph. Without the photograph we hardly ever obtain any result.

Do you use the Illustrated Circular issued by the Metropolitan Police?—Yes. It helps us a great deal, and is of great use in Essex. It would be more useful if issued oftener than three times a year.

Where do you send out the route-forms?—We send to the different prisons and different counties. To the

Metropolitan Police pretty early, being so close to Essex. Route-forms are not much use without photographs; with photographs they are of immense use.

You want additional means of taking photographs?—Not only power to take them, but to punish for refusing to be taken. Magistrates are quite ready to remand in all proper cases. They are always ready to help us.

You think it would be well if the Secretary of State made rules under the Penal Servitude Act, 1891, to compel remand prisoners to be photographed?—It would be better if compulsory power to take photographs were given to the police rather than prison authorities. When the photographs are taken at the police cells we have copies ready to send out the same day. If they are taken in prison we cannot have them before the third day at earliest. When the remand is only for seven days, the loss of two days in making inquiries is serious.

Do you think that many escape?—Very few escape identification in Essex.

Do many of your criminals come from other districts?—All the bad criminals come from outside Essex, burglars and horse-stealers especially. We hope soon to have telephonic communication with the Metropolitan Police, and this will probably be of much use. Taking all kinds of crime, 75 out of 100 are Essex local men, and of the remaining 25, the greater number are identified before trial.

(*Major Griffiths.*) Do you find that the previous convictions make much difference in the sentences?—Yes. Most judges and chairmen of quarter sessions are very much influenced by previous convictions. Some of the judges do not pay much attention to previous convictions, but that is exceptional.

Supposing power to photograph in all cases were given to the police, what proportion would have to be photographed?—About 20 in the 100. The other 80 do not require to be photographed; they are known. The photographs are done by an outside photographer, to whom the police pay 1*s*. for the first photograph, and 4*d*. each afterwards.

Would prisoners object to being photographed if they knew that it was the law that they should be photographed?—I think not, if they knew that the police had power to compel them. If a prisoner even knew that he had to be photographed in prison, he would probably not object as he does now to being taken by the police.

Inspector JOHN ROBERTSON, Liverpool City Police.

(*Chairman.*) What position do you hold in the Liverpool Police Force?—Detective-Inspector.

Are you head of the detective branch?—No, the assistant head constable is head of the detective department. I have charge of the convict branch. In addition to other duty, I have to deal with all routes received, and routes sent out, and all correspondence relating to prisoners in custody. I have the assistance of a sergeant, and the partial services of a constable.

What is your procedure in identifying criminals?—All our prisoners are sent to the main Bridewell, and each morning, either I, or the sergeant, or constable, often all of us, visit the Bridewell, and see all the prisoners charged with felony. All prisoners arrested the previous evening are then in the Bridewell; but they are not detained there more than one day. One-day remands are kept in the Bridewell; Saturday to Monday remands, and all remands for more than one day are sent to prison. Three-day sentences are kept in the Bridewell.

In the morning, from 7 o'clock to 10, the description clerk of the main Bridewell takes the description of all prisoners before they are brought before the magistrates, and many local men are at once recognised. The father's Christian name and occupation, mother's maiden name, and (if prisoner be a married man) wife's maiden name are taken. All prisoners are indexed under their own names, their mother's maiden name, and their wife's maiden name, and these indexes are of considerable assistance, as if a man alters his name, he frequently take his wife's maiden name, or mother's maiden name, or gives one or both as before, and many are traced in this way. All this work is done between 7 o'clock and 10 in the morning. There is one description clerk for felony cases, another for "disorderly" males, and another for females. Three description clerks are employed, and they have the assistance of four or five other men. When they have done their work, I and my assistants visit the prisoners, and identify those we know. We often call in outside detectives who may be likely to know them, and the books are referred to. No prisoner runs more than three years in one ledger. The ledger is only for three years; each ledger has a separate index, so that if convictions extend over a longer period they may be traced through several books; the preceding folio is quoted in the latest ledger and *vice versâ*. The felons ledgers have been preserved since 1856.

We have the advantage in Liverpool of the prison being a receiving prison for a very large area, from Birkenhead, from the adjoining districts of Lancashire and Cheshire, and from the neighbouring boroughs. I am glad to acknowledge the assistance rendered by the prison officials.

Do you visit the prison to identify prisoners from other districts?—No. No periodical visits are paid to the prison by the police.

Then in the case of a prisoner belonging to Liverpool being arrested at Birkenhead, the Liverpool police would not have any chance of recognising him in prison?—Not in prison, but the Birkenhead police would bring him over during the remand for the purpose of being seen. They bring prisoners to the Bridewell for this purpose from Birkenhead and other boroughs and from Lancashire.

But to return to what is done before a prisoner appears in court?—If neither the description clerk nor any of my branch nor the detectives recognise the person we apply for a remand and obtain the photograph of the prisoner at the Bridewell, before he is removed to prison. Some object to being photographed, but we have arranged a room where the objectors can be photographed without knowing it (witness here gave a description of the room). Some of the photographs taken in this way are good, others are not so satisfactory. There are some offenders who have been told of this room and they refuse to go upstairs. When this is the case we do not use force, we apply to the prison for photographs. Persons who refuse to be photographed by the police will submit in prison. The prison authorities are always ready to assist us in this matter. Probably 20 or 30 photographs are taken in the prison for the police in the course of a year.

Our albums contain photographs representing about 2,300 persons.

And when you have got the photographs do you issue the route-forms?—Yes, that is a matter requiring much discrimination. I feel it a tax upon the time of other police forces to route a man broadcast; we only route a prisoner when it is really necessary to do so, and then we exercise great care as to where we send the routes. Frequently we are able to obtain information from friends of prisoners as to who he is, or we may obtain information as to where he comes from or where he is likely to be known. Some forces seem to route everybody they get. We receive an immense number of routes issued quite indiscriminately and the labour of examining them is very great. From September 29th, 1892, to 29th September last we sent out routes for 74 persons, sending each to perhaps 20 places on the average, and we received about 1,200. Preston borough appears to route every person arrested; Salford send a great number, but they keep a good register and classify their album very well, and never fail to send a result of the case, a thing many forces neglect to do. The majority of the routes received would be from Salford, Leeds, Stafford, Nottingham, and Leicester; Belfast are now routing a good many, Glasgow and Edinburgh if they suspect the person has been our way. Newcastle-on-Tyne also sends a good many.

Do you use the Habitual Criminals Register?—We find that the register sent from the Home Office is invaluable. It is helpful to us not only for travelling thieves but many of our local thieves. The only suggestion I would make would be its issue at an earlier date.

The Habitual Criminals Register is useful not only for identification but also for obtaining the accurate particulars of the previous conviction. The weekly list from Scotland Yard also helps us. The Illustrated Circular is a very good thing in the Metropolis but not helpful to us except for reference.

Where do you send your route-forms?—The great centres to which we send route-forms and the places from which we receive the greatest assistance are the Metropolitan Police (the Convict Supervision Office, Scotland Yard, help us very much), Glasgow, Edinburgh; and with regard to Edinburgh the police do pretty much the same as they do in the metropolis, viz., inquire at Prison and Registry, Salford, Leeds, Stafford, and both local and convict prisons.

Do you think many prisoners escape identification?—Very few. It is very seldom a prisoner gets through the police and the prison without being recognized. After he has undergone examination by the police he has got the same to go through with the reception officer of the prison, and if not identified by either he is routed. Of 74 persons routed 64 were identified, only 10 failed.

Do you keep a record?—We have a route-book in which a man would be entered, with the date and place where his route came from.

Do you suggest any alteration in the rule as regards the photographing of prisoners?—We get very nearly all the photographs we want. We either get them ourselves or by applying to the Governor of the prison. I understand that it is the rule that remand prisoners may be photographed. I think under the Standing Order the Governor of the prison photographs prisoners without applying to the Commissioners of Prisons.

When do you get the photographs from the prison?—As a rule on the third day.

Home Office, Friday, 8th December 1893.

PRESENT:

MR. C. E. TROUP (Chairman).
Major A. GRIFFITHS.
Mr. M. L. MACNAGHTEN.
Mr. H. B. SIMPSON (*Secretary*).

Sir RICHARD E. WEBSTER, Q.C., M.P.

1. (*Chairman.*) We have been in Paris, and seen M. Bertillon's system in operation. As we understand that you have also seen it, perhaps you will give us your opinion?—I mentioned to the Home Secretary that when I was in Paris I saw it on two or three occasions and I went into it very thoroughly, and it seemed to me, for reasons that I will state, if you wish it in that way, to be by far the best system that I had ever seen or heard of and to possess many remarkable safeguards against the possible identification of innocent people for those previously convicted. I do not care so much (although of course it is very important) that previously convicted people should be identified. Its great importance or one of its principal features, to my mind at all events, was the practical impossibility, in my opinion, if it is properly worked, of the confusion of any person charged with one previously convicted or previously charged. That was the first, perhaps the leading, idea that I gathered or formed about it. And the next is the great facility for registration and classification and tracing out particular cases, assuming identity. Those are the two points which I should like to refer to subject to anything that you might wish to ask me.

2. Yes, that is what we would like from you?—I think I had better take the second first, because the reasons for the first depend on the correctness of my judgment, in regard to the second, I will tell you the practical result in regard to classification. On the first occasion when I was there, a man who had distinctly given a false name and said that he had never been charged before was measured in our presence, we asking our own questions as to the method of measurement, merely being taught as to the order of measurement, and only asking sufficient questions to understand what was being done. After five or six measurements—eight actually taken, I think—one card with the five or six measurements was given to the Attorney-General who accompanied me and myself, and without any assistance in less than five minutes we found the card, which according to the measurements, would be the man, assuming the measurements to be accurate. On that occasion the card bore a different name, the man having been identified, and we ourselves having picked out the card without the slightest assistance. There was a photograph on the first card. I should not have identified the man by the photograph; but I do not lay any stress upon that, because I do not pretend to be a judge of that class of photograph, and I am bound to say that the Attorney-General said that he should have identified the man by the photograph. However, the impression on my mind was distinct that I should not have identified him by the photograph. I may mention that in that case the previous photograph had a moustache and beard, and the prisoner, when presented before us, had no moustache or beard. I mention that because I do not want to overstate the matter, and possibly better judges would have been able to have identified the photograph at once. I should not. Having identified the man, M. Bertillon handed to us the first card, and said, "Look at the private marks,"—you are aware, of course, that after measurement private marks are taken,—and the first mark was, that if this was the man whose name was on the previous card, there was a scar on the second or third finger of his left hand, an approximate measurement in a certain direction. Prisoner's hand was held up, and there was the scar. I then looked at the first card again. It stated that if this was the same man, he had a tattoo mark of an anchor, about an inch long, on his left arm, the posterior side of his left arm, rather faint. The prisoner had got a bandage round his arm. It was pulled off, and there was the anchor. It was not that we had got a man very like him, but it was unquestionable that we had picked out that man, and I only refer to that for the purpose of showing that, assuming it to be correctly taken, the mode of classification is singularly easy to find your way about. The whole operation would not take us more than four minutes.

3. This would be a case where the whole measurements came within one class, not a case on the margin?—Your mind is entirely following the same groove as mine. I merely mentioned that with reference to the first point of registration. I then put questions upon the very point you have referred to, viz., that the leading measurement might be a misleading one. You remember the first measurement of classification is the length of the head beginning from a point above the nose to the back. I investigated myself as to how far the leading measurements were likely to be defective or misleading. I satisfied myself, as far as my judgment was worth anything, that the probability of error of measurement is very small.

4. I think 185 to 190 millimetres is the middle class: suppose you had a case where the measurement is 184 or 185?—I satisfied myself that supposing the dividing line should indicate 185, or whatever it is, the margin would not be more than, at the outside, 183 or 187; so that anything near the line could only mean double the time for the purpose of search, and further than that, that the only result in case of failure would be non-identification, not identification of an erroneous person.

5. Supposing you missed the case altogether?—The only result would be that you would miss the case altogether, not that you would identify an erroneous

person. I was further satisfied in my own mind, and I went into it as thoroughly as I could, that the measurements which are taken as leading measurements cannot be altered at the will of the person measured. There must be possibility of mistake in the act of taking the measurement. Although I myself trying subsequently, inexperienced as I was in the taking of measurements, found that in taking this measurement from back to front of the head, that the calipers, appeared to fall into position almost automatically. I do not pretend to say whether there is a possibility of large error, but if there be the possibility of error, it would only have the effect which I have already indicated. In my opinion, with regard to all the other measurements, the leading measurements seem to me to a large extent to be measurement of bones, and they use the height only to a very limited extent, for obviously the height is one in which a man can appear shorter or longer by the movement of his knee-joints and in other ways. Therefore, from the points of view of failure of measurement, it seemed to me, from an examination of—well, perhaps, altogether some three or four cases, that I do not think it was likely that with ordinary experience a man would get into the wrong class. If he got in, assuming him to be there, it was only a question of investigating two sets according to the first leading measurements, and if he were missed it would be a case of not being found, and not the wrong person identified. Then with regard to the actual measurements taken; as to whether they are the wisest or not I am not in the least competent to express an opinion beyond this, that there seems to be such a margin of variety in the various measurements taken—for instance, this one, 185, it was not a question of everybody being close to 185, but it reached up to 200 on the one side, and the next class down to 180 or 179, or something of that kind. Therefore, the allowance for error with regard to the matter was so remarkable that I do not think there was much probability of error. I have no doubt you have been informed by M. Bertillon of what he informed me—I have no reason to doubt his statement—that his system has been in operation for ten years, and they have never known a case of an innocent person being mistaken for a guilty one. I think they do miss some cases, but they have never known a case of mistaken identity in that sense. I should like to say that, from the little experience I have had, I do not value the photograph identification very much. I have no personal experience of its working to enable me to give an opinion worth anything, from what I may call practical experience, but I looked for a very large number of cards that were admittedly identical, the same prisoners, men who had not given a false name, but men who had been in more than once; certainly the changes in the photographs were very remarkable, such as the cut of the hair, the condition of the man in health, and, of course, beard and moustache, and all those things. But the condition of the man in health appeared to make the photograph very different, although they were undoubtedly the same. I think that is all that occurs to me, unless you would like to ask me a question upon the first part of the matter.

6. I do not think we have any more questions to ask on that part. And now with regard to the second part—With regard to the second point, which is the impossibility or the improbability of a person being mistaken for the same criminal as a man supposed to have been there before, I think, having regard to the numbers of definite measurements that are taken and assuming men to be impartial and fair, that the improbability of the same combinations is so great that the system checks it. And I cannot help feeling in my own mind certain that, from that point of view, the ultimate examination made of the previous prisoner for secret marks is a great safeguard, because, although it may be an extremely useful, and in my opinion is an extremely useful method of getting at the prisoner and classifying him, so as to identify him, a man might have been imprisoned at Marseilles and then appear at Paris; a prisoner could have been imprisoned at Toulon and then appear at Havre, if there were greater identity in measurements and at the same time not absolute identity. I think the marks that were subsequently taken, assuming them to exist, would in a very large number of cases prevent an erroneous identification.

7. (*Major Griffiths.*) Have you seen Mr. Galton's finger-print system?—I have not seen the enlargement of it. What I say in regard to that, if I may express an opinion without having seen it, is that it seems extremely difficult to classify.

8. (*Chairman.*) The classification is the difficulty; but from the point of view we are talking of now—proving identity—it seems to be absolutely safe?—I do not see any reason why it should not be supplementary, why it should not be added on. I have no practical knowledge of it, and from the point of view of classification, it struck me as extremely difficult. It would be, in fact, what I might call a supplemental test at the end of the others. I do not know whether or not it was possible to get much difference due to the degree of pressure put upon the hand.

9. The degree of pressure makes no difference—But with regard to the Bertillon system, I have talked it over with Colonel Talbot, the Military Attaché at Paris; he is a most able man. He told me that he had formed the strongest opinion; that in a few years it would be an absolute preventive of double enlistment, and it would enormously facilitate the work of the Department. I only mention that because it is an entirely different point of view, except in this respect, that there again you want only to be doubly careful that you do not charge a man with having enlisted before. It is not a matter that you fail to charge a man who has enlisted before, but you do not want to charge a man who has not enlisted at all.

10. Have you ever thought at all of the application of it to England?—I have thought a good deal about it. Do you mean from a practical point of view?

11. From a practical point of view?—I think there would be very great difficulty at present in applying it to persons who are only charged, and I do not see the the necessity of it myself. I am speaking first of persons who are charged as distinguished from persons who are convicted. Practically speaking, all you want to do is to measure the convicted, not an innocent person.

12. (*Major Griffiths.*) Surely the object of the whole thing is to bring out the previous convictions?—Yes, the second time. Your first measurement need only be in the case of a convicted prisoner. The only people who could wish it to be done in the case of first charges would be innocent people, that they may not be mistaken for the person who had been originally charged; but from the point of view of public sentiment, although I personally should not have any objection, there is a reason why a man charged should not be measured. It would be in all probability wiser if you were proposing to apply it that it should be in the first instance solely applied to the case of convicted prisoners, or, if you like, prisoners charged with certain offences. With regard to the practical application of it I made some inquiries about expense, but I do not think my opinion is worth very much in regard to that.

13. But in every case the jury would not be satisfied with a few measurements for identification; they would require personal identification as well?—I do not know enough about the system to answer that question.

14. Any identification of that sort would have to be followed by a personal identification by some individual who knew the man, no jury or court would accept those measurements and finger prints or measurements alone as a proof of identification?—Of course, from the point of view of evidence, they undoubtedly would not at the present time. I do not think I should suggest any attempt to establish that such measurements should be identification at present. But I am by no means certain of what might happen 15 or 20 years hence.

15. In the first instance it really is an index to find the faces of individuals?—In the first instance it really is an index to find the individuals in order to ascertain for yourselves whether or not the person has been previously convicted.

16. Exactly?—And as I say again, to prevent a person whom a jailor thinks is very like a man whom he saw 10 years ago, being charged for the same offence.

17. That is the most important point no doubt, and must of course receive proper attention?—I do not think you can bring it out too prominently. I do not think there is anybody who takes an interest in this matter who does not believe that it is infinitely more for the protection of the innocent man who is charged than it is for the punishment of person who is charged with having been previously charged. It is, in my opinion, an absolute safeguard if properly worked—fairly, honestly worked—against an innocent person

being charged, or it even being alleged against an innocent person that he has been previously charged or previously convicted. I should think it would be a good thing if, at any rate for some years, the measurements were taken quite independently by two different people. I asked M. Bertillon about that, as to whether he found that they often made a mistake, but he told me that they really had become so experienced, that they had not found a mistake in some hundreds of times. From a public point of view—at any rate tentatively — I think it would be as well that the measurements should be taken quite independently by two different people.

18. It might be taken by the police and the warders?—I do not suggest who should do it, but there might be two independent measurements.

19. When the system is fully established, we propose that all prisoners on remand should be measured?—Well, before you can make any practical use of the system that must come. If a man is not committed for trial, you do not care whether he has been convicted before or not.

20. (*Chairman.*) It is rather an important point in deciding whether he is to be committed for trial or tried summarily?—Yes. That, I thought, was a detail in the criminal law that did not affect my investigation of the system.

21. (*Major Griffiths.*) The curious thing with our prisoners is that the moment they know, or think we know, who they are, they give in and admit their identity?—That is what they also told me in France.

22. Is there anything else you would wish to add?—There is one other point which I think should be noted, that in three minutes we picked out that man out of 138,000 cards; it took us, certainly, not quite four minutes to get the right card out of 138,000, and that the information given by the measurements once recorded is available for and can be sent to every prison in the country.

Dr. John George Garson.

23. (*Chairman.*) You are one of the vice-presidents of the Anthropological Institute, are you not?—I am one of the vice-presidents.

24. And you have done special work for the Anthropological Institute?—I have been working not for the Anthropological Institute alone, but I was for 10 years assistant in the Anthropological Department of the Royal College of Surgeons Museum, and I have also done a very considerable number of examinations of the living subject.

25. By measurements?—Measurements of the living.

26. Then you have a good deal of experience yourself in taking anthropometric measurements?—Very great I should say.

27. And also in training other people in it?—Yes, I have had considerable experience in training other people also.

28. Have you gone into M. Bertillon's system of measurements? — Yes, I am thoroughly acquainted with the system.

29. The first point on which I think we should like to examine you is the question of what measurements, if we were to take some part of M. Bertillon's measurements, what measurements are best to take for the classification of criminals?—Bertillon's measurements include the height, the span of the arms, length, and breadth of the head, length and breadth of the ear, the foot, the middle and little fingers, the fore arm, the cubit; these are all his measurements, I think. There is the colour of the eye, the hair, and beard, and also any marks on the body.

30. Yes, but then the point is this, I think. For the purposes of classification we want, do we not, to have the measurements which are most permanent in the individual, have the widest range of variations in different persons, are most convenient to take, and also have the least correlation to one another?—These are the different points for which one has to look. The most important measurements are those that can be taken between absolutely fixed points of bone. Of those of M. Bertillon, which I have just mentioned, I consider the length and breadth of the head, the cubit and the middle finger, the best and most important.

31. Those four?—Those four. The length of the foot is a very good measurement too, but care has to be taken in making it. The criminal, or whoever is being examined, has to stand on one foot in rather an awkward position. Of M. Bertillon's measurements, these five are the best, I think.

32. Then you think the fifth one, the foot, is not so good as the others?—Not quite so good.

33. On the ground of the awkwardness of taking it?—Yes, and also there is an arch in the foot—in the central part of the foot—which is liable to vary somewhat.

34. (*Major Griffiths.*) Unless the foot is absolutely flat?—Yes.

35. The toe-nail causes some difficulty too—the length of toe-nail?—Yes, and another thing is in wearing boots, the large toe gets twisted round, very often a bunion forms on the great toe joint, consequently you have a reduction in the length of the foot produced as age advances.

36. (*Chairman.*) What do you think would be the danger of error in the foot; how much variation might there be?—From half to nearly a centimetre.

37. Nearly a centimetre?—That would be an extreme variation, certainly. I would consider about half a centimetre—five millimetres—would not be a very great difference in the measurement of the foot of a young man and when he is old. But for its liability to vary during life I think the length of the foot a better measurement for purposes of classification than the finger, because it is longer and has a greater range of variation in different persons.

38. (*Major Griffiths.*) I was going to ask what do you think the next best measurement after the foot or equal with the foot. (*Chairman.*) Or instead of the foot?—Instead of the foot I strongly recommend the breadth of the face, across here (*showing*), the bizygomatic breadth. I have got some statistics of it. Every year for the last seven or eight years I have superintended an anthropometrical laboratory at the British Association meetings; during that time I have measured something like 1,000 people, and I have got out the statistics of the breadth of the face, which perhaps would give you some idea of its merits. The table shows you that though there is correlation between the breadth of head and breadth of face, yet that correlation is not very close. You see that there is a certain correlation, because the figures run somewhat obliquely; but take any breadth of head that you like—this is the breadth of head down this column, the horizontal row of figures is the breadth of face—you see there is a very great range in the breadth of face correlated with it. That is to say, that a man with a head of, say, 154 millimetres in breadth may have a face breadth of from 112 to 144 millimetres.

39. There is certainly a great variation?—There is a great variation.

40. (*Major Griffiths.*) Would you hand this in?—I shall be very pleased, or I will make you a copy more suitable for printing.

41. (*Chairman.*) Perhaps you can make some summary of it?—I can make a table of it by putting in the numbers in the little squares. (*See Table, printed p. 55.*)

42. (*Major Griffiths.*) So that you would really recommend the breadth of the face, or substitute it for the foot?—Well, I think it fulfils the conditions I have indicated as being essential for a good measurement. It is easily and accurately measured, not liable to vary during life, and not closely correlated, as we have seen, to the head breadth.

43. (*Chairman.*) How is it taken, and how far would it be affected by emaciation or stoutness?—It is best taken with sliding callipers such as are used for measuring the head. Being measured between two subcutaneous surfaces of bone, it is little affected by emaciation or stoutness, certainly not more than the breadth of the head is affected.

44. Of course you would allow there was some advantage in measuring the foot simply on the ground, that to have the same thing that they have in France counts for something? — Oh, of course, that is so decidedly, and probably when you come to speak about the method of making measurements I would have something to say in regard to that also.

45. (*Major Griffiths.*) I think it was Mr. Galton yesterday or Sir William Flower who said something about the hip bones?—There was another measurement I was going to suggest, which would be a very good one, that is the breadth of the pelvis or haunch bones, not the hip bones. The only difficulty I have regarding the pelvis is this, that you require to strip a person; at least the trousers must be down.

46. That is not a difficulty with us, because our prisoners are always stripped?—Very well, then that would be a most excellent measurement. I hand in a table of pelvic measurements of skeletons, which shows considerable range of variation.

TABLE of MAXIMUM BREADTH of PELVIS (Skeleton).

Measurement of Pelvic Breadth.	Number of Cases.		Total.
315–319 - -	× =	1	65
310–314 - -	× × =	2	64
305–309 - -	× =	1	62
300–304 - -	× =	1	61
295–299 - -	× × =	2	60
290–294 - -	× × × × =	4	58
285–289 - -	× × × × × × × × × =	9	54
280–284 - -	× × × × × × =	6	45
275–279 - -	× × × × × × × =	7	39
270–274 - -	× × × × × × × × =	8	32
265–269 - -	× × × × × × × × × =	9	24
260–264 - -	× × × × × × × =	7	15
255–259 - -	× × × × × =	5	8
250–254 - -	× × =	2	3
245–249 - -	× =	1	1

47. And the measurement itself is very unvarying in each person, is it not?—Yes, very invariable; you can feel it, how very firm it is; at this point there is hard bone on each haunch which is just below the skin, so that you can get an exact measurement from point to point with a sliding rule.

48. (*Chairman.*) If a man got very stout, would that affect the measurement?—No, not particularly; the stoutness would be above or below. Another very good measurement is the height of the knee, but it requires to be done very carefully. I am not quite so certain about recommending that, considering that you would not have highly educated men to make the measurements probably. Mr. Galton did take the height of the knee at one time, but what would require to be done if it were taken would be to seat the person on a stool of perhaps not more than 12 inches high, and have some arrangement against which the leg would rest, so as to insure it being in a vertical position when measured. If you feel on the top and outside of your knee at this point, when you hold your knee in a flexed position, you will feel that there is hard bone, showing that you have got the end of the bone there perfectly subcutaneous.

49. (*Major Griffiths.*) Then as to the breadth of span of arms that M. Bertillon takes?—The span if taken as he does it, against a wall, is a very fairly good measurement; if taken in any other way, I do not give much for it, because so much depends upon the will of the person, exactly in the same way as the height.

50. (*Mr. Macnaghten.*) But I should have thought as to the height of the leg up to the knee, that a man could contract it a little?—Not if he is put into the particular position I have indicated.

51. We should have some rather dubious kind of gentlemen to measure?—Yes.

52. I thought you could contract the calf of your leg?—That would not affect the length of the leg if the foot is flat on the ground.

53. (*Chairman.*) Would not these knee measurements be more or less correlated with the cubit?—To a certain extent, but I do not know that they would be very closely correlated, but I have no statistics showing whether it is so or not.

54. (*Major Griffiths.*) What do you think of the ear measurement, which is one of M. Bertillon's measurements?—The ear may be taken, though it is somewhat delicate to measure properly, and its lower end varies in form. The Saxon ear is said always to have a distinct lobe, but in other races the lobe is wanting, so that you get a considerable amount of variation, and I think it would be rather difficult perhaps for an uneducated person—when I say that I mean persons of such education and training as you are likely to have as measurers—to take its dimensions with sufficient accuracy to use it as a measurement for classifying; besides its length is so short that an error of even one millimetre would be considerable per cent. of its length.

56. The shape of the ear comes under "distinctive marks"?—That is so.

57. You were going to tell us something about the method of taking the measurements, were you not?—With regard to the length of the head, M. Bertillon takes it from the root of the nose, but it is somewhat troublesome to get an instrument to lie upon that point.

58. In India, Mr. Gayer has invented a modification, or rather an addition to M. Bertillon's instrument, in the form of a split tube which slips on to one arm of the callipers with a transverse rest for the nose.

59. Which fastens over the bridge of the nose?—So that the point rests upon it.

60. (*Chairman.*) You have got a specimen?—I have tried to get a copy of the illustration, but have not succeeded. I can give you the reference however; it is described and figured in a report to the Government of Bengal in 1892, last year.

61. (*Major Griffiths.*) They have introduced measurements then in Bengal?—Yes, I believe so.

62. (*Chairman.*) What was the subject of the report?—It was on this subject, the Anthropometric Identification of Criminals. Then, perhaps I may say, treating of that paper also, he has also introduced a rest—a handle—by which you can hold the callipers more steadily while using them, and has attached a small spring to the limbs so that they are drawn together with a regular pressure. And then in regard to that heavy instrument by which the cubit is taken, he found it was rather difficult to get the people who make the measurements to take them accurately, he introduced a coiled spring by which the sliding arm is drawn home with more or less constant pressure. He found it most important, for the foot especially.

63. To have a constant pressure?—To have the arms of the instrument pressed against the part of the body measured with a constant pressure. That is an instrument which I have used for measuring the head. (*Instrument produced.*)

64. That is almost the same as M. Bertillon's?—Very nearly.

65. It is rather lighter?—This is Broca's; M. Bertillon's is almost the same as Broca's, only much heavier. This is the original instrument which was invented by Broca for making measurements on the living.

66. (*Major Griffiths.*) I suppose, as a matter of material, it would be better to have them in metal than in wood?—Oh, they would need to be in metal.

67. (*Chairman.*) In India do they measure the head length from the same point on the nose as M. Bertillon does?—I think they have followed M. Bertillon entirely. In all our measurements for race characters, we take the glabella, as it is called; that is to say the most prominent point of the forehead. There is less danger decidedly of putting the point into the eye in that way, and it gives you a maximum length of head.

69. A little longer than the other?—A little longer than the other; all our statistics are made by this means.

70. Do you find it as easy to fix the exact point to take it from?—Quite as easy; the only difficulty that I could imagine regarding this point is that there is an air-space underneath the glabella which increases in size as you develop towards adult age; in a child it is much less than it is afterwards. That can be the only objection that I can see that could be advanced against taking the measurement from this point.

71. (*Major Griffiths.*) And the same variation would not occur if it were taken from the root of the nose?—No, not from the lowest part of the bridge of the nose; still the glabella is a much better point generally.

72. Easier to get at?—Yes, I think so, especially if it is taken with sliding callipers like those of Sir William Flower. We consider it is far the best point to take. The root of the nose used to be taken by Professor Virchow in taking the length of the head,

but I think he has now abandoned it and follows every other person in taking the glabella.

73. Why did M. Bertillon take the root of the nose?—Why he took it I cannot understand, unless for that reason I have just mentioned, which would only apply to young criminals of less than 25 years of age at the very outside.

74. (*Chairman.*) What age do you think we can take as the age after which the size of the skull would practically not vary?—The skull, as a rule, has attained its full size from about 22 to 25; it certainly may increase after 20, but to a very small extent. I have put in my bag here an important paper by Mr. Venn, of Cambridge—The results of measurements made in the anthropometrical laboratory of Cambridge University on 1,095 students. These tables will show you the results.

75. Are those individual cases or average?—These are average cases, as far as I know there are no statistics on the same individual, showing the variations, or whether there are variations with the regular sequence of years after the age of 23.

76. There are very slight variations after 21?—Yes.

77. (*Mr. Macnaghten.*) I can understand the breadth and length increasing, but how does it happen that at 24 it goes back?—That must be the accident of chance.

78. (*Major Griffiths.*) Of the measurements?—Yes, in the series of men measured at 24 years, the number of students with small heads happened to be greater than in the preceding series.

79. (*Chairman.*) It is an average?—Yes. These anomalies are corrected in the plate which accompanies the paper, showing the curve of the increase in size of the head, of different grades of students.

80. Poll men increase most rapidly?—Yes, they do.

81. What do you think as to the adoption of the metric system of measurements?—I cannot too strongly recommend its adoption for all measurements, even for stature and span, but I consider it is absolutely necessary, if we desire accuracy, that the smaller measurements, such as those of the head, arm, leg, or other parts of the body, be made by the metric system.

82. It does not really involve any training in the metric system on the part of a man?—No, it does not.

83. They simply would tell off the instruments?—They simply would tell off the instruments.

84. They need not even know there are such things as millimetres?—Well, I strongly advocate training the men in everything, and showing them the reason why a thing is done in a certain way, as I like to get them to bring intelligence to bear upon the subject that they are doing. You get much more reliable results than if they are simply mere mechanics working by "rule of thumb."

85. But still, as a matter of fact, it is not absolutely necessary for them to understand the metric system?—Oh, no, it is not necessary; they simply know that the breadth of the head is 140 or 150, or whatever else it is.

86. What sort of men do you employ in making these measurements?—The most that I come across are all educated gentlemen, very often travellers.

87. Then what do you think about the possibility of getting prison warders to do it?—I think that anyone who is a good writer, or dexterous generally, will very probably turn out after some training a fairly good measurer.

88. You have had some experience, have you not, training police in ambulance work?—Yes. I have had very considerable experience in training and in examining police in ambulance work.

89. So that you know pretty well what the capacity of that class of men is?—Yes, I know very well what the capacity of those men is, and I have found that the older men who have got as it were into routine habits, mechanical sort of habits, are usually the worst to train; if you get an older man——

90. (*Major Griffiths.*) That is a question we have already arrived at, that those that we taught should be young men principally?—Yes, that is very essential.

91. (*Chairman.*) You do not think there would be any difficulty in getting young men taught sufficiently to be able to do it?—None; there need be no difficulty whatever.

92. Have you made any table of the margins of error which occur in these head measurements?—No, I have not.

93. You do not want it for your purpose?—In general anthropometry on the living one gets few opportunities of obtaining such statistics, but I have had considerable experience of repeated measurements of skulls and limb bones on the skeleton. In the case of head and other measurements which are well defined, the margin of error is not great. Skulls measured independently by Sir William Flower and myself in the College of Surgeons Museum, or which I have repeatedly measured, would sometimes show a difference of 1 or rarely 2 millimetres, but in most cases the results would be the same, the skulls being under similar conditions when measured and re-measured. There are certain errors liable to occur in making each measurement, and it is necessary for the measurer ever to be on the watch to guard against making them. For example, the breadth of the head, which should be measured exactly transversely to the head length, while the ends of the callipers are held exactly level, is liable to error through the callipers being held more or less obliquely in one direction or the other.

94. But do you think, in practice, the error would be more than 1 millimetre or 2 millimetres?—I do not think it should be with fairly well trained men.

95. (*Major Griffiths.*) Could not that be counteracted mechanically in some way by having a third leg to the calliper to fix it to the upper part?—I am afraid not. I prefer the sliding callipers to those of Bertillon, because you have got the bar across, which enables you to see pretty well whether you are holding it horizontal or not.

96. (*Chairman.*) You prefer the sliding callipers?—Yes.

97. Professor Flower's?—Yes, callipers of that kind.

98. How long a training do you think it would require to teach a man?—I should think about three weeks or a month would be sufficient.

99. Giving an hour or two of measuring a day, I suppose?—Yes. I would recommend that first of all the measurements be explained systematically on the skeleton in the first instance, and the men shown exactly why everything is done; then proceed to exercise them for the practical work of measuring the living.

100. Do you think the best plan is to send one or two warders to Paris to learn it from M. Bertillon, or do you think they would learn it here?—I should think they could be taught it very well here.

101. (*Major Griffiths.*) And we might teach them our own way?—Yes, quite so. I think it would be much better to train them at home.

102. I suppose you have no doubt that there is no variation in these various measurements after 25?—Not appreciable.

103. Not appreciable?—The head measurements I consider are really stationary in the adult, although variations take place in the structure of bone in old age, but that is more particularly in regard to the proportions of the various constituents of the bone—the organic become less in proportion to the inorganic.

104. (*Chairman.*) Would you undertake to teach warders these measurements?—Yes, I should be very pleased to do so, very pleased indeed.

105. (*Major Griffiths.*) To teach a few to start, at any rate?—Yes, I should be very pleased to do so.

106. And then, if you had six or seven men well taught, they could teach other men?—I do not think that would be an advisable arrangement, but would strongly recommend that all the men needed for this work should be taught by an experienced teacher who has had a scientific training, otherwise errors in method and results will be increased at an inverse ratio.

107. (*Mr. Macnaghten.*) They would form a nucleus?—They would certainly form a nucleus. I think it would be a good plan in starting anything of this sort to make it a reward to be a measurer, and so to give some stimulus to a man to do the work well. In fact you might train a greater number than you actually

want and make a selection—make competition for the posts.

108. (*Major Griffiths.*) Make it promotion to become what we call a reception warder for the purpose of measurement?—Yes, and he should have passed competent examination before you allow a warder to take the measurements.

109. You see we should want about 60 warders?—Yes. There would be no difficulty in teaching that number.

110. We should probably have to take the material we could get; we should not be able to compete very much.

111. (*Mr. Macnaghten.*) Well, but 60 would only be one man for each prison; you ought to have a kind of understudy, I suppose?

112. (*Major Griffiths.*) And in the big prisons you would want two or three in the reception office; in the smaller prisons you would only want one; that is where the difficulty comes in.

113. (*Chairman.*) Yes, there would be a difficulty; the medical officer might sometimes superintend.

114. (*Major Griffiths.*) In a small prison he is an outsider, he merely comes in for his work?—But the medical officers would have to be instructed in it also; they are not by any means up in this work.

115. It is purely mechanical?—Yes, but the measurer requires to think what he is about.

116. (*Major Griffiths.*) Perhaps you can suggest improvements upon the French instruments, or do you consider them the best possible?—No, I do not think so; I think we should get equally good instruments made in this country.

117. (*Chairman.*) Should we have to start making new instruments, or are there instruments to be got?

118. (*Major Griffiths.*) We could make all the instruments; once get your plan, there is no difficulty in producing the thing by contract in any quantity?—Yes, it would depend on the measurements that were finally settled to be made.

119. (*Chairman.*) Do you know Mr. Galton's Finger Print System?—Yes, I have worked with him for over a year on these finger prints.

120. Our idea is to classify first by four or five measurements and then by the finger prints?—Oh! You mean to classify by finger prints also?

121. Classify by measurement and supplement by finger prints?—That is distinctly the value of them; they are quite as good as photographs.

122. (*Major Griffiths.*) We shall have the photographs too as a third means of identification?—Yes, that would be very good; in fact one could almost do without the photographs if we have the finger prints, I think.

123. You like the finger prints?—I like the finger prints very much indeed; I think they are very important.

124. (*Mr. Macnaghten.*) What we thought the weak point was the means of classification there; there is not really a mode of classification?—I do not think it is possible to classify, except very roughly, by finger prints.

125. You see, you have only the arches and the loops and the whorls?—These are the only three forms you have; if you want a case identified, the imprint would require to be sent up to some central office, where some expert would go into the question and examine all the different minutiæ.

126. Of course, as a matter of classification, M. Bertillon's system is perfect?—I think it is as perfect as any system can be for purposes of classification.

127. It is an admirable system?—But the limits of the divisions or groups that are used in Paris would very probably not apply to this country.

128. (*Major Griffiths.*) No, the race characters would be different?—The race elements, of which the population of this country are composed, are blended in different proportions from those of France, indeed there are some race elements in this country which do not exist in France and *vice versâ*; the measurements also of English people are different from those of French, consequently the limits which have been fixed for the middle groups of each measurement to give an equal division of cards in the different departments of the cabinet at the central office in Paris, if adopted in this country, would produce a very unequal division of cards. For example, the limit fixed for medium heads, as regards breadth, is 154–158 millimetres inclusive. This it is calculated will divide, say, 90,000 cards into 30,000 narrow, 30,000 medium, and 30,000 broad heads. If the same limits of medium breadth were adopted in England, I calculate, from the table of measurements I have handed in, that instead of there being an equal number of cards in each group, there would be 32,368 narrow, 35,211 medium, and 22,421 broad heads. It is therefore evident that lower limits than those adopted by M. Bertillon for the medium group, as regards head breadth, will require to be taken in this country. Other measurements would vary in the same way, so that we will require to have our own divisions of the groups.

129. That is only a matter of cataloguing after all for the expert who has got his central office?—Yes.

130. You cannot settle that until you have got the measurements?—Yes, we could, as there exists sufficient materials for determining what should be the limits of the groups. We have several thousand measurements from all parts of the country now, and there should be no difficulty whatever in working out the limits.

131. So that you could give us the proportionate limits of each class, the long, medium, and short?—Yes, for almost all the measurements. The only one that there are no statistics of is the foot measurement.

132. (*Chairman.*) Suppose you had a collection (say) of 500 cards, and classified them roughly by the finger prints, could you find your cards out of that number?—Finger points alone?

133. Yes, after you had got the total number of cards reduced to classes of about 500 each?—That is rather a large number, I think, to classify by finger prints.

134. You think that is rather a large number?—Yes.

135. You think it would be better to have only about 100?—Very much better; 50 I would say.

136. Mr. Galton, you know, picks them out very quickly out of a very much larger number than that?—I know he does; but unless you are going to have experts to do it you will find difficulty.

137. It would only be done at the central office, of course, and by an expert. At the prisons, all that would be wanted is a merely mechanical process, and all the cards would be at one central office where they would be classified by an expert. Do you see any difficulty about that?—No, but I do not think it is desirable that at the central office such a system of classification should be adopted as requires minute examination of the finger prints, as with a lens, or counting ridges, before the cards could be marked and put into their proper pigeon-holes. The impressions are often not very distinct.

138. (*Major Griffiths.*) We went to Pentonville last week and we took 100 prisoners whose hands were in all conditions from picking oakum and so on, and we picked out the others?—Yes.

139. (*Chairman.*) There was not a single one of the 100 prints taken that was not easily decipherable?—Yes, but I think it would be very much better to trust in the main to measurements for classification, using the finger prints chiefly for identification.

140. Is there anything else you have got to say?—There is the subject of peculiar forms of any part of the face, like the ear.

141. Which ought to be used as a distinctive mark?—Well, of course, if you have a profile photograph, that takes in the ear, which is important. Then I was going to suggest that there might be some advantage in noting the outline of the nose. In the ethnographical survey that we have going on just now in this country we have the outlines of noses recorded. These are the various types of noses. (*A copy of "Notes and Queries on Anthropology" was here produced.*) The first five are well-known forms, and they are very easily distinguished. The measurer standing at the side simply notes, by a single numeral, which form of nose the man has, unknown to the criminal who is being measured. It might be useful.

142. (*Major Griffiths.*) I doubt if there is sufficient difference; you know we have to do with warders?—

Yes; if you have got a profile photograph it is not necessary.

143. The colour of the eyes?—The colour of the eyes is a very difficult question.

143. (*Major Griffiths.*) M. Bertillon has seven colours, has he not?—Yes.

145. (*Mr. Macnaghten.*) Do you think there cannot be more than three?—Well, the arrangement I came to in regard to this subject in "Notes and Queries for Anthropology," compiled for the use of travellers, who are usually pretty fairly educated people, was to divide the eyes into three primary groups—dark, medium, and light; we have figured four eyes to show the variations of the medium group. All these forms are included in the medium group, eyes at all darker would be called dark, and anything lighter would be called light.

146. (*Chairman.*) What is this?—That is hair colour, and the other is skin colour.

147. (*Major Griffiths.*) Skin colour is not much used, is it?—Not in this country.

148. And hair colour?—That we class in the same way as the eyes—the dark, the medium, and the light.

149. For the purposes of identification the hair colour would be useless, of course, as it changes?—After a time it changes colour.

150. (*Chairman.*) Have you anything else noted?—I think that is all that I have to say. I may add that I consider the adoption of the anthropometric method of identification in England is very desirable, and would mutually be of advantage both to ethnological science and to criminal anthropology.

TABLE showing the co-relation between MAXIMUM BREADTH of FACE in 570 MALES from the BRITISH ASSOCIATION ANTHROPOMETRIC LABORATORY STATISTICS.

Max. Breadth of Head.	Maximum Breadth of Face (from the external surface of the zygomatic arch of one side of the face to that of the other) is given in the horizontal series of Figures at the top.																																																	
	109	110	111	112	113	114	115	116	117	118	119	120	121	122	123	124	125	126	127	128	129	130	131	132	133	134	135	136	137	138	139	140	141	142	143	144	145	146	147	148	149	150	151	152	153	154	155	156	157	Total.	—
197	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	1	570
191	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	569
170	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	–	–	–	–	2	568
169	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	566
168	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	2	565
167	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	1	–	–	–	–	–	–	3	563
166	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	1	–	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	3	560
165	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	1	1	–	–	–	–	–	1	–	–	1	–	–	–	–	–	–	–	–	–	5	557
164	–	–	–	–	–	–	–	–	1	–	1	–	–	–	–	–	–	–	–	–	–	1	–	–	–	1	1	–	–	1	–	–	2	–	–	–	–	–	–	–	1	2	–	–	–	–	–	–	–	11	552
163	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	1	1	–	1	–	–	1	3	–	–	–	2	–	1	–	–	–	1	–	–	–	1	1	–	–	–	–	–	–	–	–	14	541
162	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	1	–	–	1	–	–	1	–	–	–	1	1	1	–	3	1	–	–	–	–	–	–	–	–	–	1	–	–	–	–	–	–	1	1	14	527
161	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	2	–	–	1	1	–	–	2	–	1	–	–	2	2	1	–	–	3	1	1	1	1	1	–	1	3	–	1	–	–	–	–	–	–	26	513
160	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	1	–	–	–	3	2	–	1	–	1	2	1	2	–	4	–	1	–	2	–	1	–	–	–	–	–	1	–	–	–	–	1	24	487
159	–	–	–	–	–	–	–	–	–	–	–	1	1	–	1	–	–	1	3	–	4	2	2	1	2	3	1	–	1	3	2	2	1	2	–	–	1	1	–	–	1	–	–	–	–	–	–	–	–	35	463
158	–	–	–	–	–	–	–	–	–	–	–	–	2	–	3	2	–	1	–	1	1	2	5	2	1	2	–	2	2	2	1	3	1	1	–	2	2	1	2	–	1	–	–	–	–	–	–	–	1	43	428
157	–	–	–	–	–	–	–	–	2	1	–	–	–	2	1	1	1	1	–	1	1	–	1	–	2	2	5	3	1	3	1	3	3	1	1	–	2	1	3	1	1	–	–	–	–	–	–	–	–	45	385
156	–	–	–	–	–	–	–	–	–	–	2	–	1	1	2	1	3	2	–	2	1	1	1	2	2	1	1	2	–	–	–	9	4	1	–	2	–	–	–	1	–	–	–	–	1	–	–	–	–	40	340
155	–	–	–	1	–	–	–	1	–	–	–	–	–	–	2	1	–	–	1	–	2	–	4	1	4	2	–	–	–	–	2	2	–	–	1	–	1	–	–	1	1	–	–	–	–	–	–	–	–	27	300
154	–	–	–	1	–	1	2	1	3	–	2	–	1	2	2	4	1	3	5	2	1	4	7	4	3	3	3	2	1	5	1	–	3	2	–	2	–	–	–	–	–	–	–	–	–	–	–	–	–	68	273
153	–	–	–	–	–	–	1	–	–	–	–	–	–	–	1	3	1	–	4	–	2	1	–	2	1	1	1	1	1	1	2	1	–	1	1	1	1	–	–	–	–	–	–	–	–	–	–	–	–	28	205
152	–	–	1	1	–	–	–	1	2	–	1	–	1	1	5	5	–	–	4	1	–	1	1	3	1	5	1	1	–	1	1	3	–	–	–	1	1	–	–	3	–	–	–	–	–	–	–	–	–	48	177
151	–	–	–	–	–	–	–	1	1	1	–	1	1	–	2	3	–	3	–	1	1	4	2	2	2	2	–	–	1	1	–	2	1	–	1	–	1	–	–	–	–	–	–	–	–	–	–	–	–	34	129
150	–	–	–	1	–	–	–	–	–	–	–	2	2	–	1	1	–	–	–	–	–	–	3	–	2	1	–	2	–	3	1	–	–	2	–	–	1	–	–	–	–	–	–	–	–	–	–	–	–	22	95
149	–	–	–	–	–	–	–	–	–	–	–	–	–	2	1	1	1	–	1	1	1	–	1	–	2	3	1	–	2	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	17	73
148	–	–	–	–	–	1	–	1	–	–	–	–	3	–	–	2	1	1	–	–	–	1	2	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	12	56
147	1	–	–	2	1	1	1	–	–	–	–	2	–	–	1	–	–	–	2	–	–	1	1	–	1	–	2	1	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	18	44
146	–	1	–	–	–	–	–	–	1	–	–	1	–	–	–	–	1	–	–	–	–	1	–	1	1	–	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	8	26
145	–	–	–	–	1	–	–	–	–	–	1	1	–	–	–	–	–	–	1	1	1	–	–	–	1	1	–	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	8	18
144	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	2	10
143	–	–	–	–	1	–	–	–	–	–	–	1	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	3	8
142	–	–	–	–	–	–	–	1	–	–	–	1	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	3	5
141	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	2
140	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	0	1
139	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1	1
	1	1	1	7	3	3	4	6	10	2	7	10	12	10	26	28	9	12	25	12	15	25	29	20	28	31	22	20	12	32	13	30	18	13	6	12	12	5	5	10	10	3	2	1	1	2	0	1	3	570	
	1	2	3	10	13	16	20	26	36	38	45	55	67	77	103	131	140	152	177	189	204	229	258	278	306	337	359	379	391	423	436	466	484	497	503	515	527	532	537	547	557	560	562	563	564	566	566	567	570		

LETTER to the SECRETARY from SIR WILLIAM FLOWER, K.C.B., with regard to Dr. Garson's Evidence.

British Museum (Natural History),
Cromwell Road, London, S.W.
January 11, 1894.

DEAR SIMPSON,

. . . . I have now carefully read Dr. Garson's evidence given to your Committee, and it seems to me to be all quite satisfactory.

As I told you, I have not had the experience that he has in measuring the living, all my anthropometrical work having been confined to osteological specimens, and I have had no means of testing some of the points upon which he speaks with authority. I see, however, no reason to question any of his statements or opinions. We shall be in a better position to speak positively on the value of some of these measurements when fuller data have been collected relating to the same individual at different periods of life and at various intervals of time. Changes in the length and breadth of the head, after the age of 25, must be very slight indeed, and if they occur would be all in the direction of increase. The same would be the case with the breadth of the face, as the zygomatic arches might possibly develop with great exercise of the masseter muscles. The measurements made at Mr. Galton's laboratory will I hope be the means of giving this, and still more, the greater number that will be obtained when what I trust will be the recommendations of your Committee are carried into effect.

Believe me,
Yours very sincerely.
W. H. FLOWER.

Home Office, Monday, 18th December 1893.

PRESENT:

Mr. C. E. TROUP.
Major A. GRIFFITHS.
Mr. M. L. MACNAGHTEN.
Mr. H. B. SIMPSON (*Secretary*).

Mr. FRANCIS GALTON, F.R.S.

151. (*Chairman.*) You have studied the subject of finger-prints for a good many years, have you not?—I have; I took up the subject in 1888.

152. You took it up originally chiefly from the point of view of heredity and racial distinctions?—Yes, subsequently I became interested in the matter of personal identity. I gave a lecture on personal identity before the Royal Institution in 1888, in which I described M. Bertillon's plan and added some views of my own.

153. The questions we are going to ask you now will be chiefly on one or two points upon which we want to have your evidence formally on the notes. They will be merely supplementary to the explanations you have given us already, when we visited your laboratory. The first thing we should like to hear you about is this—there are two qualities in the finger-marks which you think specially suit them for the purposes of establishing identity; the one is the persistence of the marks, and the other is the enormous variety in different individuals?—Quite so.

154. First, we should like you just to tell us in a word or two the evidence you have of the practically absolute persistence of the marks?—I hand you an album which contains all the evidence I possess, or nearly all, upon which those conclusions are based that are given in my book entitled "Finger Prints"; they are the prints of the fingers taken of the same persons at the beginning and end of different intervals of time. They refer to 10 different persons, the interval between the first and the second impressions varying in the different cases from 9 to 31 years. I have also this other packet of prints from eight different natives of India, which were taken at Hooghli in Bengal in 1878, and again in 1892.

155. That is an interval of 14 years?—Of 14 years—these are the originals—they have been photographically enlarged, and the enlargements are published in my book on "Decipherment of Blurred Finger Prints." Extracts from those in the album were published in a memoir read before the Royal Society in 1891, and part of them were reproduced on a still more enlarged scale in my book "Finger Prints."

156. And these examples go over the whole of life, do they not—I mean in periods?—They do, from childhood to past 80. Here is a case of a gentleman in advanced life who took his impression in sealing-wax in 1873, and again in 1890, he being then past 80. I show some of my earlier attempts, in proof of the carefulness of the way in which the prints have been worked out. These were enlarged, some by a camera lucida and the others by a pantagraph, from already enlarged photographs, and I have in each case marked the points of resemblance; the results have been published in my book on finger-prints. I especially draw attention to part of the palm of a hand of a child in 1877 and afterwards as a youth in 1890. I have divided the numerous points of comparison into groups bounded by coloured outlines in order to distinguish them and placed numbers corresponding to each. There are no less than 111 coincidences in these two prints.

157. In every case there is a coincidence?—Not a single exception.

158. You have never found a single discrepancy?—But one; to which I have given much prominence in my book on "Finger Prints."

159. That was the case of a child 2½ years old?—Yes, two ridges had merged into one by the time he had reached the age of 15.

160. But with that exception you never found any single discrepancy?—No.

161. And there has been no discrepancy in adults?—Not the slightest.

162. It has only been in that case of a child?—That is the only case I have met with.

163. (*Mr. Macnaghten.*) And this was after an interval of 13 years; 1877 to 1890?—Yes.

164. (*Chairman.*) Then you think the evidence of persistence throughout life is practically complete?—I think so; indeed I am sure of it.

165. Then have you gone into the question of how far they are affected by accidental injuries?—Yes, by accident and by age. I have a great many cases here in which the deterioration by age alone are shown, and others by age and hard work combined, but you will see on looking them through—these are south-country labourers—that in every case the pattern can be made out though there has been much deterioration in the clearness of the ridges. Now I show a case of a burn, it occurred to my assistant Sergeant Randall; he burnt his finger badly and took impressions subsequently, first when the burn was recent, then when it was healing, and again when it was almost healed; the finger is perfectly healed now.

166. Do the original marks re-appear exactly as before?—Just as before; there is not the slightest alteration.

167. After being obscured for some time by the burn all the ridges re-appear exactly, with no variation whatever?—Exactly, not the slightest.

168. When there is an injury leaving a permanent mark, if it is a very bad one, it may possibly obscure the pattern altogether?—One kind of injury obscures, but others only distort. Of the kind that obscures I show you some instances here (*showing*).

169. (*Mr. Macnaghten.*) Is that one obscured?—Well, this barely obscures the pattern; I could make out the pattern.

170. You have marked that with a Z, with a whorl underneath it?—Yes.

171. You can see there is a whorl?—You can clearly see there is a whorl here, but I can show you more difficult cases to deal with than this. In my book on finger-prints I have given a case in which a tailor——

172. (*Chairman.*) But the point that we want to get at is this,—the injury may obscure the pattern or distort the pattern, but does it ever produce alterations in the ridges such as to cause any confusion?—Rarely so as to cause any confusion. A cut must be deep in order to leave a permanent mark; an injury, whether it is an ulcer or a burn, must go deep, because the glands, whose ducts are included in the ridges and which appear to be the cause of the ridges, lie deep. When a deep cut is healed the ridges are distorted, much as the strata of a geological section are distorted by a subsidence or by a fault, but they are quite easy to trace.

173. It is always perfectly easy to tell that that is the result of an injury?—Oh, quite so.

174. You can never mistake it for an alteration of the pattern?—Never, not only is it easy to tell that it is the result of an injury, but the very sign-manual of the injury is remarkably definite.

175. In fact, it becomes an additional mark; it even assists the classification, does it not?—Quite so.

176. Then supposing that it were applied to prisoners, would it be possible for a person to obliterate the marks altogether?—It would be possible for a person to obliterate the marks altogether; in that way declaring that his antecedents were more or less suspicious or dangerous. If they were obliterated only to the extent to which Randall's burn has obliterated them, I presume a week in the hospital would entirely restore them, proper precaution being taken.

177. In fact he could hardly destroy them altogether except by cutting off his finger tips?—Except by serious injury; an injury that would take away the sensitiveness of the finger.

178. Then practically there would be no probability of any considerable number of people doing this?—I cannot think so; it is a matter of judgment, but I do not think so

179. (*Mr. Macnaghten.*) No, no, I quite agree with you?—Let me say that the indications on the inner surface of the hand are so numerous, that if out of the whole hand half an inch square were left intact, there would be enough in that to prove identity by comparison, but it would not do for indexing purposes.

180. (*Chairman.*) That brings us to the second question we wished to go into, the amount of variety in the finger-prints. I think you might tell us the results of your calculations on that point, we need not go into the details?—It is extremely difficult to answer the question in a few words, because some patterns are very common and others are very rare. I am now speaking of the patterns as distinguished only by the letters A, L, W, of which I submit various specimens.

181. Well, I rather wanted to go into the questions of how far the identity of two finger-prints established the identity of the person?—The probability of identity or the reverse that is given by comparing the details of finger-prints is enormous; I made a mistake in one paragraph (p. 110) in my book on finger-prints where by accident it was understated tenfold. It may be of interest to show the original experiments I made to determine the degree of trustworthiness of the evidence afforded by the details in finger-prints; their principle is described in that book.

182. The net result of your experiments was to show that the chances of two finger-prints being the same, within a limited area, was one in sixty-four thousand millions, is not that so?—Yes; that was the result of the calculation that I made upon a trustworthy basis. Still, I always fear these large numbers; I merely gave those figures as a perfectly reasonable result after very careful experiments; but I do not cling to them at all.

183. At any rate the probability is absolutely enormous?—Yes; it is enormously greater than what in popular language begins to rank as certainty.

184. And if one takes two or three fingers into account, it is so enormous that it can hardly be put down in figures?—It is like comparing the ground plans of towns, each of which consists of very many streets, many bifurcations, and of totally independent architecture; it is impossible to mistake the plan of even one town for that of another; much less to do so in two or three consecutive cases.

185. It is something like the chance of two cities being constructed by accident on exactly the same plan; that is what it comes to?—Exactly.

186. But to make out evidence of identity from these minutiæ it must be done by an expert; is not that so?—It must be done by an expert if it is to be done exhaustively. If it is to be done sufficiently to give a strong moral probability, a man with very little training could, without photographic enlargements, do it well enough to make it worth while to send it to an expert or otherwise to incur some expense to obtain fuller evidence.

187. Of course if it were to be actually used in a court of law as evidence, you would have to have it enlarged by photography and fully explained to the jury?—A finger-print should be very much enlarged by photography for easy explanation to a jury.

188. For ordinary purposes — supposing a warder states he can identify a particular man—in order to make sure that he is not making a mistake, it would be quite enough for any ordinary person to compare the two sets of finger-prints?—Quite so; supposing he had had some little experience in making these comparisons. A person who is quite raw does not know where to fix his attention; pointers of this kind (*showing*) greatly facilitate. Any person who examines minutiæ, and on whom some responsibility is thrown to do it well, ought to possess himself of a watchmaker's lens, or its equivalent, and a few of these rude tripod pointers, one of whose feet is a pin to place on the particular point to which he wants to attend.

189. Well, coming to the question of cataloguing, that involves the settling of patterns, does it not?—If this simple A, L, and W principle is adopted, a set of patterns is wanted for reference such as those on the table. There should also be typical specimens of those patterns about which doubt may reasonably arise. Then, by putting below each specimen the letter that is intended to represent it, uniformity in treatment can be ensured.

190. What is the proportion of patterns in which doubt has actually occurred? In what number of finger-prints would you find an ambiguous case, would it be one case in 20?—Ambiguity has many grades. When my superintendent marks the prints and hands them over to me, I have to make a correction—but seldom a serious one—in about one in 15 sets of finger-prints.

191. That would be one in 150 finger-prints?—Yes.

192. How often would you have to refer to these specimens to settle a pattern; should you say once in a hundred sets?—Though I have prepared these specimens only lately, I do not think I should have to refer to them often now.

193. You know the work so well now?—I am very familiar with it, and find certain ambiguous cases to recur so frequently, that when you have determined how to name them, they cease to be ambiguous.

194. It is only a question of learning which of the classes these ambiguous patterns belong to?—Quite so. May I take this opportunity of making an explanation? I was at a little disadvantage when the Committee was appointed, as I had not then determined how to class many of these ambiguous cases. My plan had been to leave it to Randall to write the title to each card and for me afterwards to revise them. Then I noted the more or less ambiguous cases; where there was decidedly room for doubt we conferred together sometimes. Then the cases of doubt became fewer and fewer, and I had intended at the end to have leisurely accumulated and photographed a good set of the doubtful cases, and finally to fix how they should be classed. But when the Committee was appointed it was necessary for me to catalogue with haste my collection, although these ambiguous cases had not been so thoroughly worked out as I should have liked.

195. Then do you think there would be any difficulty, supposing these were used for identifying prisoners, in getting one or two persons who in a reasonably short time might learn to work at deciphering patterns?—My experience is this: seven persons have been more or less connected with me in various parts of this inquiry, and I found that after a few days they all

acquired very fair knowledge; it was the want of a good set of specimens of ambiguous cases that prevented them from making further advance in that brief time. As an example of what has been done for me by others, I submit a small portion of the voluminous work by Mr. Collins, in which not only the A, L, W method of indexing was taken into account, but the particular pattern in a series of 53 standard patterns, which is a far more difficult task, yet he acquired the art very quickly.

196. Is this outline necessary in working these finger-marks?—I think not, if the A, L, W method only is used.

197. A learner ought to practise it?—A learner ought to practise it a little. It must be recollected that I wrote my book on finger-prints, in which the importance of outlining was emphasised, a year and a half ago, at all events, it was out of my hands 18 months ago, and I have studied the subject a good deal since; some things are now superseded that were said in that book.

198. You would dispense with the outlines except by way of practice in getting the forms into a beginner's head?—Quite so.

199. Then your method of indexing is taking the 10 fingers and appending to them the letters A, L, or W, according to the pattern of each?—Yes.

200. But on the forefinger you use the letters R and U according as it comes, R from the radial or thumb side or from the ulnar side?—Yes.

201. And that would give you possible combinations amounting to over 100,000 if they occurred quite indiscriminately?—With 10 digits their number is $4^2 \times 3^8 =$ 104,976, say 105,000, with six digits it is $4^2 \times 3^4 = 1,296$, but only a fraction of the possible combinations are actually met with.

202. That is if they occurred absolutely indiscriminately?—If they occurred absolutely indiscriminately the 105,000 possible titles would be equally frequent.

203. But as a matter of fact, they do not occur indiscriminately or anything like it?—No. One combination is very common.

204. That is all loops. It occurs in about 6 per cent.?—Yes, that is the per-centage for "all loops" of the U kind only.

205. What is the reason of your making a distinction between R and U in the forefinger and not in the other fingers?—Because R occurs very rarely in any digit except the forefinger.

206. Very rarely?—Very rarely, and from trying to pick out the instances and finding so few the mind becomes lulled, as it were, with a sense of security and overlooks them when they do occur; for that reason I have thought it better to avoid them hitherto in my particular way of working.

207. In fact it adds very much to the labour of getting correct formulæ without really assisting the classification very much?—Quite so; that is the reason why I have discarded it, but I am not at all clear that I should recommend the same plan as that which I have used, for your purpose. I think it might be better to do away with the letters R and U, and to substitute for them other letters that mean respectively sloping downwards from the upper right-hand corner of the paper to the left-hand lower corner, or vice versâ. It would be much simpler to get rid of the R and U, which have opposite significations in the two hands, and therefore strain the attention. In the way I now propose you would only deal with one signification. You would not care for the difference between radial and ulnar, but only for the direction of the slope, whether it was downwards to the right or to the left. The disadvantage would be that it is not physiologically accurate, but this is so only in appearance, because the way in which the title is written carries on its face its physiological meaning, telling which is the right and which is the left hand; if you desired to translate the title into R and U language, it could be done very easily.

208. I suppose you think it is desirable that we should take all of the 10 fingers; it very much increases the extent of the classification to take the whole 10?—On that point I have a misgiving; the gain is not so very great of 10 over 6 as it appears at first sight. Here you will find a number of the observed occurrences in a classification by six fingers.

209. Which are the six you take; the three first fingers of both hands, omitting the thumb and little finger?—Yes, the thumb and little finger sub-divide the rarer cases, but you may not want that particular kind of sub-division. If only six are taken it does not largely increase the number of the commoner cases. The commonest case, which is that of *ull, ull—U, U*, is raised from 164 out of 2,644 cases to 243. In other words it is only made half as common again. That is the only case which creates much difficulty if you are dealing with drawers that each contain not more than, say, 300 specimens, and I doubt if it is worth while under those circumstances to take the trouble of recording four more fingers.

210. Then you think, for the purposes we have in view, a record of six fingers would practically be sufficient?—If I understand your purpose rightly, that it is to divide primarily by measurement into 243 different drawers, so that each drawer shall contain not more than a few hundred cards.

211. Do you think the six fingers would work up to a thousand?—A thousand is rather large. The six-finger system would work even then if in the commoner cases the ridges in any one finger were counted or measured. I am a little doubtful about the advantage of indexing the whole 10 if you desire to expedite matters and secure the greatest economy of time.

212. You mean that the time taken occupied in taking the six fingers would be very much less than the time taken in taking the 10?—Yes; and there is also the largeness of the card.

213. There can be very little more time taken in putting down the five fingers than in putting down the three?—You have to roll them all individually afterwards.

214. (*Major Griffiths.*) The whole thing does not take more than a minute, does it?—If you do not fear the little additional time it is so much gained.

215. (*Chairman.*) It is only on the ground of saving time that you would take the six rather than the 10?—Yes.

216. (*Major Griffiths.*) It might lead to confusion—they might take wrong fingers?—Yes, certainly—it is better to have one impression at all events of the little fingers.

217. There is no fear about the time, it is a question of a minute, more or less. In taking the distinctive marks they sometimes occupy ten minutes?—A print of 10 fingers also means a large card, as you are aware.

218. We are glad of the other side of the card for the measurements and for the photograph?—Yes.

219. (*Chairman.*) What is the reason for not taking the formula from left to right?—The practice of beginning as I do, has grown into use for more than one reason peculiar to myself. Thus, I wanted many thousand prints from persons of different races, and the only chance of getting them was to ask for what could very easily be given. This led me to ask for the first three fingers of the right hand only, and so the practice of beginning with these was started. It has been persevered in, because of the great variety of pattern in the forefinger; it is the only one that frequently has an R. If you begin with the little finger nearly all your formulæ would begin with an L of the U kind.

220. Except a few W's?—Yes, but only a very small proportion of them and hardly any A's. I may as well now put in evidence a number of prints of palms and of complete hands.

221. But the palms must be a good deal more difficult to take than simply the finger-tips?—There must be a pad with a somewhat rounded surface to press upon, and soft paper like this should be used; it does not take much trouble.

222. Is it long since you took up the question of sub-classification?—In one sense I took it up from the very beginning.

223. But the mode of classifying by counting the number of ridges in the loop?—It was only when you met and asked me about sub-classification that I took it up in that way; I have counted ridges before, but not for the purpose of sub-classification.

224. But you think now that this sub-classification by counting ridges is the most practically useful one?—If the number of ridges in the first finger alone is counted, it would sub-divide the common titles into manageable groups, if you are only dealing with about 500 cases.

225. Take the ring finger, about how many classes would it give?—In the ring finger the number of ridges between the two selected termini (namely, the summit of the core of the loop and the place where the surrounding ridges diverge to enclose it), varies from 2 to 26 or more; the classes are about equally numerous between 3 and 16; there are thus fully 14 available grades.

226. Fourteen nearly equal classes?—Yes; if you counted to the nearest ridge there would be 14 classes and more; if you allowed for an error of one or two ridges, there would be say five very well-marked classes.

227. But perhaps the best way would be simply to arrange the cards in order of the number of ridges?—Yes; in order of the number of ridges in some one specified finger.

228. Then you would find the card somewhere near the place?—Quite so.

229. Do you think it needs a good deal more practice and skill to do that than to do the primary classification of A, L, and W?—It is extremely simple. The chief difficulty lies in the prints being on so small a scale that you require a lens, which some people may not be capable of using.

230. And can it usually be done pretty accurately?—Yes, I have found in going through a set of 164 cases of *ull*, *ull*—*ll*, *ll*, on which Randall and myself worked independently, that there were no cases of a discrepancy between us of more than two ridges; there were four cases, I think, of an error of two and about 18 of an error of one.

231. Generally speaking, you came very close?—Yes, we came very close when we had gained a mutual understanding about the exact principle by which the two termini should be chosen.

232. I think I have gone over all the points I have noted. We have one or two questions about Bertillon's system still?—I shall be glad to reply to them.

233. Before we come to that, are there any other remarks about the finger-prints you would like to make?—No, I think I have said all that I wish to say.

234. You have an arrangement by which you think it would be possible to take the finger-prints of a prisoner who resisted altogether?—I have.

235. Not fully worked out yet?—Not worked out on a resisting person.

236. But still you think it might be used?—I think so. I do not, however, rightly understand the degree and the sort of resistance to be feared. In experiments I have made with two small rollers set in a handle, one to ink the fingers and the other with paper round it to receive the impression a print can be obtained in an instant.

237. So that probably by holding the hand of the prisoner you might get the print?—I should think so. Another way would be to cut out holes or slits in a brass plate and to press the fingers upon them; then their bulbs would show through the holes and could be printed fairly well in that position.

238. Then, I think, you wanted to tell us something about the search of the cards?—(*The witness here explained a model of a drawer and cards, so arranged as to make it impossible to put any card in a wrong drawer in a cabinet of* 243 *drawers* [vide Appendix H.].)

239. You have also something to say about the use of a mechanical sorter?—I should just like to say a word about the possibility of hereafter using a mechanical sorter. I described one in my original lecture at the Royal Institution in 1888, and I find now that in the United States and at Vienna they use mechanical sorters in the Census Office dealing with many millions of cases. The use of the mechanical sorter is that it gets rid of all difficulty about transitional cases, and that it allows for a margin of error in measurement, also for the total absence of any one or more of many measures. (*The witness here explained the working of the mechanical sorter.*)

240. Then you have a criticism to make or some account to give of the degree of precision required for the Bertillon measurements?—Yes. I have been studying this subject, because it was stated in a memoir read before the International Congress in 1891, by Lieut.-Colonel Greenleaf and Major Smart, of the United States Medical Department (Trans., vol. IV., p. 294), that the Bertillon method had been carefully examined for the purpose of identifying deserters from the United States army, but was not found suitable owing to the inaccuracy of the measurers. It will be recollected that M. Bertillon lays much stress on extreme precision. My results are not yet quite ready for publication, but they show that if any set, whether of the objects to be measured or of errors of measurement, be divided into three equally numerous divisions of small, medium, and large, and if the distance between the lower and upper limits of the medium division be, for brevity, called the "medium range," then if the "medium range" of the errors is not greater than one tenth part of the medium range of the objects measured, not more than 5 or 6 per cent. of the sets of five measures will be assigned to the wrong division. If the error is two tenths, a search will fail as often as it succeeds. It therefore is very necessary to exert all the precision attainable. I conclude from experiments in my own laboratory that the precision of this ratio of one tenth would be practically attainable.

241. In all the Bertillon measurements?—In respect to the four elements that I have been able to measure, namely, the head breadth, the head length, the cubit, and the middle finger. I have not measured the foot, because it requires stripping, which would be unsuitable to the publicity of my laboratory. I conclude that Bertillonnage would be feasible if the measures were made with as much accuracy as at my laboratory, but it would fail if the measures were somewhat less accurate on account of the repeated references that would then be needed to conduct an effectual search.

242. By failing you mean to say that you would have to search in another division of the collection?—You would commonly find that your first search was incorrect, and a repeated search, once, twice, or more would be necessary. Precision is essential to success in search, though not absolute precision.

243. But a high degree of precision?—Yes.

244. Do you think that that degree is attainable for the four measurements which you have referred to?—Yes.

245. By ordinary measurers?—The measurers must be somewhat trained, but a small amount of training will do if they are intelligent.

246. The next point is the correlation; the effect of correlation upon successful classification?—It is a very important subject. If correlation is close, the advantage of using the correlated elements becomes, considerably reduced. I have worked out the theory of correlation in a Memoir read before the Royal Society, in 1888, where I showed that there exists what may be called an index of correlation. This may be taken to range between 0, which signifies complete independence, and 10, which signifies the strictest interconnexion. Thus the index of correlation between head breadths and head lengths is as low as 4 or 5. Between the middle finger and the cubit, it is as high as 8 or 9. Now when it is as high as 8, the available variability is reduced to about six-tenths. The subject is so very technical that it does not admit of a brief and full explanation, except in technical language; but you may take it for certain that when one limb is correlated closely with another, the advantage of using that second limb is small.

247. There is an instance of that, is there not, in this, that M. Bertillon now says that the little finger is so closely correlated with the middle finger that, after having made three divisions by means of the middle finger, he could not make three equal divisions by the little finger?—I should not be at all surprised. I should have thought the result would be worse than that. But now to give a practical application to what I said. Dr. Garson tells me that he has submitted to you a number of measurements of distances between the zygomatic arches. I do not know from my own experience whether that is an easy measurement to take, and I give no opinion upon it, nor whether emaciation or obesity would affect the measurement much; but this is certain, that it is much less correlated with breadth of head, and, so far as I infer, with other dimensions than almost any other measurement. The index of correlation between it and the head breadth is between 4 and 5, and its own variability is great, so that it appears to be a valuable measurement.

248. How would that stand as compared with the middle finger, supposing we substituted it for that?

—Subject to the reservation I began by making, I think it would be a very good substitution for the middle finger, but it would be advisable to measure the middle finger as well, in order to bring the set of measures *en rapport* for international purposes with those of M. Bertillon.

249. But for cataloguing purposes you think it would be better?—I think so.

250. Assuming that the measurements could be taken accurately?—Yes.

Mr. Angus Scott Lewis.

251. (*Chairman.*) You are in the office of the Director of Public Prosecutions?—Yes.

252. You undertake the prosecution of cases all over England, I think?—Yes.

253. But principally in the Metropolis?—Well, no, all over England.

254. Amongst others you take up all coining cases, do you not?—All Mint prosecutions.

255. I suppose you find that coiners, as a rule, are people who very often come back again on one charge or another?—Yes, very frequently; more so I should think than for any other class of offences.

256. Do you find that there are often cases where the police fail to identify a man when he comes back on a fresh charge?—Perhaps I had better say I have been through the cases for the present year. Since the 1st of January 156 persons have been convicted of coinage offences; of those about 50 have been proved to be old offenders.

257. Their previous conviction has been proved in court do you mean?—Yes, and charged in the indictment.

258. But have you reason to think that a larger number really are old convicts?—Out of 156 I think that possibly six who were not proved to be old convicts probably were.

259. What reason have you for thinking they were old convicts?—By the way in which they committed their offence; they showed that they knew how coins could be passed most successfully.

260. But do you find in any of those cases after the convictions it has come out that the man was an old convict, or would you hear of that if it did occur?—I do not think we should hear of that; I cannot say that in any of those cases we have actually ascertained that they were old convicts.

261. But you are very strongly under the impression that there are some cases?—Yes.

262. Are there cases where a man commits an offence first in London and then travels to some provincial town, or travels from one provincial town to another?—Yes, the second offence is frequently committed in a different place from the first.

263. So that he would not come into the hands of the same police force?—Yes.

264. Are there any special instances you could give us?—A man was convicted not very long ago—during the present year, I think—at the Hertford Sessions, and in that case we suspected that he was an old convict, from the way in which the coins were uttered.

265. Did you make any investigations about it?—We made every inquiry we could to try to identify him, but we could not find out any thing before his conviction.

266. Do you not keep an album of photographs?—Yes.

267. But you cannot afterwards trace them by the photographs?—No. I should like to refer presently to one or two instances that I have got. After conviction this man gave certain information which enabled us at once to see who he was. We could not say that he was an old convict, but he was known to the London police as an associate of coiners in London.

268. But while he was waiting trial you had not been able to get any information about him?—No; he refused all information, refused to be photographed, and nothing could be traced.

269. I suppose in cases of coining like that, the fact of previous offences makes a great difference to the sentence?—Yes, peculiarly so.

270. In fact, it sometimes actually affects the character of the offence itself?—Yes, the first offence of uttering is a misdemeanor punishable with one or two years' imprisonment as the case may be; whereas the second offence is felony punishable with penal servitude for life.

271. Then, it would be a distinct advantage to you in your work if we could establish some system which would make identification more easy and more certain?—Certainly.

272. (*Major Griffiths.*) Are we to understand that out of these 156 cases this year, 50 you identified, and 6 you had reason to suppose were convicts; are we to suppose that the other 100 were first offenders?—I think so.

273. (*Chairman.*) In any other class of cases have you come across instances where identity has been missed?—Well, there was a case that you know of here, the case of Blake.

274. Yes, we know the case of Blake very well?—I need not say anything about that case.

275. In that case the mistake was put right before he went for trial?—Yes, I think so.

276. It really originated in that case, did it not, from a warder making a mistake, or a police officer making a mistake, in the recognition?—Yes, more than one officer. On the question of mistakes, I do not know whether you would care to hear of a very recent instance of a mistake in a coinage prosecution.

277. We should very much?—No real harm was done. A woman has just been convicted at the Stafford Assizes for a coinage offence. She pleaded guilty, and after conviction a police officer stated that nearly 20 years previously she had been convicted of a coinage offence, in the name of S—— H——, and sentenced to 12 months' hard labour. She did not deny it, but within the last few days we have been looking at the old papers, and I have here the photograph of the woman who has just been convicted, and the photograph of the woman convicted at the Stafford Assizes in 1874.

278. Was this previous conviction attributed to her when she was convicted the other day?—Yes, but it was not charged in the indictment. It was stated to the court after she had pleaded guilty.

279. Then do you think they are not identical?—I think not.

280. Do you judge by the photographs?—I have something else that I judge by. At the Stafford Assizes in 1874 another woman who gave the name of A—— J—— was convicted of a coinage offence and sentenced to the same term of imprisonment—12 months. Now that is the photograph of A—— J—— (*photograph shown*).

281. (*Major Griffiths.*) These are the same women, no doubt?—I think these are the same.

282. (*Chairman.*) Then the mistake, if it was a mistake, did not do her any harm?—No; as it happened it did not.

283. It was only one name instead of another?—Yes. Of course, as it happens, it is not a serious mistake, but it might have been.

284. It shows at any rate that you have no materials for absolutely establishing identity in these descriptions?—No.

285. Is there any other case that you would like to mention?—No. I cannot think of any other case that would be of any service to you. I may say we did not charge that conviction in the indictment, because it was so long ago.

286. Are there any other cases?—I can say generally we have had cases in which it is thought that the person has been previously convicted.

287. Without your being able to establish it?—Yes, but that does not occur frequently in other charges with which we have to deal.

Mr. EDMUND ROBERT SPEARMAN.

288. (*Chairman.*) You have made a special study of the Bertillon system of identification, have you not?—Yes, I began it in 1886, and I think it was in March 1887 that I made my first communication to the Home Office on the subject.

289. And since then you have occasionally worked in M. Bertillon's laboratory?—I should not have written to the Home Office if I had not thoroughly worked at it; I worked at it in 1886 for more than three weeks.

290. Just as if you had been one of the assistants?—I went through exactly the same course as an ordinary warder would. Since then the Ceylon Government sent over Dr. Thornhill to study it, and as he could not speak any French, I had another three weeks' experience there teaching him, and of course perfecting myself still more, so that I think I know the system certainly as well as any Englishman. Hardly a month passes that I am not in the Paris anthropometric bureau.

291. You have written on the subject a good deal?—I have written a good deal, both in the public press and in the different reviews.

292. There is one article of yours in "The Fortnightly"?—The first article is in "The Fortnightly" of March 1890; another one in the "English Illustrated" in October of the same year, which, by-the-bye, was the only guide that they had in Ceylon to work from, and then another one in the "New Review" of July of this year.

293. You were the person who first brought it to the notice of the Home Office I think in 1887?—I was; very much to my surprise I found that neither Scotland Yard nor the Home Office had heard of it.

294. That was in 1887, when it was comparatively a new thing in Paris?—It had been in practice five years.

295. The first few years were spent chiefly in taking measurements?—Still I thought there would have been a general sort of knowledge of what other countries were doing.

296. At that time it was only used in Paris?—It was only used in Paris at that time.

297. Since then it has been extended?—In 1888 it was made compulsory in every prison in France and Algeria.

298. But as a matter of fact in M. Bertillon's office, very few identifications are made outside the cases measured in Paris?—Well, a good many are sent up. Unfortunately in their statistics they have not got the records of the country identifications.

299. In 1892 there were six identifications outside Paris?—There were, yes. That is all for legal purposes, because Marseilles and Lyons have got anthropometrical as well as alphabetical collections, so that anything done there would not want to come to Paris.

300. That is, local criminals in Marseilles or Lyons?—Yes.

301. But the great merit claimed for the system is that a criminal can be identified anywhere over the country by that means?—Yes.

302. In that respect it has not led to very great results?—It might appear so from the figures, but from conversation with M. Bertillon I fancy that they do identify a great number. They certainly identify men who come from the country to Paris, because I have seen them. When Sir Charles Russell was at the office there was a man from Marseilles, whom he identified, who swore he had no convictions at all, and seven or eight were found against him. When he was asked why he had sworn that he had no convictions at all he said, "I denied it because I never was convicted "in Paris; how was I to know that you knew anything "about the Marseilles convictions?" Belgium and Switzerland have sent measurements of criminals to Paris and M. Bertillon has been able to give information regarding the antecedents of their prisoners. Lyons and Marseilles, being near the frontiers, anthropometry has been found of much use in reference to expelled foreigners.

303. Still, I rather gathered from M. Bertillon that he only searches in a country case when a special application is made to him?—Yes, he only searches where the *juge d'instruction* in the country asks him to do so.

304. (*Major Griffiths.*) Which is not very frequent, he told us?—Not very frequent.

305. Because they do not understand yet?—Because they do not yet appreciate it.

306. (*Chairman.*) As a matter of fact, it is not really in full operation?—As far as the country is concerned, it is not. There is a feeling of prejudice; there was a great deal of trouble when he started it, even in Paris. One thing which must eventually have some effect on the country people is the fact that the alphabetical classification has red cards in all cases of expulsion, warrants out, &c. notified by the authorities. When any such person is arrested and his card is sent up from the country, the Identification Bureau at once notifies where he is custody to the proper parties.

307. As a matter of fact, the provincial people do not believe in it?—No.

308. (*Major Griffiths.*) As soon as they get a certain number of cases they will believe it thoroughly?—As soon as they do.

309. (*Chairman.*) In the meantime a great many duplicate returns are accumulating in his office?—The more you have, the better.

310. I mean a man may be convicted, say, at Lyons, and then convicted again at Havre, and both returns come up to M. Bertillon's office; but unless his special attention were called to the case, they would be two separate cases?—He would find the first when he puts the second in the alphabetical classification. In the anthropometric classification he would note the first conviction on the second card and destroy the former.

311. He makes a certain allowance for minor errors, and these errors might separate the cards: a good many of them would be separated, because a good many measurements are on the margin of two classes?—That may be, but it is exceptional.

312. But when you have eight measurements, the chances are that in one or other of the eight it will be pretty near the margin?—The original one, the one taken at Havre, would be on the margin too; therefore, it would go into exactly the same place. Whether at Havre, Lyons, or wherever it is, it would have to be put in the same place.

313. The point is, on what you may call the margin, a very slight error of measurement in one direction or the other will put the card in the one box or the other; if you measure a millimetre more at Havre than at Lyons it will go into another box?—Yes, provided it was exactly on the margin.

314. In every case there are eight chances of that happening, because you take eight measurements.

315. Well, we have had some criticisms upon M. Bertillon's measurements. The things that are aimed at in the measurements chosen are first to have a measurement that is invariable in the individual, but that has got a considerable range of variation in different persons, and next, to have measurements which are not correlated to one another, and which are easily taken. Well, I think the first two, at any rate—the head measurements—answer all those conditions, do they not?—Yes; there is one thing I want to say about it, the anthropologists generally want to take the head length from the glabella.

316. What is M. Bertillon's reason for using the root of the nose?—One warder may take it from one spot, and another from another, and they may go higher and higher; there is nothing to guide them. Under M. Bertillon's system it must always be taken from the same place.

317. (*Major Griffiths.*) There is no fixed starting point?—There is no fixed starting point; the anthropologists, in some of the places where the system has been adopted, have taken to measuring from the root. Why anthropologists want it taken from the glabella is because it will supply them at the expense of Government with plenty of additional data. They know that if you take to measuring criminals right and left they will have more data than they are likely to get at their own expense.

318. M. Bertillon began by measuring from the glabella?—He did, but the warders were uncertain about the spot. The least variation would make a serious difference.

319. (*Chairman.*) Does he find that the middle finger gives a considerable range of variation?—Yes; and it is easy to measure accurately, that is, to a millimetre.

320. Then the little finger varies a good deal along with the middle finger?—The middle finger is more easily measured than the little finger.

321. He also takes the little finger for classes?—Yes.

322. He only divides it into two classes instead of three?—He takes the little finger to have sufficient measurements in case the numbers increased, because, as I have already stated, you cannot add.

323. But is it not the case that the cards in the drawer are divided by means of the little finger?—No.

324. I certainly understood that?—Yes; but first, by the height.

325. Second the little finger, and then the third the eye?—Yes; and then stretch of arms.

326. In this book which you gave me M. Bertillon says that now, instead of dividing into three divisions by the little finger, he now divides it into two on account of its varying so with the middle finger?—In a note on page xxiii, he says he cannot divide the little finger into three approximately equal classes, the medium being the largest.

327. Then as to the eyes; he takes seven classes of eyes?—Seven classes of eyes. Well, the eye is a very important element, and at first sight it looks a very difficult thing to classify; but if you get the eyes in a good light it is a thing that is very easy to deal with. I have seen both foreigners and Frenchmen who have come to learn the system at first very much prejudiced against the eye classification, but after practice of four or five days they have quite seen the advantage of it. And if you are likely to send anyone over to Paris to study the system, before you give up anything, I should strongly advise you to see what the opinion of these warders may be after they have had experience there. Do nothing theoretically, but have the practical experience of two men who have been over there and learned. It is only the amount of yellow matter there is in the eye which is classed.

328. Does the amount of yellow matter make the seven classes?—Yes.

329. Then there are a good many cases transitional are there not; there are a good many cases where one warder would put it down as class number two, and another warder in class number three?—I do not think so. After all a mistake in that is not very important, as it is only the last division but one in the drawer.

330. It is not very far to go to the right one?—You have only to look through the seven, if you do not find the measurements come out in one class of eyes you go to the others.

331. Do you think it is necessary to retain the other measurements beyond those that are used for classification?—It is very easy to drop a measurement afterwards. The whole thing takes four to five minutes, the time you would save by dropping a measurement is not worth speaking of.

332. Why should we retain the measurements of the ear, for instance?—Well, you cannot add; when a man has once passed out of your hands, you cannot add anything to his description.

333. Yes, but what purpose does the measurement serve; it is not used now for the purpose of classification?—No, but you must look forward to having an enormous number.

334. We do not look forward to having nearly so many as M. Bertillon?—No, but you speak of years—eventually. Besides the ear is an additional test, it is one amongst the others on the card to show whether the cases agree.

335. If we take the finger-prints we shall not want that?—The finger-marks! I only say this, that during 11 years the Bertillon system has fulfilled all required of it. Why replace part of that system by something of which you practically know nothing, which at present is mere theory?

336. That would have been a reason at one time for not adopting the M. Bertillon system, would it not?—Perfectly; but now, at the expense of others, you are getting the benefit of their experience. May I just say, that in Berlin they have got a bastard Bertillon system? They had the prejudice that it was French. Their substitute has been a failure. In Italy they have tried the same thing; they tried additions and alterations. I saw a letter from the Italian Government the other day in which they said their system had failed signally, and that they are going to adopt Bertillonnage *pure et simple*.

337. There is one thing I want to ask you about measuring in M. Bertillon's office. You have, say, 100 persons measured there in the morning. 50 of these perhaps at once admit that they have been measured before, and there is no difficulty about them; the other 50 are all measured, but is the search for former measurements made in every case?—Yes, the first thing is, if a man when he arrives gives a name under which he says he has been convicted, they look to the alphabetical list and they check him with a few measurements. Well, then with the others a search is made, because when they put the card away, they will find the other one in its place.

338. But do they make a search in every case to be sure that they have not the man measured previously do they make a complete search?—They measure him, and if he does not confess——

339. What I want to know is whether they make a complete search, supposing duplicate, or triplicate, or manifold search?—The search is practically made when the cards are put away.

340. I cannot admit that at all. I am perfectly clear that in a great many cases double, or treble, or multiple searches must be made. There are so many cases lying near the margin that it is perfectly clear that search must be made in two drawers, three drawers, or four drawers, so as to make sure they have no other man?—Yes.

341. I want to know whether there is such a search in every case, or whether they simply put away the card?—They simply put away the card.

342. They do not make a search in every case?—Not unless the police want to know who he is. You do not admit it, but I say that in putting the card into its place, the other will be found. I think you consider those on the margin are a good deal more than they are.

343. You mean they do not make these double searches unless they are asked to do it; they merely sort away their cards?—They merely sort away their cards.

344. (*Major Griffiths.*) They do not look to see whether they have got the card in another drawer?—They simply put that away. I should explain that of these 50 prisoners who do not admit a previous measurement, some 30 have generally been arrested in their homes, or been given into custody by employers or others, and their names are well known. The remaining 20 are always thoroughly searched for. In case of minors a thorough search is always made.

345. (*Chairman.*) Supposing a man were found to have been previously measured, I mean by a warder afterwards, does that count against the Bertillon system? If a man were identified by a warder, immediately the measuring officials are fined.

346. Are the measuring fellows fined in every case where they have a duplicate entry?—In every case where a man is identified after he has passed M. Bertillon they are fined 10 francs, five of which are paid by the man who has taken the measurements or made the search, if it is a case of search, and five are paid by the rest of the men in the room. But you will see the number in 1892, I think there were three cases.

347. Yes, but do they count it in every case where the card is in the registry at all, or only in cases where special search has been made?—In every case where a man is identified, the only way they can tell it is by his being identified afterwards. They cannot tell how many men have passed and not been identified; they have no means of finding that out, but the prison warders are all on the look-out to gain the 10 francs and so bring out any mistakes.

348. But in every case where a man is passed without being identified, and is afterwards identified in the prison?—Then they are fined.

349. I suppose you have been keeping a keen look-out have you not for any cases of mis-identification in this country?—Well, unfortunately living abroad I do not get much chance of seeing them, it is only in the public papers, and I may not have spotted some. I think I have one where a question was asked in the House of Commons. "In 1887 attention was called in the House "of Commons to a case at the Middlesex Sessions, "where a detective, although closely pressed by counsel, "persisted in identifying a prisoner as having been "previously convicted in the name of Reeves, until the "real Reeves was produced in court, when he confessed "his error."

Then in 1888 Lord Coleridge called attention in the House of Lords to the case of a man convicted before him at Gloucester Assizes for some slight offence, when a Metropolitan constable swore to his identity with a man previously convicted who, it was afterwards ascertained, was not the prisoner. There was a difference of I do not know how many inches in his height. I have the full particulars of all these cases. I have got the newspaper cuttings; I can give you them. Well then there was a very good case, "In July last (1889) the case was mentioned in the House of Commons where a previous conviction of a prisoner in 1879 with a sentence of seven years' penal servitude and seven years' police supervision was proved to the satisfaction of a jury by a warder who had compared the prisoner with the official description of the man so convicted; it was subsequently proved by police and prison warder's evidence, that the prisoner was convicted in 1882 of a minor offence, and therefore could not be the man convicted in 1879, who would still be under police supervision." He was convicted at Brighton in 1882, and yet the warder swore that he had got seven years in 1879; I think he was a pretty clear case. Then there was "In May last (1889), David Callaghan, after having two remands of a week each was committed for trial and was sentenced to six months' hard labour as an incorrigible rogue and vagabond on the evidence of a mendicity officer and police constable, who swore to him in mistake for a man who had been frequently convicted. In prison he petitioned the Home Secretary, who after investigating the circumstances which led to his conviction liberated him," but he had suffered 12 weeks' hard labour. "The constable identified a prisoner as having been previously convicted, and the authorities on his evidence, were taking proceedings against the prisoner for having failed to report himself, but he proved that at the time the constable swore to his conviction in one town, he was in prison in another town miles away." That I think you will find in the Convict Supervision Department; they will give you all the particulars in that case. But they were actually proceeding against him for being on license and not having reported himself. That is all unfortunately I have been able to gather; these were all previous to 1890.

350. (*Chairman.*) There was one point I forgot to ask you about, namely, the arrangement of the cards. M. Bertillon now divides them into periods of 15 years, according to the date of birth, does he not?—Yes, but I spoke to him the other day and he strongly advises no attempt at that being made, because he says that is a thing you can always do later.

351. It involves as a matter of fact a great deal of additional labour?—Yes. It is very necessary when you have a large quantity, but it is not necessary now. In sorting away the cards in France there is the *état civil*. You have nothing here to check a man's age with, but by the *état civil* in France; they can always check it.

352. (*Major Griffiths.*) That is supposing they can find out who he is?—Supposing they can find out who he is.

353. In France where they catalogue every birth it is easy enough to; here where the population increases by leaps and bounds, the test of age always is fallacious?—Oh, yes.

354. Does not this arrangement frequently lead to a card being put in the wrong cabinet if a false age has been given, and the age is on the border?—Yes. M. Bertillon is strongly against your attempting it at present.

355. (*Chairman.*) Supposing the years from 1830 to 1845 make one division?—It might be; I do not know what they are.

356. Supposing a man gives his age as born 1844, do they sort away his card in that cabinet without having looked to see whether he was born in that year or not?—M. Bertillon is now working his system in connexion with the *Sommiers Judiciaires*, of which he has made the head.

357. It may be useful for the purpose of weeding out the old cases over 50 and over 60?—Gradually to weed it out, when you get your numbers very large; but it would be years before you want that. It is only to weed out the old ones; it is not to look for them, merely to get rid, to put away men who must be dead and gone. In the *Sommiers Judiciaires* they found records of men who were 110 years old.

358. (*Major Griffiths.*) He had been 110 years?—He would be 110.

359. They were not living?—Somewhat like annuitants one has heard of.

360. (*Chairman.*) I think those are all the questions which I have got specially to ask you; I do not know whether there is anything that you want to state?—No; having communicated so fully with the committee in Paris, and by letter, I will ascertain the exact number of identifications in cases measured outside Paris.

[*Mr. Spearman has since sent the following note:—* "The number of cases identified by measurements outside Paris in 1892 was 63, and not 6. By a clerical error in the figures furnished to me the 3 was dropped, thus reading 6 instead of 63."]

Mr. Charles Stewart Murdoch, C.B.

361. (*Chairman.*) You are head of the Criminal Department at the Home Office?—I am.

362. And all the petitions from all the prisoners in all the prisons of England go through your hands?—They do.

363. You see every one of them?—I see every one of them, I may say.

364. If any considerable number of prisoners complained of being wrongly identified as old offenders, you would necessarily see them in every case, would you not?—Certainly I should.

365. You have in the course of the year several thousand petitions?—Yes, that is so.

366. And do you find that there is any considerable number of them complain of being wrongly identified?—No; certainly as far as my official memory goes, it is quite the contrary. We have many petitions from persons who protest their innocence, but very few indeed who have ever protested that their previous convictions have been wrongly recorded against them. We have had a few cases, and some of them somewhat curious ones.

367. Generally there has not been much difficulty in settling the case one way or the other?—That is so. I have an instance here, a very curious instance of a certain man who protested against the previous convictions, recorded against him, one Thomas Williams. He was reported to the Home Office in February in 1885, as being identical with an old convict of the name of Walters or Evans. This man denied his previous convictions. The Home Office in looking over the case found the convict was positively identified by officers at Pentonville and Portsmouth as Evans; nevertheless, as he protested so strongly, it was thought well to refer to the judge. The judge reported that he had examined the evidence of identity at the time of the trial, and he thought that Evans was not the same person as Williams. Well, the Home Office was still not satisfied that he was not the old convict—the question of forfeiture of license was involved—and I think it was your present chairman who suggested that a comparison of the handwriting of an old petition with the writing of the petition in which he protested against the injustice of his previous convictions should be compared. The extraordinary resemblance between the handwritings in the two petitions was such that by Sir William Harcourt's direction, we wrote to the judge pointing this out and sending him the petitions. The judge was convinced, and finally this convict was held to serve the unexpired portion of his former sentence, so that you may say that this case shows that the examination at the Home Office had successfully tested the correctness of the identification.

368. Speaking generally, you do not think there is reason to think that there are any prisoners undergoing long sentences on account of convictions being wrongly attributed to them?—So far as I know, certainly not, and I think that they are so very willing and ready to protest against any injustice, fancied or real, that we should certainly have had petitions from anyone in that position.

369. Well, then on the other question of identifications being missed; that would chiefly come before the Home Office, would it not, in the case of license-holders being re-convicted, but not identified at the time as being license-holders?—Yes, certainly.

370. Because every question of the forfeiture or revocation of their license goes through your Department?—Quite so.

371. Do you find there is any considerable number of cases where it turns out on conviction that the man had been a license-holder?—I could hardly say a considerable number of cases. I have got some figures—which, though not complete statistics, will give you some idea of the number. They have, prepared by Mr. Wheeler, and they all relate to cases that have occurred during the present year. He has examined 72 cases in which license-holders were convicted summarily either of indictable offences, or of frequenting with intent to commit felony. In 33 cases they were known to be license-holders at the time of conviction, in 9 cases they were not so known. In the remaining 30 cases we have no definite information as to whether the identification was made before or after the conviction, but it is to be presumed that in most of them it was after conviction, for in 29 of the 30 cases the clerk of the court did not report the conviction as he was bound by Act of Parliament to do if he knew the man to been a license-holder. Among license-holders convicted on indictment the proportion of identifications is larger. In 103 cases, 88 were recognized before conviction and 5 after. As regards the other 10 we have no information as to where they were identified. Of course these figures cannot take any account of those who escape identification altogether—are not recognized at all. They only refer to cases recognized—and show that in some cases the recognition is not made till *after* conviction. Curiously enough, one or two very striking cases of this sort—in fact, more than one or two—have happened quite lately; I do not know whether you would care to have the particulars.

372. I think we might have them. Those are cases under consideration in the Department at the present moment, are they?—Yes, going on in the Department now. Here is one, Charles Edwards or O'Brien, who was convicted at the Middlesex Sessions, August 1888, of larceny from the person, and sentenced to five years' penal servitude. That convict was released into the Metropolitan Police district by license dated 25th May 1892. He was convicted at the Middlesex Sessions in October 1892 of stealing 74*l.* from the person. That shows that it was a considerable offence, and he was sentenced to 12 months' imprisonment. He was not recognised as a license-holder at the time, and in consequence, the forfeiture of his license was not exacted, and he did not pay the penalty of such forfeiture.

373. You have just discovered it when it was too late?—Yes.

374. (*Major Griffiths.*) What sentence did he get?—He got 12 months for that other offence; he was under an alias of Clancy. That is one case.

375. When was it discovered that he was a license-holder?—Just the other day, after his release. He was charged with not reporting while on license, and then it was discovered that the reason why he did not report was that he had been in prison under another name.

376. Then here is John Clayton, another case. Original sentence five years for larceny, at the Middlesex Sessions, in 1888; released in August 1892, into the Metropolitan Police district. Convicted at the Mansion House, October 1892 for stealing a watch; sentenced to six weeks' imprisonment; he was only recognised after conviction but in time to secure the revocation of his license. His license was revoked and he served his remanet, but earned the usual remission of one fourth, and was again released on license.

377. Both these men would have gone to Holloway, sent to Holloway for trial and seen by the police and warders?—Yes.

378. (*Chairman.*) That man was again convicted?—He was again convicted in February 1893; he was again recognised only after conviction, and this time too late to revoke his license.

379. That is twice at the Mansion House within the year?—Yes, both times convicted without being identified. Then there is a third case much the same, George William Hammond, originally sentenced at the Central Criminal Court for forgery in 1888 to five years' penal servitude; released on license into the Metropolitan Police District in June 1892; convicted in November 1892 of uttering a forged request for goods, not recognised then; he was again convicted at the Central Criminal Court in September 1893 for forgery and had 12 months; not recognised before conviction. His case rather difficult to explain. He had incurred forfeiture of his license by his conviction in November 1892; but as it was not discovered that he was a license-holder the penalty of the forfeiture was not exacted at the time, and now it cannot be exacted on his second conviction in September 1893, because the forfeiture having been incurred at the previous conviction, there is now no license to forfeit. It is a very technical point, but the result is clear. In consequence of the failure to recognise him as a licence-holder he has escaped the penalty due.

380. Do you think of anything else?—No, I think you have everything I can tell you. I could only say generally that any addition of a new system to the old which would ensure the absolute certainty of identifying old offenders—I mean to say a more perfect registry which would enable the County or Metropolitan Police to identify at once by marks or measurements old convicts and license-holders would be cordially welcomed.

381. Especially to spot them all over the country wherever they go?—Yes; that of course is most important.

Home Office, Thursday, 1st February 1894.

PRESENT:

MR. C. E. TROUP (Chairman).
MR. M. L. MACNAGHTEN.
MR. H. B. SIMPSON (*Secretary*).

Professor ARTHUR THOMSON.

382. (*Chairman.*) You are Professor of Human Anatomy in Oxford University?—Yes.

383. At the request of the Committee you have prepared a report on the anatomical aspects of the Bertillon measurements of the head?—Yes. I hand in that report.

384. The question I asked you to deal with in that report, was not so much the possibility of practically using Bertillon's measurements—we assumed that experience had proved that—as whether the margins he allows for error were sufficient?—Yes, it is the question of the sufficiency of the margins that I deal with.

(*Report handed in.*)

Department of Human Anatomy
Museum, Oxford,
January 26, 1894.

SIR,

IN compliance with your request, I have much pleasure in submitting the following statement.

In regard to the measurements of the length and breadth of the head as suggested by Bertillon, I consider that the limit of error (2 millimetres) is too small to apply with absolute certainty in all cases.

My reasons for holding this opinion are:—

I. That even amongst experts, when measuring skulls, on which of course there are no soft parts present to further complicate matters, there is frequently a difference of from 1 to 2 millimetres between the measurements of different observers. Such being the case with macerated specimens, it seems to me the sources of error in measuring the living are likely to be much increased.

II. Because on the living the accuracy of the measurements of different observers will much depend on the amount of pressure employed, thus leading to the compression of the soft tissues, and so interfering with the constancy of the results. This difficulty, however, might be overcome by the use of an instrument provided with a spring which would regulate the pressure, and so yield more or less constant results.

III. Because of the tissues themselves, which are liable to undergo atrophy and thinning as a result, either of disease or advancing years. It is a matter of common knowledge that the scalp of an old person is much thinner than that of an adult. The wrinkling of the skin of the forehead in the aged is an indication of such changes, and is in part a result of this atrophy.

IV. Because in addition to the ordinary tissues of the scalp (skin, fat, and fibrous tissue), there may be a source of error in the width measurement of the head due to the presence of the temporal muscle (one of the powerful muscles which raises the lower jaw).

I have made a number of measurements to ascertain the maximum width of the head in European skulls, and in the majority of cases, the greatest breadth of the bony walls of the cranial box, was found to fall within the limits of the temporal fossa, the area from which the muscle aforementioned takes its origin.

Thus the measurement of this diameter may be influenced in one or other of two ways:—

(*a*.) Either by alterations in the development of this muscle at different periods of life;

(*b*.) Or by the contraction of the muscle itself, which produces a thickening of its substance.

I have in this way (*b*.) been able to produce a difference of 2 millimetres in the measurement of this diameter in some individuals whom I examined.

V. Because, since the introduction of Bertillon's system 10 years ago, there has been no opportunity of testing the changes which may be produced by age. In other words there is no proof that the measurements taken from an individual at the age of 25 may be relied upon as absolutely accurate as a direct test to the same person at the age, say, of 60 or 65; bearing in mind always that by this system the range of difference allowable in these measurements is only 2 millimetres.

With a view to obtain further information on these points I would venture to suggest two lines of inquiry:—

(*a*.) That a series of measurements should be taken by competent observers from time to time in hospital from patients suffering from acute wasting diseases. This need not entail any inconvenience to the patients.

(*b*.) That observations should be made in the post-mortem room as to the thickness of the tissues involved in these measurements, at different ages, and on the bodies of those who have died in different degrees of emaciation.

It is difficult to say what one may regard as the normal thickness of the scalp, that must vary with the individual; nor is it easy to say what may be the extremes of difference without some further facts to go on. Yet I can easily imagine that at different periods in the life of an individual we may get a greater difference than that covered by a limit of error of only 2 millimetres.

There would doubtless be evidence of wasting and emaciation in other parts of the body, and no doubt an accurate observer would note these facts; but more reliance could be placed on the results if taken in association with the body weight at the time of observation, any marked variation in the body weight being taken as an indication of increase or decrease in the bulk of the tissues of the body.

The bi-zygomatic width is open to fewer objections than the breadth measurement of the head, because the tissues overlying the bone are less dense, and do not include muscular fibre, and thus the measurement dependent on the osseous form may be more accurately obtained. This measurement appears to me a good one, it is not correlated to the width of the cranial box, but depends on the form and development of the bones of the face.

The width of the bony pelvis is not likely to undergo much variation with advancing years, but here again the tissues overlying the bones may differ considerably in thickness according to the age and development of the individual, here, however, we have to deal with a greater diameter, and the range of error need not necessarily be so limited. Whatever the variation in man, it would probably be greater in woman, in whom the subcutaneous tissues are much more fully developed in this region, than in the male.

It seems to me that whilst those measurements (the bi-zygomatic and pelvic widths) may be useful as accessory measurements, it would be unwise to substitute them for the measurements recommended by Bertillon, as such a change would necessarily interfere with the international application of the system at present under consideration.

I have, &c.
ARTHUR THOMSON.

C. E. TROUP, Esq.,
Home Office.

385. (*Chairman.*) Your main point then is that for head measurements Bertillon's limit of 2 millimetres is too small?—Yes, I think it is too small.

386. Bertillon gives a "theoretical margin" of 1 millimetre only. Your criticisms would apply more strongly to this—Yes. I think 2 millimetres is too narrow a margin. We have no evidence to prove that these measurements can be relied upon if applied to persons of advanced age or to persons suffering from wasting diseases.

387. Supposing we made it a 4-millimetre margin for old persons?—Probably that would sufficiently cover these cases.

388. And we might have it specially stated in the rules that when a person in an emaciated condition was measured, a note to this effect should be made on the card, so that if necessary the double search might be made within a wider margin than the ordinary one?—That might be done, but with regard to emaciation I think that it should be recorded by weight, because one may examine a criminal and may never have seen him before, and one does not know what his physical condition was at that time. His present weight might be compared with his former weight.

389. That assumes that you know your man, but the difficulty is to find him. For that the only course is, I think, to allow a wider margin when there is apparent emaciation?—No doubt that is so; but when the case was found the difference in weight would explain the discrepancy in the measurements.

390. I will now go through the points in your report. You first mention discrepancies in measurements of skull by experts. What experts do you mean—experts in anthropology or in measuring?—I mean both—anthropologists accustomed to and trained in measuring skulls.

391. Might not errors arise from taking measurements in different ways or with different instruments?—They might, but the rules for the scientific measurement of skulls are very definite.

392. Would they not be avoided if measurements were taken according to fixed rules like M. Bertillon's?—I do not think M. Bertillon's rules could be more precise or definite than those used in taking these scientific measurements.

393. What instruments were used, callipers with flexible legs, such as you showed me in Oxford?—Yes, these and Professor Flower's craniometer.

394. M. Bertillon's callipers (*showing them*) are much more rigid than those you showed me?—Yes, in that respect they are better.

395. Bertillon found considerable errors occurred in the measurements when he took the head length from the glabella, but not to the same extent when he altered the place and took them from the root of the nose?—Yes. I think for your purpose it is better to take them from the root of the nose. For scientific purposes I should be glad to have them from the glabella. But for your purpose the other point is more easily fixed, and therefore better.

396. You refer next to the amount of pressure employed. We found when we tried measurements in M. Bertillon's office that we could easily distinguish 1 millimetre too much and 1 millimetre too little. If the callipers were a millimetre too wide, it did not touch the skin; if it were a millimetre too close, it pressed hard. Does your experience contradict this?—Well, I have tried some medical students in making the measurements; they were intelligent men and accustomed to use their hands.

397. And you found errors?—I found variations exceeding 1 millimetre, *e.g.*, the measurements of one head were 198, 194, 194, 193. (198 would be an obvious blunder.) Another 193, 192, 194, 192, 193. In this case you see there is a variation of 2-millimetres.

398. Had they no sort of standard to go by as to the amount of pressure?—They had the rules laid down by Bertillon.

399. Were these men trained in head measuring?—No; it was their first attempt.

400. M. Bertillon thinks three weeks' regular training is necessary?—No doubt that would make a difference.

401. You suggest that a spring should be used to give a uniform pressure?—Yes.

402. Could you rely on a spring always acting uniformly, in different instruments and at different times?—I think so. It is used in India.

403. In neither of the classes of cases you mention does the variation exceed the 2-millimetre margin. We now come to the points which you think might lead to that margin being exceeded. The first that you mention is that the tissues of the scalp are liable to undergo atrophy?—Yes; I show these models (*models exhibited*). These are taken from actual heads. They were made for a quite different purpose, but they show clearly the thickness of the scalp. One is a young person, the other a person about 70. You see the great difference in the thickness of the scalp.

404. What is the cause of thinner scalp in old age?—It comes mainly from the absortion of the fatty tissue. There may also be wasting of other tissue. I show a specimen of scalp kept in spirits (*showing specimen*). There is first the skin, then fatty tissue mixed with fibrous tissue, then tissue connecting with the bone. You see there is a good deal of thickness of fatty and fibrous tissue. In disease or in old age the fat is absorbed and the scalp becomes thinner.

405. Could it not always be noted if prisoner had an emaciated appearance, and additional allowance might be made for this?—Yes; that might be done.

406. If the margin for error were 2 millimetres, but all cases where prisoner looked emaciated were noted, and an additional margin, say 3 or 4 millimetres were allowed for the searches in these cases and in all cases of old persons, would not that remove to a great extent your objection?—Yes, that would be a practical way of meeting the difficulty; my point is that scientifically a 2-millimetre margin is not sufficient for *all* cases.

407. We come next to the *muscle*. In the majority of cases you say that the point from which the head width is taken lies over the temporal muscle?—Yes; I have found by experiment that in the majority of cases the maximum width falls within the area of attachment of the temporal muscle. That might be a further cause of error if there were growth or reduction of the muscle. But I qualify this by saying that usually on the point where the measurement is taken the muscle is very thin.

408. So that usually it would not much affect the measurement?—Not very much.

409. Would the muscle *develop* much in persons over 21 years of age?—No, not much; only very rarely and in very exceptional circumstances. What I should rather fear would be the effect of its reduction or atrophy.

410. In old age or from disease?—Yes, in those cases muscle would would waste with the other tissues, and would increase the variation already spoken of.

411. But in a person in ordinary health, within a period say of five years, there would not be much change?—Not much, not any appreciable change I should think.

412. Then as to the alteration caused by voluntary contraction of the muscle: that would be in shutting the mouth?—Yes, shutting the mouth and clenching the teeth.

413. The prisoner might slightly alter the measurement in that way?—Yes.

414. But if this were tried it might be met in the same way as other attempts at trickery in other measurements?—Yes, but it might be difficult to observe, unless the attention of the warder was particularly directed to it. Asking the man to whistle would be a means of preventing him from clenching his teeth.

415. (*Mr. Macnaghten.*) Bertillon finds greatest width of skull *above* and slightly *behind* the ear; does the temporal muscle reach *above* the ear attachment?—Yes, but, as I said, usually on a point where the muscle is very thin.

416. (*Chairman.*) Do different races differ in this respect?—Yes, that is a point of difference.

417. Do you think there would be any alteration in the bone in adults or only alterations in the muscles suggested above?—It is impossible to speak with certainty of the bone. Scientific measurements extending over all periods of life have not been made, but certainly if there is any alteration, it is so small that for your purpose, it might safely be neglected. I do not think that the change is likely to interfere with the accuracy of the measurements. I think that you have proof of that already; the Bertillon system has worked very well. I cannot however accept this as sufficient proof of its applying in all cases.

418. And any question of alteration in the bone only affects a long term of years, whereas our prisoners are mostly to be re-identified within a short period?—Yes.

419. Have you measured the thickness of any scalps?—The specimens I show you vary from 3 to 8 millimetres. These are taken from various parts of the head. The thickness of the scalp varies in different parts. I hand in two tracings which will show in these cases the thickness at the points of measurement. (*Tracings handed in of sections of heads which showed thickness of scalp (skin and tissues) at the back of the hand about* 8 *millimetres and at the root of the nose about* 4 *millimetres.*) I cannot say what the normal thickness would be. I have in my report suggested experiments which would show the extremes of possible variation.

420. Would not measurement of persons suffering from acute wasting diseases be misleading? Criminals are not usually in this state?—I only suggest them as showing the extremes of possible variation, not as fixing the rule for ordinary practice.

421. Generally would a margin of 2 millimetres cover the variation in ordinary cases where there is careful and uniform measurement, no *long* interval of years and no noticeable emaciation?—Yes, I think so. At any rate that would be a very different thing from saying that the variation can *never* exceed 2 millimetres.

422. You suggest the width of the face, the "bi-zygomatic width," as a good measurement. Do the tissues there really interfere less with the measurement than on the scalp?—Yes, the tissues are softer and the bone found more easily.

423. But that implies some pressure in making the measurement?—Yes, there would have to be some pressure not mere touching of the skin. In this case a spring should be used which would give an appreciable pressure uniform in all cases.

424. How is this measurement affected by the difficulty of finding the position for measurement?—It would not be difficult. You place the callipers in front of the ears and draw them forward till you reach the maximum.

425. On the whole are you in favour of anthropometry?—Yes, strongly. I think its principle is right,

and I think the particular measurements suggested are good ones. But I think the margin proposed to be allowed, if it is to be applied to all cases, is insufficient.

426. When M. Bertillon (Introduction, p. xxvi.), says that 2 millimetres is the limit of gross errors, beyond which one may infer *non-identity*, you object to that?—Yes, I object to that. I say that even with accurate measurements cases may occur of greater variation than 2 millimetres.

427. But these would be extreme cases, after long intervals of years, or in case of wasting disease such as consumption?—Yes.

428. Most identifications have to be made within a year or two of prisoner's release, many within a few months. An identification after ten years would rarely be required. (*Mr. Macnaghten.*) We should never want to identify after 30 years. (*Chairman.*) Nor should we often have to deal with a man suffering from wasting disease. So that in practice we should be able in most cases to disregard the extreme variations?—Yes.

429. Your argument points merely to the rules as to margins being settled carefully on a scientific basis?—Yes.

430. And the Anthropometric Registry should have scientific advice as to the exceptional cases that might occur?—Yes, that would be required.

431. But with proper safeguards you think Bertillonage practicable?—Yes, certainly. I think it most desirable that it should be adopted in some form.

APPENDICES.

APPENDIX A. (1.)

CIRCULAR OF 31ST OCTOBER 1893.

Home Office,
October 31, 1893.

DEAR SIR,

A SMALL Committee, of which I am secretary, has been appointed by the Home Secretary to consider the present system of identifying old offenders when arrested by the police, and to report to him what practical measures can be adopted for improving it. For this purpose the Committee would be much obliged for any observations you may be able to offer on the subject, and especially with regard to the following points:—

1. What are the methods actually employed by members of your force for identifying persons in their custody who are not personally known to them but who are suspected of being old offenders.
2. What use is made of the printed registers of names and of distinctive marks issued by the Habitual Criminals Registry, and whether they are frequently the means of establishing the identity of offenders.
3. Whether more identifications are effected through the agency of the "Police Gazette," the Illustrated Circular issued to subscribers from the Criminal Investigation Department at Scotland Yard, or the "route forms" sent to prisons and to other police forces.
4. Whether you consider the means at present available are sufficient to ensure habitual criminals being identified in the majority of instances should they be apprehended within your district; and

   Whether you can form any estimate of the number not identified who, in the absence of information respecting their antecedents, are dealt with as if for a first offence.
5. Whether in the case of criminals charged by your police before the magistrates who are not identified but are suspected of being old offenders it is the usual practice to ask for remands, so as to obtain time to make inquiry by "route-form" or otherwise, or whether such inquiry is more commonly made only after a prisoner has actually been committed for trial.
6. Whether you have any definite suggestions to make for putting the police in a better position for keeping habitual criminals and those who make a living by dishonesty under their supervision.

My Committee would be very glad if you could let them have your views not only with regard to your own force, but with regard to any other forces, of which you can speak from experience.

Yours faithfully,
H. B. SIMPSON.

[*This circular was issued to the Commissioner of Police of the city of London and the Chief Constables of the counties of Hereford, Somerset, Sussex (Eastern Division), Stafford and Worcester, the three Ridings of Yorkshire, and the boroughs of Birmingham, Bradford, Bristol, Doncaster, Leeds, Liverpool, Manchester, Newcastle-on-Tyne, Nottingham, Portsmouth, and Scarborough.*]

ABSTRACT OF ANSWERS RECEIVED.

1. Most forces attach the highest importance to route-forms as a means of identifying suspected persons, whose identity cannot be locally ascertained, if photographs of the suspected persons can be obtained for circulation.

The Chief Constable of Bristol and the Commissioner of the City of London Police, however, do not speak of issuing them, except in the latter case to the Metropolitan Police, and the Chief Constable of Hertfordshire does not consider them so useful as other agencies for identification.

The Chief Constable of Leeds says:—"The principal "and almost only useful means of identifying prisoners "are through the photographic route-forms sent to "prisons and to other police forces."

The Head Constable of Liverpool says:—"The route-"forms are found to be of the greatest value."

The Chief Constable of Somerset says:—"More "criminals are identified through the route-forms "than through either of the other means mentioned."

2. The opinions expressed as to the value of the volumes issued by the Habitual Criminals Registry are as follows:—

BIRMINGHAM	"Have on many occasions assisted "us materially."
BRADFORD	"Is a matter of reference only "when all other means have "failed."
CITY OF LONDON	"Used for reference, but not considered of much value in its "present form."
DONCASTER	"Have succeeded in a good many "instances."
HERTS	"Is referred to," but does not frequently lead to identifications.
LEEDS	"Of little or no use," "sometimes "(though rarely) referred to."
LIVERPOOL	"Frequent use is made," "identifications frequently" established thereby.
MANCHESTER	"A few cases have been traced by "means of it."
NEWCASTLE-ON-TYNE	"Frequently examined . . . . "occasionally with success."
NOTTINGHAM	"Useful for reference, although "not many persons are identified" thereby.
PORTSMOUTH	"Their use has not been very productive."
SCARBOROUGH	"Occasionally the means of identifying offenders."
SOMERSET	"Cannot say they are *frequently* "the means of identifying" offenders, though this is the case "from time to time."
STAFFORDSHIRE	Are valuable "in practised hands;" "in constant use" at headquarters, but not much used in divisions.
EAST SUSSEX	"Very little use;" "seldom the "means of establishing identity."
WORCESTERSHIRE	"Do not think they are often the "means."
YORKS, E. RIDING	"Sometimes the means."
N. RIDING	"Have not as a rule had occasion "to use" them.
W. RIDING	"Not found to be of much service."

3. Value of the "Police Gazette" and the Illustrated Circular issued by the Convict Supervision Office as means of identification:—

BIRMINGHAM	The Gazette "affords some assistance"; the Circular has "on many occasions assisted us "materially."
BRADFORD	They "are of little or no use . . . "in this part of the country."
BRISTOL	"All these means have proved of "great service."
DONCASTER	"Have both answered the purpose "[of establishing identity] on "more than one occasion."
HERTS	"Most identifications are obtained "through the Police Gazette, "but the Habitual Criminals "Register and the Illustrated Circular are found "to be most useful and valuable."

LIVERPOOL - - "Very few identifications" effected thereby.

CITY OF LONDON - "Most identifications are effected "through the Illustrated Circular."

NEWCASTLE-ON-TYNE "Hardly any are identified "through Police Gazette. "The Illustrated Circulars "should be useful . . . . . "but they chiefly refer to south "country thieves who but "seldom visit us except during "race meetings."

PORTSMOUTH - "Comparatively few" identifications through the Gazette. The Circular would be more valuable "were its scope "extended and its issue more "frequent."

STAFFORDSHIRE - Both are "in their way of considerable value, the Police "Gazette especially so. The "Police Gazette would be of "much greater value if published more frequently."

WORCESTERSHIRE - "Both very useful." In the Circular the photographs of criminals of one class (*e.g.*, burglars, horse-stealers, &c.) should be grouped together.

4. The answers given to the question whether the existing means of identification are sufficient, are mainly in the affirmative, but in quite general terms. On the other hand the Chief Constable of Doncaster says, "The "means at present available are not in my opinion "sufficient to ensure the majority [of old offenders] "being identified. I cannot form any reliable estimate "as to the number who escape identification here, but "feel confident a large number of those arrested and "dealt with summarily during the race meetings are old "offenders from all parts of the country who give false "names and refuse their addresses." The Chief Constable of Manchester says, "I do not consider the means "at present available sufficient to ensure habitual criminals being identified in the majority of instances." Of the West Riding Constabulary the Superintendent of the Doncaster Division says, "They [habitual "criminals] are not identified in the majority of in"stances." The Superintendent of the Selby Division says, "The means at present available are not sufficient;" and the Superintendent of the Tadcaster Division says, "The means at present available are not "sufficient to ensure habitual criminals being identified "in the majority of cases, in the absence of photo"graphs."

The Chief Constable of Newcastle-upon-Tyne observes, "The means at present available should be in "most instances sufficient, but I have reason to fear "that too often this highly important duty is per"formed in a perfunctory manner. I find it difficult "to teach the provincial police to appreciate these "duties, the successful performance of which so much "depends on strict attention to matters of minute "detail."

The Chief Constable of Portsmouth says, "These "agencies are not in my opinion either sufficient or "complete, and I am inclined to believe that probably "50 *per cent.* of habitual criminals arrested in the "provinces escape recognition"; and the Chief Constable of Worcestershire has no doubt that pickpockets and other criminals of a similar class escape identification in considerable numbers.

The Chief Constable of Herts again in answer to the question replies, "Yes, in the majority of instances, "but some system is required whereby every habitual "criminal should be certainly identified. A 'majority' "is not sufficient. The 'minority' may include the "worst criminals and those whose apprehension is "most wanted and is most desirable."

5. The replies to the fifth question in the circular indicate that difficulty is rarely found in obtaining remands for the purpose of making inquiries into a prisoner's antecedents. The Liverpool Head Constable states that, "in every case where a prisoner brought "before the magistrates is suspected of being an old "offender, a remand is asked for for the purpose of "making inquiries."

In the borough of Nottingham "as a general rule "all strangers charged with larceny are remanded and "routed before being committed."

In Staffordshire remands are always asked for, but the Chief Constable remarks that there are some benches of magistrates "who object to granting remands and "failures to identify may arise from this cause."

At Doncaster it is usual to remand for inquiries prisoners who are suspected of being old offenders; but during the race meetings this is not always done and the Chief Constable has no doubt that a good many of the prisoners dealt with in a summary manner are old offenders.

6. In answer to the question put in the sixth paragraph of the circular most of the forces urge that a power to require unconvicted prisoners to be photographed would facilitate their identification.

BIRMINGHAM - - Failure to comply with the provisions of section 5 of the Prevention of Crimes Act should involve an increase of police supervision. The Treasury should pay the expenses of the conveyance of prisoners and of the attendance of witnesses to prove identity. It is probable too that the smallness of the allowance made to witnesses at sessions and assizes deters police officers from giving information about criminals whose photographs they may recognise, in case they may be summoned to give evidence of identity.

BRISTOL - - - The Chief Constable writes, "I may suggest that from ex"perience gained when at"tached to the Detective "Department, Scotland Yard, "the police derived most use"ful and valuable information "by their weekly visits to the "prisons."

DONCASTER - - District registries should be established at different towns and returns made to them of all convictions of crime in each district; illustrated circulars to be issued monthly or quarterly therefrom with particulars of all registered criminals released from prison.

HERTS - - - "The common sense plan to "ensure the identification in "all instances of an habitual "criminal is that no prisoner "should be released from a "convict prison on license or "on the expiration of his term "of imprisonment, without a "letter, number, and the year "of his discharge tattooed on "some special spot on his arm, "say, under the upper part "of his left arm . . . . . . "Even if a convict rendered "this mark illegible by tat"tooing over it, the fact of his "being tattooed in such an un"usual place would show that "he is an habitual criminal."

MANCHESTER - - The allowance to police officers for attending to prove previous convictions should be increased from 3*s.* 6*d.* to 10*s.* 6*d.*

NEWCASTLE-ON-TYNE All important forces should issue quarterly circulars giving portraits of criminals supposed to be travelling the country. Chief officers of police should meet now and then in order to promote co-operation in the performance of their duties.

PORTSMOUTH - - Greater uniformity of practice is required; for this purpose instructions for recording personal descriptions, &c. should be issued; lists of discharged convicts and the Illustrated Circular should be supplied to all forces; the police should also be furnished with information with regard to all persons on the Habitual Criminals Register, when they fail to report, leave the country, die, or are re-convicted; police supervision of license-holders and others should be better enforced; more photographs of travelling criminals should be issued to local forces; the Treasury should pay the cost of detecting, identifying, and prosecuting criminals.

SUSSEX (Eastern Division). The photographs of old offenders to be supplied to every Chief Constable.

WORCESTERSHIRE - A reward of 5*s*. should be given to prison clerks or warders for information leading to an identification; evidence on affidavit to be made legal proof of a previous conviction in the case of a prisoner not disputing it, and arrangements made to prevent the waste of time involved in police officers' attendance as witnesses to previous convictions. Some restriction should be placed on the movements from one district to another of persons required to report to the police.

APPENDIX A. (2.)

CIRCULAR OF 13TH NOVEMBER 1893.

Identification of Criminals.

Home Office,
13th November 1893.

DEAR SIR,

IN connexion with the information already requested on the above subject, my Committee would be very glad if you could further give them figures, for any specified period, of the number of route-forms issued by your Force, and the number of instances in which a prisoner's identity was thereby traced; and if at the same time you could estimate the proportion of route-forms issued to the total number of prisoners arrested, or give any other definite figures to show the proportion that the number of foreign criminals bears to the number of criminals belonging to your district.

Yours faithfully,
H. B. SIMPSON.

ABSTRACT of ANSWERS RECEIVED.

BRADFORD:

——	No. of Prisoners charged with Felony, &c.	Photographed.	Previously Photographed.	Not Photographed, because well known to the Police.
1893.				
August - - -	33	4	10	19
September - - -	35	12	4	19
October - - -	25	4	2	19
	93	20	16	57

DONCASTER:

In 1892 407 persons were arrested for all classes of offences. Of these 272 were strangers to the borough. Of these 46 were "routed," 28 before conviction, 18 after conviction. Of these 30 were recognised as having been previously convicted. Of the 46 prisoners "routed" 26 were arrested during the race meeting.

LEEDS:

From 1st May to 31st October 1893 there were 194 persons arrested for serious offences. Of these 190 were photographed, 114 were known to the police; 80 (including the four not photographed) were total strangers. 223 route forms were issued, and the majority of the strangers were thereby ascertained to have been previously convicted.

LIVERPOOL:

Of the persons convicted of offences involving dishonesty during the 12 months ended 29th September 1893, there were 2,681 who were resident in the neighbourhood, or whose antecedents were fairly well known; 83 who were strangers and suspected of previous dishonesty; 74 were routed, and of these 64 were identified.

MANCHESTER:

During the 12 months ended 31st October 1893, there were 41 route-forms issued with photographs after conviction, resulting in the identification of 22 habitual criminals.

A large number of routes were issued without photographs attached, but with little result.

NEWCASTLE-UPON-TYNE:

During the 12 months ended 31st October 1893, there were 23 prisoners routed, of whom 12 were traced. Of 500 adults proceeded against for serious offences, 395 resided in the city, 80 belonged to the adjoining counties of Northumberland and Durham, and 25 to other parts of the country.

NOTTINGHAM:

During 1892 217 thieves were photographed. Of these 58 were "routed," and of these 43 were identified by the police of other towns.

PORTSMOUTH:

In 1890 the number of route-forms issued in respect of persons arrested for indictable offences was 8, the number of prisoners thereby identified was 5. In 1891 the number of route-forms was 4; no identifications effected. In 1892 3 route-forms were issued and one identification effected.

The total number of persons arrested for indictable offences during the three years was 338; of these 72 were not resident in the borough.

During the same three years 50 "circulations" were issued from the borough in respect of persons wanted, which resulted in the apprehension of 21 of them.

SCARBOROUGH:

During the five years 1888–92 the number of prisoners arrested for indictable offences was 474; of these 113 were "foreigners"; 70 route-forms were issued, and 34 prisoners were thereby identified.

SOMERSET:

During the year ended 30th September 1893 there were 515 persons arrested for offences involving dishonesty; of these 303 were natives of Somerset, or permanently residing in the county 12 route-forms were issued, and six prisoners. thereby identified.

STAFFORDSHIRE:

During three months ended 30th September 1893, there were 416 persons arrested for indictable offences; of these 62 were unknown at the time of arrest; of these 53 were identified either by route-forms, or by personal inquiry. Of the remaining nine one was committed for trial, and eight were dealt with summarily.

The Chief Constable remarks that in the more thickly populated part of the county personal inquiries generally succeed in procuring information about a prisoner. In the rural parts route-forms have to be more commonly resorted to. A criminal from the north of the county would be more really "foreign" in the south than a Birmingham man would be.

NORTH RIDING OF YORKSHIRE:

During three years ended 30th September 1893, there were 65 route-forms issued, and 43 prisoners thereby identified.

EAST RIDING OF YORKSHIRE:

During 12 months ended 31st October 1893, there were 10 route-forms issued, and two prisoners thereby identified. In each case two route-forms on an average were issued.

APPENDIX B.

Vide p. 18.

NUMBER of PERSONS CONVICTED during the months of January, February and March 1893, in certain Jurisdictions, showing the Numbers and Proportions per cent. of Prisoners against whom previous Convictions were entered in the Calendars, and the Numbers and Per-centage of those against whom Previous Convictions were proved in Court:—

Jurisdiction.	Number of Persons Convicted.	Number of Persons Convicted.		Per-centage of Persons Convicted.	
		With previous Convictions recorded in Calendar.	Against whom previous Convictions were proved in Court.	With previous Convictions recorded in Calendar.	Against whom previous Convictions were proved in Court.
London :—					
Central Criminal Court (London cases)	224	72	63		
London Sessions (N. and S. of the Thames).	429	236	162		
	653	308	225	47·16	34·40
Lancashire Assizes - - -	114	72	30		
" Quarter Sessions - -	108	86	63		
Yorks (West Riding) Assizes - -	41	28	15		
" " Quarter Sessions	30	22	18		
Staffordshire Assizes - - -	15	9	1		
" Quarter Sessions - -	35	26	19		
	343	243	146	70·84	42·56
Liverpool City Sessions - -	92	79	36		
Birmingham City Sessions - -	50	37	26		
Warwick (Birmingham Division) Assizes (Birmingham City).	22	12	7		
Bradford Borough Sessions - -	13	13	10		
	177	141	79	79·66	44·63
Norfolk Assizes - - -	5	4	2		
" Quarter Sessions - -	12	7	4		
Suffolk Assizes - - -	10	5	2		
" Quarter Sessions - -	9	6	6		
	36	22	14	61·11	38·88
Dorset Assizes - - - -	19	5	4		
" Quarter Sessions - -	4	3	2		
Devon Assizes - - - -	15	3	1		
" Quarter Sessions - -	7	1	1		
Cornwall Assizes - - -	5	—	—		
" Quarter Sessions - -	2	1	1		
	52	13	9	25·00	17·30

APPENDIX C.

SPECIMEN ROUTE FORM.

(NOTE.—*The MS. portions of this form are here represented by italics.*)

LEEDS CITY POLICE.

Route of Prisoner in custody on remand to *the 20th* day of *September* 1893, charged with *loitering in the streets with intent to commit a felony.*

DESCRIPTION.		MARKS.
Name, *Alexander Wilson ?* Alias Age, *31 years.* Height, *5 ft. 4 ins.* Hair, *dark-brown, mingled with grey.* Eyes, *brown.* Complexion, *fresh.* Build, *prop.* Country, *Irish.* Where born, *Belfast.* Married, or single *widower.* Trade, *moulder.*	[*Space for photograph.*]	*Scar near right elbow, scar centre of forehead, lost all single teeth in upper jaw.*

SIR,

I SHALL feel obliged if you will fill in what is known of the prisoner (if anything), and forward the document according to Route, as quickly as possible.

I am, Sir, &c.,
FRED T. WEBB,
Chief Constable.

Leeds, 19*th September* 1893.

Route.	Date Received.	Result of Inquiry.	Date Forwarded.	Signature.
City Police, London - -	*20/9/93*	*Not known* - - -	*22/9/93*	*Jas. McWilliam, Supt. Pro. R.C.*
Metropolitan Police, London -	*22/9/93*	*Not recognised* - - -	*23/9/93*	*John Shore, Supt.*
Habitual Criminal Registry -	*23/9/93*	{ *Forms R. of Alfred Bull and Henry Bull herewith for comparison.* }	*23/9/93*	*J. G. Grace, for Registrar.*

Return to Leeds Police by morning of the 25th inst.

APPENDIX D.

Bertillonage in Foreign Countries.

M. Bertillon first brought his proposals to the notice of the Prefect of Police at Paris in 1879. Towards the end of 1882 the *Service d'Identification Anthropométrique* was established in the capital. In 1883 there were 49 *récidivistes* identified by this special means; in 1884, 241, and the number has since gone on increasing. In 1892 it was 680. In 1885 and 1887 instructions were given by the Minister of the Interior for taking measurements in prisons outside Paris, and since 1888 the measurements there taken have been sent up for record at M. Bertillon's bureau. In any case where it is thought that the prisoner is giving a false name and is an old offender, a note may be made on his card and search will then be instituted at Paris for the previous registration. The original cards are retained at the place where the measurements are taken, and arranged in an alphabetical register. At Lyons and Marseilles, however, there are also anthropometrical registers on the same plan as that in M. Bertillon's office, and it is proposed that similar registers should also be established at Lille, Nancy, Nice, Toulouse and Bordeaux.

In June 1887 a circular from the *procureur général* at Paris impressed on provincial magistrates the desirability of making use of anthropometry for identifying persons charged before them instead of the costly process of photographs and *commissions rogatoires*. It does not appear, however, that so large a number of inquiries as might naturally be expected are in point of fact addressed at present to M. Bertillon's bureau from courts outside Paris. In this respect, no doubt, the system is still far from having attained its full development even in the country of its birth.

Elsewhere in Europe where the Bertillon system has been adopted, it seems to have been adopted mainly with the view of identifying criminals from other countries. In Switzerland it was introduced during the year 1890 in the Canton of Geneva, where there is a large floating population of French and Italians. About five prisoners are measured by the police daily, resistance to the process being made, by a decree of 10th June 1891, tantamount to *rébellion*. The system is thought to have achieved a very signal success; the Canton of Vaud is stated to be going shortly to adopt it, and its ultimate extension to the whole of Switzerland is highly probable.

In Belgium the method is habitually employed in the case of persons suspected of having come from Paris or other parts of France, and important identifications are effected in this way through the co-operation of M. Bertillon's *service anthropométrique*. The use of the system for the ordinary purposes of criminal justice has also been urged on the Government, but in the case of native criminals identification appears to be neither so difficult nor so important a matter as it is in England. On the one hand they are kept far more systematically under the notice of the authorities than would be possible here. Notice of every conviction is sent by the convicting court—even by a *tribunal correctionnel*—to the burgomaster of the commune where the convicted prisoner is domiciled, and this notice follows him at every change of residence. On any fresh charge the information so accumulated is given to the *magistrat instructeur* for use in the *instruction*. Since 1888, moreover, similar notices are also sent by the courts to the Ministry of Justice, and there filed for future reference. When the criminal is a foreigner, he comes under the notice, not of the Ministry of Justice, but of the *administration de la societé publique*. Thus a very complete record is kept in duplicate of the convictions of native criminals.

On the other hand the importance of proving previous convictions is much limited by the provisions of the Code, by which a severer penalty is involved only in those cases where a *crime* follows a previous conviction for a *crime*, or a *délit* follows a previous conviction for a *délit* punished by not less than a year's imprisonment. A Bill introduced in the Chamber of Representatives by the Government in 1890 in consequence, as it was stated, of the increase of *la récidive*, proposed to allow the penalties prescribed by the Code Pénal to be increased in every case where an offender is shown to have been previously convicted of an offence of a similar nature. In many cases the *projet de loi* fixed a *minimum* as well as a *maximum* sentence for the *récidiviste*. These proposals, however, have not passed into law.

In Italy also the Government is considering the question of introducing the Bertillon system, and here again the motive appears largely to be the advantage likely to arise from international co-operation.

In Austria the police at present use the height and the span of the arms as means of identification. Some years ago the question of introducing the Bertillon system was mooted; but the opinion arrived at was that it did not offer sufficient advantages to counterbalance the difficulty and expense of putting it in practice. Only a few experiments, however, were made, and the police do not claim that their opinion on the matter is final.

Experiments in Bertillonage have also been made at the prison at Ploetzensee, Berlin, but the results obtained were not satisfactory, and the system has been abandoned.

In India the system is likely to have a wide extension.

It was introduced in Bengal by Mr. Henry, Inspector-General of Police, in 1892. The measurements used for classification are the length and width of the head, the length of the left middle-finger, the forearm and the left foot, and the height. A print of the left thumb is also taken, but no use is at present made of it for classification.

The instrument used for taking head-measurements has been modified by the addition of a spiral spring, connecting the two arms of the calipers and intended to equalise the pressure, a handle, a self-registering index, and a small appendage to be used when the head-length is taken, for preventing the left arm of the calipers slipping into the eye.

The kind of criminals to be measured has not been definitely prescribed, but instructions have been given to the police for the purpose of ensuring the registration of all professional and habitual criminals, as well as of those whose identity is not known, and who may therefore be specially dangerous. During the first two years that Bertillon measurements were taken over 6,000 cards were accumulated at the Central Bureau.

It is worth noticing that experience in Bengal has already shown that racial differences may necessitate a re-adjustment of the figures fixed by M. Bertillon as the basis of his tripartite division. While the classes based on the length of the head are found, when the measurements of French criminals only are concerned, to contain an approximately equal number of cards, if the limits of the medium class are fixed at 185 and 190 millimetres, in Bengal heads that are more than 185½ millimetres are classed as long, and those only that are less than 181 go into the short class: that is to say, that very nearly all the heads that in Paris are of medium length have to be treated as long in Bengal. Similar differences are found in the case of other measurements. The inferences involved are of considerable interest to the anthropologist; but for the purposes of criminal identification the fact is of importance merely as showing the necessity of caution in adapting Bertillonage to the requirements of another country.

The Bengal police strongly urged that the system they had adopted should be extended to the North-West Provinces and the Punjaub, in order to enable them better to deal with the marauders from those parts who are accustomed to make repeated raids into the Presidency.

In view of the success achieved by the system in Bengal, the Government of India are pressing the system on the other local administrations: in the Central Provinces, Assam, and Madras the proposal has been already accepted, and by the last despatches it seems very probable that before long the whole of British India will be following the same method.

It was also introduced into Ceylon during 1892, and Mr. Ellis, the head of the Prisons' Administration, in his report for that year states that so far it had proved eminently satisfactory.

In North America an association was formed early in 1887 among the wardens and superintendents of prisons for securing the registration of criminals, with a view to the improvement of prison administration. The secretary, Major McClaughry, commenced practical experiments in Bertillonage at his own prison at Joliet, Illinois, and the Wardens' Association, at his instance, adopted the system at a meeting held at Toronto in September 1887. A school of instruction was instituted at Joliet Penitentiary. It has been attended by representatives from 10 or 12 important prisons, and some success in the system is said to have been already achieved at the Ohio Penitentiary at Columbus, the House of Correction at Detroit, the Western Penitentiary of Pennsylvania, the New Jersey State Prison at Wrenton, and elsewhere.

In the Central Prison at Toronto Bertillonage was established in 1889 for all criminals, as distinguished from tramps and drunkards; but from the Wardens' report for that year, it seems that there is no central office for keeping the records and photographs; and, from the absence of any mention of the system in subsequent reports, it may be inferred that it has not yet been carried beyond a purely experimental stage.

Lastly, careful attention has been given in the United States to the question of making use of Bertillonage as a preventive of fraudulent enlistment in the United States Army, but the system was not considered suitable for this purpose. One of the main obstacles to its adoption was that hitherto it has been applied exclusively to the criminal classes, and it was deemed objectionable to subject recruits to a process which might be supposed to carry with it dishonourable and degrading associations. The method instituted in its stead for classifying the personal descriptions of newly enlisted soldiers is based partly on the height and the colour of the eyes—two colours only, namely, brown and blue or grey, being now recognised,—and partly on a careful record of bodily marks. Some account of it is given in a pamphlet by Lieut.-Colonel Greenleaf and Major Smart, of the United States Army, read at London in August 1891 before the Seventh International Congress of Hygiene and Demography, and published in the Transactions, vol. viii. p. 294.

H. B. S.

[NOTE.—The above account of the growth of Bertillonage is founded, as regards France, on M. Bertillon's published writings; as regards other European countries, on information obtained through the Foreign Office; as regards India, on official despatches communicated by the India Office; and as regards America, on the introduction to the English translation of M. Bertillon's Instructions, issued from Joliet Penitentiary for use in America.]

APPENDIX E.

1. Instructions for taking the Measurements of Prisoners.

The following instructions have been adapted, with the kind assistance of Mr. E. R. Spearman, from those given by M. Bertillon in his "Instructions Signalétiques," Ed. 1893. Their object is to secure an exact uniformity in the mode of taking the measurements. If used for the guidance of learners, they must be accompanied by diagrams, and in most cases personal instruction will be required.

I.—*Length of Head.*

This measurement is taken from the root of the nose to the point at the back of the head, which is furthest distant from it.

This and the following measurement are taken with a callipers or compass specially constructed for the purpose with legs that are arched below and move above on a graduated scale.

1. Let the prisoner sit on a stool, with his face turned towards the light and slightly inclined downwards. Stand at his left side and with your left hand hold the point of the left leg of the compass in the hollow at the root of his nose. The end should be held between the finger and thumb, and to keep it from slipping, they should project a little beyond it, and rest on the nose. Meanwhile hold the other leg of the compass with your right hand between your finger and thumb, about half an inch from the end, and let the other fingers of both hands support the compass in such a way that the scale between the two legs may catch the light and be easily read.

2. You have now by means of the scale to find the point at the back of the prisoner's head which is furthest from the root of his nose. To do this, hold the point of the left leg of the compass steady, while you move the other point slowly from the top of the head downwards, keeping it as nearly as you can in the middle. Keep your eyes fixed on the scale, and note the highest figure shown by the index. This figure represents the maximum length of the head. To make sure you have got it correctly, pass the end of the compass once or twice up and down near the spot. Then take the compass away, and fix it by means of the screw at the figure you have noted. To do this easily, you should let the fingers of the left hand support the compass underneath, with the thumb across the left leg above. Your right hand is thus left free to bring the right leg of the compass to the proper point on the scale, and to turn the screw.

3. Now test the accuracy of your measurement with the compass *fixed*; holding the left end as before, and moving the other end to and fro and up and down at the back of the prisoner's head. If there is any part of the head over which the end of the compass will not easily pass, or if, on the other hand, the end passes over the head without touching it anywhere or scarcely touching it, the compass must be re-adjusted. When it is set rightly, you will find that it will pass without pressure over any part of the back of the head, and that at one point—that from which the measurement is taken—it will perceptibly touch the skin of the head.

4. To ensure accuracy it is essential that the measurement first taken should be verified, as indicated above, with the compass fixed, and in some cases it may be necessary to repeat the operation more than once. The true measurement is found when the compass touches at one point only, and passes over that point without pressure. If it touches nowhere, it is too long; if there is a point where it needs pressure to make it pass, it is too short.

5. Care must be taken against error being caused by the prisoner frowning or twitching his eyebrows.

II.—*Breadth of Head.*

This measurement is taken between the two points on the two sides of the head which are furthest apart on the same level and exactly opposite one another.

1. Let the prisoner sit on a stool in the same position as before, and stand exactly behind him, your heels together and your body straight so that your elbows may move freely and evenly.

2. Take one leg of the compass in each hand near the end and put the ends just above the place where the ear joins the head. Then move them slowly upwards, taking great care that they move quite evenly, that is to say, on the same level and exactly opposite one another. If now you fix your eyes on the scale you will find that in most instances the figure shown by the index increases for a time and then, as you get nearer the top of the head, begins to diminish. When by moving the compass up and down again, you reach the level at which the figure is highest, you must move the ends of the compass slowly to and from you, making sure that they still keep exactly even. In this way you will find the greatest breadth of the head. This is usually, though by no means invariably, a little above and behind the ears.

3. Then, in the same way as before, fix the compass with the screw at the point you have noted and test the accuracy of your measurement. To do this properly you must move the compass evenly up and down, beginning behind the ear and at each up and down movement pushing the compass rather further from you, so that each of its points describes a course like this. If they describe a course like this you are very likely to miss the place where the head is broadest.

Note that the movement of the compass in checking the width is not the same as in ascertaining it. To *ascertain* the width the compass is first moved up, and then at the widest point is moved backwards and forwards. To *check* the width the compass is moved forward, describing as it moves a series of close zig-zag lines, about an inch long.

4. It is of great importance that all the time the measurement is being taken the prisoner should sit evenly and that you should stand quite straight and square. Otherwise you will be apt not to move the points of the compass exactly together and your measurement will consequently be wrong.

If the compass is tilted up at one side, so that one leg is on a higher level than the other, or if it is twisted round, so that one leg is further forward than the other, the measurement will be wrong. These are the two errors specially to be guarded against. The points of the compass must be kept on the same level and exactly opposite one another.

In verifying your measurement, as in taking the length of the head, both points of the compass must perceptibly touch the skin at the spot where the head is broadest, but must be able to pass over that spot easily and without pressure.

Note.—In the case of both the above measurements you should specially note a head that is very irregularly shaped or not symmetrical.

If a wound or other injury prevents the measurement being taken or affects its accuracy, a note must be made of this.

III.—*Left Middle Finger.*

This and the two following measurements are taken with a graduated sliding rule. This rule has two fixed arms projecting on opposite sides about 4 inches and about 1 inch, and a slide consisting of two arms which also project about 4 inches and 1 inch respectively.

The middle finger is measured with the smaller arms.

The finger while being measured must be held at *right angles* to the back of the hand, and the measurement is taken from the finger-tip to the knuckle.

1. Stand facing the prisoner, holding the sliding rule with your right hand and resting the longer arms of the rule obliquely against your chest. Take the prisoner's left middle finger with your left hand and place it along the rule with the tip against the small fixed arm. The other fingers should not be bent like the middle finger, but should pass freely on each side of the rule.

2. With your left thumb on the third joint (*i.e.*, the joint next the finger tip) press the prisoner's finger against the rule so as to keep it straight, and with three fingers press gently on the back of his wrist so as to keep the middle finger at right angles with the back of his hand, and its point against the small arm of the rule. Your little finger meantime presses against the under side of the prisoner's wrist.

3. Now turn half-way round so as to have your left side towards the prisoner, raising your elbow as you do so. Do not allow the prisoner to move, but carry his left hand round with you, bringing the arm forward and bending it at the wrist. This is the best position in which to get the finger to be measured perfectly straight.

4. Standing in this position, see that the tip of the finger touches the fixed arm of the rule, and that the finger is at right angles to the back of the hand and lies close to the rule all its length and particularly at the knuckle. If necessary raise or lower the rule or turn it round a little. It is absolutely essential to have the finger in the right position on the rule. When this is secured, push up the slide with your right hand till it touches the knuckle lightly and read off the measurement.

NOTES.

The *nail*, if projecting beyond the finger tip, should be cut before measuring. If this cannot be done, an estimated correction of one or two millimetres may have to be made in the measurement.

Stiff Joints.—If there is a stiff joint which entirely prevents the finger being placed in the proper position for measurement, mark "stiff joint" on the card (specifying which joint), and take the measurement of the *right* middle finger. If there is merely a slight stiffness in the joints which prevents the fingers lying quite straight on the rule (this is common in blacksmiths, navvies, &c., and usually affects both hands), measure the length as well as you can, and in noting the measurement mark the case as one of "stiff joint," and add a figure (+2, +3, or +4), which represents, as nearly as you can estimate it, the addition necessary to show the true length of the finger. This will indicate in the Registry the limits within which a double search may have to be made.

Amputation.—If the finger is amputated, note this on the card, giving the measurement of the part left if any, and measure the corresponding finger on the right hand.

IV.—*Left Forearm.*

The left forearm is measured from the extremity of the elbow bent at a sharp angle to the tip of the middle finger. In taking the measurement, the arm is laid on a specially constructed table about 3½ feet high, and the length is then taken with the longer arms of the sliding rule.

1. Direct the prisoner to stand with his left side to the table and to lay his left arm flat—the palm of the hand downwards—on the place marked for that purpose. Standing on the opposite side of the table, adjust his arm so as to make it lie perfectly straight and parallel with the edge of the table, the elbow and the forefinger close to the edge, and the thumb projecting over the edge. Be careful to see that the middle finger, the middle of the wrist and the extremity of the elbow lie in a line.

2. Put your left hand on prisoner's wrist to prevent its moving, and then tell him to advance his shoulder by leaning forward till the upper arm comes so far forward as to make a sharp angle, *about half a right angle*, at the elbow. If necessary, direct his movements with your right hand.

This movement usually disturbs the position of the arm. If so, adjust it again, and see particularly that the part next the elbow lies on the table.

3. Now bring forward the sliding rule (which should have been previously placed on the table) keeping it parallel to the edge, and place it with the fixed arm touching the extremity of the elbow. Then with the left hand push the slide till it touches the tip of the finger.

4. The rule now gives the length of the forearm if there is no trickery on the prisoner's part—but before reading the measurement, in order to make sure that the prisoner is not altering the measurement by slightly arching his hand or fingers, press the back of his hand and middle finger and his wrist firmly on the table with your right hand; but while so doing use your left hand to keep the rule in position and with its fixed arm touching the elbow. If there has been any attempt at trickery, the pressure of your hand will flatten out the prisoner's hand and finger and move the slide slightly. Now read off the measurement.

5. If you still suspect that the prisoner by contracting his hand has slightly altered the measurement, raise his hand from the table, bend his wrist firmly, then bring the hand down flat sharply and take the measurement instantaneously. The alteration which a prisoner can make is very slight, and the attempt is easily detected; but if any such attempt is suspected, this should be indicated on the card by writing after the measurement the letters TR ("trickery").

Stiff joints, the loss of a finger, &c., are dealt with in the same manner as in the case of the middle finger. If any signs of a fracture in the forearm is noticed, the card should be marked "*fracture*" and the measurement of the right arm added.

V.—*Left Foot.*

The measurement of the foot is taken from the point of the longest toe to the heel. It is taken with the longer arms of the sliding rule.

1. Place the stool in front of the table at a distance of about 2 feet. Make the prisoner stand on the stool on his left foot, leaning forward and resting his right hand on the handle of the table provided for this purpose.

2. See that the foot *rests flat on the stool*, and particularly that the weight is not thrown on the inside edge of the great toe and that the toe is not bent.

3. In taking this measurement attention must be given to the possibility of the prisoner's reducing the measurement by bending the great toe and slightly arching the foot. This trick is easy to detect and the position of standing on one foot with the toe bent is difficult to maintain for more than a minute; the attempt can generally be checked by making the prisoner bend his knee slightly so as to throw forward his weight and spread out the toes.

4. When the body, foot and great toe are in position, lay the sliding rule on the stool along the inner side of the foot, touching the inside of the heel and the joint of the great toe, and with the fixed arm pressing gently on the back of the heel. In cases where the middle part of the foot projects so as to prevent the rule touching both the inside of the heel and the joint of the great toe, place it as nearly as possible *parallel* to the usual position.

5. Move the slide till it touches the great toe; then, if you still suspect any bending of the great toe, press down the joints with your thumb (taking care, however, not to press on the nail), and allow the slide to move back. See that the position of the rule and fixed arm has not been disturbed, and read off the measurement.

NOTES.

If the great toe is *turned inwards* towards the other toes, this should be noted after the measurement and a figure added to show the estimated diminution of the length of foot from this cause.

If the great toe is *drawn* in by a permanent contraction of the tendons, usually caused by wearing too short boots, this should be noted in the same way.

If the second toe is longer than the great toe, the measurement should be taken to the tip of the second toe and should be marked on the card with the sign > followed by a figure representing the estimated length of the second toe beyond the great toe.

If the left foot is amputated, note this and measure the right foot.

If the toes or part of the foot is amputated, measure what remains and, after this measurement, add also the measurement of the right foot. If the wound is still fresh, no measurement of the left foot should be taken.

2. INSTRUCTIONS FOR TAKING DISTINCTIVE MARKS.

The following instructions merely give the main points to be followed in taking the description and measured position of the bodily marks that can be used for identification. For use by warders, they should be amplified and illustrated by diagrams, and there should be a complete table of the abbreviations to be used.

1. Make the prisoner stand facing you, his arms straight down by his side, *the palms of the hand turned forward*. All marks on the limbs should be described with reference to this position, although you will have to lift and turn round the arms and hands, in order to find and measure the marks.

When therefore you speak of the *front* of the arm, you include the palm of the hand and all the side of the arm turned forwards in the position just described; similarly the *back* of the arm includes the back of the hand and the point of the elbow. *The inner side* is the side of the arm, which in the position given is turned *towards* the body;* *the outer side* is the side turned away from the body.

So the "inner side" of the hand and of each finger is that towards the little finger; the "outer side," that towards the thumb.

2. In describing the marks, state first the *nature* of each mark, then its *shape*, its *size*, its *direction*, and finally its *position*.

3. For measuring sizes and distances a small millimetre rule is used. The measurements are given in millimetres, but they do not require to be taken with the same accuracy as the measurement of the head, &c. Above 50, round numbers (60, 70, &c.) suffice.

Nature of Mark.

4. Say whether the mark is a scar ("SC"), tattoo ("TAT"), birth-mark, &c.

5. In dealing with *scars* it is important only to give those that are permanent. If there is any doubt, note them thus (*? not permanent*), or (*? recent*). State, if possible, whether the scar arises from a cut, a burn, an ulcer, &c.

6. In dealing with tattoo marks state the object represented "anchor," "heart," &c. If the tattoo mark consists of words or letters, copy them carefully.

Special care should be taken to detect and note any case where a tattoo mark has been removed or altered. The attempt to remove a mark can generally be detected, and the partially obliterated mark can often be deciphered.

Shape of Mark.

7. The shape of scars should be noted. Where the scar is a straight line, *e.g.*, where it is made by a sharp cut, it may be described as *rectilineal* ("RECT."). A scar may also be in a "curved line," "waved line," broken line, V. shape, Z. shape, X. shape, cross (+), &c.

Scars that are not linear, may be described as *oval*, *oblong*, *circular*, *triangular*, &c.

The shape of birth-marks should be given in the same way, and the colour should be stated. The shape of a tattoo mark is usually given in stating the object represented (paragraph 6).

Size.

8. When a scar or other mark is a line, give the length in millimetres. If it is circular, give the diameter. If oval or oblong, give both length and breadth. Give length and breadth of tattoo-marks.

Direction.

9. The direction of lines is to be given as vertical (VERT), horizontal (HOR) or slanting (SL). A line is *vertical* if, while the prisoner stands in the position already described, it runs up and down or nearly so—*horizontal* if it runs across on the same level or nearly so—*slanting* if it slopes from one side to another.

10. In the case of slanting marks you must give the direction the slope takes *downwards*, *i.e.*, say whether it slopes "inwards" (towards the body, or the middle line of the body), "outwards," "forwards" or "backwards."

11. In describing curved scars, say in which direction the *hollow* of the curve turns—hollow side *up* or *down*, to *front*, to *back*, &c.

Position.

12. The marks are taken in the following order:—

I. Left arm and hand.
II. Right arm and hand.
III. Face and throat.
IV. Chest (down to navel).
V. Back (including back of neck).
VI. The rest of the body.

13. In taking the marks work generally from left to right and from above downwards.

* Note especially that the *inner* side of the arm does not include the inside of the elbow which in the position given forms part of the *front* of the arm.

I. and II.—*Arms and Hands.*

14. In the upper arm, say whether the mark is on the front, back, inner side or outer side, and give the distance from elbow or shoulder.

Example:—

Scar rect., 25 m. long, vert.: 50 m. above elbow, back of right upper arm.

15. In the forearm, state in the same way whether the mark is on the front or back, inner side or outer side, and give the distance from the elbow or wrist.

16. On the hand indicate the position of the mark on back or front (*e.g.* "at base of thumb," "between forefinger and middle finger, &c.").

17. Each finger has three joints, and the sections of the finger between the joints are called phalanges. You can, therefore, besides saying that a mark is on front, back, &c. of a finger, give its position on first joint, second joint, third joint—or on first phal., second phal., third phal. The numbers begin from the joint next the back of the hand, the joint nearest the finger tip being the third joint.

Similarly with the thumb except that there are only two joints and two phalanges.

III.—*Face and throat.*

18. The chief points in the face and throat used for indicating positions and distances of marks are:—

The line of the hair.
The root of the nose.
The eyebrows (inner end, outer end, middle).
The eyes (inner angle, outer angle).
The point of the nose and the outer corners of the nostrils (right and left).
The ear (highest point and lowest point).
The corners of the mouth.
The larynx or "Adam's apple."

The cheek, chin, temple, &c., are used to assist in describing the position of marks, but the distances from *points* should always be given.

Examples:—

Scar rect., 20 m. long, hor.; 25 m. below outer angle of right eye.

Birth-mark, circ. 3 m. diam. on left cheek 30 m. in front of lowest part of ear.

IV.—*Chest.*

19. The measurements are taken from:

The fork of the breast-bone,
The nipples (right and left),
The navel,
The "middle line."

The latter is an imaginary line dividing the body down the middle (*i.e.*, passing through the centre of nose and chin, the larynx, and the navel).

Example:—

Scar, oval, 2 by 5; 60 below larynx, 35 to left of middle line.

Tattoo mark, anchor, 5 by 6; 40 above navel, 25 to right of middle line.

V.—*Back.*

20. The measurements are taken from the "middle line," which here follows the centre of the backbone, and the seventh vertebra. The seventh vertebra is the most prominent section of the backbone, and lies a little above the line of the shoulders. Its position is sometimes not easy to determine with exactness, and some allowances must be made for errors. When the prisoner is stout, the point becomes more prominent if he is made to bend forward his head.

Example:—

Scar, curve, hollow upwards, 50 long; 150 below seventh vertebra, 80 to right of middle line.

VI.—

21. Only very prominent marks, *e.g.*, loss of limb or toe, &c., are to be given.

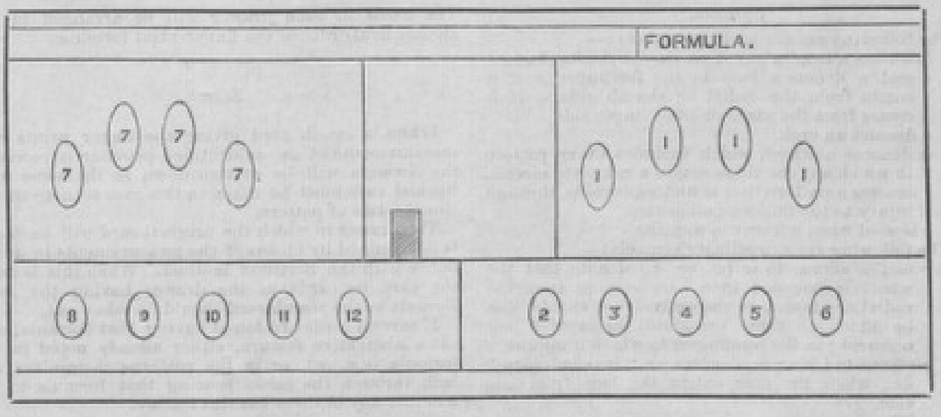
22. If there are several clearly distinctive marks on I., II., and III., no marks need be noted on IV., V., and VI., except such as are specially prominent or characteristic.

(The descriptions are much shortened by the use of abbreviations similar to those employed by M. Bertillon, and recommended in the report, page 19. Only a few examples of these abbreviations are given above. The m. for millimetre may always be omitted, as all the measurements are in millimetres.)

APPENDIX F.

INSTRUCTIONS FOR TAKING FINGER-PRINTS.

Every prison where finger-prints are to be taken will be supplied with a plate of copper, 10½ ins. by 7 or of such other size as experience may show to be most convenient, screwed by its corners and down both ends to a board 1 inch thick, an ordinary printer's roller, 9 inches in length and 3 in diameter, two tubes of ordinary printer's ink, some benzole and a stock of cards 12 inches by 5 as indicated in the report.



1. Squeeze less than a drop of ink on the copper plate and work it with the roller till it forms an even layer over the surface. The layer of ink must be so thin as to allow the copper colour of the plate to show through it.

2. Take the prisoner's right hand and lay the bulbs of the four fingers flat on the inked plate pressing them gently but firmly with your own hand. Then lay the inked fingers flat on the upper right hand division of the card pressing them as before with your own hand, so that imprints of the four finger bulbs may be taken at the places marked 1 in the above woodcut.

3. Then take the thumb of the right hand, roll the bulb slightly on the inked slab and roll it again on the lower part of the card at the part marked 2. Do the same with each of the fingers in succession, so that imprints of them may be taken at 3, 4, 5 and 6. These imprints will be more extended than those taken at 1, but are sometimes not so sharp.

4. Repeat the process with the prisoner's left hand, except that it will probably be found more convenient in taking the *separate* imprints of the fingers to begin with the little finger at 8. In any case, however, the left hand thumb must be printed on the card at 12.

5. Care should be taken in the lower range of imprints that the whole of the finger bulbs should be laid on the card well above the line that cuts off a margin at the bottom.

6. Both the roller and the slab must be thoroughly cleaned with benzole, dried with a rag, and put out of the way of dust when done with.

7. The fingers may also be readily cleaned with benzole or turpentine after the imprints have been taken.

APPENDIX G.

MEMORANDUM AS TO THE READING OF FINGER PRINT FORMULÆ.

(Revised by Mr. Galton.)

The following memorandum is intended as the basis of the instructions for the assistants in the Central Registry who have to deal with finger prints. It is not to be regarded as final, but merely as a brief statement of Mr. Galton's method in its present stage. Alterations in details will be made as the process is developed in practice.

Symbols.

The following are the leading symbols:—

l denotes a loop in any digit except the forefinger.

r and *u* denote a loop in the forefinger, *r* if it comes from the radial or thumb side, *u* if it comes from the ulnar or little finger side.

a denotes an arch.

w denotes a whorl, which includes every pattern in which any one ridge makes a complete circuit.

x denotes a pattern that is undecipherable, through injury to the finger or otherwise.

o is used when a finger is wanting.

The following are subordinate symbols:—

r and *u* affixed to *w* (w_r or w_u) denote that the whorl is enclosed in a loop coming from the radial or ulnar side respectively; *r* should also be added to those occasional cases of *l* (not occurring in the forefinger) to which it applies.

v affixed to *l*, *r*, or *u*, denotes an "invaded loop," *i.e.*, when the core enters the loop from one side.

y affixed to *l*, *r*, *u*, or *w*, denotes a pattern that suggests an eyelet hole; it is often transitional between a loop and a whorl.

k affixed to *l*, *r*, or *u*, denotes a form of loop in which the core is distinctly hooked.

Reading Patterns.

The assistant must acquire knowledge of the types of pattern mentioned above by examination of a large number of actual finger prints. He should fix his attention first on the outline of the pattern, and then on its core, and never allow it to dwell on non-essentials however conspicuous they may be, such as differences due to the impressions having been taken from slightly different parts of the finger, or being blacker in some parts than others. He should also practice tracing patterns in the manner described in "Finger Prints," page 69. He will be supplied with a book containing photographic reproductions of the forms of pattern which are transitional between the types mentioned above, showing in each case to which type the form is to be assigned. When a knowledge of these forms is acquired in this way, few cases will occur which cannot be assigned with certainty to one or other of the main types.

In reading off imprints, first determine to which type the pattern belongs and write down the symbol *a*, *l*, *r*, *u*, or *w*, as the case may be.

In the case of a transitional form, note below the line the other possible interpretation, *e.g.*, l_y, w_u. These symbols should be added even in cases where there is no doubt as to the type to which the print belongs, but where they may aid the searches in the register by indicating a well-marked feature.

Writing Formulæ.

In writing the formula for the set of 10 finger prints the symbols will be written in the following order: the first, second, and third finger of right hand; the first, second, and third finger of left hand; the thumb and little finger of right hand; the thumb and little finger of left hand. They will thus fall into four groups, divided as in the following example, *ull*, *alw*; *wl*, *wl*.

The formula will be noted at the right hand top corner of the card.

Arrangement of Cards.

The drawer in which each card is placed is determined by the measurements on the Bertillon method. The cards in each drawer will be arranged in the alphabetical order of the finger-print formulæ.

Search.

When a *search* card giving the finger prints and measurements of an unidentified prisoner is received, the formula will be written down in the same way. Special care must be taken in this case to note transitional forms of pattern.

The drawer in which the original card will be found is determined by means of the measurements in accordance with the Bertillon method. When this is done, the card or cards in the drawer having the same formula as the search card should be taken out.

If several cards are found having that formula, seek some distinctive feature, either already noted in the formula, (*e.g.*, w_r), or in the patterns themselves, and look through the cards bearing that formula to see whether any of them has this feature.

When one card has been selected, compare carefully the prints of several of the fingers with those on the search card, to ascertain whether they are imprints of the same hands.

A minute comparison of the details in the prints requires the use of a lens (a watchmaker's lens is convenient); also of two or more pairs of "pointers" to mark down corresponding points in the two imprints, from which, as from starting points, others may be successively laid down. A pointer consists of a wooden arm a little thicker than a pencil, from 6 to 10 inches long, having a common pin inserted firmly into its pointed end and then bent downwards. The arm is fixed to a short cross-bar (3 or 4 inches long), which rests on two nails with smooth heads. Thus the pointer is a tripod. The arms of the two pointers in each pair should be of different lengths to prevent their cross-bars from interference when they are both in use on the same print.

If the card is not found under the same formula, and if there are any transitional forms in the prints on the search card, search should be made under the other formula or formulæ indicated by transitional symbols.

If the card is not found there, it is not in the drawer.

Subdivision.

When the cards in one drawer bearing the same formula become very numerous, a subdivision will be necessary.

This will occur first with the formula *ull*, *ull*; *ll*, *ll*.

The principle of subdivision is to select one finger—the same in all cases (say the right forefinger), and having with the aid of the pointers determined (1) the central ridge of the loop, (2) the corner where the ridges, passing over the loop, diverge from those passing below it, to count the number of intervening ridges. The cards having this formula are then arranged according to number of ridges.

When there is no central ridge, but a narrow loop or "staple," the counting is to begin from the further shoulder of the staple.

In searching, count the ridges in the same finger in the same way, and search those of the cards of the *ull*, *ull*; *ll*, *ll* formula having the same number of ridges. Allowance must be made for a possible error of two in counting the ridges. Thus, if the number counted is 7, it is necessary first to look through the cards having 7, then those having 6 and 8, then those having 5 and 9.

APPENDIX H.

(To face p. 81.)

Fig. I. Card.

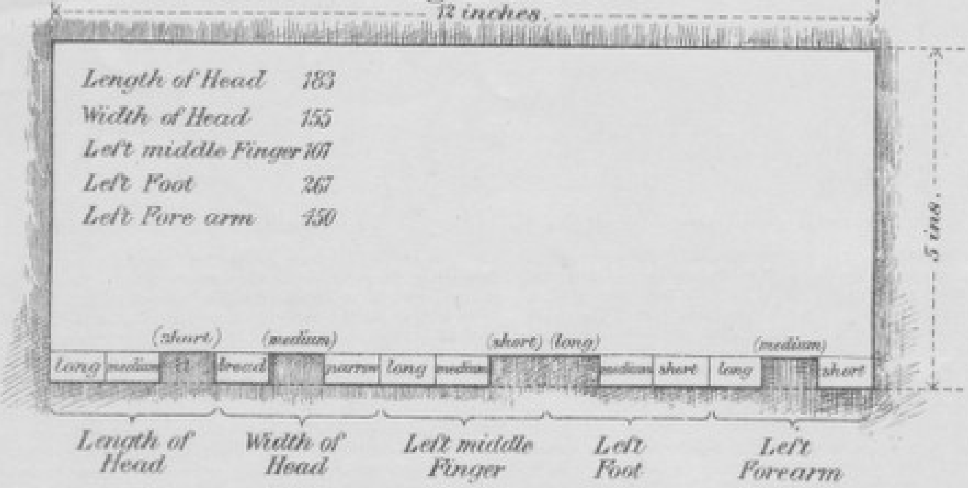


Fig 2. Plan of Drawer.

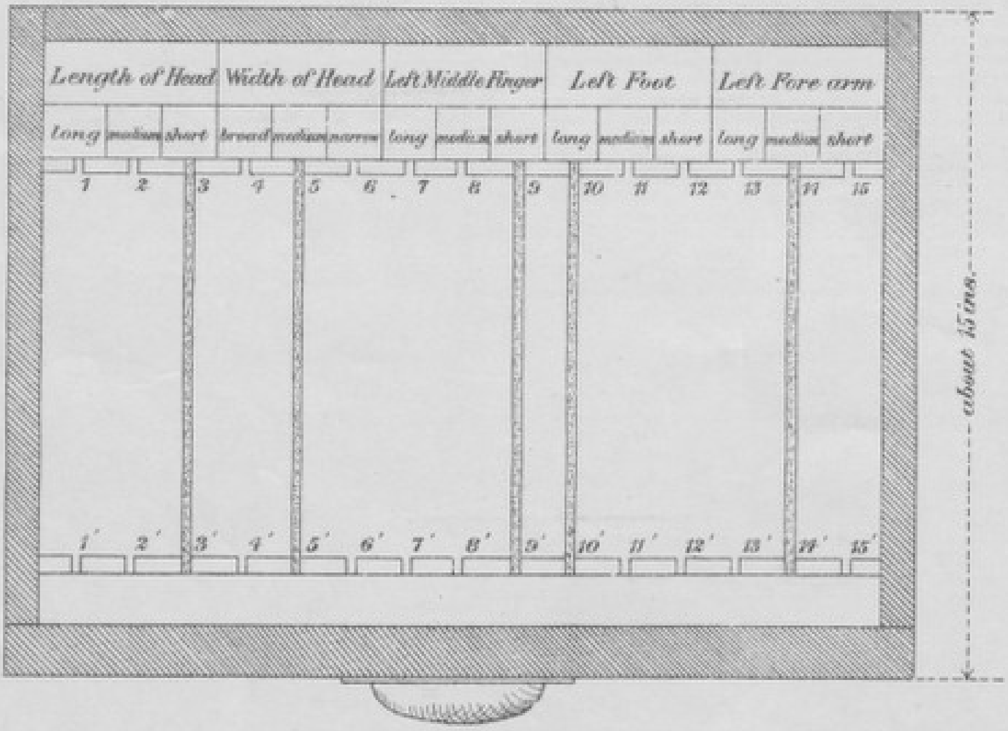
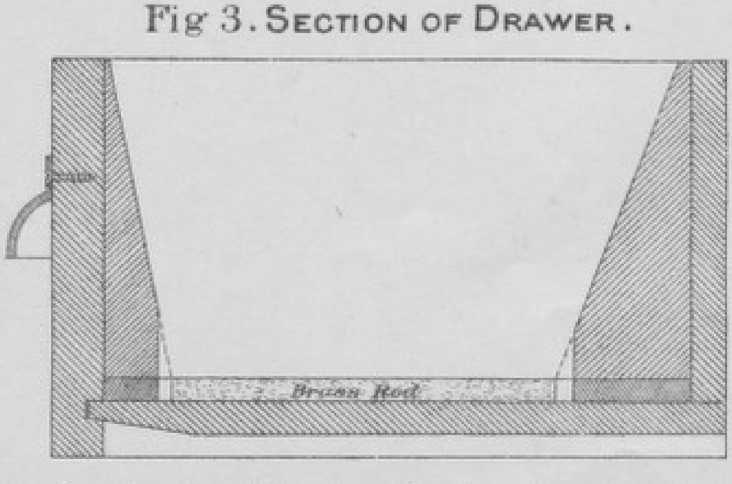
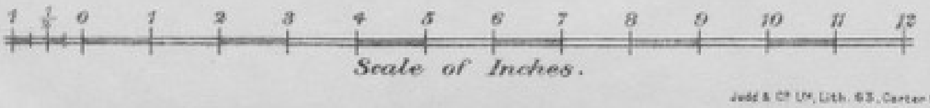


Fig 3. Section of Drawer.





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APPENDIX H.

Mechanical Contrivance to check the Sorting of the Cards in the Bertillon Cabinet.

The accompanying diagrams explain the mechanical arrangement devised by Mr. Galton to secure that each card in the Bertillon cabinet should be placed in the proper drawer. Fig. 1 represents one of the cards described on page 32 of the report with the measurements noted on the left-hand corner, the other particulars having been omitted as not essential for the present purpose. Fig. 2 is a plan of one of the drawers in the Bertillon cabinet; and Fig. 3 a section of the drawer from front to back.*

Each drawer is made 12¼ inches broad by 5 inches deep, so that the the cards, which measure 12 inches by 5, placed edgewise, may fit in with ⅛ inch play to either side from their mean position. The length from front to back of the drawer will be sufficient to allow about 500 cards standing edgewise to be placed in it. At the lower edge of each card 15 equal spaces (arranged in groups of three) are marked off, and any one or more of these spaces can be notched out. At the front and back of the drawer, in which the cards are to stand, there are 15 grooves, between which narrow rods of brass or wood can be placed edge upwards. Suppose now we have the card of a prisoner whose head length is 183. This ranks as a Short Head, consequently a notch is cut in the card at the third space (*a* in Fig. 1). In each of the drawers in the section of the cabinet for Short Heads a rod is placed at the bottom of the drawer between the third groove (3 and 3' in Fig. 2). If the card in question is placed in any one of the drawers for Short Heads, the notch will stride over the rod, and the card will therefore fit the drawer; but if the card is placed in a drawer for Long Heads (in which the rod extends from 1 to 1'), or for Medium Length Heads (where the rod extends from 2 to 2'), there will be no notch to admit the rod, and the card will be tilted up so that its upper edge is not level with those of the other cards, and will prevent the drawer shutting. Similarly with the other five measurements. Each drawer will thus have five rods running from back to front along the bottom, and each card will have five notches cut in the lower edge; and assuming the notches to be properly cut, each card will fit into its proper drawer, but will not fit into any of the 242 other drawers in the cabinet.*

* The diagrams have been drawn for the Committee by Mr. F. W. Troup, Architect.

* The width of the notches so much exceeds the width of the rods that the lateral play of the cards in their box does not at all interfere with the action of the apparatus. Each notch is 8 tenths of an inch in width, the lateral play is 2½ tenths, which reduces the available width of the notch to 5½ tenths. Consequently the rod might theoretically be a full half-inch in width, but it would be proper that to allow for defects in straightness it should not exceed a quarter of an inch.

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Kew: Royal Botanic Gardens. Bulletins of Miscellaneous Information. Volume for 1893 8vo. Boards. Price 3*s*. Monthly parts, 1894, price 4*d*.

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1. III. 94.

IDENTIFICATION OF HABITUAL CRIMINALS.

REPORT

OF A

COMMITTEE APPOINTED BY THE SECRETARY OF STATE

TO INQUIRE INTO THE

BEST MEANS AVAILABLE FOR IDENTIFYING

HABITUAL CRIMINALS;

WITH

MINUTES OF EVIDENCE AND APPENDICES.

Presented to both Houses of Parliament by Command of Her Majesty.

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1894.

EUGENICS;

ITS DEFINITION, SCOPE & AIMS

EUGENICS; ITS DEFINITION, SCOPE AND AIMS

by FRANCIS GALTON, D.C.L.; Sc.D.; F.R.S.

Read before the Sociological Society at a Meeting in the School of Economics and Political Science (London University), on May 16th, 1904, Professor KARL PEARSON, F.R.S., in the chair.

Professor KARL PEARSON, in opening the proceedings, said:—

My position here this afternoon requires possibly some explanation. I am not a member of the Sociological Society, and I must confess myself sceptical as to its power to do effective work. Frankly, I do not believe in groups of men and women who have each and all their allotted daily task creating a new branch of science. I believe it must be done by some one man who by force of knowledge, of method and of enthusiasm hews out, in rough outline it may be, but decisively, a new block and creates a school to carve out its details. I think you will find on enquiry that this is the history of each great branch of science. The initiative has been given by some one great thinker, a Descartes, a Newton, a Virchow, a Darwin or a Pasteur. A Sociological Society until we have found a great sociologist is a herd without its leader—there is no authority to set bounds to your science or to prescribe its functions. This you must realise is the view of that poor creature the doubting man, *in media vitæ*; it is a view which cannot stand for a moment against the youthful energy of your secretary, or the boyish hopefulness of Mr. Galton, who mentally is about half my age. Hence for a time I am carried away by their enthusiasm, and appear where I never anticipated being seen in the chair at a meeting of the Sociological Society. If this Society thrives, and lives to do yeoman work in science, which, sceptic as I am, I sincerely hope it may do, then I believe its members in the distant future will look back on this occasion as perhaps the one of greatest historical interest in its babyhood. To those of us who have worked in fields adjacent to Mr. Galton's, he appears to us as something more than the discoverer of a new method of enquiry, we feel for him something more than we may do for the distinguished scientists in whose laboratories we have chanced to work. There is an indescribable atmosphere which spreads from him and which must influence all those who have come within reach of it. We realise it in his perpetual youth, in the instinct with which he reaches a great truth, where many of us plod on groping through endless analysis, in his absolute unselfishness, and in his continual receptivity for new ideas. I have often wondered if Mr. Galton ever quarrelled with anybody. And to the mind of one who is ever in controversy, it is one of the miracles associated with Mr. Galton, that I know of no controversy, scientific or literary, in which he has been engaged. Those who look up to him, as we do, as to a master and scientific leader feel for him as did the scholars for the gramarian.

> "Our low life was the level's, and the night's;
> He's for the morning."

It seems to me that it is precisely in this spirit that he attacks the gravest problem which lies before the Caucassian races "in the morning." Are we to make the whole doctrine of descent, of inheritance, and selection of the fitter, part of our everyday life, of our social customs, and conduct? It is the question of the study now, but tomorrow it will be the question of the market place of morality, and of politics. If I wanted to know how to put a saddle on a camel's back without chafing him, I should go to Francis Galton; if I wanted to know how to manage the women of a treacherous African tribe, I should go to Francis Galton; if I wanted an instrument for measuring a snail, or an arc of latitude, I should appeal to Francis Galton. If I wanted advice on any mechanical, or any geographical, or any sociological problem, I should consult Francis Galton. In all these matters and many others I feel confident he would throw light on my difficulties, and I am firmly convinced that with his eternal youth, his elasticity of mind, and his keen insight, he can aid us in seeking an answer to one of the most vital of our national problems: How is the next generation of Englishmen to be mentally and physically equal to the past generation, which has provided us with the great Victorian statesmen, writers, and men of science?—most of whom are now no more—but which has not entirely ceased to be as long as we can see Francis Galton in the flesh.

Mr. GALTON said:—

Eugenics is the science which deals with all influences that improve the inborn qualities of a race; also with those that develop them to the utmost advantage. The improvement of the inborn qualities, or stock, of some one human population, will alone be discussed here.

What is meant by improvement? What by the syllable *Eu* in Eugenics, whose English equivalent is *good*? There is considerable difference between goodness in the several qualities and in that of the character as a whole. The character depends largely on the *proportion* between qualities, whose balance may be much influenced by education. We must therefore leave morals as far as possible out of the discussion, not entangling ourselves with the almost hopeless difficulties they raise as to whether a character as a whole is good or bad. Moreover, the goodness or badness of character is not absolute, but relative to the current form of civilisation. A fable will best explain what is meant. Let the scene be the Zoological Gardens in the quiet hours of the night, and suppose that as in old fables the animals are able to converse, and that some very wise creature who had easy access to all the cages, say a philosophic sparrow or rat, was engaged in collecting the opinions of all sorts of animals with a view of elaborating a system of absolute morality. It is needless to enlarge on the contrariety of ideals between the beasts that prey and those they prey upon, between those of the animals that have to work hard for their food and the sedentary parasites that cling to their bodies and suck their blood, and so forth. A large number of suffrages in favour of maternal affection would be obtained, but most species of fish would repudiate it, while among the voices of birds would be heard the musical protest of the cuckoo. Though no agreement could be reached as to absolute morality, the essentials of Eugenics may be easily defined. All creatures would agree that it was better to be healthy than sick, vigorous than weak, well fitted than ill-fitted for their part in life. In short, that it was better to be good rather than bad specimens of their kind, whatever that kind might be. So with men. There are a vast number of conflicting ideals, of alternative characters, of incompatible civilisations; but they are wanted to give fulness and interest to life. Society would be very dull if every man resembled the highly estimable Marcus Aurelius or Adam Bede. The aim of Eugenics is to represent each class or sect by its best specimens; that done, to leave them to work out their common civilisation in their own way.

A considerable list of qualities can be easily compiled that nearly every one except "cranks" would take into account when picking out the best specimens of his class. It would include health, energy, ability, manliness and courteous disposition. Recollect that the natural differences between dogs are highly marked in all these respects, and that men are quite as variable by nature as other animals in their respective species. Special aptitudes would be assessed highly by those who possessed them, as the artistic faculties by artists, fearlessness of inquiry and veracity by scientists, religious absorption by mystics, and so on. There would be self-sacrificers, self-tormentors and other exceptional idealists, but the representatives of these would be better members of a community than the body of their electors. They would have more of those qualities that are needed in a State, more vigour, more ability, and more consistency of purpose. The community might be trusted to refuse representatives of criminals, and of others whom it rates as undesirable.

Let us for a moment suppose that the practice of Eugenics should hereafter raise the average quality of our nation to that of its better moiety at the present day, and consider the gain. The general tone of domestic, social, and political life would be higher. The race as a whole would be less foolish, less frivolous, less excitable and politically more provident than now. Its demagogues who "played to the gallery" would play to a more sensible gallery than at present. We should be better fitted to fulfil our vast imperial opportunities. Lastly, 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.

The aim of Eugenics is to bring as many influences as can be reasonably employed, to cause the useful classes in the community to contribute *more* than their proportion to the next generation.

The course of procedure that lies within the functions of a learned and active Society such as the Sociological may become, 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. Few seem to be aware how greatly the knowledge of what may be termed the *actuarial* side of heredity has 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 strong reason for believing that national rise and decline is closely connected with this influence. It seems to be the tendency of high civilisation to check fertility in the upper classes, through numerous causes, some of which are well known, others are inferred, and others again are wholly obscure. The latter class are apparently analogous to those which bar the fertility of most species of wild animals in zoological gardens. Out of the hundreds and thousands of species that have been tamed, very few indeed are fertile when their liberty is restricted and their struggles for livelihood are abolished; those which are so and are other-wise useful to man becoming domesticated. There is perhaps some connection between this obscure action and the disappearance of most savage races when brought into contact with high civilisation, though there are other and well-known concomitant causes. But while most barbarous races disappear, some, like the negro, do not. It may therefore be expected that types of our race will be found to exist, which can be highly civilised without losing fertility, nay, they may become more fertile under artificial conditions, as is the case with many domestic animals.

3. Systematic collection of facts showing the circumstances under which large and thriving families have most frequently originated; in other words, the *conditions* of Eugenics. The names of the thriving families in England have yet to be learnt, and the conditions under which they have arisen. We cannot hope to make much advance in the science of Eugenics without a careful study of facts that are now accessible with difficulty, if at all. The definition of a thriving family, that will

pass muster for the moment at least, is one in which the children have gained distinctly superior positions to those who were their class-mates in early life. Families may be considered "large" that contain not less than three adult male children. It would be no great burden to a Society including many members who had Eugenics at heart, to initiate and to preserve a large collection of such records for the use of statistical students. The committee charged with the task would have to consider very carefully the form of their circular and the persons entrusted to distribute it. The circular should be simple, and as brief as possible, consistent with asking all questions that are likely to be answered truly, and which would be important to the inquiry. They should ask, at least in the first instance, only for as much information as could be easily, and would be readily, supplied by any member of the family appealed to. The point to be ascertained is the *status* of the two parents at the time of their marriage, whence its more or less eugenic character might have been predicted, if the larger knowledge that we now hope to obtain, had then existed. Some account would, of course, be wanted of their race, profession, and residence; also of their own respective parentages, and of their brothers and sisters. Finally, the reasons would be required why the children deserved to be entitled a "thriving" family, to distinguish worthy from unworthy success. This manuscript collection might hereafter develop into a "golden book" of thriving families. The Chinese, whose customs have often much sound sense, make their honors retrospective. We might learn from them to show that respect to the parents of noteworthy children, which the contributors of such valuable assets to the national wealth richly deserve. The act of systematically collecting records of thriving families would have the further advantage of familiarising the public with the fact, that Eugenics had at length become a subject of serious scientific study by an energetic Society.

4. Influences affecting Marriage. The remarks of Lord Bacon in his essay on Death may appropriately be quoted here. He says, with the view of minimising its terrors:

> "There is no passion in the mind of men so weak, but it mates and masters the fear of death . . . Revenge triumphs over death; love slights it; honour aspireth to it; grief flyeth to it; fear pre-occupateth it."

Exactly the same kind of considerations apply to marriage. The passion of love seems so overpowering that it may be thought folly to try to direct its course. But plain facts do not confirm this view. Social influences of all kinds have immense power in the end, and they are very various. 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. The multitude of marriage restrictions that have proved prohibitive among uncivilised people would require a volume to describe.

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. It has, indeed, strong claims to become an orthodox religious tenet of the future, for Eugenics co-operate with the workings of Nature by securing that humanity shall be represented by the fittest 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, seems to me one of the highest objects that we can reasonably attempt. We are ignorant of the ultimate destinies of humanity, but 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. I see no impossibility in Eugenics becoming a religious dogma among mankind, 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 will 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.

FRANCIS GALTON.

APPENDIX.

WORKS BY THE AUTHOR BEARING ON EUGENICS:

Hereditary Genius (Macmillan), 1869; 2nd Edition, 1892. See especially from p. 340 in the former edition to the end, and from p. 329 in the latter.

Human Faculty (Macmillan), 1883 (out of print). See especially pp. 305 to end.

Natural Inheritance (Macmillan), 1889. This bears on Inheritance generally, not particularly on Eugenics.

Huxley Lecture of the Anthropol. Inst. on the Possible Improvement of the Human Breed under the existing Conditions of Law and Sentiment. Nature, 1901, p. 659; "Smithsonian Report," Washington, 1901, p. 523.

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Plate taken out 29/1/24 R. P.

THE PHOTOGRAPHIC NEWS.

Vol. XXIX. No. 1389.—*April* 17, 1885.

CONTENTS.

CAPTAIN ABNEY'S CANTOR LECTURES

On Monday evening next, the 20th inst., at eight o'clock, Captain Abney will deliver the first of a course of two lectures on "Photography and the Spectroscope," the date fixed for the remaining lecture being the following Monday, the 27th inst. The following is the syllabus of the lectures :—

Lecture I. *April 20th.*—The prismatic spectrum and influence of the material on the spectrum. Dispersion and resolving power. Uses of the slit and collimator. The spectroscopic camera. Application of photography for investigating the spectrum, and of the spectrum for investigating photography.

Lecture II. *April 27th.*—The diffracture spectrum. The ordinary grating. Influence of the number of lines on resolving power. The reflection grating. The flat reflection grating. Absorption of radiation and atomic motion, and the formation of the photographic image.

Although these lectures are primarily intended for the members of the Society of Arts and their friends, the Society of Arts has always been liberal in admitting outsiders who are likely to profit by the instruction offered, so we may hope that any reader of the News who may wish to attend, will be able to gain admission by writing to the Secretary, Mr. H. Trueman Wood.

THE EDINBURGH CONFERENCES ON THE PICTURESQUE.

In our "Notes" of last week, we alluded to some of the advantages resulting from the system initiated by the Edinburgh Society. It is very difficult for our readers to thoroughly realize the great interest taken in the discussion by those present at the conferences, and it is only the fact that it is not practicable to reproduce the pictures themselves that prevents us from reproducing the discussions in full.

The following extracts from remarks made by Mr. Macbeth are interesting in themselves, and carry instruction, even although the subjects criticised are not present to the reader :—

"*Photographic Portrait of an Elderly Gentleman, seated, a Table with some Large Books on it at his Back, close to his Chair. A Three-quarter Length down to the Knee, most likely from a Wet-Plate Negative.*

"I do not imagine that the photographer could claim the merit of arrangement here. It seems so natural, and so consistent throughout with the feeling expressed. I think it is most likely this position was taken by the person himself. Only the operator has shown not a little skill in the choice of point of sight, and the particular point to which the sitter should turn his eyes. There is perhaps no more important matter than for the sitter to be well directed where to look. The eyes here are a little off the plane of face. This gives a slight feeling of reserve, not staring, not forward, but retiring and reticent. Very possibly this also came direct from the person's own act, but whatever be the cause, it is extremely good, and in beautiful harmony with the subject.

"What leads me to suspect that the operator had not much hand in the arrangement of his subject, is the 'do no better' of placing the table and books behind the back of the sitter. This should not be. The subject having been placed so much in the centre (this being thought to be necessary), the operator was obliged to fill up the space behind the chair. In my opinion it would have been better to have placed the sitter nearer to the boundary line, and filled up the gap on the other side by the same accompaniments. As it is, the line of the leg is not sufficiently extended.

"Another thing which leads me to suppose the operator did not interfere with the person's own action is the beautiful unconscious position of the hands. Fortunately, by their being so little seen and so quiet, great prominence is given to the head. There is nothing so perplexing at times, either to the operator or the sitter, as to know how to place the hands. One is always safe when the sitter naturally places them together. They should either be thus, or very widely apart; in either case they are less distracting to the spectator. If apart, and within a foot from each other, the effect is bad, and generally this is the result of not knowing where and how to place them. It is important also to notice that when the figure is sitting in a three-quarter point of view, the hands (where they come in) should not be directly below the head. This gives repose to the more important part—the seat of intellect.

"*En Reconnaisance by Edouard Detaille. From a Photo-Gravure by Goupil and Co.*

"This street scene during the Franco-Prussian war affords, as a piece of composition, many valuable examples of careful study, both in spirit and design. We feel at once in the presence of some very awful and stirring incidents. I have no doubt the elements of the composition were all carefully gathered, not from one scene, but from several, and compiled so as to embrace all that was dramatic, and thoroughly telling the story of the conflict.

"I do not know the name of the street. It bears the impress of being some of those lanes in the suburbs of Paris. In the distance is seen a French officer on horseback and his regiment making their appearance. A little farther on towards us are seen two bodies of men—French soldiers, coming into the street from both sides. The French at this moment are evidently masters of the situation. At the window, in the white house, we see its inmate on his guard looking out. Near him are two men looking over the wall, and cheering those entering on the scene. A little to the right are two boys dreadfully frightened, creeping along by the wall. Towards or near the centre is the principal group of eight soldiers and a boy. They have passed on their left a wounded German sitting up holding his wounded arm, and approach on their left, probably an Uhlan apparently dead, lying over his wounded and dying horse. To the right of the principal group a most touching incident is depicted. A poor fellow, a German, has fallen, and is attended by old people who have come out of the adjoining house. If you notice, the street takes a bend in the foreground, giving beautiful form, and producing variety in the ground plan of the picture.

"In a good design where there is much variety, the artist need not actually divide the surface of field with so many odd numbers of lines. Still, by analysis it will be found that the intersections of odd numbered cross lines will very frequently fall over the most of the important points of the subject. I submitted this work to the test of seven divisions both in length and breadth of the field, and was struck with the frequency with which the intersections fell on the points of interest. The horse is a most important object for the foreground. The lance of the Uhlan with the flag at the end of it is very valuable. Also, what by an ordinary observer would be considered accidental, because they appear so, are the hats, sword, and pistol lying in different places. The boy guide is very natural, indeed every figure is so varied in his form and position, that every one appears in his action to be quite casual. No instantaneous photograph could have realized more than is here so skilfully drawn and represented. The feet of the principal group form a great study. We often see, in instantaneous street views, some ludicrous action in the feet of those walking, so much so that we are apt to question the correctness of it. This shows that instantaneous work does not convey the impressions conveyed to us when we see people walking. Now here, as in the action of the young soldier to the right in the principal group, you see how carefully the artist placed the feet. It is one of the most difficult things to notice when one foot touches the ground, where the other is, and what position it takes. Few are aware that the moment we begin to move, the head takes the lead, and no sooner is it bent out, than either foot is put out to support it. You will always find that when a person leans on one foot, the head is plumb above it; when a person stands on both feet equally, the head is then right above between the two. These facts are more or less according to the action beautifully illustrated in this picture, more especially the head always supported in the action of every figure here presented."

Other societies might well imitate the Edinburgh Association, and organise discussions upon the merits of pictures selected for criticism. The plan of projecting them upon the screen by means of the optical lantern is an excellent one, as under the circumstances all can see the subject under consideration.

THE DEATH OF GEORGE SWAN NOTTAGE, LORD MAYOR OF LONDON.

As most of our readers have already heard from the newspapers, the Lord Mayor passed away at about half past seven on Saturday morning; the immediate cause of death being extreme exhaustion resulting from pleurisy and diabetes. He took cold at Brighton while present at the review of Easter Monday; but during the middle of the past week he recovered from the resulting illness sufficiently to enable him to distribute the Easter gifts to the boys of Christ's Hospital. Still, he did this contrary to the advice of his medical attendants. Immediately afterwards he became worse, and he expired at the time mentioned.

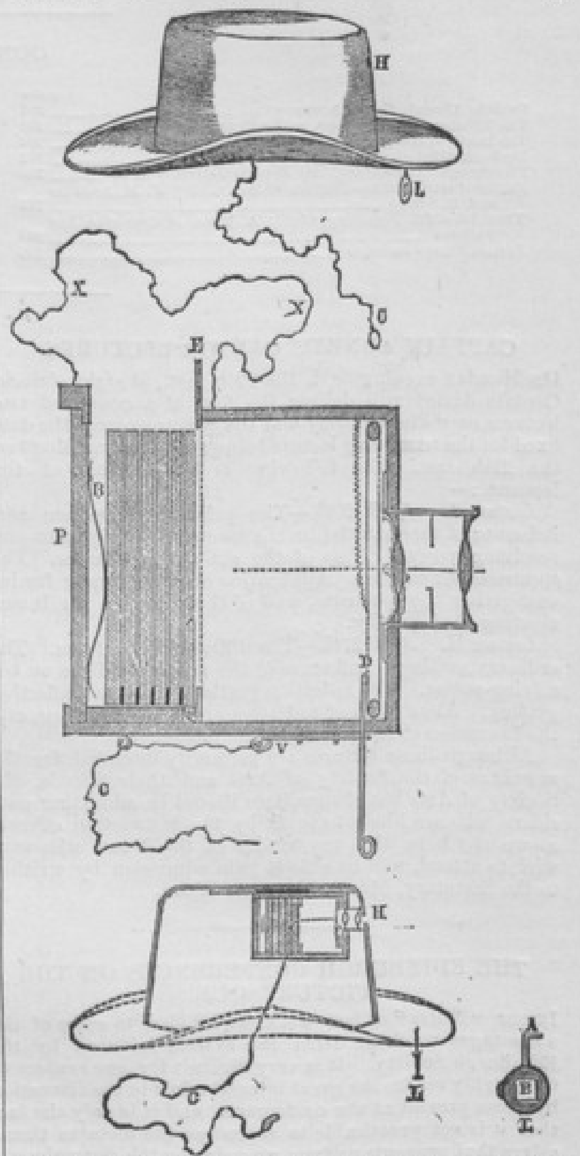
The late Lord Mayor was born in 1823, and from his early youth he evinced tastes which led him into scientific and artistic circles. He was associated with Sir David Brewster in much of his work relating to the stereoscope, and the commercial outcome of this association was the establishment of the London Stereoscopic Company. The wonder and delight with which the public received this invention will be remembered by our readers. Notwithstanding the strange circumstance that the realistically solid views of the stereoscope are no longer very much appreciated by the general public, the firm has retained its name, and has done a very extensive publishing trade. Mr. Nottage leaves a son and daughter; the former, Mr. Charles G. Nottage, was called to the bar not long ago, and the latter is married to Mr. S. E. Palmer, of the well-known biscuit firm in Reading.

It has been arranged that the funeral shall take place in St. Paul's Cathedral on Saturday next at noon.

DE NECK'S HAT-CAMERA.

SEVERAL hat-cameras have been invented of late, and one of the best we have seen is that designed by Mr. J. De Neck, a well known amateur residing in Brussels.

The subjoined drawing is taken from the *Bulletin Belge*,



and does not require very much explanation. The hanging-piece, L, is a kind of finder provided with a square opening corresponding to the field covered by the lens, and the double-blind shutter, D, is made to act by the cord C. There is no focussing screen to the apparatus, as the objective is set to a distance which fairly covers all objects at any distance beyond a known minimum, so no focussing is required.

The plates, each of which is enclosed in a very light frame of brass, are introduced into the apparatus by the door P, and the front one is kept always in focus by the action of the spring R.

After exposure, the front plate, together with its frame, is lifted by means of the extractor E, into the pocket X, and introduced into the camera again, immediately in front of the spring R. The camera slides into grooves attached inside the crown of the hat, and may be readily removed for changing the plates.

PHOTOGRAPHIC COMPOSITES.

BY FRANCIS GALTON, F.R.S.

Your request to send specimens of photographic composites as illustrations for the Photographic News happened to reach me at an appropriate time. It had just been arranged by the Council of the Anthropological Institute to assign an evening to two memoirs on the race characteristics of the Jews, and to the discussion that might arise upon them, and I had already sorted out for exhibition at that meeting a few composites of Jewish faces that I made some time ago. They are, I think, the best specimens of composites I have ever produced; they were made at the suggestion of Mr. Jacobs, the author of the more important of the two valuable memoirs then about to be read at the Institute. I therefore had them in hand, and am now able to send them to you with pleasure, understanding that Mr. Jacobs will furnish you with his own views as to how far these composites succeed, in his opinion, in presenting the typical features of the modern Jewish face. On this point I will consequently abstain from saying anything, though I may mention that the individual photographs were taken with hardly any selection from among Jewish boys in the Jews' Free School, Bell Lane, which, by the way, I am informed is the largest school of any, other than Board Schools, in the United Kingdom; and to the courtesy of whose Head Master, Mr. M. Angil, I am much indebted. They were children of poor parents, dirty little fellows individually, but wonderfully beautiful, as I think, in these composites. The feature that struck me the most, as I drove through the adjacent Jewish quarter, was the cold scanning gaze of man, woman, and child, and this was no less conspicuous among the schoolboys. There was no sign of diffidence in any of their looks, nor of surprise at the unwonted intrusion. I felt, rightly or wrongly, that every one of them was coolly appraising me at market value, without the slightest interest of any other kind.

Plate I., that accompanies this article, contains full-face portraits and composites therefrom. Plate II. will follow next week, and will contain profiles. As regards Plate I., the four large ovals, A, B, C, D, are composites, and all the rest are individual portraits. A is the composite of the five individuals a_1 to a_5. It bears strong evidence of the very dark and sharply-defined curl on the forehead of a_5, although the intensity of the original has been reduced five times by the process. B is the composite of the five individuals b_1 to b_5; C is the co-composite of A and B, with the addition of three other individual portraits to increase its sharpness. The black curl of a_5 is still traceable on the open forehead of the composite, though its original intensity has now been decreased thirteen times; but it would be probably overlooked, or mistaken for some chance shade or stain, if it were not for the abruptness of its outline. Lastly, D is a composite of five adult faces, which are not given individually for want of space.

It is unnecessary to speak at length here on the principle of the process of making composites, or about the apparatus I use, as all this has been published in full in many places, and re-published in my last work.* It will probably be remembered by many of your readers that the principle consists in throwing faint images of a succession of accurately adjusted prints (or negatives) on the same part of a single sensitized plate, so that the resultant image is an aggregate of all its components, and a pictorial average of them. Also, that the apparatus I employ (made for me by Meagher) consists of the front of a camera fixed firmly on a solid board, with its back screwing out or in for purposes of focussing, and that in front of the lens there is a carriage which can be moved to and fro on a tramway along the board, and which supports a stage provided with many adjustments, to which the print is fixed. The print can be raised or lowered, it can be moved from side to side, and it can be rotated. Consequently (within moderate limits), whatever sized print is mounted on the stage, and in whatever position it may originally have been placed there, I can produce an image of it in the camera, of any required size, and can cause that image to fall in any required position on the sensitized plate.

The required position is defined by fixed fiducial lines, to which I adapt the image by the requisite adjustments of scale and position. I cannot briefly describe the somewhat complicated and delicate arrangement that I adopt for my fiducial lines, which is fully explained in the work above referred to. Suffice it to say, that when making full-face composites, I look through an eye-hole, down upon a piece of horizontal ground glass let into the roof of the camera, and there I see two images. The one is that of delicate and bright fiducial lines, similar in shape to those in fig. 1, and the other is the image of the portrait which has been thrown upwards by a hinged

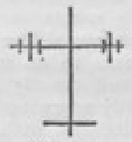


Fig. 1.

mirror that lies inside the body of the camera. While my eye is applied to the eye-hole, I have my hands free for making the adjustments. These are complete (1) when the pupils of the two eyes in the image of the print are exactly bisected by the upper of the two horizontal fiducial lines; (2) when the pupils of the eyes are equidistant laterally from the vertical line, for determining which the little cross lines give great assistance; and (3) when the parting of the lips in the image coincides with the lower of the horizontal fiducial lines. I adjust each portrait in turn in this way, and then, after capping the lens and turning the hinged mirror out of the way, I raise the dark slide. Then I withdraw the cap, give a brief exposure, and re-cap. Immediately afterwards, I close the dark slide, turn back the mirror, and remove the cap; then I am free to set to work with the next of the series of portraits, in the same way as that just described.

In taking profile portraits, I use a sloping line with two horizontal ones, as in fig. 2, and adjust the image of

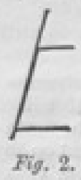


Fig. 2.

the print, so that its forehead shall be touched by the upper end of the sloping line, and that what I estimate to be the position of the front of the gums of the middle teeth in the upper jaw of the image shall be touched by the lower end of the sloping line. The line through the centre of the pupil of the eye in the profile image, and that through the parting between the lips, are then made, as before, to correspond with the upper and the lower of the two horizontal fiducial lines.

I will take this opportunity of emphasizing the value of a very simple precaution that vastly facilitates the process of reduction to a given scale, not only in making these composites, but also in other cases where reductions to scale and to position have to be made. I have not seen the plan to which I refer spoken of by others, and as I made it out for myself, I suppose it to have been hitherto overlooked, notwithstanding its simplicity. Speaking in the most general way, in every case of reduction to scale and position there are some two points (A, B) in the image

* "Inquiries into Human Faculty." F. Galton, F.R.S.—Macmillan and Co., 1883. See also Photographic News, 1881, pp. 316 and 332.

of the picture, that must be made to fit two points (A′, B′) in a fiducial line. If the precaution, of which I am about to speak, has not been taken, the process of making the fit will consist of a set of separate and tedious attempts, until the fit proves satisfactory. At the beginning of each attempt, the picture has to be removed a step further off, or nearer, as the case may be, and there must be a fresh focussing and a fresh adjustment for position. In my plan, I arrange the fiducial line A′ B′ so that one or other of its two ends—say A′—corresponds exactly with the optical axis of the camera. Then, however much the portrait may be moved to or fro parallel to the optical axis, and however large may be the corresponding focussing change in the length of the body of the camera, the point A in the image of the print will remain glued, as it were, to A′ in the fiducial line. After the line A B has been once superimposed on A′ B′ there will remain only the position of B to be attended to. In my apparatus I simultaneously work the carriage with one hand, and the focussing arrangements with the other, and the image, while retaining its sharpness, continuously waxes or wanes, as the case may be, in its size. The horizontal line that bisected the pupils at first, always continues to do so, and the vertical line still continues to stand exactly half-way between the pupils. I go on steadily screwing until the parting of the lips in the image coincides with the lower horizontal fiducial line, and then the adjustment is complete. In a solidly-made camera, it is easy to find, and mark once for all on its ground glass screen, the exact position of the intersection of the screen with the optical axis of the lens.

If we adjust the camera so that the image shall be of about the same size as the original picture, a little subsequent enlargement or reduction of the image will not require any sensible change of distance between the object and the ground glass screen. The lens can be moved to and fro a short distance, with the effect of altering the size of the image without sensibly affecting its definition. I therefore tested the position of the optical axis of my camera under these conditions. The camera and the portrait were both fixed, and as I screwed the lens in or out, the image grew smaller or larger without varying materially in sharpness, and expanded and contracted from a central radiant point, whose exact position I very soon ascertained. This being fixed, the parallelism of the tramway of the carriage to the optical axis was rectified until the to and fro movement of the carriage had no effect in causing the image A to separate from the fiducial A′. When all was satisfactorily arranged, the process of reduction to scale became swift, and very interesting to perform.

Beautiful as the adjustments of my camera are, I must honestly confess that if I had to begin quite afresh, I should employ a much more disconnected process. It would be an improvement on that which I first tried, which was merely to take prints that happened to be nearly of the same size, to adjust them under fiducial marks scratched on glass, and then to press down upon them a hinged flap, which carried two points that pricked two "register" marks in the margin of the print. The prints were successively suspended on two pins driven into the wall opposite the camera, the pins being passed through the register holes.

What I should do now would be to deal chiefly with group portraits. I formerly disparaged them for the purpose of composite portraiture as being too small and ill-defined, but they are now so frequently made on a large scale, and with good definition, and they form such useful collections of persons of the same family, profession, or race, that I should be inclined, when I have next to occupy myself with composites, to make much use of them, and to make the composites of the same size as they. Having selected portraits differing little in size, I should cut them out and paste them severally on cards, I should carefully measure the distance in each from eye line to lip line, under a lens in good light, and I should write the measurement on the card. I should also carefully estimate and write down the proper number of units of exposure, having regard to the vigour of the portrait. Then I should adjust and attach the cards to similar frames, guided by fixed fiducial lines, regarding only the upper horizontal line in fig. 1, with its short cross marks, and its intersection, the vertical line, and disregarding the mouth line. Lastly, I should prop these frames in succession in front of the camera. The points to be attended to would be, first, that the frames should be accurately propped. This would best be done by two notches, like inverted V's (Λ——Λ) cut in their lower edge, each notch straddling over a stout round peg firmly fixed at right angles to the wall. The adjustment to scale would be greatly facilitated by making the composites of the same average size as the prints, because in that case, as already remarked, a slight screwing to and fro of the lens will change the size of the image without sensibly affecting its definition. I should then carefully graduate by trial the head of the focussing screw in such a way that I need only turn it until the figure that came opposite to a fixed index was the same as that of the measurement written on the card, to ensure that the image should be correct to scale. As the point of intersection of the horizontal and vertical fiducial lines would lie in the optical axis of the camera, the image would always fall into its right place. I should use a common lens for the camera, one that did not define too sharply; but I should be very particular about the goodness of its mounting and focussing screw. These hints will suffice; the details must be filled in by the reader. The fault I find with my present camera is loss of light, due to the reflection of the image upwards from an enclosed mirror, and to the necessity of viewing it through a piece of (thin plane) glass inclined at 45°, the upper surface of which reflects the illuminated fiducial lines scratched on a blackened plate that is mounted at its side with a light behind it. I also think that my camera is too much of a jack-of-all-trades, and that I should get on much better if the portraits were successively prepared at leisure, making the actual photography of them a quick and simple process. In the plan I have just mentioned, all the preparations would be gone through in good light, and without any hurry. Then the photography would be swift, and it would become feasible to make many trials, leaving out one or other of the more doubtful portraits. As it is, I find the production of even a single composite to be an anxious and fatiguing work, and if any part of the complicated process goes wrong, all has to be repeated. There is no reason why this anxiety and fatigue should not be avoided.

There is nothing respecting composites that I should more gladly hail than the invention of a simple optical method of combining many images into one, so as to judge of the effect of a photographic composite before making it. Nothing can be better for optically combining two portraits than the prism of doubly refracting spar that I have used; but I cannot make a satisfactory and simple combination of as many as six or even of four pictures. I have described most of the plans that have occurred to me, but they all fail in some respect. The last I tried was a mosaic of pieces cut in the form of equilateral triangles, vertex outwards, from the rim of a large lens, and turned and brought close together with their vertices inwards. I then viewed the properly adjusted pictures through a small fixed telescope, in front of whose object-glass the mosaic was fixed. The method fails because the outer edges of the pictures are less bright than the inner ones; consequently the images are not equally mixed up.

In conclusion, I can only express a wish that photographers will try to make ethnological or family composites. I have been much pleased to find that both Dr. Billings, Surgeon-General of the War Department of the United States, and Mr. Thompson, lately attached to the Medical Department of the University of Edinburgh, and now to that of Cambridge, have used the method (as I also did, to illustrate a paper read at the York meeting of the British Association) to elicit the typical characteristics of the skulls of different races. It is, however, in ethnological inquiries, and in studies of hereditary and family characteristics, that I think the process would be most generally interesting.

It must be borne in mind by those who attempt to practise it, that off-hand methods will not avail. The adjustments must be made with judgment and extreme care to produce good effects. The difference between a very carefully-made composite, and one that has been combined with only moderate care, is great.

Explanation of Plate.

In the plate published this week the composites are on the left, their respective components on the right. E is the composite of the five portraits marked with small *e*; opposite it, and similarly, F is the composite of the *f's* opposite it. G is a co-composite of E and F reversed in position, and thus represents all the ten components on the right. H is a composite of five other older faces, which are omitted for want of room.

POSITIVES ON GELATINO-CHLORIDE OF SILVER.

BY W. M. ASHMAN AND R. OFFORD.

Third Article.

In the previous article, we directed attention mainly towards a clear understanding of the simple acid chloride emulsion, and its uses for certain purposes.

Those who found an opportunity to follow us by actual experiment, have doubtless felt ere this that their exertions were rewarded; we should be glad to know that such was the case, since it would bear independent testimony as to the value of our remarks, besides inciting others to test some of the capabilities of the process.

Upon the present occasion, we intend showing how a compound emulsion may be successfully prepared from materials to be found in every laboratory.

After numerous experiments with the various salts enumerated in our last article, together with others not included in that series, we have decided in favour of two compound emulsions as being satisfactory. In each case the proportions of three-fourths of a chloride emulsion to one-fourth of an organic or other emulsion, was found to yield the best result.

Mixed emulsions being always slightly washed, no very particular care need be exercised with regard to an excess of haloid, the washing waters readily adjusting the balance. As before suggested for the plain chloride emulsion, we prefer employing two chloride salts, and thus avoid crystallization or deliquescence in the finished emulsion, as instanced by the following formula:—

Gelatine	=	7 grms.	diss.	in 70	c. c. of	dist. water.
Ammo chloride	124	,,	}	20	,,	,,
Potass. ,,	88	,,	}			
Silver nitrate	6	,,		20	,,	,,

Here we have a combination by means of which the two extremes are avoided. We prefer making the silver decidedly acid, before mixing with the chlorized gelatine, and for this purpose make an addition of about ten per cent. of citric acid. The solution should be effected, and the temperature slightly raised in each case before mixing takes place.

If the quantities above given be employed, the colour-test before mentioned will not indicate the presence of uncombined silver. To mix with the above, we take either a citrate or an oxalate emulsion; the former produces a film readily toned in the ordinary gold toning bath, but possessing indifferent keeping qualities; the latter necessitates a special gold bath of the thio-cyanate form, and has the property of retaining its whiteness for a long period. Perhaps we should rather say that it is quite possible to tone an oxalate prepared emulsion film with the ordinary borax bath, provided it be treated either before or after toning with a dilute solution of ammonium thio-cyanate.

In the first instance the following formula will form a proper proportion to blend with the above:—

Gelatine	2·	gram.	dis.	in 30	c.c.s	of dist.	water
Amm. cit.	1·8	,,	,,	6	,,	,,	,,
Silver nitrate	2	,,	,,	} 10	,,	,,	,,
Acid cit.	·3	,,	,,	}			

The presence of citric acid helps to prevent the formation of a compound of gelatine and silver; it also hinders coagulation of the gelatine by the ammonium salt—an event which always takes place sooner or later with gelatine when citrate and some other salts are present, unless there has been so much washing that the resulting pictures are poor and thin. The colour test applied to this formula will be somewhat disappointing, notwithstanding the excess of silver salt used; nor is it really possible, when dealing with such organic compounds, to define the chemical changes that take place. On the application of potassium bichromate solution to a drop of this emulsion, no change may be apparent for a few seconds, but after a minute or so the red chromate of silver almost invariably appears. The argentuous citrate is a soluble salt, but, as stated by both Roscoe and Fownes, the argentic citrate is insoluble. Besides this, there are most probably two or three salts, acid and neutral, and we are inclined to think that it is either the accidental formation of an argentous or a neutral argentic soluble salt that the colour test really indicates. This most probably only takes place upon the introduction of the chromic acid, because the addition of a chloride salt to the emulsion will not prevent the exhibition of free silver upon testing. Another argument in favour of this theory is found in the suggestion of a chemical authority to use argentous citrate as a means of getting argentous chloride by the addition of sodium-chloride. Any attempt to meet the case by successive additions of ammonium citrate will end in the apparent increase of free silver, and the ultimate separation of the gelatine from the precipitate and coagulation thereof.

Thus it appears evident that the citrate group are somewhat unstable in their behaviour when in combination with silver and its haloids, bringing about a train of complications which can only be unravelled by further experiment when the precise reactions may be chemically defined.

Having the mixed chloride and the citrate emulsions prepared, a mixture may be effected by adding one part of the latter to three parts of the former, and melting—if solidified—at as low a temperature as practicable. When the citrate emulsion is thoroughly incorporated with the chloride they should be rapidly set, thus reducing the evil effects of temperature to a minimum. Although three parts of chloride to one of citrate emulsion has been mentioned, the proportions must always remain a matter of individual taste, since printing colour is thereby affected; thus, the larger the proportion of citrate employed, the redder or browner will the printing colour be; and the less the proportion of citrate used, so will the colour tend towards the purples. When four-fifths of a chloride emulsion to one of citrate is employed, a rich violet printing colour is obtained.

After an interval has elapsed sufficiently long to ensure a firm jelly, the compound emulsion may be broken up by squeezing it through coarse wire mesh or mosquito netting into a vessel of cold water, where the thready emulsion may remain five minutes; the whole should then be thrown on a filter or fine hair sieve, and a second washing in clean water be given, the duration of each washing being five minutes only. As a general rule, the more the washing is prolonged, the better the emulsion will keep, but the resulting image will be less vigorous; for this reason we would rather impress upon those who prefer this formula, to confine each successive washing to the length of time mentioned.

It is necessary, as in all washed emulsions, to extract some of the superfluous water, otherwise it would be found to lack body in coating; and half an hour's draining on a filter is sufficient to effect the purpose. The usual methods adopted for melting, filtering, coating, and drying, follow; the details of which are too familiar to require repetition.

A suitable oxalate emulsion to mix with the chloride spoken of at the commencement of this article may be formed as follows:—

Gelatine	2 grammes	in 30 c.c. of distilled water
Potassium oxalate	1 ,,	6 ,, ,,
Silver nitrate	2 ,,	10 ,, ,,

Dissolve separately, and mix, then add to the chloride emulsion in the same proportion, and in a similar manner to that recommended in the case of the citrate. The method of washing differs in no way from that already mentioned; but previous to melting, half a gramme of citric acid dissolved in ten c.c. of water should be added.

The compound emulsion with potassium oxalate is both white and slow, and is especially useful in making transparencies for the lantern, enlarging, &c. Almost any colour may be obtained, from warm brown to black, provided sufficient toning be given. This operation, as previously stated, is not a rapid one; still, it can be somewhat accelerated by the use of two baths, the first an ordinary borax, such as given in the Photographic News Formulary, and the second either before or after fixing the thiocyanate toning bath given in page 43 of the current Year-Book.

An especial feature in a compound emulsion is the facility afforded for printing negatives of a black-and-white character, for, as a rule, very much more harmonious results will be obtained by this means than in any other way. And it is remarkable to how far this effect may be carried in practice by a slight modification of the formula. Thus, by increasing the amount of haloid, and decreasing the proportion of acid, we obtain harmony; and by decreasing the haloid and increasing the acid or organic compound, we obtain brilliancy—nay, hardness; in a word, a reliable formula is a necessity; but having such, judicious modifications are allowable.

THAT DREADFUL FOCUSSING CLOTH!

(A Moan of One who has Suffered).

I cannot understand why the ingenuity of photographic apparatus makers has not yet devised an efficient substitute for the focussing cloth. There is nothing more harrassing in the whole range of a photographic outfit.

Take it in its out-of-door aspect. If there is the least wind blowing, is it not the most tiresome thing to manage possible? Hasn't it got the habit of falling over the focussing screen, after you have put the latter on the ground, and of hiding it, and you only find out what you've done when you've put your foot down and you hear a smash? Have a camera with a folding screen, you say. Yes, that's all very well, but suppose you haven't got one? Besides, allow that you *have* a camera with a screen of this kind; hasn't the focussing cloth been known to catch in some part of the camera, and when you drag it off you throw the whole arrangement out of balance, and you have to begin again? Nay, is it altogether out of knowledge that an attachment of this kind has more than once upset the camera and legs altogether?

Take the appearance of the photographer out of doors, when his head is under the cloth. Doesn't it give a horrible uncanny look, which makes him at once the object of ridicule to the unthinking and unscientific masses? When his head is thus concealed, has not the ubiquitous boy a desire to meddle with his dark slide? Hasn't he been represented in comic journals times out of number, assaulted in the rear by innumerable bulls? Not that I ever heard of a photographer in real life so troubled; but it shows the weakness of the focussing cloth when it is thus pitched upon as the mark for the caricaturist.

Out of doors bad, indoors the focussing cloth is worse. It ruffles the photographer's hair, and towzles his beard. It forces him to wear skull, smoking, and other unhealthy head-coverings which bring on premature baldness, and impart a flashy aspect to his bearing which does not properly belong to him.

If the weather is hot, putting one's head under the focussing cloth is an abomination and a terror. If the focussing cloth be too small, words cannot picture the irritation caused by straining one's eyes, and the slipping of the tormenting piece of stuff at the very moment when you think you have focussed correctly, but are not quite sure. If it be too large, the perspiration pours down the face, and you emerge hot, flushed, and fatigued from the trial of supporting its weight.

Let the focussing cloth be mislaid, and you are crushed. In despair, you seize the first thing that comes to hand. You have a rush of sitters, and they must not be kept waiting while you hunt for the missing article. You rush wildly at the camera with some black calico which you discover thrust behind the fixed background. It is as thin as gauze, and on putting your head underneath, you find the daylight streaming through the interstices. Another dart at the store behind the background. Ah! this is much thicker. You use it, focus, and re-appear with your eyes, mouth, and nose, filled with dust. The sitter smiles, and you don't know why. You accidentally catch sight of your reflection in the mirror, and then you discover. The black calico has been used in some remote period to stop out light in the studio roof, and your perspiring face is well peppered with smuts. Eventually you find that the friend of the sitter has been sitting on the missing focussing cloth all the time.

It is very easy to say that one ought to have a studio dark at one end, and move about one's screens to shut off the light, but what is a man to do if his studio refuses to be managed that way? Besides, why shouldn't we do away with the focussing cloth? Has anybody any vested interest in a focussing-cloth? Has a focussing-cloth any interest in itself? Why then should it be held sacred?

I read some time ago in an American paper, this:—"Our Chicago amateurs are trying to do away with focussing-cloths." Now, what I want to know is, *have* they done away with them? If not, why should not our English amateurs try their hands and abolish the hateful thing?

In this hope I live.

FRENCH CORRESPONDENCE.

Gelatino-Chloride of Silver Paper—Photo-Tracing Process — Phototypes — Gelatine Plate-Making Machine.

Morgan's Gelatino-Chloride of Silver Paper.—The process of obtaining positive prints by developing takes a long time in becoming popular. Photographers are slow in adopting new methods requiring learning on their part. The firm of Morgan and Co. have, however, facilitated matters by offering an excellent gelatino-chloride of silver paper. I have been trying it, and with complete success, not without hesitating the first three or four times as to either the length of exposure, or the right time at which to stop developing. But the thing is so speedily accomplished; and what great advantages such a process offers by allowing of printing at any time, day or night, under absolutely regular conditions! It is easy to obtain a light of practically equal luminosity after making a trial beforehand to find out the requisite length of exposure, and placing the paper always at the same distance away. It is better to under-expose rather than over, as in the latter case the prints are not brilliant. The same developing bath, composed as follows, is sufficient for a good number of prints. These formulæ are given by Mr. Morgan himself:—

Developer.

No. 1.—Neutral oxalate of potash...	250	grammes
Bromide of ammonium ...	15	,,
Hot water	1,600	,,
No. 2.—Sulphate of iron	40	,,
Hot water	1,600	,,
Citric acid	15	,,

Filter, and mix in equal parts, adding No. 2 to No. 1.

No. 3.—Alum	125	grammes
Water	2500	,,

Toning Bath.

No. 4.—Acetate of soda	160	grammes
Chloride of lime	8	,,
Water	2,500	,,
No. 5.—Chloride of gold	1	gramme
Water	125	grammes

Fixing.

No. 6.—Hyposulphite of soda ...	150	grammes
Water	1,000	,,

Daylight may be used as well as any artificial light. About five centimetres of magnesium ribbon are sufficient to print from a normal negative. After exposure, the print ought to appear fainter rather than too deep in tone; in toning it comes out in very agreeable warm colouring. To preserve a brilliant surface, it is recommended, after the final washing, to press it against a plate coated with talc, and rub with blotting-paper to squeeze out the excess of moisture. When dry, a finely-glazed image is obtained without the application of anything else, as in the so-called enamelled prints. Thanks to Mr. Morgan, we are able to print quickly and with excellent results, with this gelatino-chloride paper, while, under ordinary circumstances, in this dull weather, we should have to spend a whole day to get a single print from the same negative. The one little remaining question is that of the cost of this paper; but no doubt, when in general use, it will be reduced to that of chloride of silver albumenised paper.

Photo-Tracing Process.—I have already stated that the process known as *photocalque* consists in directly tracing by hand with a pen over photographic prints without interposing tracing paper. However transparent the tracing paper, it always hides certain details in the shadows, and it is troublesome work in that case. This process is very useful at times, as the photograph can be entirely effaced, leaving only the tracing in absolutely black lines, which may be made thicker in the shadows, and a reduced negative taken from the tracing can be employed for typographic or other purposes. By this means one has not to contend with the inability of a draughtsman, as he has only to be supplied with a silver print on salted paper, fixed in hypo, but not toned. If it is the case of an autographic transfer, the print is sized on the under surface with a solution of tapioca in hot water, and the tracing is done in lithographic ink. No reducing is permissible in such a case; the transfer is direct, and of the same size as the original. If it is required to be reduced, the tracing is made in Indian ink. To cause the image to disappear the print has only to be immersed in a solution of—

Bichloride of copper	15	grammes
Water	100	,,

If after reproducing the tracing in the camera, it is desired to bring back the photograph after previously wetting, dip it in a bath of neutral oxalate of potash in saturated aqueous solution of sulphite of iron. About one part of iron to five or six of the potash will be required, the image reappears immediately, and after washing, it is finished. It may also be made to disappear in a saturated solution of bichloride of mercury in 20 c.c. of hydrochloric acid to 300 or 400 c.c. of water. In this case an eight per cent. solution of hyposulphite of soda will bring the image back. If it is desired to completely destroy it, one has only to use a three per cent. aqueous solution of cyanide of potassium; to one-twentieth of the bulk add enough iodine to colour it, and mix the whole together. The process by bichloride of mercury has been used in making the prints by which the image is made to appear by dipping in water as in the so-called "magic photography" when it is pressed between blotting-paper impregnated with hypo. I think in certain cases, to see the work better, it would be preferable to use ferro-prussiate prints, as on the blue image the black tracing would be more easily seen.

MM. Boussod, Valadon, and Co.'s Phototypes.—This firm is now practising on a large scale the preparation of phototype blocks. The coming Salon number of *L'Illustration* will be printed entirely from phototype negatives by the Manzi process. The same publishers have just issued a splendid album from Detaille's military drawings. Our friend Mr. Woodbury, to whom I showed these results, declared that as yet nothing had been produced in England to come up to them. I am directing my attention at present in the application of new methods of phototypography to ceramics, and my first trials have met with perfect success.

Gelatine Plate Machine.—My colleague, M. Stebbing, has just organised a gelatine plate making machine of the kind specified on page 199 of this year's Photographic News. It works capitally, and with such speed, that 600 plates of medium size can be easily prepared in an hour with perfect regularity; and with these plates I have obtained the finest results. The emulsion is free from all defects, and the apparatus works so regularly that the film is quite uniformly spread.

Leon Vidal.

Patent Intelligence.

Applications for Letters Patent.

4234. William Middlemiss, Holmefield Mill, Thornton Road, Bradford, Yorkshire, for "Improvements in photographic cameras."—7th April, 1885.

4288. Scotto Clark Nash, 33, Chancery Lane, London, for "Improvements in portable photographic cameras."—7th April, 1885.

4378. Edward Marlow and Henry Bishop, 4 and 5, Arcade Chambers, Corporation Street, Birmingham, for "Improvements in the construction of dark-room lamps or lanterns for photographers' use."—9th April, 1885.

4528. William Ford Stanley, 4 and 5, Great Turnstile, Holborn, Middlesex, for "Improvements in photographic cameras."—(*Complete specification*).—13th April, 1885.

4529. William Ford Stanley, 4 and 5, Great Turnstile, Holborn, Middlesex, for "Actinometer for photography."—13th April, 1885.

4531. William Low Sarjeant, 19, Enmore Park, South Norwood, Surrey, for "Spring shutter for camera."—13th April, 1885.

Patents Void through Non-payment of Duty

10. A. M. Clark (*Corbassiere*).—Photographic printing.

27. A. M. Khotinsky.—Lime light lamps.

Specification Published during the Week.

7201. Thomas Samuels, of Monken Hadley, in the County of Middlesex, Gentleman, for "An improvement in photographic cameras."—Dated 3rd May, 1884.

The patentee appears to claim the use of a double swing-front.

Patent Granted in America.

314,811. Benjamin J. Edwards, London, County of Middlesex, England. "Apparatus for coating photographic plates."—Filed October 22nd, 1884. (No model.) Patented in England June 5, 1884, No. 8643.

This apparatus is described and figured on page 541 of our volume for 1884.

Notes.

A supplement illustrative of the remarkable results obtained by Mr. Francis Galton's method of composite portraiture is given with the present issue of the News, and another illustration of the same subject will follow next week.

In Mr. Galton's article on page 243 will be found much detailed information; and it is to be hoped that opticians will endeavour to supply Mr. Galton with the instrument he requires—an apparatus for immediately presenting to the eye a composite of any reasonable number of components.

From an artistic point of view, much might be said, the composites recalling the ideal pictures of the great masters. Perhaps the most real difference between a photographic portrait and a good painting is that the former is merely an exact representation of one phase of the sitter's individuality, while the latter may be a composite of an indefinite number of phases.

A feeling of universal sorrow has been experienced in photographic circles in consequence of the sudden death of the Lord Mayor.

A chill, taken at the Brighton Review, developed into pleurisy, but on Wednesday he was so much better that he took part in some ceremonies connected with the Bluecoat School. He, however, did this against the doctor's advice; and was, no doubt, prompted by a kindly feeling, which made him feel that the boys would be disappointed at his absence.

In the photographic department of the Inventions Exhibition things seem rather more forward than in other departments. A good show may be expected.

The historical collection of photographic mementos which is to be exhibited under the auspices of the Photographic Society promises to be one of considerable interest, and the committee have been well supported by those who possess relics of the past having photographic interest. Still there are phases in photographic history not represented, and any of our readers who have old stock they may be willing to lend, would facilitate the work of the committee by immediately forwarding a list to Prof. W. F. Donkin, Honorary Secretary of the Photographic Society, 5A, Pall Mall East. We are informed that some of the early lenses would be very acceptable.

(Scene — *A Photographic Studio near St. Thomas's Hospital*). *Photographer*—"I can't take your portrait, sir, if you hold your head so low down. I just get the bald part, and the rims of your glasses." *Medical Student*—"Capital; just what I want. This portrait's for my father, and I want him to see that I'm working so hard it's affecting my health. *I've asked him for the cash to go out of town for a fortnight!*"

The prospects of a supply of platinum from New South Wales seem to be encouraging, this metal existing to a very notable extent in the sand of the sea shore near Richmond River; while a nugget weighing over half-an-ounce was recently found near Wiseman's Creek. Should the platinotype process of photographic printing become general, fresh sources of the metal will be needed.

One of the oddest suggestions made for a long time is that of Mr. T. Kay, of Stockport, who, the other day, in a paper read before the Manchester Literary and Philosophical Society, proposed to make sea water drinkable by treating it with citrate of silver! Mr. Kay gravely sets forth the chemical changes which would take place—namely, the displacement of the chlorides by combining with the silver, and the formation of the harmless citrates of sodium, potassium, magnesium, &c. He observes that sea water thus treated would be slightly aperient and diuretic if taken in large quantities, but would be suitable for moistening the parched throats of shipwrecked mariners. No doubt. But setting aside the cost, are we to imagine a shipwrecked crew rushing to the medicine chest for citrate of silver, before betaking themselves to the boats, and subsequently performing a delicate chemical experiment in the open sea? We have also doubts as to the usefulness of citrate of silver, supposing more be put in than the equivalent of chlorides. And with the constant rocking of the boat, how is the chloride of silver to settle, or is a filtering arrangement to be taken? The idea of drinking turbid chloride of silver is not pleasant. Mr. Kay's notion savours too much of the quasi-scientific romances of M. Jules Verne to be thoroughly acceptable.

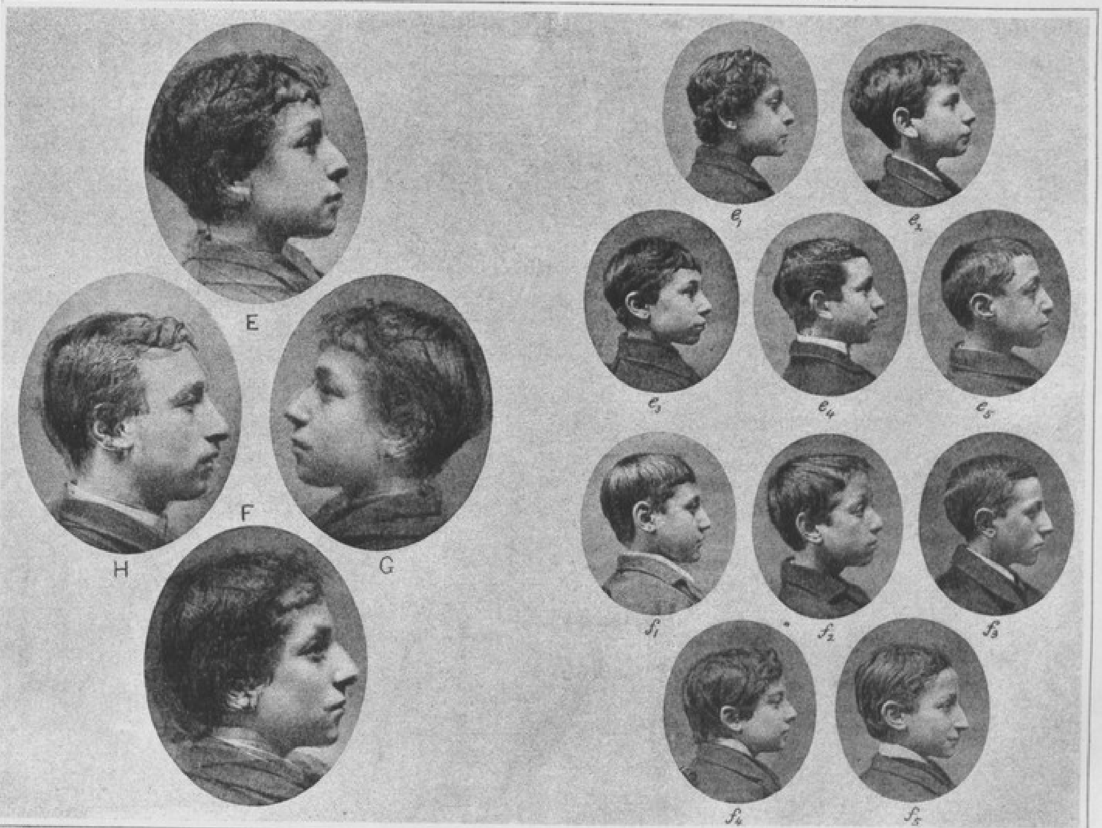
To make sea water potable by distillation is a tolerably easy process, and the ingenuity of shipwrecked sailors has now and then rendered it possible to carry on a rough process of distillation in an open boat at sea; the condensing arrangement being a piece of waterproof sail, folded so as to form a bag which can hold some pints of sea water.

The *Chicago Times* has a curious story of a painting o the "Deathbed of Lincoln," executed by an artist named Lichfield. President Lincoln, it will be remembered, was shot in the theatre. An army surgeon, Dr. C. S. Taft, happened to be present, and he it was who went to the assistance of the wounded man and remained with him until he died. When the picture was painted, Dr. Taft naturally supposed he would be represented, and so he was; but the picture now appears, not with the portrait of Taft as the medical attendant, but with the portrait of Surgeon-General Crane. No one would have been any the wiser, had not the picture been photographed when first painted. It seems that Surgeon-General Crane had the head of Taft scratched out and his own portrait inserted.

The Beauty Competition at Paris, which had to be decided by judging the photographs of the candidates, and not the candidates themselves, is over. The prize has been carried off by a young Parisian lady, twenty years old,

SUPPLEMENT TO "THE PHOTOGRAPHIC NEWS" 17TH APRIL, 1885.

THE JEWISH TYPE. *Profile.*



COMPOSITES. *Components.*

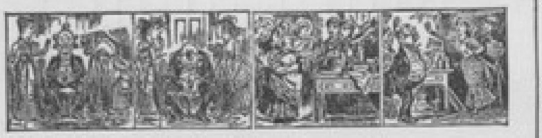
FRANCIS GALTON, F.R.S. PHOTO.

ILLUSTRATIONS OF COMPOSITE PORTRAITURE.

named Mathilde Corlin. Whether Madlle. Corlin is as beautiful as her photograph, and whether the judges are right in their opinion of her beauty, the public will have an opportunity of seeing, as the prize-winner has promised her assistance at a *soiree*, when copies of her portrait will be sold, and the proceeds given to the wounded of the French army in China.

The age of a lady is always a delicate subject. We admire the cautiousness of a San Francisco photographer, who labels two photographs of Madame Adelina Patti respectively thus: "Madame Patti at eighteen—" "Madame Patti at—present."

Funny Folks pictures the advent of the camera into the domestic circle. First we have the smiling father under the lens, but after having had an experience extending



over six hours, he smiles no more. The difficulty with the cook having been arranged amicably, and sole possession of the kitchen obtained, Pa and Ma step in and spoil all by opening the door.

Boxwood is becoming rapidly scarce, according to Mr. J. R. Jackson, of Kew Gardens; and notwithstanding the extent to which the various "process blocks" are used for journalistic illustration, the demand does not slacken.

Paper, as the basis on which by far the greater number of photographs are made, must always be a subject of study to the photographer, and it is interesting to note a batch of suggestions as to the progress in this branch of manufactures which are to be found in the current number of the *Journal of Chemical Industry*.

Herré adds soluble salts of zinc, calcium, or aluminium to the pulp, and afterwards mixes in soap, so that insoluble metallic soaps are precipitated; these serving to make the resulting paper both partially waterproof, and far more fire-resisting, than the ordinary article.

The treatment and manufacture of wood-pulp have been studied by Coethen, and some advantage appears to result from subjecting the wood which is to be pulped, to a preliminary baking in a hot air bath; this being more especially the case with coniferous woods rich in turpentine.

In working the sulphite process of bleaching wood pulp, Flodquist uses a steel boiler lined with lead; the lining being fixed by screws provided with lead-covered heads, so that no iron comes in contact with the contents of the boiler.

Dulfus makes an uninflammable pulp by incorporating eighty parts of asbestos, five of silicious earth, five of silicate of soda, and one and a-half of silk fibre. It is proposed to use this material not only for making paper, but also for insulating electric wires.

A photographer who wants gold for making chloride of gold, obtains exactly twenty shillings' worth by dissolving a new sovereign; but in doing this he puts the country to an expense of about three-halfpence—the actual cost of manufacturing the sovereign. Still, one would be a considerable loser by dissolving silver money, as the metallic value of an English silver coin is not equal to the nominal value of the piece. In America, this difference is so great that spurious half-dollars are manufactured; weight, style, and fineness of the false coin being equal to the genuine article, and the illicit manufacture yields a profit of about twenty-five per cent.

The dodges of the itinerant photographer are not to be despised, and we must confess we admired the dexterity of a couple of the fraternity who, a few evenings ago, we saw photographing a horse and its rider near Battersea Park. The horse is a somewhat difficult subject to take, and this particular quadruped was so restive that its rider was some time before he got him in front of the camera. But this done, all the rest was easy. While one operator focussed and made ready with the cap, the other, taking up the focussing screen, walked in front of the horse about ten feet away and tapped sharply on the glass; instantly the head was turned, the ears pricked up, and every muscle on the alert, and yet motionless for a sufficient time for the exposure to be made. Really a capital picture, considering the surroundings, was obtained. Horses are like children; they must be taken at once, or not at all.

Photographs of the old school may sometimes be improved by artistic retouching; it is the reverse with the paintings of the old masters. An odd story has just come to our knowledge, which, if its truthfulness were not vouched for by good authority, would scarcely be credible. A gentleman of means, by profession a painter of heraldry, had a fancy for collecting pictures, and, investing judiciously, got together a good number of valuable works, including a Gainsborough, several Morlands, a Cuyp, and a Rembrandt, though we have doubts as to the genuineness of the latter. Anyway, his gallery was valued at several thousands of pounds. On his death, some two years ago, the pictures were sold, and realised £200! The reason for this was, that they were put up for auction in a suburb of London, the chief auctioneers for works of art refusing to have anything to do with them because they had been "touched up" by the owner. The fact was that the latter had a fancy for "improving" the old masters, and especially their skies, and where he thought a picture might be benefited, he applied a dab of colour, no matter whether the artist was Gainsborough, Smith, Rembrandt or Jones.

Reviews.

Conference sur la Decoration Ceramique par Impression. Par Léon Vidal (*Paris: A. Quantin*).

We have here a work which, if not large, is of very exceptional interest, as it brings the whole question of decorating ceramic ware by means of photographic blocks well up to date. A photo-typographic block (Meisenbach) is given, after which are printed the various dissections of the same block, which will, when each is printed in its proper colour, give a polychrome image. We hope to present our readers with a translation of portions of the work before long.

A Practical Treatise on the Fabrication of Glue, Gelatine, &c. By F. Davidowsky. Demy octavo, 297 pages and thirty-five engravings in the text. (*London: Sampson Low, Marston and Co. Philadelphia: Baird and Co.*)

A handbook treating of gelatine and glue could not come at a more opportune time than the present, as gelatine has practically replaced collodion as a vehicle for the sensitive salts of silver in making negatives, and it threatens to oust albumen from its long held tenure in connection with positive prints.

The subject is introduced in some fifteen pages, after which about sixty pages are devoted to describing the routine work and ordinary fixtures of a glue factory. The finer gelatines are afterwards treated of, and numerous uses and applications of gelatine are considered. The directions for making gelatine foils and coloured veneers are specially interesting.

We cannot find space for extracts this week, but hope to give our readers a sample of the book shortly.

A Practical Guide to Photography. (*London: Marion and Co., 22 and 23, Soho Square.* 1885.)

This is a very clearly written and neatly got up handbook, and we have no doubt that any intelligent person might become a photographer by a careful study of it.

HARDWICH ON THE ETHOXO LIME-LIGHT; WITH A SAFETY JET TO PREVENT EXPLOSION.

BY T. FREDERICK HARDWICH.*

The "Ethoxo," or, as I prefer to term it, the "Oxy-ether light," is the invention of Mr. W. Broughton, of Manchester, but I do not think sufficient credit has been given him for it. Much fault has been found by critics who either have not tried the process, or have used it in a way contrary to the instructions. My own experience is that it is a very efficient substitute for the oxy-hydrogen, when coal-gas cannot be obtained, and that it is quite safe in the hands of experienced persons, with proper precautions.

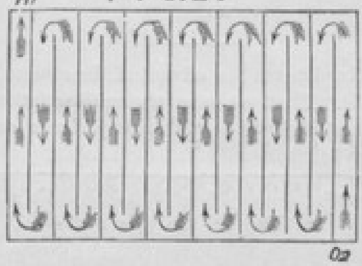
Presuming that the members of your Society are familiar with the general outlines of the process, I proceed to speak of what I have found to be the best form of tank for holding the ether. The oxygen gas in this form does not pass through the liquid, but over its surface; the advantage of which is, that there is less fear of ether being forced back into the bag, and also the light is steadier. In the old form of tank, when granules of pumice or other substances are used for safety, the flame at the jet mouth will be seen to rise and fall as each bubble of gas passes through the ether; but when the gas passes merely over its surface, the resistance offered by the granules is overcome. In the former case, also, the pressure of the gas is reduced at least one-third, whereas in the latter it not only suffers no reduction, but is sometimes even increased by the elastic force of the ether vapour.

An inspection of the following diagrams, which are drawn to a scale of one-fourth, will at once show the construction. Fig. 1 is a vertical, and Fig. 2 a horizontal section. The tank, 7½-inches long, is divided into two floors by a septum passing across, at a height of 1½ inches (inside measurement), the lower division being used for generating the vapour, and the upper forming two reserve chambers; so that, if any overflow of ether should accidentally take place, it would not be driven into the pipes. The oxygen enters at the point marked O_1 and O_2, and after saturating itself with ether by passing in the direction indi-

FIG. 1.

VERTICAL SECTION OF THE TANK.

FIG. 2.



HORIZONTAL SECTION OF THE TANK.

cated by the arrows in Fig. 2, escapes at H_2 and H_1. A T-piece, with tap, is screwed in at O_1, and a simple exit pipe, with tap, at H_2. There is also a cap screwing down upon a leather washer at A, to secure the orifice used for filling and emptying the tank.

The quantity of ether required will be about a pint; it should be poured into the tank until it reaches to a quarter of an inch, or a little more, from the horizontal septum. If it be then returned into a bottle, and a label placed on the bottle at the surface of the liquid, the exact measure required for the future will be indicated.

In making this tank, great care must be taken in the soldering, or a portion of the oxygen will travel by a shorter route than is intended, and there will be a danger of an explosive mixture being formed. The bottom must be put on last, resting on the edges of the vertical septa, but not attached to them, and notched here and there to allow the ether to flow underneath.

To test the tank, fill it with ether, and place it for two hours in melting ice. Now connect it with the safety jet of the lantern and pass oxygen through it. The flame should be long and blue slightly tipped with yellow. If, on the other hand, it burns with a small flame of a pale violet colour, and renders the lime incandescent without any further admixture of oxygen from the O tube of the jet, the tank is improperly made, and the oxygen not sufficiently saturated with ether vapour.

The Ether.—I find a great advantage from the employment of the lightest and best methylated ether. Being comparatively free from alcohol, and water, it can be used over and over again by simply filling up to the original bulk with fresh ether. It is sold in commerce as ether of ·720; but if my specific gravity bottle is reliable, it is very little more than ·710 at 60° Fahr. After having been once used, the specific gravity was ·715, and after three times ·725 at 60°. Unless a really light and good ether can be obtained, I do not advise that the ethoxo lime-light should be attempted. Store the ether in a cool, and especially in a dark place, as the action of light causes it to absorb oxygen, and become less volatile. See, also, that no flame is near at hand when you are pouring it out to fill the tank.

The Jet.—Any jet, of the oxyhydrogen kind, which answers for coal gas may be used with ether; but to obtain the best results it must be well made, as the ether vapour is more liable to what we term "roaring" or "hissing," than either coal gas or hydrogen. The orifice should be somewhat smaller than the $\frac{1}{20}$ inch usually recommended, or the flame will be liable to pass back in dissolving, when the bye-pass is low. One twenty-fifth will be sufficient, and I do not find the light sensibly lessened by reducing the orifice to this point. To secure as strong a pressure of gas as possible during emission, the bore should ex-

* Communication to the Newcastle-on-Tyne and Northern Counties Photographic Association.

pand from $\frac{1}{25}$ to $\frac{1}{16}$ inch after passing a tenth or an eighth of an inch inwards. I have known jets in which the narrow part of the bore was $\frac{1}{4}$ or $\frac{3}{8}$ inch long, but these jets do not burn as much gas nor give as much light, unless the bag is more heavily loaded.

With the most careful instructions for the manufacture, you will find that scarcely two jets will agree in the amount of pressure which can be put on without producing hissing. You must, therefore, blow through the jets beforehand with the breath, and pick out the noisy ones. On taking off the nipples, and looking through them against a strong light, you will, probably, find that with those which work silently the small bore of $\frac{1}{25}$ inch is exactly in the middle of the larger $\frac{1}{16}$ inch bore, whilst with the roaring nipples, it is one side. Failing this, all that you can do with the noisy ones will be to select a needle of the right size and polish the bore with it, after dipping it in a cream of oil and knife-polishing powder. The same may be done with good effect in the case of an old jet which has become corroded or choked up with lime dust.

The Granule Safety Chamber.—I do not consider that anything of this kind ought to be necessary; but as I usually lecture in crowded rooms and often to children, I have used the safety chambers since the Chadderton accident. They may be applied in the form of tubes attached to the taps of an ordinary jet, or the jet may be made purposely with the safety chamber in front, immediately below the nipple. The biunial which I show to-night has a safety jet in the top lantern packed with granules of binoxide of manganese, and safety tubes in the lower lantern with granules of pumice.

The safety chamber must be circular in form, $\frac{1}{2}$-inch wide and $\frac{1}{2}$-inch deep, interior measurement, with a disc of wire gauze of sixty meshes to the inch next to the granules, and a second of thirty meshes to strengthen it, both resting on the top of the chamber. These two disc must be five-eighths of an inch across, and be kept in position by a ring of wire fitting into a groove in the brass. Two other similar discs are to be pressed or screwed tightly upwards against the bottom of the chambers, but in such a way as to be easily removable for cleaning or renewal.

Below the chamber thus formed, there should be an empty space holding half a fluid drachm, to serve as a mixing chamber for the gases before they enter the granules. Above the chamber is no cavity of any kind, but the cap screws down nearly close, leaving only sufficient room for the gases to pass freely to the nipple.

The difficulty with this jet will be not so much in extinguishing the exploding gases, as in getting enough pressure with one hundredweight on the bag. Therefore no attempt must be made to economise space by contracting the diameter of the granule chamber at the top; the upper discs of wire gauze must be as large as the lower, and all the discs must be changed if they become rusty or choked up. I have seldom had occasion to change them, but a few minutes will suffice for the operation.

The safety tubes ought to be a little longer than the chamber in the safety jet, about three-quarters of an inch instead of half-an-inch, but they need not be quite so wide, say half-an-inch exterior measurement instead of interior, with discs of wire gauze, exactly as before, in the screw caps at the ends; the whole to be made air-tight by springing a piece of vulcanised rubber over the tube and shoulders of the caps, until it is flush with the milled heads at the ends, and then securing it by tying with red silk.

The Safety Granules.—I know of none superior to the pumice, originally proposed by Mr. Broughton, in its power of extinguishing flame; but it is inclined to be dusty, and is easily disintegrated by the action of the gases. When this happens, a flickering will be seen from small particles of pumice projected into the flame, and eventually the tubes, on being opened, will be found not to be quite full. The flickering is very slight after the tubes have been a little time in use, and some persons do not observe it; but if you wish to avoid it, granules of *black oxide of manganese* may be substituted. These granules are not entirely equal to the pumice in power of arresting flame, but they are strong enough for use in the jet or tubes now described. The hardest and purest variety of the mineral should be selected, and it may be crushed in a steel mortar. This operation is somewhat troublesome, but a little goes a long way and lasts a long time.

In reference to the size of the granules, wire gauze sieves of forty and fifty meshes to the inch are the best for screening;* all that passes through the former and rests upon the latter being retained. Granules screened by Nos. 50 and 60 are too small to allow of a free passage for the gases, and 30 and 40 are too large to stop the flame. I succeeded with this size when *crushed slate* was used, but on trying *pulverized glass* the flame passed easily through them.

The granules of binoxide of manganese are in appearance like coarse grains of gunpowder, and being very heavy they sink by their own weight and keep the chamber closed. In filling the chamber, it should be tapped gently, and the granules scraped to a level surface; but they must not be pressed tight together, or the flow of gas will be impeded.

As regards the relative merits of the safety-jet and safety-tubes, the jet is a neater and more compact arrangement, the granules being readily accessible, and well secured in position; it also has an advantage in the chamber being upright. On the other hand, the tubes are available for a jet already in use, and although the volume of the exploding gases is larger, the granules, being in a cool place outside the lantern, are better able to withstand the shock of an explosion. The upper jet in a biunial becomes strongly heated by the hot air from the lower lantern, and it is well known that heat facilitates the passage of flame through fine tubes and orifices. What you gain in one direction, therefore, by using the jet, you lose in another, and it is hard to say to which side the balance of advantages belongs; practically, either jet for tubes will answer the purpose, if kept in working order.

Supposing the safety-tubes to be used, with pumice, they must be opened from time to time to see that they are full; for, not being vertical in position, the granules, in wasting away, and sinking down, will eventually leave an empty space, along which the flame might travel.

The India-rubber Tubing.—I must repeat the caution given in a previous paper, not to use any but the best and thickest quality of tubing. The ether softens the thin kinds, and speedily makes them rotten.

Red rubber is very elastic, but I have found it sticky, and apt to cut with the string used in tying. The pure grey vulcanised rubber is far tougher, and will last a long time. Half-an-inch outside diameter, and a quarter of an inch in the bore, is a good size for carrying the ether vapour.

I object to putting the ether-tank on the floor, because it involves the use of a long tube between it and the lantern; and this tube absorbs a quantity of ether, as you may prove by drawing air through it when the lecture is over. With the tank on the table close to the lantern, you have only a short piece of tubing, of nine inches or so, to look after, or two pieces if you use a biunial.

These pieces of rubber must be examined from time to time, because if they were to give way, there would be a rush of vapour, liable to ignite at the nearest flame, and to run down the tubing, burning fiercely. I consider this danger more real and obvious than that of ether passing back into the bag, or forming an explosive mixture in the tank, neither of which could easily happen with good ether and a tank properly constructed.

Setting up the Apparatus.—The lime cylinder ought, undoubtedly, to be a little further off from the tip of the jet with ether than with hydroden gas, because ether, being rich in carbon, deposits it on the lime if you bring it too near. A quarter of an inch from the orifice to the point where the burning gases impinge upon the face of the cylinder is an average distance; but all depends upon the angle of incidence of the flame. Mr. Lewis Wright mentions 35° as the nearest to a right angle obtainable without risk of throwing a shadow on the screen, and this agrees with my own experience; but if the angle is less than 35°, then the lime may be brought nearer. You can tell when it is right by looking at the lime itself; it should be worn away into a depression of an oval or elongated form; if *a small round hole* is drilled by the flame, then the cylinder is too near.

The weight on the oxygen-bag should be about a quarter more than you are in the habit of using with an ordinary jet. I seldom find it necessary to exceed a hundred-weight, unless with a very large bag and a biunial lantern, in which case a hundred-weight and a-quarter may be put on. Turn the taps in a regular order, beginning with the bag, and ending with the taps of the jet. In the old form of ether-tank, it was recommended to relieve the pressure of ether by opening the H tap of the tank first; but in the tank described in this paper, it is comparatively immaterial which tap you open first, provided that you open them both at the same time. You must not, however, open the oxygen-tap of the tank, and leave it for a long time with the

* Wire gauze of any size can be obtained of Messrs. Bedford & Steer, wire-workers, of Long Lane, Bermondsey, London.

H tap closed, or some diffusion of ether vapour backwards might gradually take place, in spite of the pressure from the bag. In the ordinary way, nothing of this kind happens, and I have often smelt the residue of oxygen in the bag at the close of the lecture, without detecting any trace of ether.

When the jets are lighted, put on as much pressure as you can without producing hissing. After a little while, when the lantern warms up to its work, you will be able to turn the taps a little more, and then they will need no further touching for at least an hour and a half, if the ether be light and good. Rotate the lime about once every ten minutes.

To preserve the granules from disintegration and dust, the following mode of putting out the light at the close of the lecture may be adopted. The H tap of the jet to be turned off slowly, and the oxygen from the O tap allowed to blow out the flame. If you turn off both taps at once a "snap" will pass back, and the inside of the jet will gradually be coated with fine pumice dust. Then the next time you light up you will see quite a little shower of scintillations in the flame; whereas when small explosions of that kind are prevented by proceeding in the way described, the surface of the brass in the safety-chamber remains bright and clean. My impression at first was that the oxygen left blowing in this way would make the lime cylinder crack and "fly," but I have not found such to be the case.

When you have completed your lecture, leave all the jet taps open, and draw air through them to suck out the ether vapour, which, if allowed to remain, would turn acid and corrode the metal.

My experience of the safety-jet exhibited this evening has been principally with ether vapour. I have, however, used pure hydrogen gas, carefully freed from air, with good effect. This pure hydrogen explodes more strongly, when mixed with oxygen, than that prepared in the common way by dilute sulphuric acid and iron turnings, but it is extinguished without any difficulty in the granule chamber of the safety-jet.

In regard to ether, the treatment to which I have subjected the jet may, I think, be considered exhaustive, for the chemicals were of the best quality, the gases mixed in various proportions, and the jet heated by a spirit lamp until the india-rubber washers showed signs of melting. The result, however, was invariably the same, a faint snap at the orifice, but the flame could not pass the granules to the chamber beneath. The jet would be more perfect if this mixing chamber beneath could be dispensed with, but I have not been able so far to do more than reduce it in size.

In the numerous experiments I have had occasion to make, one point has struck me forcibly, viz., the very moderate amount of pressure which suffices to keep even the most explosive gases burning quietly at the mouth of the jet without passing back. The exhibitor at the Chadderton Town Hall, although he allowed ether to enter his oxygen bag from a wrong construction of the tank, would, I believe, have escaped any accident if the bag or tubing had not been touched. Hence, whilst recommending a safety-jet, I recommend also that the ordinary precautions for keeping up the pressure should be observed. The whole of the gas used in maintaining the light in this process passes from the bag through a single tube, and it is therefore obvious that if this tube was stepped upon, the pressure would at once be taken off.

A Letter from A. S. Herschell, Esq., M.A., F.R.A.S.,

Professor of Physics and Experimental Philosophy in the Durham College of Physical Science.

Dear Sir,—I can now confidently vouch for your jet's perfect safety under all conditions of burning with explosive oxyhydrogen gas, as I have put it to as severe a test as can possibly be applied in its normal state, and it gives way to none of them.

Taking off the fine nozzle of the jet, I placed over the chamber instead of it, the tin lid of a round vesta match-box, which just fitted on the outside, so that it could slide up and down and cover the wire gauze exit of the chamber with a gas measure of variable volume between it and the escape orifice, which was a small hole pricked with an auger point in the middle of the match-box lid. A short riband of paper had to be gummed round the outside of the cylinder to make this tin cap fit it and slide on to it quite tightly. The greatest capacity of the cap above the wire gauze was about half a cubic inch (or a trifle less), and it could, when necessary, be pressed down close upon the chamber top.

In no position of this cap could I make the gas flash back, though in order to strengthen the violence of its explosion, I narrowed the touch-hole orifice by forcing a taper glass tube into it, with a very small opening at its point, and lit the gas at the large open end of the tube, so that its flame blew back into the cap. Even with this assistance, I could not get a violent enough explosion in the tin cap to throw it off its fitting on the chamber (although it was *just* knocked off once or twice), and on pressing the balloon nearly empty with the hand, I could feel the *puff* of the tin cap explosion quite strongly, making it probable that backward escape through the pumice was at least a considerable cause of the weakness of the explosions.

Feeling satisfied that half a cubic inch of gas on the front side of the wire gauze was insufficient to drive back the flame through the pumice, I then proceeded to use the body instead of the lid of the tin match-box. This held about one cubic inch, and could not be slid down to less than that, but was easily packed and tied down tightly to the top of the chamber. It had also a small pin-hole pricked through the lid. The "puff" of this, as before, did not injure or move the box, but the very first ignition passed through the pumice and inflamed the balloon, bursting it with a pistol sound.

Finding that the large box of one cubic inch always blew up the balloon, I next tried the effect of varying the rate of egress of the gas by holding the short india-rubber tube, leading from the balloon to the jet, between the finger and thumb, and pinching it. In this way, with the small $\frac{1}{16}$ inch cap, I was able to burn the explosive gas either at the mouth or inside the cap, on the surface of the wire gauze with a fizzing sound.

The tin cap being raised to $\frac{1}{4}$ inch above the chamber, and the chamber filled with copper filings instead of pumice, the simple snap-explosions were not effective in driving back the flame, but left it kindled on the gauze inside, making a rushing noise from escaping steam, and after a little while, as the copper became heated, firing through into the balloon.

I then took out the copper filings, and tried the same experiment with pumice sand in the chamber. Here neither "puff" nor "fizz" would take any effect, although the steam formed and rushed out copiously. On looking at the wire gauze after it was over, I found that it had fused into pinholes in places, and had melted itself up with the pumice, which actually formed a semi-vitrified cap of $\frac{3}{16}$ or $\frac{1}{16}$ inch deep on the top of the sand; and this sand came out whole, and was partly solid and consistent. Yet, in all this violent heating, the gas did not fire back through into the balloon, so that the pumice appears to be a safer material to use than the copper filings.

The tin-box experiments were preliminary to a concluding trial with the jet itself; and, therefore, putting in new pumice sand, I proceeded to raise the nozzle to a good height from the gauze by means of a thick leather washer round the screw. The "fizzing" and "snapping" phenomena could then be got at pleasure, in turns, by varying the rate of gas flow. But, as I expected, no continuation of this process would make the gas fire back into the balloon, and the gauze and sand, after a trial of some time, were quite unharmed.

Of course, with the nozzle screwed close down on to its thin washer, as in the jet you sent me, the security against ignition will be still greater, and I do not see how it could, by any possibility, be made in that state to burn injuriously. The pumice sand is evidently *sanspariel*—an intercepting material of first-rate excellence.

ON PLATINOTYPE.

BY J. S. POLLITT.*

The platinotype process, though of comparatively recent origin, is by no means a stranger amongst us, as it has been rather extensively practised in various parts of the country, and very successfully by some members of this Society. It is a process which, for many kinds of work, has much to recommend it, the results being characterised by a quiet beauty which invariably pleases an artistic taste; and its freedom from the meretricious glaze of albumenised paper affords a sensation of repose to the eyes; but the great value of the process is still further enhanced by the supposed permanent nature of the printed proofs.

It is now, I believe, a matter of history that the late Roger Fenton, who went out to the Crimea during the Russian war about 1854, and took a large series of fine photographs, which were afterwards exhibited in the Exchange of this city, gave up photography because, as he said, there was no future before him, so many of his photographs having faded. Such a complaint, however, cannot be made against platinotype, and the knowledge

* Abstract of a communication to the Manchester Photographic Society.

that in working the process we are obtaining results which, according to present experience, will be as perfect one hundred years hence as now, is not the least encouraging feature in the printing manipulations of this beautiful process. As regards the chemistry of platinotype, the subject is a somewhat complex one. The image, as the name implies, is formed of chloride of platinum reduced to the metallic state.

In sensitizing, the paper is brushed over by a mixture of chloride of platinum and sesquioxalate of iron, or, as it is more generally called, ferric-oxalate, this ferric-oxalate having the peculiar property of changing, by exposure to light, into ferrous-oxalate, and as ferrous-oxalate in combination with potassic oxalate (or the neutral oxalate of potash used in developing the prints) is a powerful reducer of chloride of platinum to the metallic state, it follows that, on the application of the plain hot solution of oxalate of potash, a combination is formed in the paper itself, which immediately develops the image by reducing the platinum to the metallic state. It will thus be seen that the faint impression which is distinctly *visible* before development is really formed by the darkening of the ferric-oxalate, used in combination with the platinum, by the action of light converting it into ferrous-oxalate, and that hitherto the chloride of platinum has taken no part whatever in forming the image; and further, that it is only when the hot solution of potassic oxalate is applied to the surface of the print, that the union of the two salts takes place and forms a reducing agent on the platinum, varying in intensity precisely in proportion to the different strengths of light passing through the negative, and representing the light and shade or gradation of tone in the picture.

It may be that the commercial paper supplied by the Platinotype Company is prepared in some way different from the above description—no doubt the large experience they have had in the working details of the process may have suggested many alterations and improvements—but, broadly speaking, the principles will be much the same.

Correspondence.

A NEW DANGER.

Sir,—The following will, I think, surprise your readers, and, at the same time, put them on their guard.

About a fortnight since, I received an application (addressed to 49, King William Street, E.C.) from a party in Northampton, wishing to become a canvasser, and naming someone as recommending him. As I do no club work, and employ no canvassers, I thought there must be some mistake; but, about a week later, another application came from another person residing in Northampton, on the same business, and my suspicions were aroused. Soon after this letter came a gentleman from Northampton, to enquire if I employed canvassers in Northampton, explaining that his niece had paid some 8s. as deposit on a photograph. I immediately put myself in telegraphic communication with the victim, and this resulted in my making tracks for Northampton. My first visit was to the police. The sergeant received me civilly, telling me that the party spoken about by the victim had recently been released from prison, having been convicted for embezzlement, and that he was powerless to act, as his superior was absent.

I made my way to an address of one of the candidates, who explained to me that he had been induced to subscribe some 20s. on account of two photographs, believing that they were to be done at my address, and while I was talking to this gentleman, who should pass the door but the canvasser in question. He was called in, and I taxed him with obtaining money under false pretences. He denied this, and when I asked him to accompany me to the police station, he refused; but, by dint of a *little gentle persuasion*, I got him there. The inspector had not returned, so the sergeant and I went in quest of him. On our getting back to the office, we found he had returned. I was met with a rather indifferent remark, "that it was no business of theirs." "What," I said, "not when a man is obtaining money under false pretences all over the town?" He advised me to take out a warrant, and that I could get one at the Town Hall at three o'clock—an hour and a-half to wait! I thought it better to put the matter in the hands of a solicitor, who will place the facts before the magistrate with the view of a police prosecution.

I was almost forgetting to say that the explanation given about my address being on the card was, that the printer had put it on without his (the canvasser's) knowledge or permission.—Yours, &c., A. L. Henderson.

49, King William Street, E.C., April 11th.

THE PRESERVATION OF MEMORIALS.

Dear Sir,—In reference to your note on the "Preservation of Memorials," in last week's News, allow me to state that two photographs were deposited in the cavity of the foundation stone of St. Catherine's Church, Pontypridd, some twenty years ago. The photographs—which represented the exterior and interior of the old room in which church services had been held—I prepared as follows:—Made reduced collodion transparencies, which I cemented with Canadian balsam in contact with opal glass. I see no reason to doubt the absolute permanence of the pictures, especially as they were sealed in a glass jar with the usual papers, coins, &c.; and I venture to think that if ever that jar is opened, those little photographs will be considered the most valuable and interesting of its contents.—Yours truly, Thos. Forrest.

Pontypridd, April 11th.

[Now that photographs in vitrified enamel may be so readily made, they should certainly be used in preference to any other kind of photograph.—Ed. P.N.]

INTERNATIONAL LANTERN SLIDE EXCHANGE.

Sir,—You will be interested in knowing that the Lantern Slide Exchange that I started in the autumn has been carried to completion, and that it has now ceased for the season. All the members have seen the slides contributed by others, and the only mishap has been the breakage of eight of the slides in my own set. On the whole, the matter has gone off in the most satisfactory manner, and has had the effect intended of giving each member the use of a large number of slides during the winter. Next winter matters will probably be managed in a slightly different manner, and instead of an "Ever-Circulation" of slides weekly, a list of members, with the slides they have, will be supplied, so that those in the Club can make direct borrowings and lendings.

My chief object in addressing you now is to call for members willing to take part in an "International" Exchange which I propose to arrange, in conjunction with Mr. Dresser and with Mr. Beach, the President of the Society of Amateur Photographers of New York. The object of the Exchange will be to place in the hands of members in England a number of lantern slides taken in different parts of the United States and other parts of America this year These slides will be so made up as to be readily taken apart, so that members may take copies of those they may take a fancy to by contract. This can be done by binding the two glasses together by thin metal strips.

The general conditions would be that each member should contribute ten approved slides either quarter-plate size, or the ordinary three-and-a-quarter square, and pay a season's subscription of 5s. to pay costs of working the Exchange; the ten slides to be sent to me by the first week in September next, with a description of the subject, which must be some view of general interest, either in England or the Continent. Suitable metal strips will be supplied to each member to bind his slides with. As soon as all are collected together, they will be sent in bulk to Mr. Beach, of New York, and will remain the property of the American section of the Exchange. In like manner Mr. Beach will ship the slides he gets in, corresponding in number with those I send to him, and upon arrival they will be divided into batches, and distributed in turn to each English member of the Exchange, who will keep them a certain time, and take copies if he feel so disposed.

This is the general idea, and I shall be glad to have the names of British photographers who will undertake to carry out the arrangement sketched out. We shall thus get modern views, and many of them at a very small outlay.—Yours truly, H. Smith.

Proceedings of Societies.

London and Provincial Photographic Association.

A meeting of this Society was held on Thursday, the 9th inst., Mr. A. Cowan in the chair.

A series of chloride transparencies from negatives made by the Chairman at the outdoor meeting of the Photographic Club, on Bank Holiday, was shown to illustrate the absolute safety of his new combined camera and changing-box when in the field.

The Chairman also exhibited Mr. Matthew Whiting's new lantern slide carrier. The chief characteristic was the attachment of two portable chambers, one on either side of the opening, capable of containing a number of slides retained in position by means of a spiral spring. Each time the shutter was actuated one slide was released from the feeder, and so travelled along the platform, and through the lantern. By means of a spring the slide was then thrown out of the square sufficiently to be underlapped by the succeeding slide, and from thence into the chamber provided for its receptacle. The Chairman illustrated the practical utility of the arrangement by rapidly passing about thirty slides from one chamber to the other.

The following question was then read:—"Why does the precipitate formed upon the addition of iodic acid to nitrate of silver dissolve in ammonia?"

A discussion ensued, but no direct evidence was brought forward bearing upon the question. The general opinion was, however, that as the majority of the silver salts are soluble in ammonia, it might fairly be expected that silver iodate was among them.

Mr. H. S. Starnes then spoke of certain plates giving green fog when developed with ammonia or carbonate of potash containing restraining bromide, but not so when the bromide was omitted from the latter; he used 3 drops of a 250-grain solution of the latter alkali, with 2 grains of pyro, and the exposure was only half that required with the ammonia developer.

The Chairman said that Mr. Newton did not advise the use of a restrainer, and his proportion of alkali was much greater—about 12½ grains of each carbonate per ounce of developer.

Several members said they were enabled to reduce their exposures materially by employing the carbonate of soda or potash developers.

Mr. C. Heinrick Trinks stated that he had received a communication from a friend on the Continent, who desired to know how far the splitting up of combination or doublet lenses might be carried to advantage, and which were the most suitable for the purpose.

The Chairman remarked that any of the rectilinear forms might be so treated.

Mr. Trinks then asked Mr. Debenham what the effect would be if the stop were not placed in the optical centre. From recent observations he believed he obtained better results in some cases by varying the distance of the stop.

Mr. Debenham did not consider it right to assume that because the position of the stop had been varied in the cases alluded to, it was not in the centre. There was only one correct place in a properly constructed lens.

Mr. Trinks noticed that when using a single lens it was necessary to put the stop nearer than when using one of the doublet form.

Mr. J. Barker: When only part of a doublet is used the focus is too long, and therefore the lens is slower in action.

Mr. A. Mackie's experience was, that the rectilinear form would not work with so large an aperture as the single. In the best lenses the place for the stop was found by practice, and not mathematical calculation.

Mr. A. L. Henderson: Are we to understand that the insertion of a stop lengthens the focus?

Mr. Debenham replied that in a properly-constructed lens the central rays would not be altered in any way. He then illustrated, by means of the black-board, the effect likely to be produced both by varying the position of the stop, and covering up the margin of the lens.

Glossop Dale Photographic Society.

The members of the above Society had their first "turn out" for the season 1885 on Good Friday, and the muster was the largest out-door meeting since the Society was established. The "meet" was at the Society's rooms at 10 o'clock. The morning was very fine, with nice diffused sunlight, though the distance was somewhat obscured by mist, which, however, soon disappeared, and for a short time the prospect seemed all that could be desired. Unfortunately, this state of things was of brief duration, for about 11.30 an exceedingly dense fog was seen to be approaching, and very shortly the whole neighbourhood was encircled with gloom, entirely frustrating any attempts at photography. After a while, however, the fog gradually disappeared, and advantage was taken to secure a few groups, &c. The remainder of the day was bright, with a slight mist, and some good work was accomplished, most of the plates exposed subsequently turning out very fair negatives.

The ordinary monthly meeting was held at the Society's Room on Tuesday, the 7th inst., the chair being occupied by Mr. J. Merry. The minutes of the last meeting were read and confirmed, and the Secretary then reported communications from the London Stereoscopic Company, Messrs. Fry, and the Woodbury Fund Committee.

Mr. Hardman exhibited a patent camera.

It was incidentally mentioned that one of the members had received two letters from a firm of solicitors, threatening him with proceedings for *indecent behaviour* in a churchyard, the alleged offence being that he had "carried a photographic apparatus into consecrated ground," and, horrible to relate, had actually taken a photograph of the exterior of the church! The announcement was received with roars of laughter. It must be understood that the gentleman in question was not acting (contemptuously or otherwise) in opposition to the expressed wishes of anyone in authority. On the contrary, he had the full permission of the vicar's churchwarden, who was then and there present.

Hyde Amateur Photographic Society.

This Society had its first ramble of the season on Good Friday, the route being through Bottoms Hall Wood and on to Mellor, through the Willows (Marple), and on to Compstall, where they had tea in the Gardens.

On Monday, the 6th inst., a few of the members availed themselves of a trip to Liverpool, and several really good pictures were taken of docks, shipping, and other views.

On the 1st inst. the Society held its meeting in the Mechanics Institution, there being a crowded room, Mr. F. W. Cheetham, President, in the chair.

After confirming the minutes of the previous meeting, and nominating several new members, the Chairman called upon Mr. John Pollitt to read a paper on "The Influence of Photography on Popular Taste and the Graphic Arts."

Mr. Cheetham here vacated the chair, and Mr. John H. Brooks was voted to preside. Whilst Mr. Cheetham and Dr. Sidebotham got ready for the lantern entertainment, the secretary, Mr. John Crowther, exhibited several prints from America, received from Mr. J. E. Dunmont, of Rochester, N.Y., amongst them being "Niagara Falls in Winter," "Harper's Ferry," showing three States, Maryland, Western Va., and Virginia, "By the Quiet River," "A Hudson River Steamer in Rapid Motion," "View of Steamboat Landing in Albany," the two latter views being taken from on board a rapidly moving steamer. Mr. Crowther had mounted these views. Mr. Pollitt exhibited many of the old style of photographs which were truly works of art. The views shown through the lantern were from various districts both in and around Hyde, also many views from Devonshire. The Solgraph Company, Guisborough, had forwarded a few slides and negatives of various subjects, which had been developed with zixtol, which were greatly admired, especially one of "A Mill House in Cleveland." One of Mr. McLean's lantern views was shown, and received its due share of praise. Mr. Cheetham presented a view from "Bettws-y-Coed, North Wales," and a copy was given to each ordinary and honorary member; the picture was taken by himself, and was beautifully got up and mounted. Other specimens were placed on the table from the following firms:—Messrs. Fry and Co., Mr. J. Martin, and others.

At the close of the entertainment a vote of thanks was passed to the exhibitors.

Edinburgh Photographic Society.

The sixth meeting of the current session was held in Queen Street Hall, on the evening of Wednesday, 1st inst., Mr. Norman Macbeth, R.S.A., in the chair.

After the minutes of last meeting had been read and approved, Messrs. Wm. Moir, Stuart Fowler, C. J. Burton, Henry Murray, Alexander Asher, and A. L. Henderson, were admitted ordinary members of the Society.

A "Conference on the Picturesque" then took place, when a number of works were submitted and fully criticised.

Newcastle-on-Tyne and Northern Counties Photographic Association.

The ordinary meeting was held in the committee room of the Literary and Philosophical Society's Institute, Newcastle, on Monday evening, 13th inst., Mr. Payne in the chair.

Mr. Herbert Spargo and Mr. John Jackson were nominated.

The Secretary reported that Prof. Herschel had very kindly presented to the Society a case for holding such lantern slides as the Association might from time to time accumulate, and he had also sent a few slides by way of making a commencement.

The members then adjourned to the lecture theatre of the Institute, where the Rev. T. F. Hardwich, M.A., read a short abstract of his paper on the "Ethoxo Light" (see page 259), and gave, in conjunction with Mr. Allison, a lantern demonstration; the slides consisted of a series of microscopical, followed by a collection of Egyptian, slides by the collodion and Woodburytype processes. The audience was very large and appreciative.

Lochee Amateur Photographic Club.

The annual meeting of this young but flourishing Club was held in the studio belonging to the Club on Tuesday, 7th April, Bailie Ogilvie in the chair.

The Treasurer's report showed a balance in favour of the Club.

The Secretary, in his report, congratulated the members upon the very successful year just passed, and attributed the enthusiasm and *esprit de corps* of the members to the fact of their having a studio of their own to meet and work in. The studio has now been in use for a year. It is fully equipped with a splendid studio camera (suitable for either cartes or cabinets), three backgrounds (plain, interior, and exterior), a very convenient dark-room, and other accessories. It is very largely used by the members, and has been the cause of great improvement in the work of the members generally.

The Club now numbers forty-one members, and several gentlemen were proposed for membership.

The following were elected office-bearers for the ensuing year:—

President—Bailie Ogilvie.
Vice-President—Mr. D. C. Watson.
Secretary—Mr. W. G. Weatherall.
Treasurer—Mr. D. Henderson.
Committee—Messrs. R. B. Keir, G. Bell, W. Ogilvie, jun.

Manchester Amateur Photographic Society.

The first meeting was held on Tuesday, the 14th inst., in the Technical School, the Rev. H. J. Palmer presiding; and there were present thirty-six gentlemen.

For some time the want of such a Society had been felt, and it transpiring as the result of an announcement in the Photographic News of the 13th ult., and other publications, that a fair number of gentlemen were willing to join, the meeting was called to constitute one. The meeting was of a purely business character, rules being considered and adopted, and the following Council elected, viz.:—

President—Rev. H. J. Palmer.
Vice-Presidents—Prof. Gamgee and Dr. Tatham.
Committee—Messrs. Bathe, Champ, Dawson, Duncan, Flowers, Harrison, Hay, Lane, Widdop, and Williamson.
Hon. Treasurer—Mr. J. G. Jones.
Hon. Librarian—Mr. R. Graham.
Hon. Secretary—Mr. W. Stanley, 21, Howard Street, Eccles New Road, Salford.

The Society will hold its meetings on the second Tuesday in each month throughout the year. The subscription is 5s. per annum.

Sheffield Photographic Society.

The above Society held their monthly meeting in the Masonic Hall on Tuesday, the 7th inst., Mr. W. B. Hatfield in the chair.

Mr. W. Askwith was elected a member.

A resolution was unanimously passed to resume monthly subject competitions for the next six months, commencing with "Interiors" for May, the subject for the remaining five months to be selected by the President.

A committee was elected to arrange and conduct Saturday afternoon excursions.

The members then rigged up the screen and lantern, as this night had been fixed to try the one they had just received from the Sciopticon Company; one hour on slides of members' own making, and the remainder on a number of Woodbury slides kindly lent by Mr. F. Barber. The lantern, after some little delay, worked very well, and a good disc was got with camphorated paraffin.

The subject of the May meeting will be judgment of competition pictures of "Interiors," and the work on our first trip of the season to Syam and Froggat, which takes place on Monday, the 20th inst., by coach from the Masonic Hall at 8.30.

Photographic Society of Ireland.

The annual lantern meeting open to members and their friends was held on Friday evening in the Lecture Theatre of the Royal College of Science, Dublin, when, notwithstanding the counter attractions connected with the Royal visit, there was a fairly good attendance.

The lanterns were manipulated by Messrs. T. A. Bewley and Woodworth, while the description of the slides was undertaken by Mr. Greenwood Pim. A new feature was introduced on this occasion, many of the transparencies being the work of members of the Society; whereas, on former occasions, all had been made professionally from members' negatives. The principal contributors were Dr. Scott (whose beautiful instantaneous views of the arrival of the Prince of Wales elicited hearty applause), Messrs. J. L. Robinson, Samuel Baker, Roberts, T. A. Mansfield, and Bewley.

Bristol and West of England Amateur Photographic Association.

The ordinary monthly meeting was held at the usual room at the Queen's Hotel, on Wednesday, 25th March, one of the Vice-Presidents, Colonel Playfair, in the chair.

The minutes of the previous meeting having been confirmed,

The Hon. Secretary brought before the meeting the matter of the Woodbury Fund, with reference to which a feeling was expressed by some of those present that it would interest many who admired Mr. Woodbury to know what misfortunes had followed his great successes, thereby rendering such an appeal necessary.

Mr. W. B. Wright, of Brislington, near Bristol, was elected an ordinary member of the Association.

A few formal matters having occupied the attention of the meeting, the Chairman called upon Mr. H. A. Hood Daniel to give a few of his experiences with the soda and potash developers.

Mr. Daniel said: Now, on former occasions, others and my-

Plates [illegible] 29/1/24 K P

THE PHOTOGRAPHIC NEWS.

Vol. XXIX. No. 1390.—*April* 24, 1885.

CONTENTS.

CAPTAIN ABNEY LECTURING AT THE SOCIETY OF ARTS.

It is only now and then that one sees the Lecture Room of the Society of Arts so full as it was on Monday last, when Captain Abney spoke of photography and the spectroscope.

"My text is the spectrum," said the lecturer, "and you see it on the focussing screen of this camera." The source of light was an electric arc sustained by the battery of accumulators belonging to the Institution, and Captain Abney made the whole arrangement of apparatus (arc, collimator, slit, two prisms, objective, and camera) clear to his audience, so that the text was well impressed upon them, after which he exposed a plate to the spectral image, and very successfully developed it upon the lecture table.

"Before telling you anything of the nature of the influence exerted by the spectrum upon the sensitive plate," resumed the lecturer, "let me tell you something about matter itself."

Captain Abney then gave an exposition of the atomic doctrine, and pointed out that although this doctrine is based entirely on indirect evidence, it is one well worthy of consideration and study. He told his audience that they must try to form a mental picture of matter, and he would try and assist them in doing so. First, as regards molecules. The molecules of which physicists suppose matter to be built up are similar, insomuch that they all occupy the same amount of space, but their weights differ considerably. A molecule may be—indeed, generally is—built up of several atoms; but the atom is the ultimate and undivisable constituent of matter. Sir William Thompson tells us something as to the probable size of molecules, but he does this with caution and reserve; his figures are "less in diameter than one two-hundred-and-fifty-thousandth of an inch, and greater in diameter than one twenty-five-thousandth of an inch." and we may accept this estimate without more reserve than is usual in matters of scientific speculation.

If we can imagine a globe of water the size of a foot-ball, magnified to the size of the earth, its molecular structure would become visible, and we should find—according to Sir William Thompson's view—the molecules to be larger than small shot, and smaller than cricket balls. One more aid to the imagination. Let us suppose matter to be magnified to the utmost limit which is possible by the use of our best microscopes, and the magnified image to be again magnified to the same extent, then would the molecular structure become visible.

Every atom is charged with energy, but it is doubtful whether we ought to call this energy electricity, although, as in the case of electricity, we may recognise two equal and opposite forms of the molecular force. These we may call plus and minus, or positive and negative variations of the atomic energy, and when chemical combination occurs, the liberated energy takes the forms of heat and light.

"See," said the lecturer, "I dust a little powdered antimony into this jar filled with chlorine gas: now note how energetic the combination is, the bright white light showing the degree of heat generated by the combination, and the intensity of the reaction. Next I will sprinkle some finely-divided silver into a similar jar of chlorine gas, and you will see that the reaction is not nearly so intense, only a dullish red heat being the result. In one case the penta-chloride of antimony is formed by the combination of the antimony and the chlorine; while in the other case, chloride of silver is formed by the combination of silver with the same halogen. The constituents of the former combination are held together with far more force than the constituents of the latter combination: the difficulty of separating the constituents of a compound being (with certain qualifications) proportionate to the energy manifested during their combination.

Still, one must not suppose that attraction is the sole force which governs the molecular or atomic constitution of matter, as in reality there are also repulsive forces, and the contrary forces are oftentimes to be regarded as contending for the mastery.

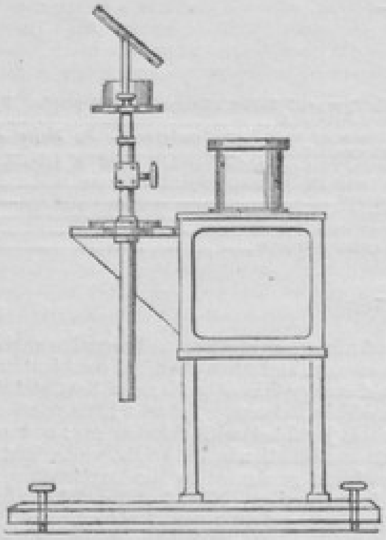
A striking experiment was shown to enable the audience to form some kind of a mental picture of this condition of things. In a glass cup placed on the stage of a vertical lantern, a number of small magnetic needles, each having a cork float at one end, were placed, and as the floating ends were all of similar polarity, they mutually repelled each other; but when a coil through which a strong electric current was passed was brought over the top of the glass, the floating poles were so strongly repelled towards the middle of the field as to be held in this position, in spite of these mutual repulsions.

By shifting the governing coil into positions where its action became less intense, the mutual repellant property of the similar poles became once more apparent, but was again overcome by including them in the intense magnetic field of the coil.

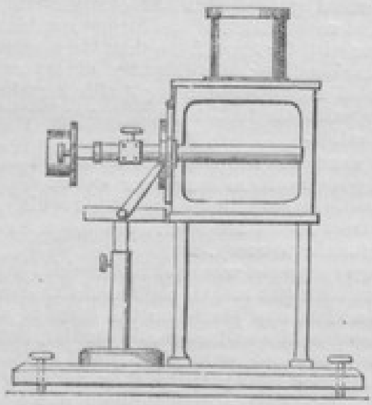
It should be mentioned that Captain Abney used a demonstrating lantern of the form designed by Professor Morton, and we need do no more than give the diagrams on the next page to enable our readers to understand how—easy it is to use the apparatus either for horizontal objects as, for example, the magnets floating in a glass of water—or for vertical subjects.

The audience were cautioned against taking a too realistic view of the possible movements of molecules or atoms, and to remember that in the case of the magnets used to illustrate the matter, the movement could only take place in one plane.

A spectrum was now projected on the screen, and its action on the plate was again referred to. It was explained how it may be expected that when any molecule is acted upon by a ray of light having a rate of vibration synchronous with its own, the extent of the swing is



increased; but a ray which is not synchronous with the vibration of the atom may be expected to diminish the range of the swing. This was illustrated by blowing



periodically upon a pendulum. It was next pointed out that it is quite conceivable that in some cases not only the extent of swing, but also the rate of swing, might be modified by the impact of light vibrations; and to enable those present to form some kind of a mental picture of the way in which this may happen, a pendulum consisting of a weight suspended by an elastic thread was shown; this vibrating more slowly as the swing becomes greater, owing to the stretching of the thread. So it may be that light rays may act on the atoms, even though the rate of swing may not be identical.

The action of light on the sensitive plate may be increased by assisting it with the motion of heat; the increased swing due to the action of heat is taken up and still more extended by the action of light, and to illustrate this a plate was warmed by placing a slightly heated flat iron outside the dark slide containing it; after which the plate was exposed to light and developed. The increased action on the heated part was shown by the appearance of the outline of the warm iron.

The use of the thermopile in searching out and mapping the distribution of heat in the spectrum was then illustrated and explained; and it was pointed out that only one-hundredth of the total radiant energy of the spectrum exercises any photographic action, except so far as the heat rays may help indirectly. Now it is easy to understand how it is that the eye of the camera does not see the colours of the spectrum just as the human eye sees them.

An interesting experiment was now made. The colours of the spectrum were first united by means of a lens, and it was shown that white light results. Afterwards, those rays which are of little or no photographic value were cut out, and the essentially "photographic light" was exhibited on the screen, when it appeared as a beautiful rich blue, with a tint recalling (but differing from) sea-green. Other experiments were made on the combination of colours, and the lecturer pointed out that an apparatus such as he was using could be used with advantage in experimenting on the best light for the dark-room.

The results obtained by varying the form of the slit-aperture were then exhibited, the much-overlapping spectrum obtained by the use of a mere round hole being first shown, then the results with zig-zag slits and ring slits.

The second lecture, which will take place next Monday evening, will deal with the diffraction spectrum, and the formation of the photographic image.

PLATINOTYPE PRINTING.

The paper by Mr. Richard Keene on the "Platinotype Process," which appeared in a recent issue of the Photographic News, will have been read with interest by many, as it describes the practical details of working the process on a considerable scale. It is probable that, when the platinotype process of printing becomes the property of photographers in general, and may be worked by them free of all restrictions, it will be one of the most popular of all printing processes.

Probably all photographers are sufficiently familiar with the platinotype process to know that in it the paper is coated with certain salts of platinum and of iron; that in the printing frame a faint image in the iron salt is obtained; and that subsequently, by a development which may be called one of "substitution," metallic platinum is reduced from the salt and is deposited on the paper in a very fine state of division, forming the image.

The greatest advantage of the process is that the results are regarded as permanent. This is a matter which is certainly not so much considered by photographers as it should be. This ought to be the first question asked of a printing process, "Are the results permanent or not?" We fear it is generally the last. Witness, for example, the manner in which the printing process which gives probably the most fleeting results of any—namely, that of silver printing on albumenized paper—has held its own against all others.

In talking of the permanency of prints, it should always be borne in mind that *permanency* does not mean *indestructibility*. We think that a print on paper might fairly be called permanent if it can be said of it that, subject to ordinary atmospheric conditions, the image will last as long as the paper on which it is supported. There can be little doubt that this can be said of the platinotype process, and likewise of the carbon process.

The appearance of a platinotype print must be known to all. The surface is without gloss, and the colour is either an engraving-black or an approach to sepia. Comparisons are proverbially odious, so we shall avoid comparing the appearance of platinotype and silver prints. The question as to which kind of print is the most pleasing must remain one of opinion. There can be no doubt, however, that the decision of artists of the brush is, as a rule, decidedly in favour of platinotype prints.

The facility of working the platinotype process as compared with any other printing process that we know of—except, perhaps, the "blue process" for copying drawings—is very considerable. After very little practice it is quite as easy to judge whether a print has had sufficient exposure in the frame as it is in the case of albumenized paper; whilst the time taken for printing is considerably less. This latter quality is, of course, specially advantageous in dull weather. The manipulations of the print after exposure in the frame are of the simplest nature. We have more than once taken several prints from one negative, and have had them finished and mounted within an hour of the time when the first piece of prepared paper was placed in the frame.

When the platinotype process was first laid before the public it was undoubtedly the case that the contrast obtainable in prints by it was very considerably less than that obtainable in those on albumenized paper. In fact, if the best results were to be obtained it was necessary to have specially prepared negatives which would have given hard silver prints. This drawback has been to a great extent removed of late. With batches of paper which we have recently had we have got prints giving very nearly as much contrast as could be got on albumenized paper. The contrast has certainly not been quite as great; but, on the other hand, the *gradation* appears to be truer.

The most troublesome part of the working of the process is to be found in the necessity which exists in keeping the paper absolutely dry up till the moment of development. This difficulty does not by any means counterbalance the saving of labour in other parts of the process, but it is one which must not be entirely lost sight of. The paper must be kept in a "chloride of calcium tube" both before and after printing, and during printing must have a sheet of rubber placed on the back of it. The rubber must be of very good quality, or it will soon become hard. It is therefore somewhat expensive. We may mention that in our own practice we use a very cheap substitute for the rubber sheets, and that it appears to answer admirably. We make use of the waterproof sheets intended to be placed between the leaves of the copying book in press copying of letters.

The expense of working a process ought within limits to be entirely disregarded by the photographer; that is to say, it should be considered a matter of no importance as compared with final results. It is, however, to be feared that it is considered a matter of very great importance indeed. It cannot of course be expected that prints in platinum should be obtained as cheaply as those in silver. The difference is, however, not very great, and it has lately been made somewhat less than it was by the fact that the sheets of platinotype paper are now turned out of dimensions which allow them to be cut up into the regular photographic sizes with much less waste than can the usual albumenized sheet.

THUMB-NAIL NOTES.

(Society of British Artists, Suffolk Street.)

Being a photographer, I naturally regard pictures through a photographic eye, and strolling the other day through the Suffolk Street Galleries, the first to open of the Annual Spring Exhibitions, I made a few notes from my own photographic point of view.

Suffolk Street is the home of the "Impressionist." The artist who endeavours to seize with his brush the scene as it strikes him, who is content with all the crudities which he may perpetrate, and who refuses to elaborate or embellish, is an "Impressionist," and to some extent he corresponds to the photographer who revels in instantaneous snap-shots. Of the two I prefer the photographer. Take Mr. Whistler's "bits" in Suffolk Street. There are some half a dozen of them, "a note in grey," "grey and brown," "green and silver," and so on. They are all vague, indistinct, and like nothing in nature.

Perhaps, considered as "arrangements" in colour, they may have merit, but as "drawings" they are beneath notice. What can a photographer learn from them? Absolutely nothing. The contrary may be said of the same artist's portrait of Signor Sarasate. This is masterly in pose and drawing, and the violin is a wonderful bit of colour. But why does a kind of smoke tinge the whole picture? It is called an "arrangement in black," but it is such an arrangement as every photographer is familiar with. In plain words, it is under-exposed, the development is forced, and fog has made its appearance. Mr. Whistler's followers are very particular in copying his favourite fog. Mr. Sidney Starr's "In the Lobby of a Theatre" is a capital example of this. An ugly young lady is sitting all alone, possibly waiting for some one, though there is nothing in the treatment, pose, or expression to show this. She wears a dingy dress, she is stuck against a sombre background, and this is all. Her face is seen through a veil of smoke, and, were the picture a negative, it would be put aside as a failure. Mr. Harper Penington gives us smoke of a different kind. It is cold and slaty in tone, and altogether cleaner than Mr. Whistler's variety, but it is still smoke. It is especially marked in a picture of what he calls "A Little White Girl." This picture is notable, by the way, for the extraordinary chair on which the child is seated. The back leg, though farther away from the eye than the front one, is much longer, and if really drawn from nature would tilt the seat downward three or four inches. Photography has an awkward habit of finding out faulty perspective, and had Mr. Penington photographed the chair he would have discovered this.

The same sad tone which appears as smoke in so many figure-subjects takes the aspect of sunlessness in landscapes. Walberswick, in Suffolk, has lately become the artist's happy hunting ground, and there are no less than seven views in the Suffolk Street Rooms, but nearly all show a grey, sad sky, and an absence of shadow. Is there less sun at Walberswick than anywhere else, and does this endear it to the "Impressionist" school?" I should have thought so had I not by a lucky accident seen, after my visit to Suffolk Street, a number of photographs of Walberswick taken by Mr. Valentine Blanchard. Here there was no lack of sun nor of picturesqueness. To the painter's eye, Walberswick consists of level, monotonous dykes, long stretches of muddy water, desolate patches of grass, and dreary skies. To the photographer, Walberswick abounds in sunlight effects, in quaint pictures of weather-beaten fishing boats full of suggestiveness, and in odd bits of river scenery, not flat and tame, but vivid with human interest. The pictures of the painter are fragments; the photographs are complete, and the eye is satisfied.

In Mr. T. H. Potter's "A Blue Jar," we get a very patent example of the modern school. The subject is a young lady with a muddy face and dark maroon dress, holding a piece of blue china. I have no doubt the sample of pottery is rare and valuable, but why should everything else in the picture be subservient to it? Why should the young lady's face be dirty—shadow does not express the peculiar tone—in order that the blue jar may be brilliant? Modern art is certainly eccentric, if nothing else.

There were many other pictures which present a mark for the foeman, but I pass them on. The predilection for smoke tints seems to be the most salient feature from my point of view, and I have confined myself to that alone.

Wide Angle.

GALTON'S COMPOSITE PORTRAITS.

BY W. E. DEBENHAM.

Mr. Galton's paper on "Composite Portraits," and the exceedingly interesting illustrations which accompany it, recall an experiment which I occasionally made many years since. At the time of which I speak, about five and-twenty years ago, stereoscopic pictures were much in vogue, and many photographers acquired the knack of combining the images without the use of any instrument. This was accomplished in two different ways: one by squinting, so that the right hand picture was sufficiently displaced in the image as perceived by the observer to the left, and the left-hand picture to the right, for the two to coincide. This method, however, was not a proper one, but gave rather what is called a pseudoscopic effect. The other, and better plan, was to preserve the axes of the eyes parallel, as if looking at a distant object, whilst the focus was brought to bear at the distance at which the slide was actually held.

The method of acquiring the ability to see stereo slides in this manner with a stereoscope is the following. Two pictures, which may either be identical, or as nearly so as the two halves of a stereoscopic picture, are mounted on separate cards, and cut so narrow as to allow them to be placed much nearer together than the two halves of a slide are generally mounted. These pictures are laid upon a plain surface where no disturbing bright object comes near them, and where they will be equally illuminated. A slip of blackened card is then placed in such a position as to resemble the partition in a stereoscope—that is to say, so that each eye sees only one picture. If the pictures are placed square to one another, and at an even height, and very near together, so that the same point in each is not more than an inch or an inch and a-half apart, the eyes will generally easily combine them into one, and they may then be gradually separated without their union being destroyed. When by practice they combine at a distance of from two and a-half to three inches, the ability to see a stereo slide stereoscopically without an instrument has been acquired.

As stereo slides were commonly mounted at a distance (between some fixed point in each) greater than that which exists between the two eyes, it was necessary that the ability to use the axes of the eyes rather divergent than parallel, whilst preserving the focus for near objects, should be gained. I therefore considered that it should be practicable, where objects to be combined might be viewed from a distance of a few feet, that such objects should be capable of being combined in the observer's brain when they are further apart from each other than three inches or so, which was their separation when viewed from a distance of a few inches only. Acting upon this idea, I placed two persons in a similar position, and succeeded in combining their features into what is now called a composite portrait. This I did on several occasions, and perhaps Mr. Galton may be interested in repeating the experiment.

As to a method of directly combining several images without necessarily photographing, there are two plans which I would suggest. One plan is to place a series of plates of flat polished glass (which should be as thin as possible, so as to reduce the effect of double reflection from the two surfaces to a minimum) at such an angle as each to reflect an image of one of the sub-

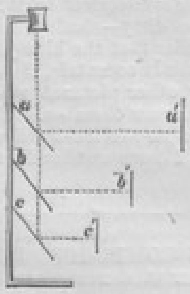


Fig. 1.

jects it is desired to compound. Let fig. 1 represent a stand with an eye-piece at the top, and glass plates, *a*, *b*, *c*, placed at the angles indicated. The cards, *a'*, *b'*, *c'*, will then be reflected in the glass plates, and if they are placed at such distances from the plates that the total distance—from the eye-piece to the plate, and plate to card—be the same, they will all be in the focus of the eye at the same time. One card may be laid on the base of the stand, and it will be seen through the glasses and unite with the others.

Another plan, and one which will serve both for viewing combined images and for photographing them, is to have two or more lenses mounted as close together as possible. The lenses should not be large in proportion to their focal length. An illustration of two lenses will suffice to show the effect, whether that number, or three or four, or even more, are employed. When studying or copying photographs is in question, it is obvious that the pictures (*a a*, fig. 2) should be placed at just a distance that their

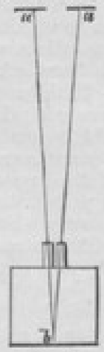


Fig. 2.

images shall fall upon the same point, *b*, of the plate. A final exact adjustment, close to the operator's hand, could be obtained by vertical and horizontal rack movements to the part of the camera front upon which one of the lenses is mounted. Several may be thus united simultaneously; but it would probably be convenient, in photographing a series, to keep the one which was used as the first always in place, opposite one lens, and arrange the others as they are placed in position opposite a second lens, so that the two images coincide as much as possible. In viewing the images of two individuals, the lenses might be slightly separated, if in the particular case the sitters would otherwise be inconveniently close.

More than two persons might be brought to the same place on the plate by the use of additional lenses, but the number would be more limited than in the case of combining photographs, as the latter can be arranged vertically as well as horizontally, an arrangement which would not be convenient with living subjects unless some had larger features than others, when they could stand behind at such a distance as to come to one size in the camera.

REMARKS ON PHOTOGRAPHING PAINTINGS, AND ISOCHROMATIC PHOTOGRAPHY.

BY W. M. ASHMAN.

PHOTOGRAPHING paintings is not by any means an easy operation, unless they are freshly painted. Even then, certain colours in the original do not render up their true value to the photographer.

There may have been two reasons at work in the mind of the Hon. Treasurer of the Parent Society when he determined on reading his recent paper at Pall Mall.

The first, we may assume, was to elucidate an interesting discussion which might enable experiments of recent date to be shown the light, and thus pave the way for the future enquiry suggested by the author of the paper; or, as an authority upon the subject, Mr. Bird might have felt that British photographers awaited his opinion concerning the action of the authorities who went out of their way so far as Messrs. Braun and Co.'s establishment at Dornach, in order to have the pictures in our National collection properly photographed. It is probable that both these reasons influenced the decision, for a discussion took place; albeit, no new points fell from the speakers who were engaged therein, and the author, in that portion of his paper which dealt with the cosmopolitan nature of art, gave certain reasons tending to justify the selection.

Those who had the opportunity of examining some of the examples with which the paper was illustrated, were by no means agreed as to how far the results were due to skilful retouching beyond the legitimate limit, and how far to chemical and other means; neither were they assisted in their deliberations by any information as to Messrs. Braun's *modus operandi*, beyond a claim by the said firm that their process of reproducing paintings was a secret one, and better than any one else's.

It is pretty generally understood by English copyists how to photograph a painting; some manipulators prefer to make the negative in direct light, at the same time carefully providing against all possible reflection from the surface of the painting, beyond that which is necessary to obtain an image; others give preference to an equally diffused light, such as may be obtained in the open air.

Sunlight, too, has its adherents. Then, again, we find that many successful results are obtained by the use of the reversing mirror. Whichever of these methods of illuminating be adopted, it is usual to make a preliminary negative somewhat smaller than the original, and for this purpose the gelatino-bromide process is, for several reasons, peculiarly suited. Putting aside for the moment the gain in rapidity, and the absence of many defects met with in the wet collodion process, we find that bromide of silver in gelatine tends to the direction of rendering detail not easily obtained with collodion containing a large proportion of one or more iodides; this result, added to the more harmonious rendering of the strong lights, enables the photographer to make a negative in which he has a truer representation of the painting, and one in which harshness, cracks, and fissures are less obtrusive. Upon a preliminary negative of this kind, a considerable amount of modification can be worked, both sides being utilizable for this purpose; here it is that the method mentioned by Mr. W. England, in the discussion referred to, proves of especial value, always provided the truth is not sacrificed.

Another power of no less magnitude is the skilful manipulation of a transparency from this dodged preliminary negative. Such a transparent positive may be either from collodion, gelatino-bromide, chloride, or carbon tissue, the two former processes allowing for enlargement; the latter not so. As a rule, the first or last named is generally preferred; in skilled hands such positives are amenable to an immense amount of alteration, and frequently of improvement, the final negative, which is usually by the wet collodion process, requiring nothing beyond the merest touches for the removal of ordinary defects. There are other methods of producing photographic copies of paintings, wherein the artist's brush is more or less brought into requisition. These methods are doubtless familiar to all practical photographers, and need not be dwelt upon now, since sufficient has already been mentioned to illustrate the value of mechanical contrivances artistically pursued.

That certain modifications of the sensitive film assists us towards successfully photographing colours, in accordance with their true value as regards luminosity, is a fact which has been proved beyond question. Sir J. Herschel's and Mr. R. Hunt's experiments in this direction were valuable, but they have only been considered practical since the researches of Major-General Waterhouse, Drs. Vogel, Lohse, Captain Abney, Mr. F. E. Ives, and others, have been recorded. It will be remembered that the particular influence exerted —and it is a marked one—in the recent experiments is effected by staining the sensitive film with a substance which exalts the sensitiveness of the rays of lower refrangibility, without increasing the blue and the violet materially, in some cases, and decreasing their energy in others. Working upon the same lines as the earlier experimenters—namely, photographing through coloured mediums—it is found that the refrangibility of blue and violet rays can be greatly lowered in this way without exercising any corresponding disadvantage upon the yellows, greens, and reds. Thus, if we photograph two bands of colours—say, one of them yellow, and the other blue—under ordinary circumstances, with wet collodion, we should obtain a dark band in the first instance, and a light band in the second; but if we had repeated the operation, having first taken the precaution to stain the film, and interpose a suitable medium between the object and the objective, a totally different result would have been obtained—the luminosity of the yellow would have asserted its supremacy. Captain Abney's "canary stained glass" forms a suitable medium for the purpose; but it must be borne in mind that the interposition of this medium necessitates a prolonged exposure.

Among the recent experiments published by Dr. Vogel, we find that in addition to interposing a medium similar to the above, he obtains the most decided advantages by incorporating eosine with his sensitive film; the results were exhibited before the German Societies, and much appreciated. The treatment of collodio-bromide plates with chlorophyll recommended for a similar purpose by Mr. F. E. Ives, an illustration of the capabilities of which is published in the current Year-Book, shows conclusively how much can be done by chemical means alone. Another familiar substance, recommended by Dr. Lohse, "Turmeric," has special qualities which recommend its employment, not the least advantage being that it may be used without an interposing medium or extra trouble beyond soaking a gelatino-bromide plate for the space of a few minutes in an alcoholic extract of turmeric root immediately before exposure. It does not appreciably increase the exposure, and much of the colour leaves the film during fixing. Doubtless many plants would yield alcoholic extracts presenting similar qualities if the subject were investigated in the manner its importance deserves.

I have before me two negatives representing twenty-six squares of coloured ribbons, mounted to form five distinct bands, containing in the aggregate all the colours ordinarily met with by the photographer. Thus, in the first band we start with pale buff, and terminate with a deep and rich orange; and in like manner bands containing varied shades of red, blue, and green are made up. These negatives, together with the tablet of mounted ribbons, was sent to me by Mr. Offord nearly a year ago; one of the negatives having been exposed on the tablet in the ordinary way, and the other had been previously soaked, as recommended by Dr. Lohse, in a solution of turmeric. The difference between these two negatives was so remarkable that I considered the matter of sufficient importance to repeat the experiment with the view to its mention at the photographic societies. My results being similar, led me to try other substances, among them being an alcoholic extract of the petals of the French marigold, which exercised a similar, though more marked influence, than the turmeric; but like Mr. F. E. Ives' chlorophyll, its value depends very much on its freshness.

Upon the occasion of mentioning these experiments at the August technical meeting of the Photographic Society of Great Britain, I advocated the formation of a committee whose duty it should be to make a series of comparative experiments with the substances already published, as well as with the isochromatic plates of foreign manufacture. So far as known to me, no such series of experiments have yet been officially carried out, therefore I heartily second the Hon. Treasurer's request that the Parent Society of photographers shall give this subject their attention.

TO THE CAPE.

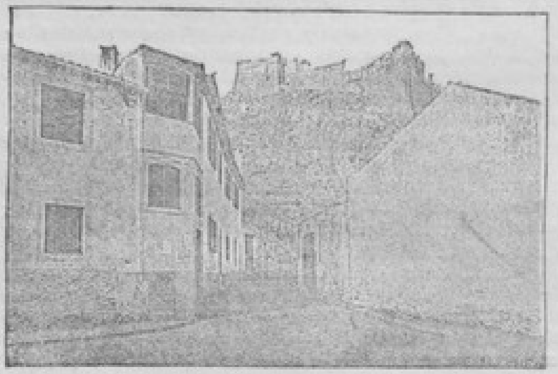
BY C. RAY WOODS.

Ding dong! "Any more for the shore?" Ting-a-ling is heard from the little bell that warns the engineers to "Stand by!" Splash, splash goes the screw as it is first put in motion, and the vessel is steaming slowly down the harbour. The rain is coming swiftly down, almost drenching us; the wind is blowing a bit, and the whole appearance of things bids in favour of dirty weather. And we get it.

As soon as the Needles were passed, the vessel began to roll, the roll steadily increased, and from the moment we left the shores of England to the moment Madeira appeared in sight, the ship rolled heavily, and the masts appeared to swing from side to side like stalks of corn in a fitful breeze. For six days only a mere handful of passengers took a walk on deck, and these few, mostly old sea-goers, were heartily glad when those six weary, monotonous days were over. The forward part of the vessel was one wet bath, and aft, on the quarter deck, the slippery timbers and the blinding spray rendered exercise a somewhat difficult thing to get, and only the exertion of keeping upright and accommodating the body to the motion of the vessel was available for keeping some of the muscles in order when one sat down. The sight of what seemed like a huge wall of water, the top of which was the horizon rising suddenly up before the eyes, remaining for a second suspended above, then lowered as rapidly as it rose quickly, made one dazed and giddy, so that, good sailor as I have found myself to be, I was never without a fear lest I might eventually succumb to that to me unknown, but to many less fortunate individuals familiar, sensation known as *mal-de-mer*. I like a little rough weather occasionally—but on this occasion the long monotony of it tired me out, and I shall not only be content with, but even grateful for, a little smoother water in any future journey I may have occasion to take. Only one incident of any moment occurred during the first week of our passage: a great wave sweeping over the fo'c'stle snapped the fastenings of a few of the articles on deck, and carried them away. A hencoop or two were seen, and a poor dog struggling bravely but vainly with the waves; but it was a yellow dog, and, being a yellow dog, it failed to strike a chord of pity in the hearts of the spectators.

What is there about a yellow dog that carries with it a touch of the ludicrous? Is it because yellow will not harmonize with, but contrasts, the *blues?* Is it because——

We only stopped for two hours at Madeira, but that two hours gave me sufficient time to take a few views. I got a tar from the ship to carry my camera, which I put in its



leather case, for the landing is bad at Funchal, and both passengers and their effects are apt to get a damping as the boat is run through the surf; slides might get the salt water into them if carried in a canvas satchel. Safe on shore, we walked through the town in the direction of an old fort, which stands on a little eminence commanding the harbour.

My first plate was exposed on the fort as it appeared between the houses of a short and narrow street; but not feeling satisfied that I had hit the right or a workable exposure, I exposed another plate on it from a slightly different point of view. Fifty yards farther on, and just outside the town, I came on a fairly attractive view from a bridge, under which ran a little stream which came down the valley in front of me. The road above the stream, the hills in the distance, and the vine-grower mending his trellis, made a view characteristic of Madeira; but the clothes drying by the side of the stream, though perhaps equally characteristic of the place, did not add to the picture. But it does not do to be too finicking when you have not much over an hour to spare, and so I took the view with its drawbacks as well.

Another view from a bridge, in the midst of the town, showing one of the churches and some of the houses built, as it were, on the river's embankment, then a view of a quiet street a little further on, and five plates have been exposed. I had one more left, but the time of the steamer's departure was approaching, and besides, two policemen were watching me with very suspicious eyes, and policemen may make themselves disagreeable when a man is taking anything. I made my way towards the quay, therefore, and just before going for the boat I made a busy street the subject of my last shot, and humoured the vanity of a few of my fellow-passengers by placing them in a conspicuous position. Thus I occupied the brief time I had at my disposal in Madeira, and if my results are only what are usually termed "shots," I must console myself with the reflection that that is all one usually gets during a brief stoppage of an hour or two in an unfamiliar place.

After leaving Madeira our passage was smooth enough, and it was quite refreshing to be able to take a meal without pouring your soup into your lap, without having to clutch your plate, knife, and fork with your hands, and the table with your elbows, and without being bombarded by saltcellars and other missiles which, scorning to be kept within bounds, bounced over the fiddles which ineffectually tried to keep the tables in something like order. And then, too, our fellow-passengers suddenly recovered their health and spirits, and friendship-making, quizzing, flirting, scandal-spreading, and all the other usual accompaniments of ship life asserted their sway. There was one passenger, a colonial genius, who tackled me on astronomy, and imparted to me an original and somewhat startling theory on comets.

"Look here," said he; "this is my theory. You know some scientific chaps; suppose that this earth of ours is like a bombshell, filled with melted stuff. Now, I say that these here comets arn't exactly like our earth, but only something like it. They're just filled up with water, and when they get near the sun the water boils, and out come jets of steam, and that's just what makes the tail."

"But some comets have more than one tail."

"Well, can't these shells have more than one hole for the steam to come out of? Hasn't this earth got more than one volcano?"

"Then some comets have curved tails."

"Perhaps that's because there's a wind blowing."

"But how do you account for the tails pointing away from the sun?"

"Look here! If these shells have blown out any steam, they can't be full now, can they? And the less water that's in them the nearer will the centre of gravity be towards the outside of the shell. Now the centre of gravity, which will be where the water is, will be nearest the sun, and therefore the steam will blow out of the other side."

"But what about the fact that the planets exercise so little disturbing influence on comets? How can that be if they are great globes of water?

"They needn't be *great* globes. A little water makes a lot of steam, doesn't it?"

"But what about the rapid motion of the tail as the comet sweeps round the sun, and how do you account for the ——

"Look here! I'm just going to ask you a question before you go on. If the tail isn't steam blowing out of something, what is it now, eh?"

I confessed that I did not know.

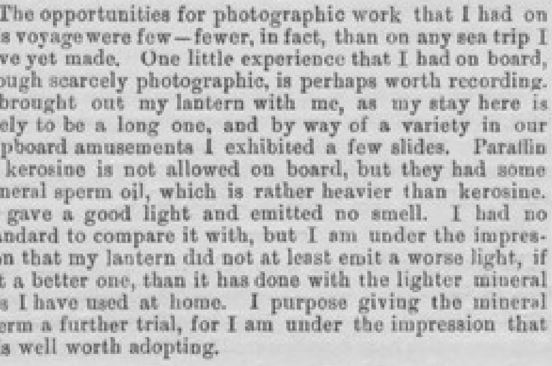
"That's just what I thought! You haven't got a theory! None of you have got a theory! Now I *have*, and when you fellows have got a better one, then it will be quite time enough for you to pooh-pooh mine!" and he walked away triumphantly.

On the 5th of February we passed the far-famed Peak of Teneriffe, and had the satisfaction of seeing it free from cloud, although there was a fair amount of haze about. I took a shot at it with a lens of about 13 inches focus, but the result was very poor, and not worth keeping. I felt no very great regret, for I could not go into enthusiasm over the Peak as so many people do.

Compared with most glimpses of land that one gets from the sea, it is tall and imposing, but then one always estimates a spectacle by comparing it with something he has seen before. Teneriffe, though high and steep, is not so steep nor so rugged as the mountains of Switzerland, and one sight of the Matterhorn is worth fifty of Teneriffe.

St. Helena was reached in the early morning of February 14th, and our stay there was again very short, as we had to make up for the time lost between Plymouth and Madeira. This time I did not take my camera on shore, and the one photographer there who sold views was able to show his customers little more than mere transcripts of the scenery, and no photographs that presented any attraction as specimens of the art. Of all places of desolation, St. Helena must carry off the palm.

The opportunities for photographic work that I had on this voyage were few—fewer, in fact, than on any sea trip I have yet made. One little experience that I had on board, though scarcely photographic, is perhaps worth recording. I brought out my lantern with me, as my stay here is likely to be a long one, and by way of a variety in our shipboard amusements I exhibited a few slides. Paraffin or kerosine is not allowed on board, but they had some mineral sperm oil, which is rather heavier than kerosine. It gave a good light and emitted no smell. I had no standard to compare it with, but I am under the impression that my lantern did not at least emit a worse light, if not a better one, than it has done with the lighter mineral oils I have used at home. I purpose giving the mineral sperm a further trial, for I am under the impression that it is well worth adopting.



We arrived safely at the Cape on the appointed day of arrival, February 19th, and since then I have been making rapid progress in preparing for the work. Preliminary experiments in photographing the stars have turned out better than might have been expected, and almost as well as one might wish.

A few days after my arrival, I ascended with some friends belonging to the Meteorological Department here, to the top of Table Mountain, where some thermometers and rain guages are placed and examined at certain intervals. I succeeded in obtaining photographs of their apparatus, one of which is given above.

CARBON PRINTING.

BY W. LANG, JUN.*

It seems to me that the three main considerations which stand in the way of the amateur adopting the carbon process are the following :—

1. The necessity of having reversed negatives where the single transfer method is made use of.

2. When the ordinary negative is employed, the troublesome operation of double transfer.

3. The fact that the exposure, as in silver printing, cannot be judged by seeing the actual progress of the picture. Perhaps I should add a

4th. The trouble of sensitizing the tissue.

If, however, we develop our carbon picture on a glass support and leave it there, we have the image in optical contact with the glass, and all that is needed is a backing of white paper to bring out the details of the picture. It is this method of finishing a carbon picture I bring before you to-night. It does away with the necessity of the reversed negative, and no transferring operations are required. The only precaution to be taken with the negative is to provide what is known in carbon printing as the "safe edge," which is easily formed by running a black margin all round the negative with black varnish. Instead of black varnish, strips of orange paper may be pasted down on the negative. These margins, as you will see from the specimens lying on the table, in no way interfere with the negative being used for ordinary silver printing.

The third consideration is the one that perhaps will weigh most with amateurs.

From the nature of the process no visible image can be discerned, and the exposure has to be determined by means of the photometer; but the method of employing this instrument is soon arrived at, and when once the printing density of a particular negative known, there is no further trouble to be apprehended in this direction. As it is with the developing of the carbon image we are mainly concerned to-night, it will be unnecessary to go further into details. You will see that it is a very simple affair—no turning, no fixing; all that is required is a fair amount of tepid water. There is no necessity to work in the dark; a subdued light is all that is demanded. The plates on which I propose developing the prints have been coated with a gelatine substratum made as follows :—80 grains gelatine dissolved in 5 ounces of water, and 1 ounce of water in which 3 grains chrome-alum have been dissolved, added to warm gelatine solution. The coating that is thus given to the plate is practically invisible.

To develop on a plate thus prepared, all that is required is to take it, plunge it into cold water, and bring the exposed tissue, which also has to be put into cold water, on to the prepared surface, squeeze them into contact, and let them stand for a little with a weight super-imposed, and finally immerse the whole in the developing bath of hot water—say at a temperature of about 100° F. Do not disturb the plate for a little, but wait till the paper of the original tissue begins to show signs of coming away of its own accord. It may then be gradually lifted off, leaving the image, which is still hidden under a layer of unacted-on gelatine, fixed to the glass. By gentle washing, the soluble portions are by degrees removed, and very soon the image comes into view. When the development is considered complete, the picture is to be washed in a bath of cold water, and to render it insoluble it is immersed in a solution of alum. All that remains is to soak a piece of transfer paper in hot water and squeeze it on to the picture, and allow the whole to dry. If all the foregoing operations be properly carried out we should have a picture which it would be difficult to tell from a silver print, provided, of course, that the tissue used in the first instance has been of that particular shade. As you are doubtless aware, tissue can be obtained in almost any colour, brown, black, purple, but the

* Communicated to the Glasgow Photographic Association.

colour which comes closest to the silver print tone is that known as chocolate red. For transparencies, a tissue in which Indian ink forms the colouring matter is the one most generally employed. I have exposed several prints to-day, using the two kinds of tissue specified, and these I will now proceed to develop.

I have said nothing about the sensitizing of the tissue, but there is no difficulty here. The bath employed is a five per cent. solution of bichromate of potass, and the time of immersion varies from one to four minutes, all depending on the temperature of the bath. As a general rule, it may be stated that the tissue should remain in the bath till it begins to feel soft. It is removed by one of the corners and allowed to drain, and afterwards fixed to a clip and allowed to dry spontaneously in a darkened apartment. The drying may be hastened by squeezeeing the excess of the bichromate solution out of the tissue before hanging it up.

Notes.

The illustration of Galton's composite portraiture which is given with the News this week shows four composites in the large ovals (A, B, C, and D). A is the composite of the five individuals a to a_5, while B is a composite of b to b_5, C is a co-composite of A and B, and D is a composite of five adult faces. The influence of the black curl of a_1 is very noticable in A and C.

The atmosphere is as a dark glass through which we see the sun, according to Professor Langley, who lectured at the Royal Institution on Friday last.

In order to eliminate as much of the atmospheric effect as possible, Professor Langley studied the solar spectrum from the top of one of the peaks of the Alleghany Mountain in California, and he found that the heat spectrum extends far beyond what has been looked on as the limit, while the blue rays are over ten times more intense at the top of the mountain than at the bottom. Were it not for the protective action of the aqueous vapour in the earth's atmosphere, mercury would freeze in the direct rays of the sun.

Every now and then a patent is obtained for a new method or process of making or obtaining gold, the alchemist of the present day evidently preferring the prospect of a close monopoly for fourteen years, rather than taking the chance of preserving his secret during a longer period; but of all the ancient and modern schemes for gold making, the most extraordinary process we have yet heard of is that originating with Mr. Harry Fell, Mercantile Clerk, of "Fairlight," Avenue Road, South Norwood Park. The patent specification of this gentleman (New method for getting gold from wheat; No. 14,204, 1884) may be obtained for twopence at the Patent Sale Office in Cursitor Street; but we give our readers the essential features of it.

The material (whole-wheat straw) is steeped in slightly warm water for ten hours, and strained off into a shallow pan; the pan being allowed to stand in a moderately-warm place for twenty-four hoars, a scum appears on the surface of the liquid, and this is caught on a cylinder of some cool substance, as china or earthenware. "Then let this skim dry," says the alchemist, "so getting some results of films of gold."

TYPE-WRITING, REDUCED BY THE CAMERA AND MADE INTO A PRINTING-BLOCK BY A PHOTO-TYPIC PROCESS, HAS BEEN USED IN AMERICA INSTEAD OF LETTER-PRESS MATTER FROM THE ORDINARY MOVEABLE TYPES; AND WE HERE GIVE AN EXAMPLE ILLUSTRATING THE NEW USE OF THE TYPE-WRITER.

Who knows but what photography may some day make the compositor a personage of the past, just as it now threatensto extinguish the wood engraver? Type writers are now made weighing only a few pounds, and so small that one of them may be carried in a hand bag, while, by using manifold paper, several copies can be obtained. It is easy to suppose the possibitity of newspaper matter and reports being first produced as type writing, being then photographed and finally converted into blocks by a photo-type method.

One notable advantage of this method would be the facility it would afford for including in the text hastily-made pen-and-ink sketches, or autographic matter.

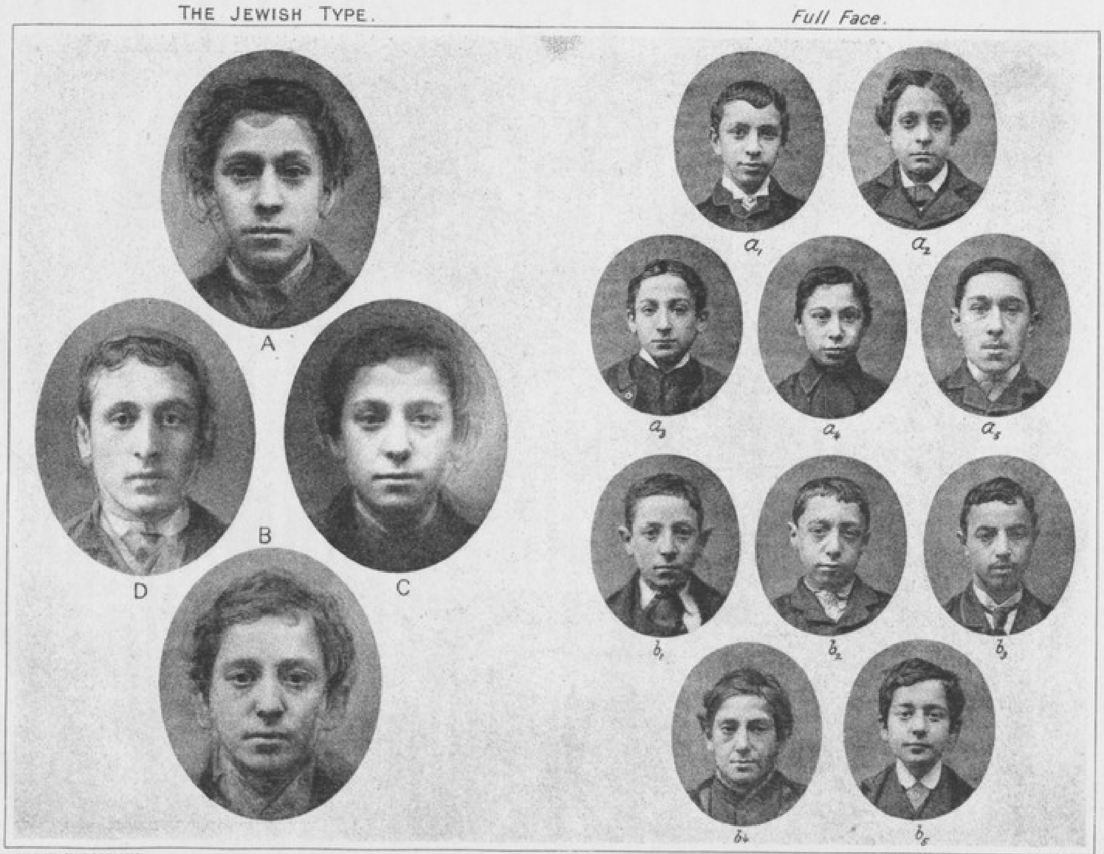
Adopting the ordinary chemigraphic method, some two or three hours would have to elapse before the block could be made; but one may expect improvements to be made as regards rapidity.

The direct transfer of type-written matter to stone for lithographic printing has been occasionally done, but during the present month a small journal produced in this way has made its appearance. It is called *The Monotype*, and is published at 9, Portugal Street, W. C.

The photographer has been following closely in the track of the Prince and Princess of Wales during their tour in Ireland; and the already astounding total of negatives which have been secured from first to last of this popular couple will have been increased by at least a score by the time they return. The public will be most interested, perhaps, to know that an admirable portrait of the Princess Alexandra, in her robes as *Mus. Bac.* of the Royal Irish University, was taken. For one of the negatives she actually sat in the "mortar-board," which literally "caps" an academic costume; but it is not likely that copies of this particular portrait will be published for sale.

Apropos to these photographs of Royalty, it may be interesting to state that amongst the most valued of the art treasures in Her Majesty's private apartments is the series of albums in which are arranged, in chronological order, copies of every photograph of a member of the Royal Family which has ever been taken. The custody and arrangement of these albums—which now form a small library of themselves—are left in the hands of one of the

THE JEWISH TYPE. *Full Face.*



INK-PHOTO, SPRAGUE & Co LONDON

FRANCIS GALTON, F.R.S. PHOTO.

COMPOSITES. *Components.*

ILLUSTRATIONS OF COMPOSITE PORTRAITURE.

Queen's most trusted private attendants; and admirably are the duties performed. The portraits of the Prince and Princess of Wales alone reach a total of over 600, and now that her Majesty's grandchildren are marrying, the Royal circle is extending at a most rapid rate. It may become necessary, in fact, seeing how photography is still developing, to appoint a special Court official to act as "Comptroller of the Royal Photographs," "Keeper of the Cartes," or something of the kind.

The primrose is a very pretty flower, and we have nothing to object in the setting apart a day for wearing it. But does not the *St. Stephen's Review*, in its perfervid adoration of Primrose Day and Lord Beaconsfield, go perilously near the ridiculous when it extols some card photographs because "they have the merit of being cheap, and all taken from actual Hughenden primroses?" We always thought one primrose very like another, but evidently, according to the *St. Stephen's Review*, the Hughenden primroses are "distinctly precious."

The *Lady's Pictorial* cites the "military photo frame" as an instance of the tendency just now to assimilate ornament to affairs of public importance. It is interesting to learn that "it has become *de grand genre* for officers to relieve their sombre photographs by the addition of a soldier painted in full uniform on one side of the leather frame, on the lower margin is often put a gun, two crossed swords, a casque or helmet with gauntlets." This description is rather vague, but one can see what is meant, and if the fashion spreads we should not be surprised if frames suitable for the portraits of bishops were made ornamented with mitres and croziers; frames for members of Parliament decorated with the mace and a copy of the oath for an inscription; and frames for lawyers in the shape of a woolsack, and emblazoned at each corner with a representation of the Lord Chancellor's seal!

A monument to the memory of the Rev. F. F. Statham will probably be erected in St. Peter's Church, Walworth. He was essentially the friend and helper of the poor in his parish, and the humbler classes of the parishioners are taking the initiative in the movement. In the *South London Press* we notice a long list of subscriptions; and if any reader should desire to make the list longer, the best way will be to send a cheque to the Editor of the paper in question.

A phototype reproduction of one of the Tironian Psalters will be published shortly by the Royal Stenographic Institute of Dresden, and notes by Dr. O. Lehmann will accompany the reproduction.

"Landscape," written by Mr. P. G. Hamerton, and just published by Seeley, would be valuable to the photographer, even if without the illustrations; but considering that the four hundred folio pages of text are supplemented with plates in the proportion of one to each ten pages, and that each plate teaches a definite lesson, the work is one that every out-door worker with the camera may reasonably desire to possess.

But for the system of photographing criminals, the perpetrator of the recent ghastly murder in Paris might not have been discovered. Marchandon, the butler of Madame Cornet, the murdered woman, had undergone a term of imprisonment in 1881, and the police, suspecting he might be the murderer, had his photograph shown to the keepers of ready-made clothes shops, as it appeared Marchandon had systematically obtained situations as butler, and had absconded with stolen goods. One of the clothes-dealers recognized the photograph as that of a regular customer who had once mentioned that he lived at Compiègne. The photograph was also recognized at Compiègne railway station, and the man himself was at once arrested.

An effort is being made to secure the publication of Gordon's journals in facsimile by one of the photo-zincographic processes which so admirably reproduce an origina MS., or whatever it may be.

The difficulty at the Home for Lost Dogs at Battersea is to dispose speedily of the good dogs that from time to time find their way there. It has been suggested to the committee that the photographic camera might with advantage be brought to their aid, and that the circulation of (say) a weekly circular containing photographs of alll the more eligible of the canine refugees, in London and the Provinces, would probably bring about the desiderated sales.

Herr Wilde, of Görlitz, tells a queer story. A request that he would act as a juror in the photographic department of the Technical Exhibition at Liegnitz was delivered by mistake to a cousin having the same name, and this gentleman, who had no idea of any mistake being made, accepted. As the other photographic juror (Herr Siebe) did not put in an appearance, there was but one acting juror, and this one a gentleman knowing nothing of photographic matters.

Herr Quidde comments on the worthlessness of medals given under such circumstances—indeed, he evidently thinks that medals should be done away with altogether; and he speaks approvingly of the exhibitors at the recent Technical Exhibition in Berlin, they having agreed to take no medals. As a rule, Herr Quidde remarks, medal-seeking exhibitors do not care whence the medals come, so long as they are medals.

This view is perhaps an extreme one, but, as regards the medals of the Photographic Society of Great Britain, it is a question whether it would not be more satisfactory to have them done away with altogether than for the suburban tram-cars to be decorated with portraits, each portrait being accompanied by a notice to the effect that the firm producing it has received the highest possible honour in connection with photography—the medal of the Photographic Society.

Our reference to Mr. Kay's proposal to make sea water potable by means of citrate of silver brings us two communications: one from Mr. Allison, of Hull, descriptive of his castaway locker, which is a small case containing a still, and enough petroleum to produce 100 lbs. of fresh water, to say nothing of what can be produced by burning other materials; also fishing tackle, concentrated food, compass, and other necessaries. The whole apparatus will float when packed. The other communication is from Mr. Kay himself. He asks our opinion of the usefulness of a sealed tube containing the exact quantity of citrate of silver required to make a pint or a quart of water drinkable. Even if one sets aside the variation in the amount of chlorides in sea water, it must be remembered that the use of an exact quantity of silver citrate requires equally exact measuring out of the pint or quart of water.

In the Mississippi State Female College, at Columbus, U.S., photography is to be regularly taught. We give this on the authority of the *St. Louis Photographer.*

Ferrotype dry plates have long been enquired after, and Mr. Gray, of Newcastle-on-Tyne, not only tells us that he has been successful in making them, but he sends us some very good examples of work. May we then not expect collodion to be ousted from one of its last strongholds—the tent of the peripatetic photographer who supplies the public with a portrait complete for sixpence?

It is quite possible that many tourists who would like to have memorials of their travels, but who hesitate to take several dozen glass plates, will be willing to take the more portable and less fragile ferrotype plates.

Patent Intelligence.

Applications for Letters Patent.

4786. Edward Marlow, 4 and 5, Arcade Chambers, Corporation Street, Birmingham, for "Improvements in photographic dark slides."—Dated 18th April, 1885.

Patents Sealed.

5134. Louis de Roux, of Begles, near Bordeaux, Gironde, France, for "Improvements in engraving by photography."—Dated 19th March, 1884.

5647. James Thomson, 21, High Park Street, Liverpool, Lancashire, Photographer, for "Improvements in photographic camera stands for use out of doors, on land or at sea."—Dated 29th March, 1884.

10,558. Count Stanislas Julian D'Ostrorog, 5, Conduit Street, New Bond Street, Middlesex, trading under the name of "Walery," for "Improvements in obtaining pictures on enamel, fixed by fire."—Dated 24th July, 1884.

Specification Published during the Week.

8463. Samuel Dunseith McKellen, of 18, Brown Street, Manchester, in the County of Lancaster, Watch Manufacturer and Jeweller, for "Improvements in photographic cameras."—Dated 20th February, 1884.

The claim is, in a photographic camera, the use of the two pinions for the purpose of giving motion to the sliding frame carrying the front board and lens, or carrying the camera body: such motion causing the front board and the camera body to approach to or to recede from each other.

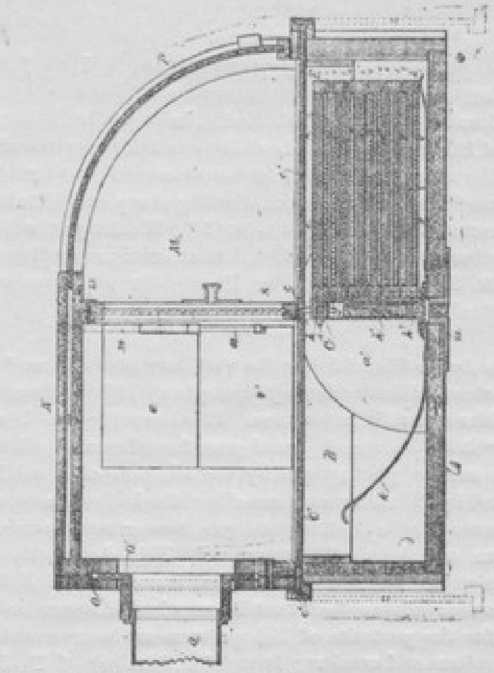
Patents on which the Fourth Year's Renewal Fee of £10 has been Paid.

1751. Ferdinand Hurter, Ph.D., of Widnes, in the County of Lancaster, Alkali Manufacturer, for an invention of "Improvements in actinometers or photometers, or instruments for measuring light."—Dated 23rd April, 1881.

Measuring the intensity of light by causing rays of different refrangibility to be received by or pass through different colours, and to be absorbed by the two sensitive parts of a differential thermometer, and measuring the difference of temperature thus produced, whence the intensity of the light may be ascertained.

Patents Granted in America.

315,156. Scotto C. Nash, Harrisburg, Pa., "Portable camera."—Filed June 14, 1884. (No model.)



Claim.—1. The combination, with a camera, of a box and a series of plate-holders united by a belt, a roller for moving the plate-holders up into position successively, and means for holding the plate in its position whilst the picture is being taken, substantially as set forth.

2. The combination, with the belt and plate-holders, of a box surrounding the same, means for moving such belt of plate-holders, a camera above and connected to the plate-holder box, and lids to the plate-holder box, substantially as set forth.

3. The box B, with a movable bottom, A, and side pieces, a', in combination with the belt and plate-holders, the roller for moving the same, and the partition and roller within the belt and between the sides a', substantially as set forth.

4. The combination, with the plate-holders, of a belt, clamping plates to connect the belt to the plate-holders, and a polygonal roller with teeth for moving the plate-holders and belt, substantially as set forth.

5. The combination, with the box A B, of the plate-holders, the belt connecting the same, the polygonal roller for moving the belt, and the springs upon which the outer ends of the plate-holders rest, substantially as set forth.

6. The combination, with the plate-holders and the belt connecting the same, of the polygonal roller, a square axle passing through the same, the journals for the axle, and the knobs upon the ends of the axle, substantially as set forth.

7. The combination, with the plate-holders, the belt connecting the same, and the means for moving the belt, of a box inclosing the parts and sliding covers composed of slats upon a flexible material, substantially as set forth.

8. In combination with the lens-holder and a flat septum attached to the same, a camera-box having a double front end, with a space between the two parts of the end, into which the septum is received, and within which it can be moved laterally, substantially as set forth.

9. The combination, with the plate-holders and a belt to connect the same, of a box, sliding lids for the same, and a camera

that can be reversed, so as to take pictures upon plates at either side of the plate-holders, substantially as set forth.

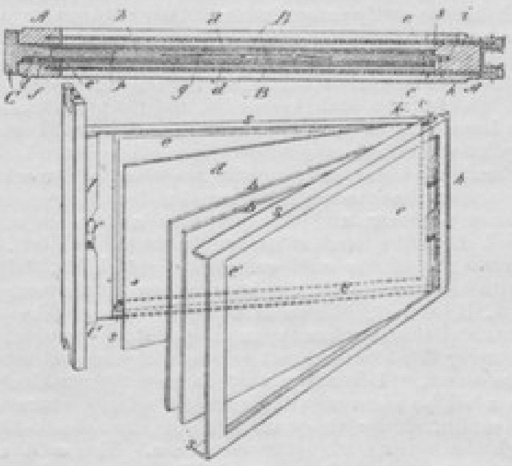
10. The camera-box having a front, end, and sides with segmental grooves, in combination with a cover formed of slats united by flexible material, the ends of the slats being within the grooves, substantially as set forth.

11. The forked stop, spring, turning-head, and notched case, in combination with the plates *n*, and the plate-holders or focussing-frame, substantially as specified.

12. The combination, with the camera-box, of sectional shields within the box, and the axles of the same extending through the camera-box, and turn-buttons or heads for the same, substantially as set forth.

13. The combination, with the plate-holders, the belt for connecting the same, and the roller for moving the belt, of a counting device for indicating the plates that are brought successively into place for the pictures, as set forth.

315,219. Erastus B. Barker, New York, N.Y., assignor to E. and H. T. Anthony and Co., same place, for "Photographic plate or sheet-holder."—Filed Nov. 18, 1884. (No model).



Claim.—1. In a photographic plate-holder, the combination with the septum of the holder, of an open hinged or swinging-frame, constructed to clamp or hold the marginal portions of the sensitive sheet on or against the septum, substantially as specified.

2. A septum for a photographic plate-holder, made substantially as herein shown and described, consisting of pivotted boards, in combination with pivotted open frames, constructed to clamp the marginal portions of the sensitized sheets on or against said boards, essentially as described.

3. The combination, with the septum boards and the pivotted frames, of the separating spring or springs *g*, substantially as described, whereby the edges of the sensitized sheets will be firmly held between the septum boards and the edges of the pivotted frames, as set forth.

4. The pivotted frames *e e'*, constructed with flanges or lips, *s*, substantially as shown and described, whereby the frames will fold one within the other, with the septum boards between, as set forth.

5. The outer frame, *e'*, constructed with a slot, *k*, as shown and described, whereby said frame is made to slide longitudinally upon the other frame and the septum, to facilitate the opening and closing of the several parts, and the insertion and removal of the photographic sheets, as set forth.

6. The fastening, *f*, in combination with the pivotted frame *e'* and the septum head or stop *l*, as shown and described.

7. The combination, with the extremity of the frame e_1, of the projecting fin *h*, as and for the purposes shown and described.

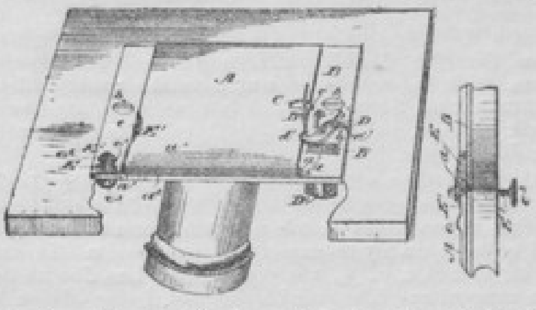
8. The frame *e*, fixedly secured to the septum head or stop *l*, and serving to carry both the outer pivotted frame and septum boards, substantially as specified.

315,296. Harry W. Kellogg, Greenfield, Mass, for "Instantaneous shutter for cameras."—Filed July 12, 1884. (No model).

Claim.—1. In a photographer's camera, the combination, with an instantaneous shutter, of an opening-spring mechanism and a closing tensioned spring, said springs being of unequal resiliency, and situated at opposite sides of the door, substantially as described.

2. In a photographer's camera, the combination, with an instantaneous shutter hinged at its lower side, of a self-setting exposing device consisting of a coiled spring having a spring-finger, which, acting excentrically with a cam operated by a thumb-nut, is brought into engagement with the under side of a lug rigid on the shutter, and the closing mechanism, substantially as described.

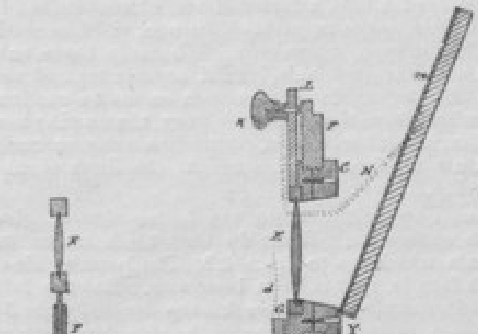
3. In a photographer's camera, the combination, with an instantaneous shutter having a self-setting exposing device, of a closing mechanism consisting of a coiled spring, E, having a



spring-finger, E', extending from the main coil and engaging with the lug or projection *e* on the shutter, and a tension-screw, E^2, for varying the exposure, substantially as described.

4. In a photographer's camera, the combination, with an instantaneous-flash shutter or light-door hinged at its lower side, of the opening and closing devices situated at opposite sides of the bottom of the door, and acting conjointly, a thumb-nut for operating said opening and closing devices, and a metal plate upon which the shutter and all the operating mechanism is mounted, substantially as described.

315,450. Wm. C. Strong, Kent's Hill, Me., "Adjustment for solar cameras."—Filed March 22, 1883. (No model.)



Claim.—1. The combination, with the foundation F, having a circular hole near its centre, of the two flanged collars C, fitted to the said hole, the lever L, secured to the inner collar and provided with the knob K, pivoted to it, the mirror *m*, hinged to the outer collar, and the cord H, secured at one end to the mirror, and at the outer end to the knob K, to be wound thereon, substantially as described, whereby the mirror may be both rotated and elevated, or depressed by means of the said knob.

2. The combination, with the foundation F, the mirror *m*, and lens E, secured thereto, as described, and the cross-piece I, also secured to the foundation F, of the lens-bar N, pivoted at *t* to the cross-piece I, the lens R, and the telescoping joint P, supporting the lens and provided with the slide O, fitted to the bar N, as shown and described, whereby the lens R may be raised or lowered, may be caused to approach or recede from the lens E, and may be swung horizontally out of parallel therewith, as described.

THE JEWISH TYPE, AND GALTON'S COMPOSITE PHOTOGRAPHS.

BY JOSEPH JACOBS, B.A.

Most people can tell a Jew when they seen one. There is a certain expression in Jewish faces which causes them to be identified as such in almost every instance. Being engaged in some investigations into Jewish characteristics generally, I was anxious to discover in what this "Jewish expression" consists. It occurred to me that Mr. Galton's method of composite portraiture would enable us to answer this question with some degree of exactitude. I accordingly applied to him about two years ago, and he kindly consented to "compound" some Jewish types. I procured for him photographs of Jewish lads from the Jewish Working Men's Club, and of Jewish boys from the Jews' Free School, and he was good enough to compound them, with the results presented to the readers of the Photographic News this and last week. I am now to speak of them and their scientific value as representing the Jewish type.

Of the fidelity with which they pourtray the Jewish expression there can be no doubt. Each of the eight composites shown might be taken as the portrait of a Jewish lad quite as readily as any of the components. In some cases, indeed, *e. g.*, f_3, the portraits are less Jewish than the composites. The individuality and, I may add, on Mr. Galton's authority, the beauty of these composites are very striking. It is difficult, even for those who know the process, to grasp the fact that the composite E exhibited last week is anything but the portrait of an individual; and the same may be said of D, the composite of five older lads, whose portraits are not shown. A, again, the composite of the five *a*'s, reminds me of several Jewish youngsters of my acquaintance, and might be taken for a slightly blurred photograph of any of them. This is the more curious since A does not resemble very closely any one of its components. These facts are something more than curious: they carry with them conclusions of scientific importance. If these Jewish lads, selected almost at random, and with parents from opposite parts of Europe, yield so markedly individual a type, it can only be because there actually exists a definite and well-defined organic type of modern Jews. Photographic science thus seems to confirm the conclusion I have drawn from history, that, owing to social isolation and other causes, there has been scarcely any admixture of alien blood amongst the Jews since their dispersion.

These composites, there can be no doubt, give the Jewish expression. What do they teach us as to the elements which go to form it? The popular idea of a Jewish face is, that it has a long nose. But the full-face composites exhibited this week have decidedly the Jewish expression, though the shape of the nose does not appear; and further, in composite H, as well as in co-composite G, which represents ten Jewish boys "rolled into one," the shape of the nose is markedly blurred, showing that there is no uniformity in this respect. The popular impression seems, then, to be disproved by these composites. Yet it contains a part of the truth, as do most of those rough averages which we term impressions. The nose does contribute much towards producing the Jewish expression, but it is not so much the shape of its profile, as the accentuation and flexibility of the *nostrils*. This is specially marked in the composite C. Take a narrow strip of paper and place it over the nose in this composite, and much, though not all, of the Jewish expression disappears. And in the profile *components* of last week it will be observed that every face has the curve of the nostril more distinctly marked than would be the case in the ordinary Teutonic face, for example.

A curious experiment illustrates this importance of the nostril towards making the Jewish expression. Artists tell us that the best way to make a caricature of the Jewish nose is to write a figure 6 with a long tail (fig. 1); now remove the turn of the twist as in fig. 2, and much

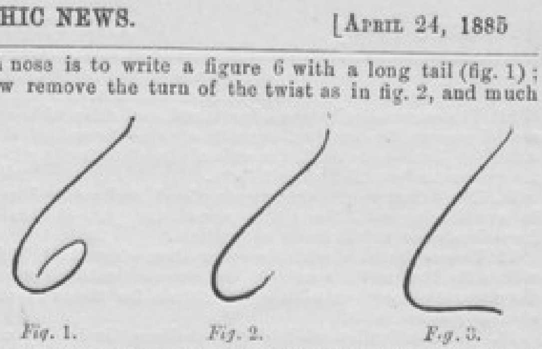


Fig. 1. *Fig.* 2. *Fig.* 3.

of the Jewishness disappears; and it vanishes entirely when we draw the continuation horizontally as in fig. 3. We may conclude, then, as regards the Jewish nose, that it is more the nostril than the nose itself which goes to form the characteristic Jewish expression.

But it is not alone this "nostrility" which makes a Jewish face so easily recognizable. Cover up every part of composite A but the eyes, and yet I fancy anyone familiar with Jews would say, "Those are Jewish eyes." I am less able to analyse this effect than in the case of the nose. The fullness of the upper lid, and the protuberance of the lower may be remarked, as well as the scantiness of the eyebrows towards the outer edges. The size, brilliance, and darkness of the iris are also well marked. Many persons have remarked to me that Jewish eyes seem set closer together, and this property is seen in composite A, D giving much of its expression to the latter. I fail to see any of the cold calculation which Mr. Galton seems to have noticed in the boys at any of the composites A, B, and C. There is something more like the dreamer and thinker than the merchant in A. In fact, on my showing this to an eminent painter of my acquaintance, he exclaimed, "I imagine that is how Spinoza looked when a lad," a piece of artistic insight which is remarkably confirmed by the portraits of the philosopher, though the artist had never seen one. The cold and somewhat hard look in composite D, however, is more confirmatory of Mr. Galton's impression. It is note-worthy that this is seen in a composite of young fellows between 17 and 20, who have had to fight a hard battle of life even by that early age.

There remain the forehead, mouth, and chin to add their quota to the Jewish expression. The predominating characteristic of the forehead is breadth, and perhaps the thick and dark hair encircling it has something to contribute to the Jewishness of the face. The thickness of the lips, and especially a characteristic pout of the lower one, come out markedly in components and composites, both full face and profile. One may observe, too, the dimples (if one may use the term) which mark the termination of the mouth, and are seen in an exaggerated form in a_1. Finally, the heavy chin, especially marked in the profile composites, confirms the popular association of this feature with the quality of perseverance, so ingrained in the Jewish nature.

We learn, then, from these composites that the Jewish expression is considerably more complicated than is ordinarily thought. If I have analysed it aright, the Jewish face has accentuated flexible nostrils, largish mouth, with ends well marked, and pouting under lip, heavy chin, broad forehead, with prominent superciliary ridges scantily covered with hair towards the outer extremities, and large dark eyes, set closely together, with heavy upper and protuberant lower lid, having a thoughtful expression in youth, transformed into a keen and penetrating gaze by early manhood. But words fail one most grievously in trying to split up into its elements that most living of all things, human expression; and Mr. Galton's composites say in a glance more than the most skilful physiognomist could express in many pages. "The best definition," said the old logicians, "is pointing with the finger" (*demonstratio optima* *definitio*); and the composites here given will doubtless form for a long time the best available definition of the Jewish expression and the Jewish type.

There is one consideration which lends an interest other than scientific to these composites of Mr. Galton. Most of the readers of this journal will be familiar with the portraits of spirits which gratify the curiosity of spiritualists, and cause ironical laughter among those who know the easy trick by which these plates may be manufactured. But here we have something more ghostly than a ghost, more spiritual than a spirit. The thing, person, spirit, ghost, idea, type, or what you will that looks at us in A has no bodily existence; and yet there is life in its eyes. And it has a definite expression which can only mean that it expresses something. In the present instance, as the components can in all probability trace back to a common ancestor, the composite face must represent, if it represent anything, this Jewish forefather. As the spectroscope has bridged over the abysms of space, and has told the composition of Orion's Belt, so the photographic lens seems, in these composites, to traverse the æons of time and bring up into visible presentment the heroes of the past. In these Jewish composites we have the nearest representation we can hope to possess of the lad Samuel as he ministered before the Ark, or the youthful David when he tended his father's sheep. Or, if this is saying too much, we may see in the composites the Jewish youngsters who "coolly appraised" the ancient prophet, as their components seem to have done with the modern scientist.

Let me, in conclusion, recommend—of course with an eye to my own hobby—any photographer who intends taking up composite portraiture, to try his hand—but not his 'prentice hand—on composites of Jewesses. I am inclined to think that there is less variation in Jewesses than in Jews, so that the composite ought to be even more individual. Besides, in their case portraits can be taken at a later age without fear that any part of the face will be concealed from view, and we should thus be able to study the features when more set than in the case of the Jewish lads. The enterprising photographer who adopts my suggestion need have no anxiety about procuring components. I make bold to assert that my fair co-religionists are as constant visitors to photographic studios as any class of her Majesty's subjects. And if Mr. Galton has succeeded in producing such individual and beautiful composites of Jewish boys, not of a very refined class—well, my argument is obvious, and I leave some of the professional readers of the Photographic News to carry it into effect.

PHOTO-LITHOGRAPHY AND PHOTO-ZINCOGRAPHY.

BY MAJOR J. WATERHOUSE, B.S.C.,
Assistant Surveyor-General of India.

Chapter XVIII.—Photo-Lithography in Half-Tones.*

III. We now come to the processes in the third class, in which the photographic image is broken up by a network or grain interposed in some way between the negative and the sensitive paper or other surface.

Messrs. Bullock Brothers, of Leamington, who patented a method of photo-lithography in half-tones in 1865, appear to have been the first to recognise and practically carry out the principle of breaking up the photographic image by a definite grain, though Mr. J. W. Swan patented a different process of the same kind about the same time.

In a paper read before the London Photographic Society in April, 1866 (News, vol. x., p. 185), Mr. J. Bullock gave a short resumé of experiments he and his brother had made in this direction.

Making a grain upon the glass plate previous to its being coated with collodion to form a negative was found promising, but troublesome. Forming a grain upon the finished negative by coating it with a granular varnish, or by making the surface slightly adhesive and depositing a fine powder upon it, were tried, but not found satisfactory. Producing the grain in the camera by placing before the prepared plate a glass plate covered with a fine grain was also tried, but a long exposure was required, and an unpleasant pinhole appearance was given to the parts which should have been quite dense. Other methods of placing a grained medium between the negative and the paper were tried, but without success.

The methods which finally were found most successful were—first, by reticulating the negative in the following manner:—A piece of ground glass, or a photographic copy on glass of some reticulated surface, was placed face to face with the negative, and a transparency taken from the two whilst in contact. From this transparency any number of negatives could be produced, each containing a granulation so marked as to give the prints on transfer paper a well-marked half-tone composed of lines or dots.

The second plan, which seems to have given very good results, was to imprint a grain, by aquatint or otherwise, upon the unsensitised gelatine paper—all that was required being to sensitise the paper with bichromate of potash, and print from an ordinary negative. The exposed surface was then inked and washed in the ordinary way. Some excellent specimens were produced and published in the News and other photographic journals, but the process seems to have fallen out of use.

In Mr. Swan's process he first produced what he termed a crayon photograph—*i.e.*, one in which gradation of shade was produced by dots of opaque matter more or less aggregated as the shading is darker or lighter, and therefore corresponding to a crayon drawing in the principle on which the gradation of shade is produced. Such a crayon photograph may be produced in various ways, but Mr. Swan preferred to obtain it by means of a gelatine tissue containing a certain proportion of charcoal or other opaque substance, in such a state of division or granulation that when diffused through the gelatine solution and spread upon glass in a thin layer, the opaque particles will show distinct granularity between particle and particle. This tissue was either prepared ready sensitized with bichromate, or could be sensitized as required. It was exposed under a photographic negative or positive, that surface of the tissue which was uppermost during the solidification of the gelatine being placed next to the cliché, and care being taken that the light might fall at right angles to the plane of the cliche. After exposure the exposed surface of the tissue was coated with a solution of india-rubber and dammar in benzole, and mounted on a glass coated with the same solution, the solvent being allowed to evaporate. The image was then developed in warm water in the usual way. The crayon photograph could also be produced by the dusting process.

Having obtained the crayon photograph by the above methods, it was used instead of an ordinary negative or positive for the production of a transfer print in the usual way.

A process of this class by which have been produced some of the best results in photo-lithographic half-tone the writer has seen, was published last year in the *Photographisches Correspondenz* by Herr Mariot, of the Military Geographical Institute at Vienna, who has done a great deal in perfecting various photo-mechanical processes. Herr Mariot's work is specially interesting because he has thoroughly investigated the question of grain, and has succeeded in working out a method which, being equally applicable to Photo-typography Photo-lithography and Photo-gravure, may be termed a "Universal Photographic Printing Method" (see News, vol. xxvii., p. 802).

Herr Mariot's process is based upon the well-known principle that if we copy a negative of a line or point, the image of the line or point will increase in breadth and

* Continued from page 60.

coarseness according to the length of exposure; this goes so far that the images of two thick lines or points standing close together (within certain limits) will finally join. This effect will be more readily produced if the covered spaces which should remain white allow some action of light to take place through them under long exposure.

If we expose a network of fine lines at a distance apart of about $\frac{1}{50}$ of an inch to diffused light under a sensitive plate, and cover it over in slips, so that the first slip shall have a normal exposure, and the remainder be over-exposed from twice to four times, it will be found that the first slip will correspond with the original, while the rest will show distinctly thicker and thicker lines, till finally they join together.

The same effect occurs when the lines are exposed under a half-tone negative. In the clear parts the lines will be found closed up after a sufficient exposure, and will gradually open out until finally those under the densest parts (the lightest half tones) appear in their proper dimensions. To produce the proper effect, therefore, the exposure must be prolonged until the lines or points which represent the finest tones in the lights are sufficiently clearly developed.

(*To be continued.*)

AN ATTEMPT TO PHOTOGRAPH THE CORONA.

BY W. H. PICKERING.

It occurred to the writer that the late partial solar eclipse would be an excellent chance to repeal Huggin's experimentz on photographing the corona. A 3-inch refractor of about 40 inches focal length was employed. A drop-shutter was attached to the lens, giving an exposure which was estimated at about a fifth of a second. A piece of deep-violet glass was procured, which could be inserted just in front of the plate, or removed, at pleasure. By its use a negative image of the sun's disc was obtained, but without it the plate gave a reversed image; the sun being a positive and transparent, while the surroundings remained negative and were dark, the appearance being strikingly similar to that of a photograph of a total solar eclipse. Both bromide and chloride plates were provided; but, as with Mr. Huggins, the latter proved proved to give much the better coronal effects. A ferrous-oxalate developer was employed, which contained a large proportion of potassium bromide. The weather throughout the eclipse was wholly favourable; and we began photographing at ten o'clock, two hours and twenty minutes before the eclipse began, and continued at work until five minutes past four, or an hour and ten minutes after it had terminated. Photographs were taken every half-hour, with extra ones interpolated at the more interesting phases, making twenty-nine pictures in all.

Very corona-like effects were certainly produced, faint rays here and there shooting out perpendicularly to the sun's surface. But, unfortunately, no two of the pictures were alike, and the corona in front of the moon was quite as well marked as that on the other side of the sun. Indeed, the most corona-like ray produced, appeared in one photograph stretching directly towards, and terminating at, the centre of the moon. Nine photographs taken in succession showed one side of the halo stretching to a greater distance than the other; but in one of these the darkening was carried so far out, that it became nearly separated from the rest of the corona, and appeared as a distinct dark circle of the same size as, and by the side of, the image of the sun. This, of course, showed it to be merely an internal reflection of that image, and nothing more. During the course of the experiments, the object-glass was revolved about its optical axis, photographs being taken in four positions. No effect, however, was discernible upon the plates.

The conclusions I should draw from my experiment are 1st, that, though it is very easy to obtain a corona-like image, one may readily be deceived in such matters, and the same effect be obtained by our atmosphere, without the aid of the solar corona, combined with little defects in the gelatine film (this, I think, is conclusively shown by the extension of the pseudo-corona in front of the moon); 2nd, that chloride plates are more suitable than bromide for obtaining an atmospheric corona, just as Mr. Huggins has claimed that they are more suitable for taking a solar one; hence I think one must not rely too much on the ultra-violet region sensitiveness of the chloride plate for a separation of the two; lastly, though my experiments fail to corroborate Mr. Huggins's results, they do not, of course, show that his corona may not be solar, but merely indicate that under very favourable circumstances I could obtain no trace of it.

I have before me a print made from a negative by Dr. O. Lohse, in October, 1878, showing effects very similar to those obtained by myself, except that his view was not taken during an eclipse. He considers that the halo is wholly atmospheric, and not coronal.

MOUNTING LARGE PHOTOGRAPHS.

BY F. J. HAYNES.

Having recently had a varied experience in mounting full sheet prints, I would be pleased to inform your numerous readers of our method of mounting and preventing warping, if the following is of sufficient importance to justify its publication.

If mounting on cardboard 22 by 26, have a stretcher made of 3-inch strips well put together, 23 by 27 outside measure; this allows half-an-inch margin over the cardboard; mount the print as usual on the dry cardboard, place it face down on the stretcher, and cover the back with a piece of strong Manilla paper large enough to bind the print and cardboard securely to the stretcher. Allow it to remain on the stretcher until thoroughly dry. If your back paper is poor, the contraction will break it loose from the stretcher, and the desired effect will not be obtained. But if a first quality of paper is used it will hold it securely; and when the cardboard is cut loose, it will be as straight as the original cardboard, and will remain so.

Large views mounted in this way can be placed in portfolios on easels, &c., with some pleasure; they will not be like a lot of stove pipes. We have mounted during the last three months some two thousand, and can recommend this plan as entirely satisfactory, and certainly one that is appreciated by the purchaser of the views.

Any number of these stretchers can be made; but with twelve we have been able to get along rapidly enough, as the print dries in a few hours, when it can be cut off, and the stretcher is ready for use again.—*Photographic Times.*

Correspondence.

THE NEW DANGER.

Sir,—The subjoined extracts from correspondents will prove the interest taken by the profession in the above case. As the matter is still in the hands of my solicitor, I refrain from making any remarks.—Yours truly,

49, *King William Street, E.C.* A. L. Henderson.

A Northampton photographer writes:—

"Dear Sir,—I have just returned from a short holiday, and on picking up my News found your letter, 'A New Danger.' I must confess to feelings of thankfulness that you had the promptitude to unmask the wretched cheat. I have no clubs here, so naturally I was pleased to read your letter. I knew that the individual, on being liberated from prison, was going on for a London firm; but I little thought he had ventured to use your name. I am afraid I can be of little service, but please command me if necessary in this matter."

"Dear Sir,—Seeing a letter in the Photographic News of last week of yours, I beg respectfully to call your attention to what I think you may, perhaps, have overlooked in connection with the 'New Danger.' First, your letter would only be seen by at most about sixty or seventy persons, as not more than that number of copies of the News are circulated in the town. Second, as your letter does not mention the name of the canvasser referred to, it throws discredit upon *all* canvassers in this town. Knowing you have for many years past exposed various shams and frauds in connection with the profession, I feel sure you would not willingly wish to brand all for one. I would suggest that if action be not taken before the magistrates, that an advertisement in the Northampton *Daily Reporter* or *Daily Chronicle* disclaiming *your connection* with the so-called firm would at once set the matter at rest. I trust you will pardon the liberty I am taking in this matter, but I am speaking on behalf of canvassers of two or three firms represented in the town, whom I am sure you would not wish to suffer under a ban for another's ill-doings."

Proceedings of Societies.

London and Provincial Photographic Association.

A meeting of this Society was held on Thursday, the 16th inst., Mr. A. L. Henderson in the chair.

The preliminary business having been disposed of, the Chairman invited Mr. W. K. Burton to deliver his lecture on "Modern Developers."

Mr. Burton commenced by saying that "Modern Developers" might be said to have dated from 1861, when alkaline development was first made known. Before this date some acid, always accompanied with nitrate of silver, was employed to build up the image; but in the year mentioned it was discovered, that if an alkali be substituted for the acid and free silver, an image could be developed, thus preparing the road for the present methods. In 1877, Carey Lea pointed out that alkaline pyrogallol could be replaced by organic iron salts. This form of development the lecturer did not consider the best for plates, but it was better for paper. The question then arose, which alkali should accompany the pyrogallol? He believed ammonia was used almost invariably. He then passed round a series of transparencies wherein each of the carbonates had been employed. A similar series, which included the caustic alkalies, followed. Forty-eight examples were then shown, in which the proportions of pyrogallol per ounce varied from ½ to 2 grains, bromide of ammonia ¼ to 2 grains, and ammonia ½ to 4 minims. He then referred to the addition of sulphite of soda, a subject upon which opinions widely differed. So far as his experiments went, he was unable to trace any particular advantage to its use, neither would he say it was injurious. Hydrokinone was then spoken of and illustrated; three grains, together with a quarter of a grain of restrainer, bromide of ammonium, being used, the result of which was satisfactory. Hydroxylamine, on the contrary, failed to give an image with any of the caustic alkalies. He had not tried the salt with either of the carbonates, as he assumed the result would also have been *nil*. All the examples illustrating comparative values of ammonia pyro and bromide, received an exposure of twenty seconds to a duplex paraffin lamp, and were all developed for as much detail as could be obtained from them in half an hour. He did not recommend keeping the stock developer in one solution for obvious reasons, but to show that it was practical, he passed round examples which had been mixed for a month; they were of a brown colour, but had not lost any developing power, a fact afterwards satisfactorily demonstrated. The solution was composed of ammonium bromide, pyrogallol, and caustic soda in methylated spirit, the limit being governed by the solubility of the bromide salt. At the conclusion of the lecture, a discussion took place.

Mr. A. Cowan preferred the ammonia carbonate to either soda or potash, and instanced the development of plates by a solution which had been kept twelve months, after developing eight plates. He had diluted some developer with sixty-four times its bulk of water, and obtained an image in fifteen hours.

Mr. A. Mackie remarked upon the yellowness of the images so obtained, even with one hour's development.

Mr. W. J. H. Wellington was in doubt whether another brand of plates would yield similar results to those obtained by the lecturer, since formulæ differed so much. He had lately been trying Dr. Eder's sulphocyanide of ammonium formula, and his plates then gave green fog; with sulphite of soda they were perfectly clear.

The Chairman called attention to the amount of halation as well as density in one of the examples, and remarked that ferrous oxalate development was due to Willis rather than Carey Lea.

Mr. Burton said it might be taken as an axiom, that the same plate would increase in halation as it increased in detail. His information regarding Carey Lea was derived from the journals of that time.

The Hon. Secretary showed a plate illustrating the disadvantage of an impure sample of sodium sulphate, a little sulphide being apparently present, and the Chairman illustrated similar defect, together with red and green fog; other plates of the same subject developed minus the sodium sulphite were in both these cases perfect.

Mr. C. H. Trinks pointed out the necessity of testing samples of sodium sulphite for alkalinity, and correcting the same with an acid, to avoid green fog.

Mr. Haddon referred to a series of experiments he had conducted some time ago; he found that working under exactly similar conditions, some plates gave green fog, and others not. Regarding the preservation of pyro in solution, he had not met with anything so good as sulphurous acid. He dissolved one ounce of pyrogallol in 10 ounces of water, containing ½ an ounce of sulphurous acid. He had some which had been in solution more than six months, and was now perfectly clear. He used ammonia, and needed no restrainer, as a little sulpihte of ammonia was formed.

A vote of thanks was then accorded to Mr. Burton for his interesting lecture, and it was announced that on the following lecturette night Mr. G. M. Satchfield will demonstrate "Toning and Fixing the Acme Paper."

Chicago Photographic Association.

The regular monthly meeting was held on Wednesday evening, April 1st. After the transaction of some routine business,

Mr. Greene related some peculiar experiences that had lately happened to him in the matter of silvering albumen paper. At a recent meeting he had mentioned the fact of his paper curling obstinately; since that time he had been badly troubled with "measly" prints, and thinking his bath might be weak, he tested it with the hydrometer, and found it to read 42 grains. This induced him to precipitate the silver with chloride of sodium, when, after washing and drying the precipitate, he found that he had 1¼ ounces of silver chloride from 55 ounces of solution. His theory was, that the nitrate of silver now in the market was largely adulterated with (probably) nitrate of potash, which, not being taken up by the paper, accumulated in the bath, and accounted for the false reading of the hydrometer. To guard against being misunderstood, Mr. Greene said he was well aware that the hydrometer test was unreliable, except with a new bath.

Dr. Garrison, in reply, said he believed all the leading makers of silver nitrate furnished a practically pure article, and that adulteration with potassium nitrate was readily detected by the difference in the shape of the crystals of the two salts. He considered that the density shown by the hydrometer was legitimately accounted for by the nitrates formed in the bath by double decomposition between the nitrate of silver in the latter and the chlorides in the paper.

Mr. Greene asked how it happened, if this theory held good, that when he was, for a number of weeks, silvering 100 sheets a day, this trouble never occurred, although he relied on his hydrometer for keeping up the strength of the solution?

Being asked how often he boiled down his bath,

Mr. Greene replied that, until this winter, it had not had a thorough boiling for fourteen or fifteen years. He occasionally gave it a slight boiling to coagulate any albumen it might contain. He added that, since making a new bath, he was no longer troubled with paper curling.

Glasgow and West of Scotland Amateur Photographic Association.

The usual monthly meeting was held on Tuesday, the 14th inst The following new members were admitted:—Messrs. John, Gillespie, H. B. Collins, and John G. Walker.

After various letters had been read,

The President exhibited and explained Warnerke's sensitometer, with special reference to Mr. Goodwin's paper at the March meeting on "Aids to Correct Exposure."

A short discussion followed, which was taken part in by various members.

Mr. W. Lang, Jun. then read his paper on "Carbon Printing" (see page 263), and developed a number of carbon prints, which were handed round.

A specimen of the result on an isochromatic plate was then handed round, and compared with a print from one of Wratten's plates, and it was considered that any advantage gained in one colour was lost in another colour.

As this was the last regular meeting of the season, it was agreed to hold informal meetings on the second and fourth Tuesdays of each month at 7.30 p.m. It was also agreed that the Council should procure an album, each member to be asked to contribute prints towards same.

Gloucester School of Science Philosophical Society.
Photographic Section.

The usual monthly meeting was held on Tuesday, the 14th inst., Mr. J. M. Collett in the chair.

The minutes of the previous meeting having been read and confirmed,

The Chairman said that the subject set down for consideration was, the "Best Form of Tripod Camera Stand."

Mr. F. M. Bridgewater, introducing the subject, said that he had, soon after taking up the pursuit of photography, become convinced of the desirability of obtaining a stand which it would be possible to carry without so obviously announcing to the general public the business that the bearer was abroad upon, as did the one supplied to him with his camera, and which was simply a two-fold one. Practically, the bearer of an ordinary tripod stand might as well be labelled "Photographer," and in an article in one of the year-books, stands of the ordinary type had been appropriately designated "The Tell-tale Legs." Being desirous of overcoming what he considered an objection to the prevailing forms of stand, he had given some attention to the consideration of the best means of so shortening the length of the stand, when packed up, as to allow of its being placed in the receptacle in which the camera was carried. It became clear at once that there were only two possible methods of securing the desired end—viz., by multiplying the number of folds or pivots, so as to shorten them, or by some arrangement of a telescopic character. The first of these alternatives did not appear very practicable, unless accompanied by an increase in bulk and weight, which would go far to counteract its advantages in other respects. The second seemed quite feasible, and in consequence he came near doing, in regard to a telescopic camera stand, what he had fully succeeded in doing with reference to one or two other things—viz., evolving a contrivance which had been invented and perfected by somebody else long before. It happened, however, that just when his ideas had reached the point indicated, he met with an advertisement of just such a stand as he had in his mind's eye, and which had been patented. By the kindness of the patentee he was enabled to show them one of these stands. It would be observed that it was made of brass, the joints telescoping one within the other. The height of the one then before them was, when erected, 4 feet, and the length of each of the three legs, when closed, was 12 inches. There were no loose screws or pieces of any kind, simply the three legs and the head into which they screwed. Stands were also made, so as to close into three 10-inch lengths, with a slight addition to the diameter of the tube. The two principal requisites in a tripod stand were, he considered, rigidity and portability, and these were more perfectly secured in the stand before them than in any other he had seen. The weight of the stand was $2\frac{1}{4}$ lbs., while his oak sliding-leg stand, to carry a similar camera (half-plate) weighed $3\frac{3}{4}$ lbs.

Talk in the Studio.

Photographic Society of Great Britain.—The next Monthly Technical Meeting of this Society will take place on Tuesday next, April 28th, at 8 p.m., at the Gallery, 5a, Pall Mall East, when some questions relative to the "Chemical Reaction of Alum, Tannin, and Bichromate of Potash on Gelatine," will be discussed.

Miss Mary Anderson's Photograph.—Alexander M. Rossi, an artist, of 177, Adelaide Road, St. John's Wood, appeared to answer two adjourned summonses, taken out by Henry Van der Weyde, of 182, Regent Street, one for infringement of the copyright of the photograph of Miss Anderson and selling copies of the same. Mr. Rain, barrister, supported the summons, and Mr. Corrie Grant, barrister, was for the defendant. Several witnesses were now called, who said there was no likeness between the face in the photograph and the defendant's picture. Mr. de Rutzen reserved his decision in the case.

Balloon Photography.—The experiments in Paris by the Triboulet system of photographing all the country seen from a captive balloon, by opening the valve of a panoramic object-glass with a current sent from the ground, has succeeded wonderfully well. As the operators remain on the ground, a very small balloon is sufficient to carry the photographic apparatus. The impressions, being taken on films, can be inspected with a microscope, and are useful for military purposes.—*Nature.*

Photographic Club.—The subject for discussion at the next meeting, on Wednesday, April 29th, will be on "The Preparation of Lantern Slides." This is a Lantern Night. Visitors invited. The Saturday Outdoor Meeting will be held at Hampstead Heath; after, meet at "Bull and Bush." Trains from Broad Street every quarter of an hour.

To Correspondents.

*** **We cannot undertake to return rejected communications.**

W. T.—You have made use of sulphuric acid instead of sulphurous acid, and there is nothing to be done but to throw away all the developer you have prepared. It is fortunate that you did not do yourself some injury.

Hypo.—Cut a print in two parts, and mount one half upon the questionable card, and the other half on a sample upon which you can depend. Now keep both side by side in a damp place, and note what happens. If you purchased the cardboard from a general stationer, who did not know the purpose for which you bought it, and it causes the prints to fade, you have no remedy; but if, on the other hand, you bought it from a dealer in photographic requisites, and it was sold expressly to be used as mounts for photographs, the dealer is responsible for their quality, and should compensate you for loss of trade and reputation.

Enquirer.—1. Probably half or one-third. 2. Lambert's registering frames were supplied by the Autotype Company; but we do not know whether they keep them in stock now. 3. Probably next week; it is as you suppose.

Thos. Smith.—1. There are very useful articles in Spon's Cyclopedia, and some other publications; but we do not know of any special book treating exhaustively on the subject. 2. It is not what you suppose, but a true enamel; perhaps dusted on an adhesive ground, and then fired.

B. Cunningham.—They may be very conveniently made of wood, and if well varnished with a bitumenous varnish, they answer very well. The varnish may be made somewhat elastic by incorporating with it a little melted india-rubber. The rubber should be unvulcanised, and ought to be heated until it becomes viscous, like treacle.

A. B. Boland.—We congratulate you on your success, thank you for your offer, and will write in a day or so.

Henry Spink.—Cowell's clearing solution is perhaps the best for the purpose:—

Alum...	1 part
Citric acid	2 parts
Water	10 ,,

E. L.—1. We cannot ascertain for you whether the lithograph is copyright, but one may reasonably expect that it is. 2. We do not quite understand your position, but assume that you have bought a right to reproduce from some person who has no power to sell the right. 3. The prints and stamps are returned by post.

A. Miller.—The suggestion is a good one, and we intend to adopt it when the proper time comes round.

Alum and Water.—1. Details will be given before long. 2. Let them run on thin brass wires stretched across.

C. P. G.—It may be from the use of old hyposulphite; indeed, we can suggest nothing else.

J. M.—From the Silvertown India Rubber Co., 100, Cannon Street, London, E.C.

J. Yelder. Will you send us a description and sketch of the arrangement? We shall also be glad to receive an account of your method of printing.

The Photographic News.

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STUDIES IN EUGENICS[1]

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I. RESTRICTIONS IN MARRIAGE

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,[2] 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 armchair criticism to historical facts.

To this end, a brief history will be given of a few 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

[1] Read before the Sociological Society of London.

[2] Eugenics may be defined as the science which deals with those social agencies that influence, mentally or physically, the racial qualities of future generations.

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 recognized 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 afterward the Jewish kings emulated the luxurious habits of neighboring potentates and carried polygamy to an extreme degree. For Solomon see 1 Kings 11 :3; for his son Rehoboam see 2 Chron. 11 :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 Judea 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, which 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 license 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.[3]

It is unnecessary for my present purpose to go further into the voluminous data connected with these marriages in all parts

[3] *Encyclopædia Britannica*, Vol. XVI, p. 827.

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.*— 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 neighboring 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, etc. 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 Judea, is that known as the Levirate. It shows that family property and honor 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.[4] 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

[4] Deut., chap. 25.

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[5] 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 vigor; 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.*— 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 tribe, and is usually in force among 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

[5] See marginal note in the Revised Version.

earliest, and perhaps the most successful, of all attempts to decipher prehistoric customs by means of those now existing among barbarians, and by the marks they have left on the traditional practices of civilized 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 supply themselves only by capturing those of neighboring 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 subclassed into four subdivisions: A1, A2, B1, B2. The rule is that a man may marry those women only whose letter and numeral are both different from his own. Thus, A1 can marry only B2, the other three subdivisions, 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 forms 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 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 recognizable 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 civilized nations, may not be ideally perfect, nor may it be uni-

versally 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 thoroughbred stock, causes some sexual indifference; thus highly bred dogs lose much of their sexual desire for one another, but will rush to the arms of a mongrel. (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 civilized 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."[6] 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.

[6] Gen. 20:12.

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 not only the potency 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 afterward 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 toward 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 civilized 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 realize 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 civilization, can be deduced form 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.

II. STUDIES IN NATIONAL EUGENICS

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 program that binds no one, not even myself; for I have not yet had the advantage of discussing it with others, and may hereafter wish largely to 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."[7] 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, that do not rank as "gifted," are to be collected, and kept in manuscript, for statistical use, but with option of publication.

c) Biographies of families, which, 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,[8] 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.

[7] See *Sociological Papers*, Vol. I, p. 85. [8] *Ibid.*, p. 62.

II. *Effects of action by the state and by public institutions.*

f) Habitual criminals. Public opinion is beginning to regard with favor 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 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 toward 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 outdoor 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 recognized, 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 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.

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 traveling 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 molded 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 could do this largely so far as the evolution of humanity is concerned, and he has already affected the quality and distribution of organic

* This section was communicated to the Sociological Society in supplement to three papers, viz.: "Eugenics: Its Definition, Scope, and Aims" (*vide American Journal of Sociology*, Vol. X, pp. 1–25), and the first two sections of this article.

life so widely that the changes on the surface of the earth, merely through his disforestings and agriculture, would be recognizable from a distance as great as that of the moon.

As regards the practical side of eugenics, we need not linger to reopen 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 vigor 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.[10]

[10] Space does not permit publication of the comments upon Mr. Galton's papers. A portion of the discussion at the two sessions of the Sociological Society devoted to them will appear in the department "Notes and Abstracts" of the September number.

F. 9.

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NATURE

A WEEKLY ILLUSTRATED JOURNAL OF SCIENCE.

"To the solid ground
Of Nature trusts the mind which builds for aye."—WORDSWORTH.

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THURSDAY, MARCH 31, 1910.

A FRENCH TREATISE ON PHYSICAL GEOGRAPHY.

Traité de Géographie physique: Climat, Hydrographie, Relief du Sol, Biogéographie. By Prof. Emmanuel de Martonne. Pp. viii+910, and maps. (Paris: Librairie Armand Colin.) Price 22 francs.

EVERY writer of text-books is faced by the difficulty that the science of which he treats inosculates with and is overlapped by other branches of science. One of his most important tasks, therefore, consists in the exercise of a wise judgment as to what should be properly included, and what excluded, in the treatise he is preparing.

It has sometimes been suggested that all science may be regarded as falling into the two divisions geography and astronomy, the former dealing with everything that relates to our own planet, the latter with matters concerning the outside universe. But however logical such a scheme of classification of the sciences may be regarded, it cannot be commended on the score of convenience. There is no branch of physical or natural science which is not a part of "earth-knowledge," but it would be clearly impossible in a single treatise to deal with the foundations and superstructures of physics, chemistry, geology, botany, zoology, and anthropology. A work on geography must take for granted a certain amount of preliminary knowledge of science, and be contented with showing their application to the explanation of the various phenomena exhibited on the surface of the globe.

In the preface to the work before us, its author suggests as natural divisions of physical geography the following:—Morphologie, l'hydrographie, le climat, la biogéographie, et la géographie humaine; and the bulky volume now issued deals only with the first four of these divisions. The author justly remarks that it is almost impossible for any man to have a complete personal knowledge of all these subjects, but that specialisation becomes necessary; he has therefore sought and obtained assistance from various colleagues and friends in dealing with different departments of the subject.

In the first division of the work, devoted to general notions on the subject, a clear account, occupying 25 pages, is given of the history and evolution of geological science, and in this part, as in all following divisions, a very useful and complete bibliography of the subject is supplied. The question of projection is fully treated, and, for the very numerous maps of the whole globe given throughout the work, the conventional system of Molweide (or Babinet) is adopted in preference to that of Mercator, thus avoiding the extreme polar distortions of the latter system.

The 160 pages devoted to meteorology, and constituting the second division of the work, contains an excellent summary of that branch of science. The chief meteorological instruments are described, and clear statements given on the temperature, the hygroscopic characters, and the movements of the atmosphere in different areas; and the bearings of these several factors in producing different types of climate are discussed with much skill.

The subject of hydrography has more than 100 pages devoted to it, and constitutes the third division of the book. A good *résumé* of the observations made in recent years in the deep oceans is given with abundant references, including those to the latest published works. This is followed by chapters on the movements of the oceanic water, on the lakes, and on the rivers of the globe.

The fourth part of the work, dealing with the forms of the great land masses of the globe, constitutes the largest division of the book, occupying no fewer than 340 pages. After a sketch of the methods employed in surveying and of the different ways of representing the results on maps, a list is given of the chief published topographical maps of different parts of the world. We notice here some singular omissions. The Ordnance maps of England and Wales and of Scotland, on the one-inch scale, are referred to, but there is no mention of the existence of maps on other scales, or of any of the Ordnance maps of Ireland! The maps of the trigonometrical survey of India are included, but no notice is taken of any British colony, although Algeria and Tunis receive full consideration. This is a matter which certainly calls for rectification in a future edition.

The forms of the land surfaces resulting from different kinds of erosion are dealt with somewhat fully on the lines rendered familiar by the writings of Prof. W. M. Davis. Under the title of palæogeography a chapter is devoted to the forms and dimensions of land-masses during former geological periods, and the somewhat problematical questions connected with the subject are dealt with in considerable detail. The map of "Gondwanaland" on p. 587, in which the great continent of Permo-Carboniferous times is made to include, not only India and a large part of Africa, but to extend over Australia and the western half of South America, will naturally excite criticism from those who believe in the permanance of ocean-basins; as will also the map of the world in Cenomanian times, in which, following de Lapparent, Haug, and Frech, the author represents a northern and a southern Atlantis, and, more doubtfully, a Pacific continent. The chapters on glacial and desert conditions, and the surface features resulting from them, are fully up to date, and contain much useful information of a kind not usually found in text-books.

In the 180 pages devoted to "biogeography" there is much useful information included, but opinions will differ as to how far much of this matter should legitimately form a part of a treatise on physical geography. Such subjects as commensalism in plants and animals, the fertilisation of plants, domestication and its influence, mutation and saltation—important as they undoubtedly are—seem scarcely to form a portion of *geographical* science, and if they are included it is difficult to understand why many similar questions are omitted.

Apart from this difficult question of the limits which the author should adopt for his subject, the work before us is a mine of information, and especial praise must be given to the great wealth of useful illustrations it contains. These include 396 figures and maps in the text, 48 plates with very beautiful photographs of scenery, &c., and two folding coloured maps. J. W. J.

THE PREHISTORIC EVOLUTION OF ITALY.

The Stone and Bronze Ages in Italy and Sicily. By T. Eric Peet. Pp. 528; maps and plates. (Oxford: Clarendon Press, 1909.) Price 16s. net.

THIS book gives a clear and exhaustive description of the results of the numerous excavations made by Italian archæologists and a critical discussion of the material obtained. The author succeeds in giving a remarkably complete record of the evolution of culture in Italy from the Palæolithic age down to the Iron age. In arriving at his conclusions he relies almost entirely on technological data, which, though of great value in determining the state of culture of the peoples with which he deals, are of much less value than the data of physical anthropology in solving racial problems. Large numbers of skeletons appear to have been discovered in the immense number of tombs that have been investigated by the Italian archæologists, but only in two or three cases does the author give us the measurements of these skeletons. As a result, many problems have to be left unsolved which with the assistance of physical data would apparently be easily soluble. For instance, a type of Neolithic pottery is found in a cave at Villafrati, in north Sicily, which differs from the Neolithic types found in other parts of the island, and has analogies with pottery found in certain neighbouring countries. The author is unable to decide whether this pottery was introduced by the immigration of a new race or by trade intercourse with foreign countries. He appears to have overlooked the important fact, mentioned by him in a footnote, that four skulls having an average index of 82·2 were found in the same cave as the new type of pottery. Knowing that the average index of the ancient Mediterranean race is 74–75, the physical anthropologist would have no hesitation in saying that the probability was immensely in favour of the new type of pottery being introduced into Sicily by the immigration of a new race.

The difference in the technique of the Neolithic implements and pottery in north and south Italy leads the author to the conclusion that the populations of these regions were two branches of the Mediterranean race who arrived in Italy by different routes. The southern branch almost certainly came by sea from Crete; about the route of the northern branch there is not the same certainty. Towards the end of the Neolithic period, pottery of the "dolmen" type appeared in south Italy, north Sicily, and Sardinia, and superseded the older types.

In the period coming after the Neolithic, which the author, following the Italian archæologists, calls the Eneolithic period, copper makes its appearance alongside of stone. The rock-hewn tomb is introduced in south Italy and Sicily, and a great advance takes place in the technique of stone implements. Several new types of pottery appear. One of these is distinctly Ægean, so there can be no doubt that there was trade intercourse in the Eneolithic period between Crete and south Italy and Sicily.

A remarkable type of pottery occurs in the early Eneolithic period in south-west Sicily in association with rock-hewn tombs. The ornamentation consists of rectilinear patterns painted in black on a ground of "white slip," with which the clay pot is coated. The distribution of this pottery is interesting; it is not found in Crete, but it has been found in Thessaly and in other parts of north Greece; fragments have also been found at Molfetta, in Apulia, south Italy. It looks, therefore, that there was a second route of trade or of migration from the east, across north Greece, the Adriatic, and south Italy to Sicily, which is quite distinct from the Ægean sea route along which the greater part of the trade of south Italy with the east, passed.

The author leans to the view that the great cultural changes of the Eneolithic period were not due to the immigration of a new race, but to foreign influence. Measurements of skulls found associated with the "painted white slip" ware might possibly change this opinion. The average cephalic index of four skulls found at Castelluccio with this ware was 77·9, which looks significantly higher than that of the Mediterranean race.

The Bronze age in Italy is treated topographically. A very painstaking and up-to-date description is given of the material found in the lake dwellings, in the Terremare, and in Bronze-age hut-settlements and caves of north Italy. Chapters are then devoted to the Bronze age in south Italy, and to the Bronze age in Sicily and Sardinia.

In a chapter on the racial problem, the author deals with the racial affinities and origins of the peoples who introduced bronze into Italy. There are two theories in the field, that of Brizio and that of Pigorini. The author favours the latter. According to Pigorini's theory, the hut villages and caverns of the Neolithic age in north Italy were inhabited by a dolichocephalic race (called usually *Ibero-Liguri*) who inhumed their dead. At the end of the Neolithic period a new race appeared in north Italy which cremated its dead. This race planted the first lake dwellings in Lombardy. In the full Bronze age another branch of the same race invaded the eastern district of north Italy, and planted the lake dwellings of the Veneto and the Terremare of Emilia. At the end of the Bronze age, part of the new people crossed the Apennines and entered Tuscany and Latium. This new people Pigorini calls the *Italici*. He considers that they were of the same race as the Swiss lake dwellers, and therefore probably brachycephalic. There is no direct evidence of this, as cremation was an invariable burial custom among the Italian like dwellers and the Terremare folk.

The volume is well printed, contains many excellent illustrations, and four valuable maps showing the distribution of sites in the Neolithic, Eneolithic, and Bronze ages. No student of the prehistory of man in Italy, or indeed in Europe, can dispense with reading this volume. It is, we believe, the first treatise in English which has fully utilised the remarkable archæological discoveries of the last decade in Mediterranean countries, and the author is to be congratulated on the thoroughness and ability with which he has accomplished his task. J. G.

BOTANICAL PHOTOGRAPHS.

Vegetationsbilder. Seventh series. Parts iii. to viii. Part iii., Der nördliche Schwarzwald, by Otto Feucht; part iv., Vegetationsbilder aus Dalmatien, by L. Adamović; part v., Charakterpflanzen des abessinischen Hochlandes, by Felix Rosen; parts vi. and vii., Pflanzenformationen aus Ost-Bolivia, by Th. Herzog; part viii., Vegetationsbilder aus Dänisch-Westgrönland, by M. Rikli. Price 4 marks, each part containing 6 photographs. Edited by Prof. G. Karsten and Prof. H. Schenck. (Jena: Gustav Fischer, 1909–10.)

THIS unique botanical publication is being continually extended, so that a seventh series is now completed. The first double part of the volume, dealing with the colonisation of volcanic lands in Java and Sumatra, has been previously noticed. The third part is devoted to the vegetation of the northern area of the Black Forest, which is characterised by its coniferous trees and moorland. A typical bit of high moor shows bushes of *Pinus montana*, clumps of *Scirpus caespitosus* and tufts of *Juncus squarrosus*. On another plate the same pine is seen as a tall tree, contrasting with the adjacent Scots pine and spruce. The author has also been very successful with his representations of the two subalpines, *Andromeda polifolia* and *Athyrium alpestre*, and of the umbellifer, *Meum athamanticum*. Dr. L. Adamović has brought together a most attractive set of photographs from the sunny climate of Dalmatia. They illustrate a strip of shore and rocks of the littoral, sublittoral and montane regions. So carefully have the spots been chosen and the photographs taken that the author can point out most of the individual plants. Especially charming is the first plate, showing *Aster Tripolium* with species of Statice and *Inula crithmoides* on the shore, and the second picture of sublittoral rock where *Dianthus dalmaticus* and *Iris germanica*, with other plants, are easily recognised. Trees provide the chief feature of the views on the Abyssinian plateau. The rosaceous plant *Hagenia abyssinica*, which tardily assumes its arboreal shape, is very striking; the unripe fruits are esteemed by the natives as a valuable specific for internal complaints. A fine specimen of tree Euphorbia is depicted, which the author suggests may have developed an arboreal form when it passed from a dry to a moist climate. Even more singular is the tree Lobelia, formerly known as Rhynchopetalum. Illustrations are also given of a huge spreading Ficus and a tree Entada.

Dr. Th. Herzog has provided a fine double part relating to the remote territory of East Bolivia. Along the Paraguay, on savannah land subject to inundations, is the home of the wax palm, *Copernicia cerifera*, where it is associated with tall grasses, species of Paspalum and Andropogon. Another plate represents the growth of the palm *Acrocomia Totai*, on the sandstone highlands of Chiquitos. A "monte" or thicket formation occurs on parts of the plain of the Rio Grande, where thorny scrub and succulents predominate; the plates depict species of Cereus, a Trithrinax and the bromeliad *Aechmea polystachya*, which last is valuable to travellers, as it generally holds a store of water. Other palms selected for illustration are *Orbignya palmata* and *Mauritia vinifera*, typical of the savannahs, *Astrocaryum Chonta* and *Iriartea exorrhiza*, denizens of the rain forests; Iriartea produces remarkable thorny prop roots. The last two plates portray succulents, of which *Pilocereus celsianus* is the most striking, on account of its silvery crown of hairs.

The concluding part contains some typical aspects of vegetation in Danish West Greenland. In the southern area birches alone attain to the height of trees as seen in the first plate; the second indicates the importance of *Salix glauca*. A brilliant photograph of an Arctic meadow would be better appreciated if a key to the plants had been supplied. There is an effective photograph of cotton-grass growing by the edge of a lake, and another of clumps of *Glyceria distans* which attract attention on account of the peculiar lie of the stems.

MAGNETIC CHARTS.

Magnetische Kartographie in historisch-kritischer Darstellung. By G. Hellmann. Veröffentlichungen des Kg. Preuss. Meteorologischen Instituts, Abhandlungen, Bd. iii., Nr. 3. Pp. 61. (Berlin: Behrend and Co., 1909.) Price 6 marks.

DR. G. HELLMANN, as head of the Prussian Meteorological Institute, which controls the magnetic observatory at Potsdam, and as a lover and collector of antique magnetic literature, is conspicuously qualified for the work he has undertaken in the present volume. It aims at giving a complete list of all magnetic charts of any importance. All time prior to the year 1700 is regarded by Dr. Hellmann as preceding the era of charts, but he devotes a few pages to Columbus and other pioneers, whose work relates to the discovery that the magnetic needle is usually inclined to the geographical meridian.

Time since 1700 is divided into two periods. The first, extending until 1835, was heralded in by the famous chart of Halley; it is briefly discussed on pp. 10–11. The second period, extending from 1835 to the present day, saw the introduction of magnetic surveys on land.

The earliest work of this kind, according to our author, took place in England on the initiative of the British Association. On pp. 11–17 there is an enumeration of all the principal land surveys; while pp. 18–27 summarise the present state of our knowledge of the distribution of the magnetic elements. There is a useful list on p. 26 of the epochs of the principal surveys since 1891, with particulars as to the number and density of distribution of the stations. A number of propositions are laid down in pp. 28–29 as to the objects to be aimed at in magnetic surveys and other work preliminary to the construction of charts. The author considers it most important that the exact observational work at sea commenced under the auspices of the Carnegie Institution should be extended as soon as possible to all seas. He advocates international cooperation to ensure continuity in the drawings of magnetic lines in frontier districts, and emphasises the importance of adequate determinations of secular change.

The terminology, units, &c., employed in the description of the charts are explained in pp. 30–31. The charts themselves are divided into those dealing with the whole or the greater part of the earth, those confined to the oceans, those dealing with the several continents, and, finally, those devoted to individual countries or districts. The information given usually includes the area, the epoch, the magnetic element or elements dealt with, the interval—in specified units—between the successive isogonal, isoclinal, or isomagnetic lines, the geographical scale of the map, also the locus and date of publication. The title in each case, when there is one, is given in the original language. There is a separate list on pp. 60–61 of charts based on theory.

The list of charts seems very complete. As evidence that it is up to date may be mentioned the fact that it includes the British and American world charts published respectively in 1906 and 1907, Commander Chetwynd's charts of the South Polar regions published in 1908, Dr. Schmidt's charts of North Germany, and Prof. Beattie's South African charts published in 1909. The volume is clearly printed in good-sized type, and should prove a valuable work of reference.

C. CHREE.

ELECTRICAL BIOGRAPHY.

Makers of Electricity. By Brother Potamian and Prof. James J. Walsh. Pp. vi+404. (New York: Fordham University Press, 1909.)

THIS is not a work on central-station engineers, but a series of biographical sketches of the chief pioneers in the science of electricity in its historical development. Of these sketches there are twelve, as follows:—Peregrinus and Columbus; Norman and Gilbert; Franklin and some of his contemporaries; Galvani; Volta; Coulomb; Oersted; Ampère; Ohm; Faraday; Clerk Maxwell; Lord Kelvin. As the first three, together with those on Oersted and Lord Kelvin, are signed by Brother Potamian, it may be assumed that the rest are by his colleague, Dr. Walsh, who is the author of several others works, "Makers of Modern Science," "Catholic Churchmen in Science," "Makers of Modern Medicine," and "The Popes and Science," which appear to have a great vogue amongst Roman Catholic readers in the United States. Brother Potamian, better known to his English friends as Dr. O'Reilly, is one of those who has made the bibliographical history of electricity his own; and his masterly annotations of the catalogue of the Wheeler collection of electrical books (formerly the library of the late Mr. Latimer Clark) in the possession of the American Institute of Electrical Engineers show him to possess abundant qualifications for writing biographies of the pioneers. If the chapters on Peregrinus and Columbus, Norman and Gilbert, add nothing to previous knowledge, they are valuable in presenting very readable summaries of the results of recent antiquarian research into the achievements of these early investigators of magnetism. The account of Peregrinus is particularly good, and avoids errors too often attaching to accounts of his long-forgotten discoveries. The article on Gilbert is also replete with the details which have been unearthed in recent years, though by a slip on p. 49 he is said to have blamed Stevinus for certain "vain and absurd" views about the variation of the compass in southern regions of the earth. It was not Stevinus whom he blamed, but "certain unnamed Portuguese mariners." Gilbert's Copernican views are discussed fully, and criticised.

Franklin's work in electrical observation is treated at some length, as is natural in a work intended primarily for American readers; but all readers should be grateful for the very clear way in which Brother Potamian has laid out the historical position of Franklin with respect to those contemporaries of his—De Romas, d'Alibard, and Divisch—who have been alleged to have anticipated him with respect either to the kite experiment or the invention of the lightning rod. One amusing reminiscence is recorded in this chapter of the controversy which arose upon knobs *versus* points, and was referred to a committee of the Royal Society. In that committee the Hon. Henry Cavendish and Dr. Benjamin Wilson were opposing partisans. Sir John Pringle, the President of the Royal Society, supported Cavendish in favour of using points. But points had been advocated by Franklin, whom to support at that moment was "unpatriotic." His Majesty George III. accordingly ordered that the points of the lightning conductors at Kew Palace should be replaced by balls; whereupon Sir John Pringle, replying with dignity, "Sire, I cannot reverse the laws and operations of nature," resigned the presidency. This evoked the following witty epigram:—

> While you, great George, for knowledge hunt,
> And sharp conductors change to blunt,
> The nation's out of joint;
> Franklin a wiser course pursues,
> And all your thunder useless views
> By keeping to the point.

The chapters devoted to Galvani and to Volta call for little comment. That on Coulomb gives a better biography than is accessible in English elsewhere. Those on Oersted, Ampère and Ohm are each good in their way; but that on Ohm lacks proportion. One might think that the whole of mathematical physics began and ended with Ohm's "Law."

The lives of Faraday, Clerk Maxwell, and Lord Kelvin are compiled with a knowledge and sympathetic comprehension. The one phrase to which one must take exception in the account of Lord Kelvin is the suggestion—*apropos* of Lord Kelvin's saying at his jubilee that the most strenuous of his efforts for the advancement of science had ended in "failure"—that "because Dame Nature did not open to his sesame, but persisted in her reticence, the philosopher grew pessimistic and disappointed." "Pessimistic" is the very last adjective to be applied to Lord Kelvin in his cheery and undaunted battling to the last with the deepest problem of mathematical physics. No trace of disappointment soured the serene close of his strenuous life.

There are eight portraits and a score of illustrative cuts in the work, which is well and clearly printed.

OUR BOOK SHELF.

Syllabus of the Lessons on Marine Biology for Fishermen, given at the Marine Laboratory, Piel, Barrow-in-Furness, by the Lancashire and Western Sea-Fisheries Joint Committee. Revised January, 1910. Pp. 35; 7 plates. (Liverpool: C. Tinling and Co., Ltd., 1910.)

THIS handbook, which has been prepared by Mr. James Johnstone, is written in a clear, direct style, and is illustrated by good text-figures and seven excellent plates. The author is to be especially congratulated on the skill with which he has, throughout the book, avoided the use of technical terms without sacrificing scientific accuracy. The desire to avoid the use of the word protoplasm has, however, led to the use of another term in an unusual sense; on p. 13 the author, dealing with Peridinians, writes:—"They, like the diatoms, are jelly-fish, and have shells . . . ." It would be better to avoid the use of the term jelly-fish, in such a connection, in view of its more generally accepted application to organisms of a higher class.

The book contains outlines of lessons on those branches of marine biology which are of special interest to fishermen—the general anatomy, physiology and development of the mussel, the structure of the cockle, the food of these molluscs; the structure of shrimps, crabs and lobsters, their growth, "casting" (ecdysis) and reproduction; the anatomy of the haddock or whiting, the fecundity of various fishes, especially of flat fishes, that of the flounder being studied in detail; the food in the sea, plankton; the different kinds of spawn found on the shore; the elementary chemistry of air and water, the temperature of the sea, &c. This list will serve to show the range of subjects comprised in this admirably planned course of scientific instruction. The book is certain to stimulate the interest, not only of those who attend the classes, but also of other fishermen, to whose notice it will be brought by their more fortunate fellow-workers who have passed through the classes and used the book.

The Sun a Habitable Body like the Earth. By Sree Benoybhushan Raha Dass. Pp. xiv+130. (Naldha: Published by the Author, 1909.) Price 5s., or 3 rupees.

THIS is, typically, a book "published (and distributed) by the author," and perhaps the kindest statement to make about it is that it is an anachronism. Apparently the author attempts to explain all solar phenomena as electrical effects, and, as a prelude, describes the actions of, and discharges from, insulated conductors; but the language is so often obscure, and, where intelligible, is so devoid of connected reasoning, that no clear idea can be obtained as to the ultimate conclusions. Quotations from great authorities, including Herschel's conclusion as to the sun's habitability, give the volume itself an air of authority which is rudely dispelled on a closer acquaintance.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. *No notice is taken of anonymous communications.]*

The "Reindeer" from the Lorthet Grotto.

I HAVE just been consulting in *Science Progress* for July, 1909, the very interesting paper of Prof. Sollas on the Palæolithic races, and I venture to direct attention to the title of an illustration on p. 25. It is entitled "Reindeer and Salmon Incised on a Piece of Horn from Lorthet." This legend is taken from the original figure by the late M. Piette in *L'Anthropologie*, 1904, p. 160; but is the Cervus there engraved rightly ascribed to *tarandus?* Is it not *megaceros? Tarandus* has no brow tines like those delineated on this horn plaque. They are more or less palmated, while in *megaceros* they are differently directed, present a different section, and are bifid as are those figured in *Science Progress.* It may or may not be now or later of importance to ascribe correctly this particular drawing, but the determination of the species in prehistoric cave-engravings has an important bearing on the age and climate of the horizon from which they come.

May I venture, if Prof. Sollas will allow me, to refer also to p. 26 of the same important contribution, where occur the words ". . . Saiga antelope, the same animal as that which is sculptured in so masterly a manner on the spear-thrower mentioned on p. 20 (Fig. 3)." The animal sculptured—also after M. Piette's figure in *L'Anthropologie*—on the implement (from Mas d'Azil) referred to can hardly be a *Saiga.* The position and form of the nostrils and the uninflated nose-sac which the side-view reveals preclude this determination. The creature must be a goat or a chamois, or belong to a nearly related genus.

HENRY O. FORBES.

The Museums, Liverpool, March 20.

Centre of Gravity of Annual Rainfall.

THE ordinary method of exhibiting the annual distribution of rainfall for any station or country is a graph the ordinates of which represent the monthly rainfall. Though this pictorial method is both useful and interesting, it does not lend itself to the ready comparison of a number of such graphs for different places or for the same place in different years. By a similar graphic method we can exhibit the yearly rainfall totals for a number of years, but we cannot show differences of internal distribution for each year unless we are at the trouble to graph each month of each year separately.

Another method, which may be called the analytical method, I have been applying recently to the study of the rainfall of the province of Mysore, India, upon which I had to report officially from 1893 to 1908, and I have been much surprised at the results brought out by this method. The same method applied to the rainfall statistics of England, Scotland, and Ireland, as given by Dr. H. R. Mill in "British Rainfall" for 1908, shows also curious and interesting results.

The method consists in the application of the well-known formula for finding the position of the centre of gravity of a number of weights placed along a straight rod, viz. $X = \Sigma(wx) \div \Sigma(w)$. If we imagine the rainfall for the months of the year January, February, . . ., December to be weights placed along an axis at distances 1, 2 . . . 12 units from the Origin, or end of the axis (January 1), multiply each month's rainfall by its distance from the Origin, and divide the sum of the products by the total rainfall for the year, we get the position (or date) at which the year's rainfall might be supposed to have fallen all together to give the same effect as the separate monthly falls.

The Mysore Province, which has about the same area as Scotland without the Isles (28,000 square miles), is divided into eight *Districts*, which differ greatly in the amount of yearly rainfall, as well as in the monthly distribution. Each *District* is divided into a number of parishes, called *Taluks*, and the rainfall is gauged at the chief town of each Taluk. The mean of these is taken as the rainfall for each District. From the average monthly rainfall of each District for the past thirty-nine years I have found the C.G., also for the year 1908, and they are given for comparison :—

District.	No. of Taluks.	Average for 39 years. Rainfall	C.G.	1908. Rainfall.	C.G.
1. Bangalore ...	10	30·48	7·93	25·49	6·47
2. Mysore ...	13	27·53	7·74	24·24	6·64
3. Hassan ...	8	37·95	7·62	27·98	6·74
4. Chitaldrug...	9	21·10	7·51	14·03	6·93
5. Tumkur ...	10	25·90	8·01	15·76	7·00
6. Kolar	11	27·89	8·14	16·00	7·07
7. Shimoga ...	9	66·60	7·34	67·31	7·14
8. Kadur	7	73·00	7·40	63·74	7·06
Province ...	77	36·79	7·68	29·94	6·91

The Shimoga and Kadur districts each include three stations where the rainfall is enormously greater than at the other stations; yet though the thirty-nine-years' average annual rainfall for six of the Shimoga Taluk stations is only 35·78 inches, and for the three stations of great rainfall it is 128·24 inches, I find that the mean position of the C.G. is 7·28 for these three stations, while for the whole nine stations it is 7·34. In the same Shimoga District there are, besides the nine Taluk stations, fourteen additional rain-gauge stations, among which are Agumbi, with a mean yearly rainfall of 333·17 inches, Aralagode, with mean of 237·79 inches, and Karur, with mean of 115·79 inches, and I find the C.G. for these is at 7·29, 7·21, and 7·13 respectively.

It is to be noted that the great deficiency of rainfall throughout Mysore Province as a whole for the year 1908 is indicated, not only by the diminished yearly totals, but by the displacements of the C.G. for each District and for the whole Province. This means, of course, that the deficiency was in the "latter rains"—or those for the north-east monsoon—but the important thing is that we have a simple numerical measure, by combining the displacement of the C.G. and the total rainfall defect, of the real rainfall deficiency for the year. Thus while the rainfall average for the whole Province was 18·6 per cent. less than the yearly normal, the deficiency of the *rain-moment*, as we may call it in the language of mechanics, was 26·8 per cent., which agrees better with the agricultural effect.

This has led me to examine Dr. H. R. Mill's "British Rainfall" for 1908, and the results of working out the C.G. for a large number of stations, and for the 1908 mean rainfall of England, Scotland, and Ireland, are interesting. The position of the C.G. for the monthly mean rainfall of 122 stations in England and Wales is 6·54, of 55 stations in Scotland 6·37, of 53 stations in Ireland 6·72, and of 230 stations in the whole British Isles 6·55. For Greenwich, with rainfall 23·78 inches, it is 6·48; for Borrowdale (Cumberland), with rainfall of 127·38 inches, it is 6·54; for Glenquoich (Inverness), with 107·40 inches, it is 6·21; for Kenmare (Co. Kerry), with 70·91 inches, it is 6·59.

From the Journal of the Scottish Meteorological Society for 1908 I find the following results :—

	Year's Rainfall.	C.G.
Means of the eight principal towns of Scotland	33·05	6·54
Means for all Scotland for 1908	37·55	6·48
Means for all Scotland for fifty years (1856–1905)	39·19	6·87
Means for 1908 of eighteen Lighthouses on the Scottish coast	30·74	6·68

It is remarkable that the rainfall should be so small at the Lighthouses, and that the law of rain-distribution throughout the year should agree with that for the land-stations. The smallest rainfall for 1908 was at the Isle of May Lighthouse, where it was only 18·33 inches, with C.G. at 6·90; and the heaviest rainfall was at Ardnamurchan Lighthouse, where it was 50·99 inches, with C.G. at 6·63.

This method is readily applicable to the graphic presentation for a series of years either of the C.G. or of the *rain-moment*. Thus I have worked out the results for Bangalore from 1867 to 1908, and find that while the average position of the C.G. is 7·81, the positions for 1875 and 1876, the successive droughts of which caused the great Mysore famine, were 6·82 and 6·72, and while the average *rain-moment* is 276, it was for those years only 151 and 117 respectively. I also find that for the two years 1907 and 1908 the C.G. for Bangalore was at 6·77 and 6·08 respectively, and that the rain-moments were 214 and 157; which agree with the fact that Mysore narrowly escaped another serious famine quite recently, and give a measure of the margin by which it escaped the disaster caused by the rain deficiency of 1875 and 1876.

It is evident that we might easily graph on the same sheet for a sequence of years (1) the total rainfall; (2) its yearly C.G.; and (3) its rain-moment or coefficient. This principle will also give the data for charts of the general distribution of rainfall in a country for any year or series of years. That each station and country has its rain-constant which can be expressed numerically seems to be more than a mere theoretical curiosity.

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30 Hermitage Gardens, Edinburgh.

Lycopodium Spores.

MISS EDITH A. STONEY states (NATURE, January 6, vol. lxxxii., p. 279) that with a large aperture microscope objective and oblique illumination, Lycopodium spores are seen to be coated with hair-like projections. We believe this appearance to be illusory. Owing to the transparency of the outermost layer of cells, the margin of the spore is quite invisible under certain conditions, giving to the radial cell walls the appearance of hair-like projections.

Photomicrographs of some of these spores reproduced in the *Physikalische Zeitschrift* of February 1, p. 78, show the effect in question in some parts of the field, and evidence the correctness of the explanation given.

JOHN ZELENY.
L. W. MCKEEHAN.

Dr. H. J. Hansen and the Copenhagen Museum of Zoology.

I BEG permission to acknowledge the receipt of the open letter sent me through your Journal of March 10, by the leading zoologists of Great Britain and Ireland, regarding my resignation from the Copenhagen Museum and my zoological investigations. I am deeply conscious of the great honour done me in sending me such an address, and I regret that I am unable to write to all personally; but for that reason I would request them through your columns to accept my most sincere and heartfelt thanks.

H. J. HANSEN.

5te Juni Plads No. 1, Kjöbenhavn, F., March 17.

Title of the Natural History Museum.

WHAT has history, in its present sense, to do with the subject? What have the Muses to do with it? Certainly Terpsichore is not included at any of the museums. The N.H.M.(B.M.) is not a museum, but a Natureum. Might not a ten-syllable name on the other side of the way be replaced by the Arteum? Then Bloomsbury might use the name Historeum. The address need not include London or England, as no other place uses these terms. For all scientific reference one word would be complete.

W. M. F. P.

The Meaning of Ionisation.

IN his lecture at the Royal Institution on March 11, Dr. Brereton Baker proposed the term electromerisation instead of ionisation when applied to gases. May I venture to suggest the word "electronisation" as more euphonious, and as indicating the essential difference in the process, viz. the freeing of electrons instead of ions?

W. DEANE BUTCHER.

Holyrood, Ealing, March 18.

NUMERALISED PROFILES FOR CLASSIFICATION AND RECOGNITION.

WHEN children or savages attempt to draw a human profile, the result is usually a rude figure that lays stress on five cardinal points. These are the notch between the brow and the nose, the tip of the nose, the notch between the nose and the upper lip, the parting of the lips, and the tip of the chin. Supposing these five points, B′, N′, U′, L′, and C′, to be located with fair precision, as will shortly be shown to be feasible, then Fig. 1 is directly deducible from them, together with the vertical and horizontal axes, C′B′, and C′X at right angles to C′B′. The position of the five cardinal points varies in different profiles much more than the probable error of measurement. So though Fig. 1 is a mere skeleton, which determines what may be called the set of the features, and corresponds to the primary triangulation of a country, other points are to be derived from it, and similarly utilised. Among these are the intersections with the outline by perpendiculars, drawn from the middle or other specified division of the lines. This skeleton serves as an excellent basis for the classification of profiles and for anthropological statistics.

Peculiarities of profile, as a racial or family characteristic, can be expressed numerically by an extension of this system in a way that promises to be serviceable for eugenic records. It was, in fact, largely with this object in view that I began the

FIG. I

FIG. 2

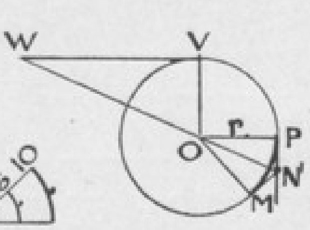

FIG. 3

inquiry. The replacement in all scientific work by numerical values, in the place of vague adjectives, is a gain of first-class importance. There is no way known to me, other than this, by which likenesses can be "lexiconised," that is, arranged as words in a dictionary. A needed portrait may by its means be discovered by a formula, as a spoken word is found in a dictionary, by the letters that express its sound. There are many simple purposes of newspaper interest to which this same method might be applied, but with more elaboration.

The practice of cataloguing profiles may perhaps become useful as a secondary means of identification when the number of persons who may require to be identified shall have become too large to be readily dealt with by finger-prints alone.

It will be shown (Fig. 5) that four telegraphic "words" are sufficient to convey a very fair profile likeness. The cost of sending an extra four words by telegram to any part of the British Isles being only twopence, and of a moderate amount over-seas, the practice of telegraphing profiles of persons of current interest, might become common. A refugee criminal could easily be outstripped by his portrait, sufficiently like to him to justify, in connection with corroborative evidence, his being placed for a while under police observation. The measures of profiles must, of course, be reduced to uniformity. Thus, by utilising two out of the five cardinal points to give direction and scale, the mean positions of the remaining three points may be determined for any given race or family, together with the frequency of deviations of any given amount from those mean positions, and such other deductions as can be reached by the modern methods of statistics.

The corrected values are here described by the same letters as the original ones, but without the dashes. The standard scale that is used is such that BC, the corrected value of B′C′, shall be always 50 units in length (see Fig. 5). The reduction is, of course, effected by multiplying each measure in the portrait by 50 divided by its B′C′. The number 50 is preferable to 100, which would probably first suggest itself, for a variety of practical reasons, into which I need not now enter. Two figures are assigned to each measure, so the values 0, 1, 2, -.9, have to be written 00, 01, 02, .09. The measures are recorded to the nearest integer, there being no room for fractions, decimal or other. A millimetre is a convenient unit for purposes of drawing, more so than one-tenth of an inch; therefore, in reproducing the corrected measures, BC becomes 50 millimetres, and the other measures are altered in the same proportion.

A thick beard interferes with determining L′ and U′, but their positions can usually be inferred with a useful degree of precision in moderately bearded faces.

The accuracy with which the five cardinal points can be located differs considerably. The most exact determinations in an unbearded face are those of the points C′ and N′, and the direction of the line C′B′. U′ comes next in order of exactness, then B′, and, lastly, L′. The distance between a line joining C′N′ and a parallel line tangential to U′, can be fixed with precision but is not used here. C′ and N′ are each defined by the intersection of two tangents, as shown for N′ in Fig. 3.

It is well to examine these conditions more closely, as they bear on the treatment of curvatures generally. A knowledge of them permits rough and ready drawing, in which the principal matters are attended to, the less essential ones being more or less disregarded. One of the tangents is parallel to C′B′, which is treated as vertical; the other is inclined to the vertical at 45°. Consequently, the curve of N′ is contained in an obtuse angle of $180° - 45° = 135°$. The tip of each prominence and the bottom of each hollow is represented by one or other of the three short circular arcs shown in Fig. 2, which are sufficiently numerous for the purposes to which they are here applied. The centres of all circles that touch both the vertical and the diagonal will necessarily lie in the line that bisects the obtuse angle between them; consequently, N′O forms an angle with the vertical that is equal to half 135°, or $67\frac{1}{2}°$. The tangent of this angle is 2·4142; therefore the position of the line of centres may be found by laying off a point V in a vertical direction, at 10 units of length from O, and by drawing another line from V horizontally to W, at a distance from it equal to 24·14 of the same units as before. Then the line of centres passes through O and W. It is easily shown (Fig. 3) that the points of contact between the circle and the two tangents are exactly 45° of arc apart. The length of the chord of that angle is equal to about three-quarters of its radius. The shortness of the chord, when the radius is small, is well seen in Fig. 2, and must be borne in mind; it accounts for the scarcely noticeable differences in the curvatures, and consequently for the fewness of the standard arcs that are necessary. The arc of 45° is shown by a heavy line in Fig. 3, where the circle has a radius of 10 mm. There is often a colloquial confusion between the obliquity of the planes between which an edge lies and that of the sides of the edge itself. The former may be very acute, and the angle of the edge would be equally acute if the planes were prolonged until they met; but usually they do not meet, the edge itself being more or less rounded. The acutely inclined faces of a knife may have a blunted edge, that fails to cut the skin without much pressure, while a broken piece of glass, the fracture of which is perpendicular to its face, but the edge of which is not blunted, only too readily makes a gash.

The arcs of 45° in Fig. 2 all refer to the cutting edge, so to speak. The direction of the lines within which the cutting edge is situated is determined by the adjacent cardinal points towards which they point.

Referring back to Fig. 3, $OP = OM = r$, the radius, $ON = r \times 1{\cdot}082$, therefore the distance between N and the circle is only $r \times 0{\cdot}82$, which for a radius = 10 mm. is about three-quarters of 1 mm. This small value is diminished in proportion for lesser values of r (see Fig. 2), so for practical purposes N (and similarly C) may be considered to lie only just outside the convex circumference of the arc by which they are in each case represented.

The values of r which are used as standards for the lesser curvatures are 2 mm., 6 mm., and 10 mm. The drawings in Fig. 4 are not exactly on this scale, but the differences are unimportant. It is unnecessary to divide these small curves into concave and convex, as their condition in that respect is indicated by the part they play in the profile. Two other curvatures of the larger radii, 25 mm. and 30 mm., are used to express and to define the concavity or the convexity of the ridge of the nose.

It is well, at the risk of some repetition, to describe in a single paragraph the nomenclature of the five cardinal points in the original portrait. B′ is the point in the fronto-nasal notch at what is judged to be its deepest part; N′ is the tip of the nose, found in the way already described; U′ is the point of contact between the naso-labial notch and a tangent, drawn diagonally to it; L′ is a point half-way between the furthest positions at which the lips would touch one another if they were lightly closed; C′ is found by a similar method to that used for N′.

The portraits are described by numerical formulæ. Each formula consists of four groups of figures, five figures in each group. The shapes of the profile at and immediately adjacent to the cardinal points, and those of the intermediate links, are expressed by single numerals, as set forth in tabular form in Fig. 4. Not more than 0 to 9, or 10, varieties of shape are provided in each case. Thus, the radius of the standard curve that best fits the fronto-nasal notch, *b*, is expressed by its appropriate numeral, as shown in the first line of Fig. 4; also the inclination of the brow immediately above *b*, whether it slopes forwards, backwards, or is upright. The ridge of the nose *g* is counted as either sinuous, concave, or convex, in two or three different degrees, or else as straight. The letter *n* includes both the very tip of the nose and the outline underneath it, which leads towards the naso-labial notch. The letter *u* includes the naso-labial notch and the first portion of the upper lip. The lips require two statements, and therefore two separate figures; the former, *lp*, shows whether the lips are shut, parted in the portrait by 1 or 2 mm., or open by 3 mm. or more, and, again, whether they project evenly, are overhung or underhung. The latter notation, *ll*, expresses the sizes of the upper and lower lips respectively, whether they are small, medium, or large. The outline between the lower lip and the chin is always notched, and *k* describes the size and position of the notch, whether it be small, medium, or large, and whether it be high, medium, or low. The curve of the chin itself at C′ is not given.

I have called these profiles "numeralised" to express the fact that they are transformed into numerical formulæ. Twenty figures enter into each formula; they are arranged, as for telegraphy, in the way already described, into four groups of five figures in each group. A "figure," in telegraphic language, includes not only the ten numerals, 0 to 9, but the three symbols in addition, of a stop (.), a hyphen (-), and a short oblique line (/), such as is used in fractions. The arrangement in groups of five, or in "quintets," proved suitable to other similar work on which I was engaged, so it has been adopted throughout. In the four quintets, or, we may say, in the four *words* that compose a formula, the first three refer respectively to N, U and L, and in that order. The first two and the last two figures, in each of the first three quintets, give the position of the point in question in X and in Y to the nearest whole millimetre. The middle figure of the quintet is derived from Fig. 4 to describe the peculiarities of the profile at and immediately adjacent to that point. The fourth and last of the quintets is preceded by a dot (.), to show that it belongs to a separate category,

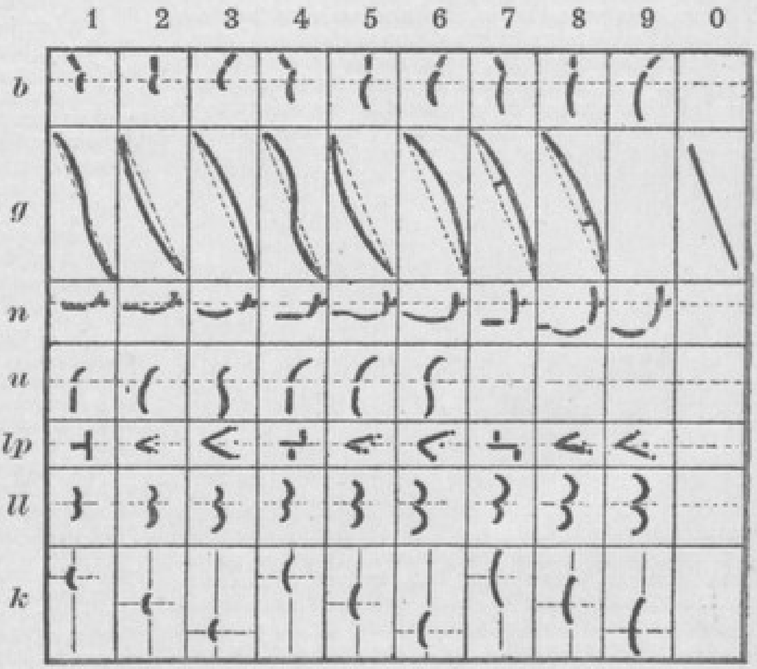

FIG. 4.

namely, to the peculiarities of *b*, *g*, *ll*, and *k*, as set forth in Fig. 4, and in that order. When proceeding to draw a figure from a formula, it is advisable for a beginner to use tracing paper. Then, after drawing an arbitrary line in any convenient place, of 50 millimetres in length, to serve for BC and, therefore, for the vertical axis, and another line CX, at right angles to CB at C, for the horizontal axis, to plot the positions of N, U, and L; then, laying the transparent paper upon Fig. 4, to trace, or copy on an enlarged or reduced form, according to the space available, the figures of *n*, *u*, and *lp*, very faintly. Next to do the same to *b*, *g*, *ll*, and *k*. Afterwards to harmonise the whole tentatively, with faint and brush-like strokes; lastly, with a free and firm hand to draw the outline through them. Tracing paper may otherwise be convenient, because when the original profile looks to the left, by the simple act of turning the traced outline it affords an almost equally clear profile, looking to the right.

When transforming the portrait into a formula, the reverse process has to be followed with little alteration. Before finally adopting any formula, the portrait should be reconstructed from it and the formula revised where necessary. It is easy after a little practice mentally to compose a formula so far as the seven small letters are concerned, from a brief inspection, either of the picture or of the living face; also to reproduce by copying by eye the symbols from Fig. 4 without caring to trace them. In short, the whole operation may be satisfactorily gone through by an intelligent person in a rapid and off-hand way. This might become a popular game for the members of a party to practise their art upon one another, care being taken that the five cardinal points should be truly laid down, perhaps by tracing a shadow.

Eight couplets of very different features are given in Fig. 5, both for illustration and for the reader to practise upon. Each couplet contains the original portrait on the left, its formula along the bottom, and the reproduction from the formula (to the standard scale) on the right.

It will be seen from Fig. 5 that four-word portraits are by no means deficient in resemblance to their originals. I think they are considerably more like to them than the sketches, usually printed in the illustrated newspapers, are to the public characters whom they profess to represent. They are, to say the least, of considerable negative value, sufficing to eliminate at the rate of about nineteen out of every twenty individuals as *not* being the person referred to.

Any form of telegraphy suffices to transmit these four-word profiles. In other respects they are far inferior to those complete pictures now transmitted between certain offices, by means of costly and delicate apparatus, by a method at present not developed to its utmost.

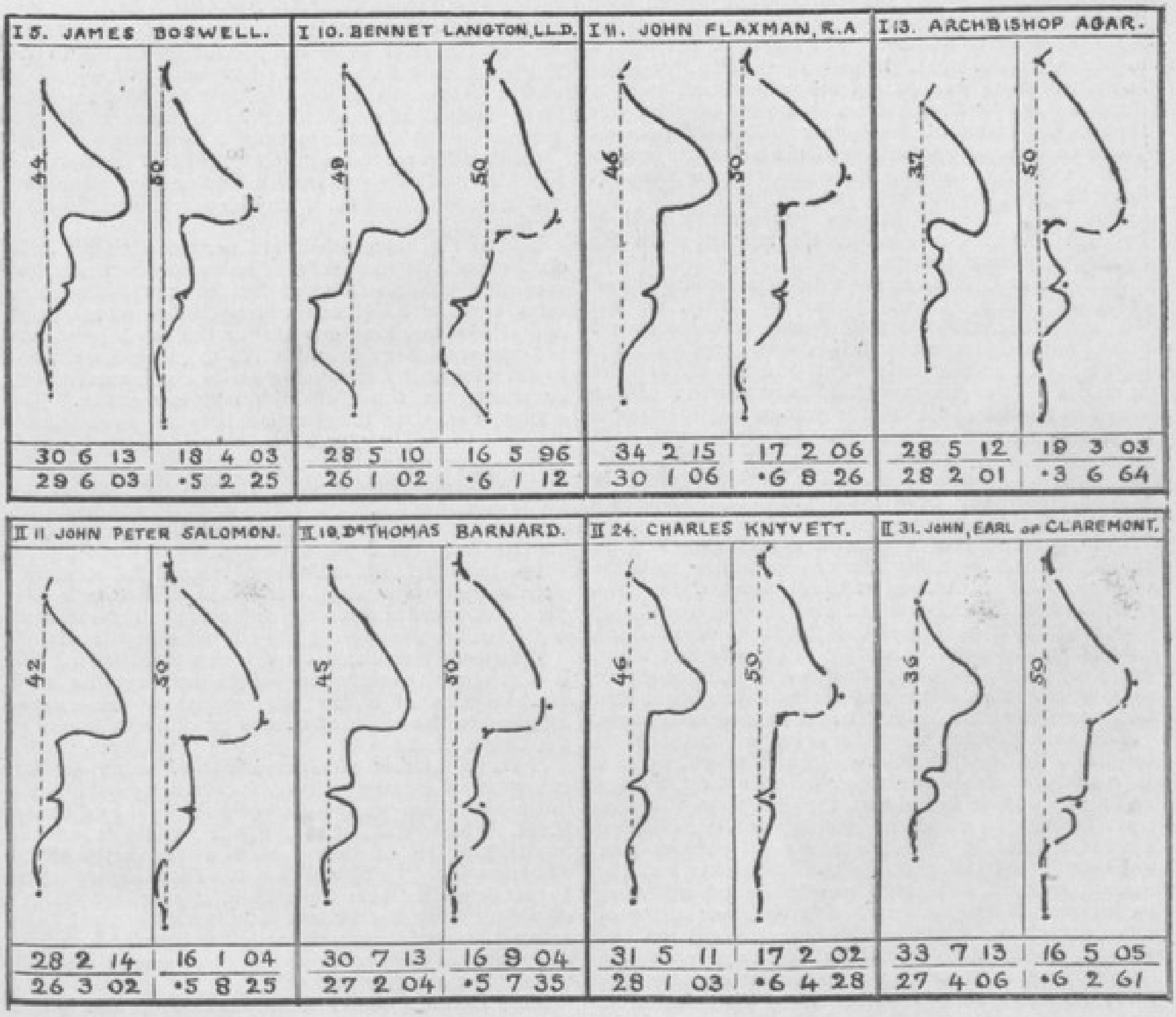

FIG. 5.

Explanation of the first formula, namely, that of James Boswell; the others are to be read on the same principle. N_x, 30; N_y, 13. U_x, 28; U_y, 03. L_x, 18; L_y, 03. The small letters are, *n*, 6; *u*, 6; *lp*, 4. *b*, 5; *g*, 2; *ll*, 2; *k*, 5.

It will be observed that in the second of the portraits, namely, that of Dr. Bennet Langton, the point L lies to the left of CY, and has therefore a negative value. This is −04, but is expressed here as 96, an artifice which practically transfers the horizontal measurement from CY to another vertical line drawn parallel to CY and 100 mm. to its left. No confusion need arise through this transformation, since it leads to very large values lying adjacent to very small ones, and therefore showing that they belong to a different category. The *minus* values in Y are similarly treated. The process may also be extended beyond the eight squares of 0 to 100 mm. in their sides, that surround the primary one.

My experiments have been chiefly made upon the "Collection of Portraits by George Dance, R.A., Sketched from Life and Engraved in Imitation of the Original Drawings" (Longmans and Co., 1809). They were convenient to work with, being all drawn on scales differing little from that of the standard. All the portraits are unbearded and in exact profile, with three or four exceptions. Those that are available are sixty-eight in number. The name of the person to whom each of the eight portraits in Fig. 5 applies is written along its top, and the volume and page of the two folios by Dance, from which the original was traced, are given in the upper left-hand corners. There are several notabilities in his collection besides those in Fig. 5. Among them are Horace Walpole, General Paoli, Haydn, and John Philip Kemble. An exhibition of Dance's pictures was recently held in London. He had a considerable reputation in his time as a portrait painter.

Methods have been used to aid the recollection of dates and other figures. That by Gray, in his "Memoria Technica," was to transform each numeral into either a consonant or into a vowel or diphthong, as might be the most convenient, and thereby to build up words easy to pronounce and to remember. Those who are familiar with such a process might apply it here, and convert the four quintets of numerals into four words, getting over the difficulty of employing the three additional symbols as best they can. If they succeed, the phrase of "four-word profiles" would be literally exact.

I do not find that a general resemblance can be much increased by using one or a *few* more quintets or words. A fifth, or even a sixth, quintet might, however, be usefully employed in extending the range of the profile, if it contained one figure to describe the chin and just below it, another to describe the brow, and two figures, 00 to 99, which would perhaps suffice to give the size and general shape of the head, also to define the mustachio and beard of unshaven faces.

The next distinct stage in order of accuracy is separated by a great distance from the present one. It requires so large a number of dots that straight or slightly curved lines drawn through them will flow smoothly when seen at the ordinary reading distance from the eye. It needs as many as perhaps fifty quintets to describe a profile with exactness and the rest of the head with rough precision, and still more to include the eye and ear. I have made many of these, which, when reduced to the standard scale of BC=50 mm., are practically identical with the originals, when viewed in a somewhat careless way by a normally sighted person at a distance of 12 inches. A special use is made in this case of the middle figure of the quintet. Thus, the numeral 1 means that a half-unit is to be added to the first two figures; 2, that it is to be added to the last two; and 3, that it is to be added to both of them. This power of doubly minute description is often wanted in the outline that joins and includes the nose-tip and the two lips. Another use for the middle figure of the quintet is to tell that a dotted line should be drawn from the preceding point, to signify doubt of some kind. A hyphen (-) in the middle of the quintet means to begin; an oblique line (/) to end; and a point (.) means an isolated point. But I will not go further into this now; neither will I do more than hint at the way of dealing with portraits that are not in exact profile, by multiplying their horizontal measures into the secant of the angle through which the profiles are turned away from it.

Much more might be added on extensions of this method, especially as regards its facilities and limitations in conveying plans—ceremonial, strategic, and others—for newspaper use. But its general principles have been explained, and as this article is already too long I will end it abruptly here.

FRANCIS GALTON.

TIDAL OBSERVATIONS IN THE ENGLISH CHANNEL AND NORTH SEA.

FOR the purpose of tracing correctly the progress of the tidal wave throughout its course in the English Channel and North Sea, observations of the vertical movement of the tide at a distance from the land, and similar observations by means of tide gauges on the shore, are equally necessary.

Information on the rise and fall of tide far from the shore may be obtained from a ship or boat at anchor, and in certain localities it may be of considerable value to the navigator.

But, if the observations are carried out with sufficient exactitude to satisfy scientific requirements, the procedure hitherto followed for that purpose necessitates weather conditions which do not often occur, and seldom last long enough for the object in view.

There are other difficulties also to contend with, due to the stream of tide running at its maximum rate at, or about, the times of high and low water. The stream, reversing its direction between those times, causes a mark buoy, or boat, however, tautly moored, to swing over a certain area during the interval; the undulating character of the surface of the ground, and the action of the strong tidal stream on the lead-line, thus tend to introduce elements of uncertainty which increase with the depth of water.

The practical difficulties experienced in obtaining strictly accurate results by this means involve loss of time disproportionate to the value of the observations, and therefore the attempt has not often been made.

Trustworthy evidence on the rise and fall of tide is thus almost entirely confined to the coast-line, with the exception of a few observations of tide-gauges attached to the masts of wrecks on off-lying banks in the North Sea.

A large number of observations of value for the reduction of soundings have, however, been obtained in the North Sea during the years 1886–90 by Captain T. H. Tizard, R.N., C.B., F.R.S., while commanding H.M.S. *Triton* in the course of the survey of the shoals fronting the Norfolk coast. These, being taken by the lead-line from the ship at anchor, on the assumption that the bottom was perfectly level, can scarcely be considered sufficiently trustworthy for scientific purposes until confirmed by more precise methods which were not then available.

More rigorous observations were carried out in the North Sea by the late Captain W. Hewett, R.N., commanding H.M. surveying ship *Fairy* in 1838 and 1840, with the object of verifying the prediction of the late Dr. Whewell as to the existence of an area situated eastward of Orfordness and about midway between the coasts of England and Holland, where the rise and fall of tide was expected to vanish.

The method employed by Captain Hewett was to moor a boat head and stern as tautly as possible by means of lead-lines attached to anchors laid out in the direction of the tidal streams. A remarkable elevation in the form of a ridge on the bottom, with a depth of 18½ fathoms over it, having been previously detected, the boat was moored at slack water as nearly as possible directly over the ridge. Another boat dropping down with the tide, with lead kept just on and off the bottom, the summit of the ridge was felt with the lead, and the depth over it registered every half-hour from 5.30 a.m. to 8 p.m., August 25, 1840.

The observations, being carried out with the utmost care under exceptionally favourable conditions of weather, gave an absolutely uniform depth throughout the day, showing conclusively that at that spot there was no rise and fall of tide. The position in which the observations were made was in lat. 52° 27′ 30″ N., long. 3° 11′ 30″ E., the moon's age being 27·6 days, and the maximum strength of the tidal stream 1·6 knots.

The particular spot for observation, as indicated by Dr. Whewell, was about 30 miles S.S.W. (true) from the above position, but circumstances did not permit of reaching it, and no observations have yet been made there.

On a former occasion, on July 5, 1838, the moon's age being 13·4 days, at a position about 20 miles S.S.E. (true) from Dr. Whewell's position, using precisely the same method and under conditions only slightly less favourable, Captain Hewett found a rise and fall of tide of 6½ feet.

The question of tidal observations in deep water having recently engaged attention at the Hydrographic Department of the Admiralty, an apparatus has been devised which obviates to a great extent the difficulties referred to above.

This apparatus, for use from a ship at anchor, is based on a principle similar to that of the pneumatic self-recording tide gauge now under trial by the Admiralty. It consists of india-rubber tubing having a bore of about ½ inch, supplied in a sufficient number of lengths joined together to allow one end open to the sea to be attached to a weight lowered to the bottom near the anchor. The inboard end of the tubing is attached to the upper part of a closed vertical cylinder 4 inches in diameter and about 6 feet high, on the top of which is fitted a small Bourdon gauge of ordinary pattern. The lower part of the vertical cylinder is in connection with an air-reservoir, and is also connected, by a separate pipe of small diameter, with a large Bourdon gauge of special construction.

The air-reservoir, charged by a powerful air pump, consists of four cylinders, each of which is similar in size and pattern to the vertical cylinder. The large Bourdon gauge is 12 inches in diameter, very delicately made, capable of indicating pressures up to 250 lb. on the square inch, and graduated on a reflecting surface to obviate the effect of parallax in reading off. It can be accurately read to within 1/10 lb.

The method of using the apparatus is as follows:—With the ship lying at anchor, and having sufficient cable veered, the india-rubber tubing should bear no strain. The 12-inch Bourdon gauge being shut off by a needle-valve controlling connection with the remainder of the apparatus, air is pumped into the air-reservoir, flowing from thence to the sea through the vertical cylinder and india-rubber tubing. Pumping is continued until the small Bourdon gauge ceases to rise, thereby showing that all the water is expelled from the tubing, and that the air is escaping freely from the submerged end, at each stroke of the pump. The valve at the junction of the india-rubber tubing and vertical cylinder, controlling connection with the sea, is then closed, and the air reservoir and vertical cylinder charged to a pressure considerably exceeding that of the head of water due to the depth. The compressed air being then admitted to the 12-inch Bourdon gauge by turning the needle-valve, the whole apparatus is again placed in direct communication with the sea by means of the valve for that purpose.

The air pressure as shown by the 12-inch Bourdon gauge will then steadily fall as the air escapes into the sea, and will continue to do so until the pressure in the apparatus exactly balances that due to the column of water represented by the depth over the submerged end of the india-rubber tubing. When the pointer of the 12-inch gauge ceases to fall and remains quite stationary, the gauge is read off.

As a column of sea water 1 foot high, with sectional area of 1 square inch, weighs 0·445 lb., it follows that the depth is obtained by the multiplication of that factor by the pressure in lbs. per square inch as indicated by the gauge. The variation in pressure, provided the weight at the submerged end of the india-rubber tubing has not moved its position, is therefore a measure of the rise and fall of tide.

Observations with this apparatus have been made successfully in depths of 35 fathoms, and the results, when compared with observations of an ordinary tide-gauge on the beach in the immediate vicinity, were found to agree very closely. For purposes of comparison, simultaneous observations were taken afloat and ashore at half-hourly intervals during several days. In fine weather an occasional difference of 2 or 3 inches might be noted, but it seldom exceeded one inch, or even less.

On one occasion when observations were being made during bad weather, force of wind 5 to 6, with the ship rolling and pitching considerably, difficulty was experienced in reading the gauge accurately; the differences observed were consequently somewhat larger, but in no case exceeded 8 inches. The Bourdon gauge used on that occasion has, however, since been vastly improved by the addition of the reflecting surface for the avoidance of parallax, besides other modifications tending towards greater accuracy and facility in reading off. The improved gauge may be expected to give results on which reliance may confidently be placed within a very small margin of error, even under unfavourable conditions.

The apparatus having been thus satisfactorily tested, the officer commanding H.M. surveying ship *Triton* was directed to make observations at certain positions in the English Channel, using the improved Bourdon gauge, with the view of verifying the co-tidal lines as drawn by the late Dr. Whewell from theoretical considerations.

The results given in the following table are very interesting, and show that the theoretical co-tidal lines require considerable modification. It may be hoped that with an apparatus available which enables accurate observations to be carried out without unnecessary loss of time, further information may eventually be obtained in many parts of the English Channel and North Sea.

A. M. F.

English Channel.—Tidal Observations by Capt. W. P. Dawson, R.N., H.M.S. "Triton," May, 1909.

Date 1909	Position	Depth Fathoms	Wind	Time of High Water h. m.	Time of preceding Moon's Transit h. m.	Lunitidal Interval h. m.	Time of Low Water h. m.	Range of Tide ft. in.	Max. Current knots	Bar. and Thr.	Time of H. W. at Dover h. m.
May 18	50° 22′ 35″ N., 0° 35′ 40″ E.	24	West 2·3	9 30 a.m.	10 33 p.m. May 17	10 57	4 30 p.m.	24 0	1·5	30·10 / 53	9 40 a.m.
May 19	50° 9′ 30″ N., 0° 49′ 15″ W.	27	Calm	10 30 a.m.	11 26 p.m. May 18	11 4	4 30 a.m.	13 9	2·6	30·40 / 55	10 30 a.m.
May 20	50° 26′ 0″ N., 1° 8′ 0″ W.	19	E. b S. 0–1	10 30 a.m.	0 21 a.m. May 20	10 9	4 15 a.m.	10 11	4·0	30·30 / 52	11 24 a.m.
May 21	50° 24′ 10″ N., 2° 2′ 10″ W.	23	Lt. air W.	8 50 p.m.	1 46 p.m. May 21	7 4	1 0 a.m. May 22	6 6	3·5	30·16 / 55	0 7 p.m.

All times are G.M. Time.

MODERN AËRONAUTICS.[1]

(1) WITHIN about three hundred pages Mr. Turner gives a popular account of the whole field of aërial navigation, including balloons, airships, and aëroplanes, in his survey. He first gives an account of the history and principles of each branch of the subject. In the chapter on the principles of ballooning the expansion of the gas in a balloon appears to be attributed mainly to the heating by the sun's rays, and only a sentence, by the way, refers to the expansion due to the diminished pressure at an increased height, which, of course, affects the gas in the balloon and the surrounding air equally, and also materially affects the vertical stability of the balloon's equilibrium in the air. The natural variation of the temperature of the air with altitude might also be with advantage discussed more fully. In treating of balloons of the non-rigid type, the action of the *ballonet* in preventing flabbiness might be made clearer. On p. 181, after exposing the fallacy of an airship tacking, Mr. Turner seems to say that aëroplanes are on a different footing. Of course, the speed of an aëroplane is relative to the air just as an airship's is, and an aëroplane and an airship capable of travelling at the same speed are under the same conditions as to the directions in which they can travel in a wind. The aëroplane can have the advantage only so far as its speed exceeds the airship's.

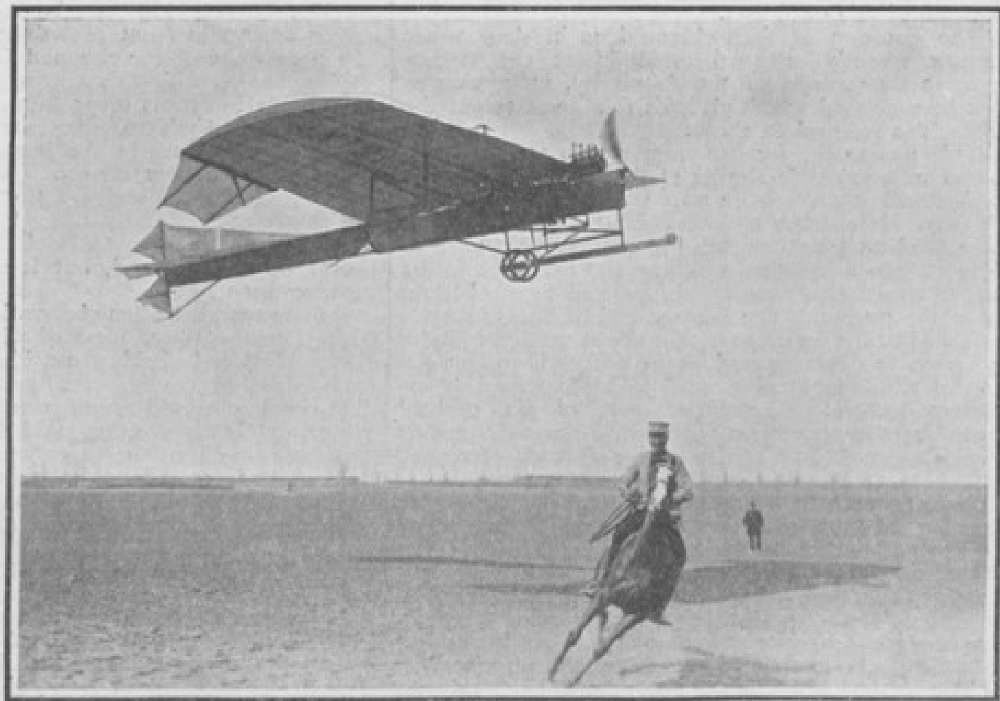

FIG. 1.—Scouts: old and new. From "Aërial Navigation of To-day."

The second part of the book deals with various problems which have to be solved. Very interesting speculations are made about the aërial law of the future, and the landmarks, sign-posts, and alighting stations which will be provided for aviators. In chapters on military and naval aëronautics and strategy and aërial invasion, Mr. Turner discusses questions which interest everyone at present. A very clear account is given of the limitations which make some of the achievements that have been attributed to aërial craft impossible, and others improbable of execution, while at the same time full justice is done to the great services they can render within their limitations. Mr. Turner's discussion of these important matters can be recommended as sane and reasonable. Other chapters deal with the possibilities of exploration and long-distance travel in general by the air, and in a chapter headed "Work to be Done" attention is directed, among other things, to the need for increased stability in aëroplanes and for a trustworthy light motor.

While those who wish to construct aëroplanes will have to have recourse to fuller and more technical treatises, this book fills a want, and a second edition is already advertised.

In a couple of places characteristically English remarks are made at the expense of mathematicians and men of science in general. These are the more uncalled for in view of the very hazy notions which the book itself shows up, regarding stability and similar questions, that are capable of exact mathematical treatment, as well as experimental tests. The references to stability alone show a lack of exactness in the use of well-known mathematical and physical terms. Thus in the glossary at the end we have the following definitions:—

"*Equilibrium.*—In flying machines the term is used in the same sense as stability."

"Horizontal stability is the same as longitudinal"; while on p. 291 the author says (of dirigibles):—

"To maintain horizontal stability—that is, to enable the airship to move forward in a straight line without veering to one side or the other—fixed vertical planes at the rear of the frame are used. In addition, there is a fixed vertical plane surface at the rear of the gas-envelope."

The "useful tables" and "glossary" at the end are good features.

(2) Mr. Samuelson's pamphlet, a continuation of a previous publication of the author's, begins with a description and drawings of a model flying machine on the principle of "rowing" flight, and concludes

[1] (1) "Aërial Navigation of To-day. A Popular Account of the Evolution of Aëronautics." By C. C. Turner. Pp. 327. (London: Seeley and Co., Ltd., 1910.) Price 5s. net.
(2) "Flight Velocity." By Arnold Samuelson. (English edition of "Fluggeschwindigkeit.") Pp. 56; 5 plates. (Hamburg: Boysen and Masch; London E. and F. N. Spon, Ltd., 1906.)
(3) "The Conquest of the Air, or the Advent of Aërial Navigation." By Prof. A. Lawrence Rotch. Pp. x+192; 36 illustrations. (New York: Moffat, Yard and Co., 1909.)
(4) "Aërodynamik: eine Gesamtwerk über das Fliegen." Von F. W. Lanchester; übersetzt von C. und A. Runge. Erster Band. Pp. xiv+360. (Leipzig and Berlin: B. G. Teubner, 1909.) Price 12 marks.

with a proposal to form a company to construct a full-sized machine from the author's plans. The principles on which the author relies are not those generally accepted. He maintains that the centre of pressure for a plane does not vary with its inclination to the line of flight, that the normal pressure is independent of the inclination, and that flapping wings can be constructed so as to be mechanically more efficient than a screw propeller. To establish these principles he seems to rely on rough experiments with kites and small gliders. The accuracy of the observations and the deductions made from them both seem open to question. Plotting v against t, the graph of

$$v = \frac{1}{\frac{n}{M}t + \frac{1}{v_0}}$$

is said to be a straight line, because v and t only appear in the first power. The author maintains that his observations prove Langley's to be inaccurate, and attempts to explain away the discrepancy between his principles and Langley's experiments in a way which is not convincing.

(3) The preface to Mr. Lawrence Rotch's book is dated April, 1909, and when we think of the number of flights performed since then it will be evident that no writer could possibly bring out a book containing the most up-to-date records in aviation. The author has, on the other hand, brought into prominence several aspects of aërial navigation which are apt to be forgotten in these days, when the breaking of records by 'planes (not to mention other breakages of a regrettable character) is the all-absorbing topic. For example, in chapter i., the ocean of air, we have an account of the results of meteorological observations in which the author has played a most important part. It is illustrated by diagrams showing the greatest altitudes reached by mountains, balloons, and *ballons sondes*, also variations of temperature and wind velocity with the altitude, and it well shows up the efficiency of kites and *ballons sondes* in exploring regions of the atmosphere to which man can never hope to penetrate. In the second chapter—the history of aërostation—the author reproduces the letters of Benjamin Franklin to Sir Joseph Banks, P.R.S., describing the first balloon ascents made in France. The following extract from one of these letters is worth reading at the present day:—

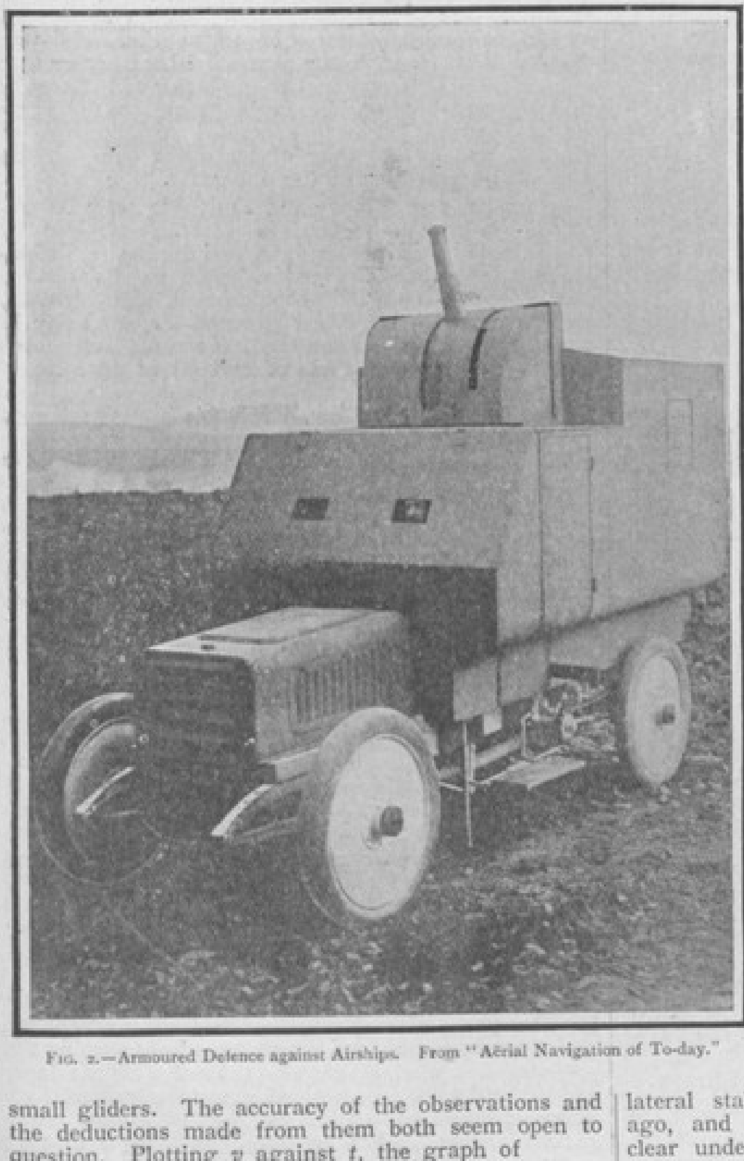

FIG. 2.—Armoured Defence against Airships. From "Aërial Navigation of To-day."

"I am sorry this Experiment is totally neglected in England, where mechanical Genius is so strong. I wish I could see the same Emulation between the two nations as I see between the two Parties here. Your Philosophy seems to be too bashful. In this country we are not so much afraid of being laught at. If we do a foolish thing we are the first to laugh at it ourselves, and are almost as much pleased with a *Bon Mot* or *Chanson*, that ridicules well the Disappointment of a Project, as we might have been with its Success. It does not seem to me a good reason to decline prosecuting a new Experiment which apparently increases the power of Man over Matter, till we can see to what Use that Power may be applied. When we have learnt to manage it, we may hope some time or other to find Uses for it, as men have done for Magnetism and Electricity, of which the first Experiments were mere Matters of Amusement."

How true this all sounds to-day! *In England there does not, we believe, exist at the present time a single prize for any scientific investigation bearing on aërial navigation.* Had such a prize existed the theory of longitudinal and lateral stability could have been disposed of years ago, and aëroplanes could have been built with a clear understanding of their stability or lack of it. It should surely have been worth while also for those who spend such large sums on construction of dirigibles to take some steps to obtain a theory of their stability, but this has not been done. There are several other problems, including one or two in discontinuous motion, awaiting solution; and it is not the mathematician alone who is handicapped by the persistent refusal of English people to provide any adequate recognition of *original work*.

We should be greatly surprised if members of the engineering profession would not be glad to make use of a similar encouragement to carry out experiments of rather a more scientific character than would be otherwise compatible with their business requirements. As it is, there appears to be no such inducement in England for anyone to initiate, undertake, and publish original work, whether on stability, stream lines, propellers, motors, or strength of materials. Indeed, there are very strong inducements for having such work undone, unwritten, and unpublished.[1]

The next two chapters deal, respectively, with the dirigible balloon and the flying machine. In the former we have an illustrated historic description, tracing the gradual progress that has been made in dirigibles since the first idea of one was suggested by Franklin in 1784; while in the latter the evolution of the power-driven machine from the mere glider is briefly but sufficiently well discussed. "The Future of Aërial Navigation" is a subject on which anyone with an imaginative mind can write something which people will read with eagerness, and this being the case, we think that Mr. Rotch has been wise in only devoting twenty pages to it, in preserving the historic order, and in giving numerous references to what has been written. The book is, of course, much smaller and less compendious than Mr. Turner's.

(4) That such English people as are able, in spite of their national disabilities, to undertake *original work* find their efforts appreciated in Germany is well shown by the publication, by the Teubner Press, of a translation of part i. of Mr. Lanchester's book within a comparatively short time of its appearance in England. The English preface is dated October, 1907, the German preface, by Prof. C. Runge, August, 1909, none too long for the work of the translators and printer. We cannot do better now than quote from Prof. C. Runge's German preface in the following terms:—

"The present book contains so many important original ideas and investigations for the development of free flight that German engineers and men of science will be grateful to the publishers for having provided a translation of it.

The author has in some places altered the text, and in others the text has been altered by the translators in consultation with the author, so that the translation may be regarded as a revised edition. A complete retrospect of existing literature was, however, not attempted; this would have altered the character of the whole book and necessitated completely re-writing it, which was not contemplated by the translator.

For men of science the principal charm of the book lies in the ideas on fluid resistance, and the expression of these by exact mathematical formulæ should be the next problem of hydrodynamics."

Does not the last sentence confirm what has been stated above as to the need of prizes for which mathematicians as well as physicists and engineers are eligible?

G. H. BRYAN.
E. H. HARPER.

PROF. K. J. ÅNGSTRÖM.

By the death of Prof. Knut Johan Ångström, physical science has lost a conscientious and capable worker, in a field which requires long and continuous experience before success can be achieved. For this reason his departure will be felt more severely than that of many men, who perhaps have gained a greater reputation, but have been fortunate enough to interest others in the line of research they have been pursuing.

Knut Ångström bore an honoured name. Those who still remember the early days of spectrum analysis know how much that science owed to the pioneer work of his father, Anders Johan Ångström, whose map of the solar spectrum remained until Rowland's time the standard to which all wave-lengths were referred.

The son was born on January 12, 1857, and received his school and university education at Upsala, where he spent almost his entire life. He was appointed assistant in the physical laboratory of that university in 1882, graduated as Doctor of Philosophy in 1885, and became lecturer in physics in the same year. In 1895 he was appointed to the chair of physics, and at the time of his death occupied the position of pro-rector of the university.

So far back as 1889 we find Knut Ångström investigating absorption phenomena in the infra-red by means of the spectro-bolometer, and during the following two years he obtained valuable results on the absorption spectrum of carbonic oxide, carbonic acid, and marsh gas. He also discovered the similarity in the characteristic absorption of the same substances (ether, benzene, bisulphide of carbon) in their liquid and gaseous states.

We owe to him, further, a valuable investigation on the infra-red absorption of aqueous vapour, carbonic acid and ozone. All these gases are constituents of our atmosphere, and the effect of the two latter on the temperature of the earth may be considerable, not so much because they absorb a certain portion of the solar radiation, but chiefly on account of their much greater comparative influence in preventing the heat radiated from the earth from being dissipated into space. An interesting and instructing controversy took place in connection with the effect of carbonic acid. Arrhenius in 1896 had given a very ingenious explanation of the Glacial period by assuming that the quantity of carbonic acid in the atmosphere had increased since that time. If it be assumed that the absorption is proportional to the total quantity present, it can indeed be shown that a small variation in quantity would exercise a very considerable effect on the temperature; but, as pointed out by Knut Ångström, the proportionality between absorption and quantity only holds when the quantities are sufficiently small, and he showed that the quantity of carbonic acid in the atmosphere must be reduced to about 20 per cent. of its present value before an appreciable effect in the total absorption can take place.

In the course of the further discussion of the subject Ångström carried out important observations on the effect of pressure, and showed that by increasing the pressure, but diminishing the thickness of the layer so that the total quantity of absorbing material remains constant, a marked increase of absorption is noticed at the higher pressure. It follows that in order to find by optical means the quantity of carbonic acid in our atmosphere, it is not sufficient to determine the amount of gas necessary in our atmosphere, it is not sufficient to produce the same absorption as shown by the atmosphere, but account must be taken of the conditions of pressure. Observations on the absorption of ozone also led to the interesting result that there must be considerable quantities of that gas in the upper regions of the atmosphere.

Knut Ångström's name has become more particularly associated with recent researches in the measurement of solar radiation. He constructed an instrument, the essential portions of which consist

[1] Mr. Alexander has offered a prize of 1000*l.* to the Aërial League for the best and most trustworthy motor of 20 h.p. capable of running unattended for twenty-four hours. While fully appreciating the importance and value of such prizes, it should be pointed out that the worker who attempts to penetrate more deeply into the *thermodynamics* or *general theory* of the internal combustion engine, with the view of paving the way for future improvements, has no prospect of reward, whereas the successful competitor for such a prize *may* have other prospects of a return for his exertions in the form of patents.

of two strips of platinum blackened at the front surface and carrying a thermo-junction at the back. One of these is exposed to the radiation to be measured, while an electric current passes through the other. This electric current is regulated until the two thermo-junctions are at the same temperature. The intensity of the current necessary for this purpose gives a measure of the radiation after certain corrections have been applied. The use of the instrument is simple and convenient, and found so much favour with observers well qualified to judge that the International Union of Solar Research recommended it as a standard for measurement of solar radiation.

Since then the instrument has shown itself liable to certain systematic errors which render further experimental investigations necessary. Its intrinsic merit is, however, so great that it is pretty certain that it will re-establish its reputation, but it is much to be regretted that Prof. Ångström's experimental skill is no longer available for the purpose. When the International Union of Solar Research made its recommendation, it was well aware that for a complete determination of the solar constant it is necessary to divide the spectrum into portions sufficiently homogeneous to allow the application of Lambert's law, but such complete determinations need only be carried out in one or two places. Abbot is doing excellent work, and if this be repeated at another station, say in India, the ground will be pretty well covered. In addition to these standards, we require, however, some instrument which is easily transported, and serves to record the radiations received at different times and in different localities. Ångström's pyrheliometer promises to serve that purpose admirably, as soon as more ready means have been found to standardise it easily from time to time, or to obtain a more permanent absorbing surface of the platinum strips. The coloured glasses which Ångström recently used to absorb parts of the spectrum chiefly affecting the absorption of aqueous vapour or carbonic acid will probably increase considerably the utility of the instrument.

It remains to notice an important contribution of Ångström's in the field of radio-activity. He measured, by means of a Bunsen ice calorimeter, the heat set free in a given time by radium salts, and found it to be constant and independent of the substance in which the radium is placed.

Ångström's charming personality endeared him to all with whom he came into contact, and we condole with Swedish science and the University of Upsala in the loss they have sustained. ARTHUR SCHUSTER.

NOTES.

WE notice with great regret the announcement of the death of Prof. Alexander Agassiz, on Monday, at seventy-four years of age.

SIR JAMES DEWAR, F.R.S., has recently received two foreign diplomas, namely, that of Doctor, *honoris causa*, of the University of Brussels, and that of honorary member of the American Chemical Society.

THE Oceanographical Museum at Monaco, established by the Prince of Monaco, was opened on Tuesday by the Prince in the presence of representatives of European Governments and scientific societies. An article upon the museum and the opening ceremony will appear in a later issue of NATURE.

THE third International Physiotherapeutic Congress was opened by President Fallières on Tuesday at the School of Medicine, Paris. A large number of members of the French Government and of the Diplomatic Corps in Paris, including the British and American Ambassadors, were present at the ceremony.

THE council of the South African Association for the Advancement of Science at a recent meeting resolved by a unanimous vote to offer the presidency of the forthcoming meeting in Cape Town to Dr. T. Muir, C.M.G., F.R.S., and he has accepted the invitation to occupy that office. The actual date of the meeting has not yet been fixed.

LORD KINNAIRD will preside at the dinner to Sir John Murray on Tuesday next, April 5, in connection with the *Michael Sars* expedition for the exploration of North Atlantic waters. The dinner will be held at the Criterion Restaurant, and tickets may be obtained from the honorary secretary of the Atlantic Union, 13A Cockspur Street, S.W.

AT a meeting of the National Geographic Society at Washington on March 26, President Taft presented the gold medal of the society to Sir Ernest Shackleton, and in doing so he remarked :—" It is evidence of the society's high appreciation of the marvellous work you have done in the cause of science, of the endurance, courage and intelligence you have shown in the pursuit of a definite object." On March 28 the explorer was presented with the Cullum gold medal of the American Geographical Society, New York.

ON March 23 the Mayor of Doncaster, Councillor Halmshaw, formally opened a municipal museum at Doncaster, for which purpose some of the rooms in a fine mansion, known as Beechfield, have been set apart. These are devoted to specimens illustrating local geology, archæology, and natural history. Mr. T. Sheppard, of Hull, who a short time ago was asked by the Doncaster Corporation to report on the lines the museum should take, was called upon by the Mayor to give an address. In this he dwelt more particularly upon the educational advantages of museums, and the necessity of provincial museums being of local interest. Subsequently the visitors were conducted round the collections, which reflected great credit upon the curator, Dr. Corbett.

ON Tuesday next, April 5, Dr. A. Harden will begin a course of three lectures at the Royal Institution on "The Modern Development of the Problem of Alcoholic Fermentation"; on Thursday, April 7, Dr. T. G. Longstaff will give the first of three lectures on "The Himalayan Region"; and on Saturday, April 9, Mr. W. W. Starmer will commence a course of three lectures on "Bells, Carillons and Chimes" (with musical illustrations). The Friday evening discourse on April 8 will be delivered by Prof. Percival Lowell, on "Lowell Observatory Photographs of the Planets"; on April 15 by Prof. W. J. Pope, on "The Chemical Significance of Crystal Structure"; and on April 22 by Mr. T. Thorne Baker, on "The Telegraphy of Photographs, Wireless and by Wire."

AFTER a number of slight earthquake shocks, an active eruption of Mount Etna commenced on March 23. Signor Ricco, the director of the observatory there, reported in a telegram from Nicolosi, a suburb of Belpasso, that the lava was advancing on March 24 in a stream more than 1500 feet wide, at a rate of upwards of 60 feet an hour. On March 25 he reported that the violence of the eruption had increased notably during the night, and that quantities of scoriæ were being thrown up, accompanied by great explosions and rumbling. Five new craters on the south declivity of the mountain, in the same place as those of former eruptions, have been reported. Though on this day the lava stream was larger, it was descending more slowly. The *Times* Rome correspondent reported that on March 27 the activity of the eruption had diminished considerably, and that the lava streams had ceased to flow. The lava appears on this occasion to have flowed farther than in the eruption of 1892. There was renewed activity in the craters on March 28, and a fresh descent of lava, though in more moderate quantities. As yet there is no real anxiety for the safety of Nicolosi or Borello.

THE Reale Istituto Lombardo has awarded the following prizes:—the mathematical prize for an essay on theory of transformation groups is awarded to Prof. Ugo Amaldi, of Modena, for his essay on the determination of all the infinite continuous groups of analytic point transformations in three-dimensional space; the Cagnola prize, relating to miasma and contagion, is awarded to Prof. Aldo Castellani, of the hospital for tropical diseases at Colombo (Ceylon). From the Brambilla foundation for industrial prizes, awards have been made to Elia Bianchi, for his system of constructing dwelling houses formed of hollow concrete blocks, and to Renaldo Rossi, for whole-meal and anti-diabetes bread. The Fossati prize is awarded to Prof. Giuseppe Sterzi, of Padua, for his two published volumes on the central nervous system of vertebrates.

PROF. J. W. H. TRAIL, F.R.S., recently offered to the council of the Linnean Society a sum of money for the purpose of encouraging the study of protoplasm by means of an award to be made periodically. This generous offer has been gratefully accepted, and a special medal has been struck in bronze for presentation with the award, bearing on the obverse a portrait of Linnæus and on the reverse the words "Trail Award" and the name of the recipient in a wreath. It is proposed to make an award about once in every five years for original work bearing directly or indirectly upon the "physical basis of life," and, in accordance with the wishes of the donor, a wide interpretation will be given to the scope of the investigations. The first recipient of the award will be Prof. E. A. Minchin, professor of protozoology in the University of London, whose researches on sponges and protozoa have done so much to advance our knowledge of protoplasmic structures, and who is also the translator of Prof. Bütschli's well-known work on protoplasm.

THE February Bulletin of the Société d'Encouragement pour l'Industrie nationale contains the president's address delivered by M. Bertin at the general meeting in January last, and particulars concerning the award of prizes and medals on the same occasion. We notice that a grand gold medal was awarded to Sir Robert Hadfield, F.R.S. M. L. Baclé, representing the association's committee of chemical arts, points out that Sir Robert Hadfield has at least thirty-one memoirs to his credit extending over the period 1888 to 1909, and that these have been presented to various learned societies in England and America. Among the numerous other awards, we notice that the Lavoisier medal was awarded to M. le Comte de Chardonnet, for the creation of a new industry—that of artificial silks—and that the first award of the recently established Michel Perret medal for scientific workers, who by their researches have contributed to the progress of industrial chemistry, was made to MM. Gall and de Montlaur, for their electrochemical work.

THE New Zealand Survey Department is undertaking, in conjunction with the Marine Department, an inquiry into the tides of New Zealand. Hitherto the tide-tables for New Zealand in the New Zealand "Nautical Almanac" have been supplied by the U.S. Coast and Geodetic Survey, there having been no facilities in New Zealand for the necessary work. We learn from the *New Zealand Times* that the New Zealand Government has been invited by the Secretary of State for the Colonies to supply particulars as to the tides for insertion in the British Admiralty manuals for the use of the Navy and mercantile marine. It is hoped that the results from New Zealand, Australia, and other parts of the Pacific will lead to the thorough investigation of the tides of the Pacific Ocean, of which the available information is at present meagre. The latest scientific apparatus is being obtained from England, and the work has been placed in charge of Mr. C. E. Adams, secretary of the New Zealand Surveyors' Board, and is to be carried on actively at once.

IN the year 1891 Prof. Flinders Petrie found a curious mummy in a plundered tomb (supposed—though there is no positive proof—to have been that of a certain Ranefer or Ranofir) in the neighbourhood of the Medum Pyramid of King Snefru (*circa* 2900 B.C.). The fact that this was the oldest known mummy was duly recognised at the time, not only by its discoverer, but also by Prof. Maspero (see "The Dawn of Civilisation," p. 362), and with due care it was transported to England and lodged in the Museum of the Royal College of Surgeons. The significance of this mummy was not fully realised at the time, because it was generally supposed that the practice of embalming was as old as the history of Egypt, and many museums contained so-called "mummies" almost, if not quite, as ancient; and the importance attached to it seemed to diminish during the following decade, when some archæologists began describing earlier, even pre-dynastic, "mummies" (see "Guide to the First and Second Egyptian Rooms," British Museum). When, however, it was discovered (see *Cairo Scientific Journal*, May, 1908, p. 205) that there were no genuine mummies in the Cairo Museum (or in the British Museum) earlier than the time of the New Empire (*circa* 1580 B.C.), and that the bodies embalmed in the times of the tenth dynasty (*circa* 2200 B.C.) and twelfth dynasty (*circa* 2000 B.C.), found in 1907 by Mr. Quibell and Messrs. Lythgoe and Mace, respectively, were so fragile that they could not be moved without becoming reduced to mere bones and powder, the importance of the Medum mummy was more than rehabilitated, as was pointed out in NATURE in 1908 (vol. lxxviii., p. 342). The age of a mummy such as this was always open to question, seeing that it was found in a plundered tomb; but the important researches carried on by Dr. George A. Reisner at the Giza Pyramids during the last few years have now supplied the data which, when applied to the curious distinctive features of the Medum mummy, fix its age definitely at the period of the fifth dynasty (*circa* 2700 B.C.). Thus the specimen in the Royal College of Surgeons is of the utmost importance to the student of the history of embalming in Egypt, for it is more than 1100 years older than any *actual* mummy exhibited in any other museum, and 500 years older than any other mummy ever found.

MR. C. PEABODY has reprinted from the Putnam anniversary volume a valuable paper on certain quests and doles. He deals first with the rite of Hogmanay practised throughout western Europe, from the Isle of Man to France, in the period extending from late Advent to January. He connects it with a pre-Christian solstitial ceremony prevailing throughout northern and western

Europe. The English Waits fall into the same class. The French Dimanche des Brandons is connected by its fire rites with the ancient Pagan ceremonies at the vernal equinox. The paper is remarkable for a very elaborate collection of references to English and Continental folk-lore.

INFANTILISM and idiocy, and gigantism and idiocy, are the subjects of two papers by Dr. A. Marie in the *Bulletins et Mémoires de la Soc. Anth. de Paris* (5th Sér.), x., pp. 101, 113. He gives a classification of the various forms of dwarfing (nanism and infantilism). He believes that the nanism of the degenerate is nothing else than the permanence of an infantile stage through which all normal persons pass. One may consider the unprofitable age (*l'âge ingrat*) of transition between infancy and the final sprouting of growth at puberty as a kind of transitory normal acromegaly. Gigantism is only the acromegaly of infancy, the unprofitable age prolonged. Giants as well as dwarfs occur in families of degenerates.

THE Touareg, who have been exhibited at Paris, have been investigated by Dr. Atgier (*Bull. et Mém. de la Soc. Anth. de Paris*, 5th Sér., p. 222). The individuals studied were extremely limited in number, and necessarily belonged to the servile classes, as the upper-class Touareg would be too proud to exhibit themselves; consequently they represent a mixed group. On this slender foundation the following results have been arrived at. Excluding the Semitic element (Arabs, Jews, &c.) and the negro element (which is evident in those investigated), one finds the same ethnic groups as those which have peopled Europe—Indo-Europeans or Aryas—that is to say, blonds, brown brachycephals, and brown dolichocephals. Thus the expression "Berber" does not denote a race or variety of the human race, but a conventional term simply signifying those peoples of North Africa who are neither Semitic nor Negroid. According to Dr. Atgier, North Africa, like Europe, has Iberian, Celtic, Basque, and Kymric types, to which the term Aryas of Africa may be applied.

WE have received from the publishers (Bowes and Bowes, Cambridge) a copy of a lecture recently delivered by Mr. W. C. D. Whetham, F.R.S., in Trinity College, entitled "Eugenics and Unemployment." From his book on "The Family and the Nation," the lecturer cites evidence "that, with a few exceptions, the successful families in all classes are voluntarily restricting the number of their children, that their birth-rate has halved since 1876, and that the average number of children to the fertile marriage is now about three. About four children to the fertile marriage is the least number that will maintain a population unchanged. . . . But the population of the country as a whole is still expanding. Hence it follows that the unsuccessful families must still be multiplying rapidly. . . . We . . . are breeding fastest from our less efficient or definitely diseased strains." Having reached this conclusion, the lecturer begs his audience to dismiss any preconceptions and prejudices they may have as to pauperism and unemployment, and to look with him at the facts. He shows a curve based on the annual percentage of the unemployed members of trade unions, but points out that it "is roughly coincident with the cycles of good and bad trade," and "bears very little relation to the curve of general pauperism" or to "the total amount of distress in the country." He shows next how the curve based on the average number of paupers relieved per 1000 of the English and Welsh population has been declining "with natural fluctuations" from 1851 onwards. Since 1900 "there has been a slight increase, so slight that it is difficult to be sure that it is more than a temporary fluctuation on a curve which shows yearly changes." Despite this warning, Mr. Whetham subsequently suggests that "our failure to go on diminishing pauperism of late years may be due to a slight lowering of the average character and efficiency value of our population," arising from an artificial reduction of birth-rate among "the thrifty, the prudent and the far-seeing, quite as much as by the selfish and pleasure-loving."

EVIDENCE is steadily accumulating to show that most of the forest mammals formerly supposed to be restricted to the west coast of Africa extend eastwards into Uganda. The latest instance of this is afforded by the lemuroid pottos, of which Mr. O. Thomas described an East African species of the genus Perodicticus at a recent meeting of the Zoological Society. Sir H. H. Johnston had long since announced the existence of a Uganda potto, but no specimen was forthcoming.

IN No. 4 of the first volume of the *Queensland Naturalist* Mr. G. F. Bennett relates some of his early experiences in hunting and observing monotremes. On one occasion, after digging out the burrow for a distance of about 20 feet, he came upon a nest containing two young duck-bills, probably about a month old, each rolled up into a ball with the tail lying flat on the beak. In other instances the tail covered the head, and the beak rested on the stomach. All young ones of about a month old are plump with a greyish, bare skin.

AT the close of an article on the courtship of spiders, contributed by Prof. T. H. Montgomery, jun., to the March number of the *American Naturalist*, it is argued that Dr. Wallace's theory that the generally less conspicuous colour of female birds (as compared with their partners) is due to their need for greater protection will hold good also in the case of the Arachnida. "For the males do not develop their ornamentation until maturity, and they have much less need of protection than the females because they live usually not much longer than a few weeks after maturing, and take no part in the care of the young. The males have fulfilled their main function after impregnating the females, and they are of no use to the species thereafter. But the females live at least several months after maturing, in some cases several years, and they have the whole charge of the eggs and young."

THE March number of *Naturen* opens with an obituary, illustrated by a portrait, of Hans Christian Printz, Norway's oldest *savant*, who was born on April 13, 1817, and died, from an attack of influenza, on January 15 of this year in the ninety-third year of his age. On completing his education, Prof. Printz devoted much of his time to botany, and in 1864 made an important collecting tour; but about 1870 his attention was largely directed to meteorology, to which science he devoted much of his time in subsequent years. In addition to this, he was an enthusiastic egg-collector, and at one time possessed between 4000 and 5000 specimens, mainly, it would appear, Scandinavian, among which his greatest treasure was an egg of *Garrulus infaustus*. About 1871 this collection was acquired by the Bergen Museum.

THE nature and arrangement of the bony armour of the dinosaur Stegosaurus are discussed by Dr. R. S. Lull in the March issue of the *American Journal of Science*. In the specimen restored by Marsh a number of small ossicles were found adhering to the under surface of the lower jaw, and these, in the opinion of Dr. Lull, not only formed a gular shield, but also extended over a considerable part of the body, as it is unreasonable to suppose that any portion of the skin of an armoured reptile would be unprotected. As regards the great vertical dorsal plates and caudal spines, the former of which Marsh regarded as forming a single series, it is practically certain that all were arranged in a double row. The vertical plates are considered to be nothing more than an ultra-development of the longitudinal vertical ridge on the horizontal scute of a crocodile or an unspecialised dinosaur like Ancylosaurus. Throughout the back the ribs are T-shaped in section in order to bear the weight of the plates. In the neck the latter are borne on short and notched transverse processes, but in the back these processes become longer and stouter, while in the sacral and anterior caudal region the bases of the plates are approximated and supported on the summits of the tall and expanded neural spines. On the other hand, the terminal third of the tail apparently formed a flexible aggressive weapon, in which the laterally divergent spines were inserted in the muscles between the neural spine and the centrum. Although the caudal spines of the English Kimeridgian Omosaurus or Dacentrus are structurally identical with those of one of the American species of Stegosaurus, in the lack of evidence as to the presence of vertical plates in the former the author is indisposed to admit the generic identity of the Old World and American types.

Dipterocarpus tuberculatus, known locally as the In tree, one of the most important members of the family Dipterocarpaceæ which bulks largely in the Burmese forests, forms the subject of a Forest Pamphlet (No. 13) compiled by Mr. R. S. Troup, and published by the Government of India. As a rule, it is a dominant tree, and an idea of its characteristic gregariousness may be obtained from computations, which estimate fifteen to twenty good-sized trees per acre. Fine specimens attain a height of 90 feet, with a clean bole of 60 feet and a girth of 10 feet. The wood is resinous and heavy, requiring bamboos if it has to be rafted; it is in considerable demand, as it works well, but is not durable if exposed.

A RECENT paper by Prof. G. Klebs, published in the *Sitzungsberichte der Heidelberger Akademie der Wissenschaften* (part v., 1909), and obtainable as a separate brochure, describes the modifications produced in flowers of Sempervivum when exposed to special cultural conditions, and incidentally contains some pertinent opinions on the subjects of variation in plants and inherited characters. The species, *S. acuminatum*, chosen for experiment is a recognised natural species. Plants were grown in rich soil and kept at a high temperature. The first inflorescences were cut off when quite young, and dormant inflorescences showing abnormalities were developed, from which self-fertilised seed was collected. Plants raised from the seed were grown, and increased vegetatively for three years. On flowering, the terminal inflorescences were removed as before, and the later flowers produced abnormalities, some new, others similar to those obtained before. These abnormal characters the author recognises as pathological modifications, yet regards their origin as intermediate between fluctuating variations and mutations.

MR. T. PETCH is responsible for three recent Circulars (vol. iv., Nos. 21–3) dealing with fungus diseases, issued from the Royal Botanic Gardens, Ceylon. A bark disease on Hevea and tea that appears during the south-west monsoon is attributed to *Corticium javanicum*. A more insidious disease of Hevea, known as "die-back," is started by a Glœosporium which paves the way for the destructive parasite, assigned to the genus Lasiodiplodia. The third pamphlet discusses very fully the stem-bleeding disease of the cocoa-nut caused by *Thielaviopsis aceticus*, a known parasite on sugar-cane in Java. The author communicates a number of details regarding the structure of the cocoa-nut palm. He distinguishes two types of tree, the one with a uniform columnar base, the other with a swollen base, and suggests that the latter, which is the less desirable, has been selected unconsciously by planters.

ACCORDING to the *Agricultural Journal of the Cape of Good Hope*, a certain amount of work on the hybridisation of wheat is being done in Cape Colony. At present less than half the wheat required for consumption is grown, the rest being imported; steps are therefore being taken to increase the area under crop. One of the chief difficulties about wheat-growing in the colony, and particularly in the western provinces, is the vast amount of destruction caused by rust; indeed, this was at one time so serious that farmers almost despaired of making wheat-growing a success. The importation of certain varieties more or less resistant to rust rather relieved matters, but none has yet been found fully to meet the local requirements. A cross between Gluyas and Darling promises to give useful results; Gluyas is resistant to rust but possesses very weak straw; Darling, on the other hand, possesses exceptionally strong straw. A hybrid, Union, has been picked out possessing strong straw and also resistant to rust. Another promising cross is between Gluyas and Du Toits, probably the finest milling wheat in the colony.

IN the *Sitzungsberichte* of the Vienna Academy of Sciences (Bd. cxviii., Heft vii.) P. Vujević discusses at some length the results of five years' temperature observations (1902–6) made at Belgrade. The readings were taken from freely exposed mercury thermometers, with cylindrical bulbs, at the earth's surface and at 0·4, 1·0, and 2·0 m. above it. The results are of special interest in view of the plea for such observations recently put forward in this country. The excess of the mean temperature from hourly readings of the freely exposed thermometer at 2 m. above the mean temperature in the screen at the same height is $-0·1°$ C. in January, $+0·6°$ in July. The mean difference is greatest ($+2·0°$ C.) at 1 p.m. and least ($-1·0°$ C.) at 8 p.m., in both cases in July. The occurrence of the minimum difference immediately after sunset is attributed to the retention of warm air in the screen. It is probably also due in part to the heat capacity of the screen itself. The point is of importance in connection with the analysis of the daily variation of temperature. Comparisons showed that the freely exposed thermometers gave higher readings at all levels on clear days, and lower readings on a cloudy day, than the aspirated thermometer of the Assmann instrument. The disturbance of the natural condition by the artificial aspiration would have some influence on these results. The observations from the freely exposed instruments are compared with one another without any attempt at correction. Throughout the year the temperature at the earth's surface is lower by night and higher near mid-day than that in the air. The extreme differences between the hourly means for surface and air are approximately $+1·5°$, $-0·5°$ C. in January, $+9·1°$, $-1·6°$ C. in April, and $+15·2°$, $-1·0°$ C. in July. Deposition of dew diminishes the value of the negative difference, while clear weather increases both the positive and negative differences. The temperature on the exposed earth's surface was found to be considerably below that of the neighbouring upper surface of snow. It is assumed that the results are inter-comparable because the thermometers are similar, an assumption which is not justified unless the ventilation is the same for each; this is probably not the case. No attempt is made to find the effect of varying natural ventilation.

MR. HERMANN GEWECKE sends us a dissertation on the influence of changes of internal structure on the physical properties of copper, electrical conductivity and density being the properties chiefly considered. In this pamphlet of ninety-three pages the author discusses at length the experimental results and theoretical conclusions of previous workers in the same field, and also describes his own measurements of electrical conductivity and determinations of density carried out on a series of copper wires drawn under known conditions. His results show that the effects of wire-drawing depend upon two actions, which occur simultaneously, but to a different relative extent when the circumstances of the drawing process are altered. These two actions are longitudinal extension and lateral compression, and their effects on conductivity and density are opposite in character. The net result is that, as wires become more severely hard-drawn, their density first increases and then decreases again, while the electrical conductivity is reduced—in some cases to an extent exceeding 1·5 per cent. Mr. Gewecke has also studied the annealing process in these wires, but although a temperature of 210° C. is found to mark the beginning of rapid annealing, this temperature is found to vary with the duration of heating. This supports the view of Turner and Levy that the change in the copper is rather of the nature of a continuous re-arrangement of structure than a transformation from one allotropic phase into another, as suggested by Dr. Beilby.

WE have before us a draft report of the science standing committee of the Concrete Institute relative to a proposed standard algebraical notation for formulæ and calculations employed with reference to reinforced concrete. It would appear that this was considered last September at Copenhagen by a committee of the International Commission on Reinforced Concrete (established by the International Association for Testing Materials), which approved of a three-alphabet system, the three alphabets employed to be Roman capital letters, Roman small letters, and Greek smalls. The principle of the initial letter is also adopted in the report, though this cannot be made a basis for agreement with Continental nations; it is held, no doubt rightly, that the use of a self-explanatory notation is in this matter more important than international uniformity. The use of Latin smalls for linear dimensions, intensities of forces, &c., and constants, Latin capitals for areas and volumes, and total forces, Greek smalls for angles and constants, is recommended. The notation can be extended by the use of subscript letters; thus B_c may be used for "bending moment at the centre of a beam." The scheme is not put forward as part of a comprehensive system; indeed, it is pointed out that letters fail if any attempt is made at a comprehensive system for engineering formulæ alone, not to speak of physics generally. It is, however, clearly desirable that some general plan should be agreed upon, by engineers at least, before an attempt is made to work out a detailed notation for each branch of engineering work. It may be that the only plan possible is the adoption of some general principles, and those suggested are sufficiently in accord with existing usage. Possibly the Engineering Standards Committee may be able to look into the matter.

A PAPER on compounding and superheating in Horwich locomotives was read at the Institution of Mechanical Engineers on March 7 by Mr. George Hughes, the chief mechanical engineer of the Lancashire and Yorkshire Railway. A number of comparative tests have been made by the author on compound and simple engines, leading to the conclusion that the compound engine is more economical and efficient than the simple. The compound engine developed a comparatively greater pull on the draw-bar for the same indicated horse-power. The Aintree to Accrington trials show an economy of 23 per cent., and the Goole to Smithy Bridge tests an economy of 22·5 per cent., in favour of the compound, based on the steam consumption per indicated horse-power. On the basis of total steam consumption per hour, the savings in these trials are 39·7 and 33·3 per cent. respectively. In fuel consumption the savings by the compound per indicated horse-power per hour are 16 per cent. and 8·3 per cent. respectively. As the horse-powers developed by the compound are less than for the simple engine, the total fuel savings are 36 per cent. and 23·7 per cent. respectively. Using Schmidt's system of superheating on a six-wheeled coupled goods engine, comparative trials show an economy in coal of 12·93 per cent. per ton-mile for the superheater. Tests on five passenger engines having Schmidt's superheaters, extending over some months, show a coal saving of 21·4 and 21·9 per cent. per ton-mile, computed from the drivers' and guards' returns. Mr. Hughes is to be congratulated on his success in dealing with very difficult problems when applied to locomotives.

AN improved form of mouth blow-pipe is submitted for inspection by Messrs. W. and J. George, Ltd., Great Charles Street, Birmingham. It is a burner and air-tube combined, connecting directly with the gas supply by means of rubber tubing, and dispensing with a Bunsen burner. A metal collar at the mouth-piece end keeps the latter clean by preventing it from coming into contact with the bench when laid down. A similar collar at the burner end keeps the flame from scorching the wood. If desired, the instrument can be clamped to a retort stand for use at any convenient height or angle, and it serves instead of a foot blow-pipe for many small operations, such as flame, charcoal, and "bead" tests, and light glass-blowing. The article is neatly and strongly constructed, and for convenience of renewal the several parts are made to a standard size.

THE January number of the *Bulletin de la Société d'Encouragement pour l'Industrie nationale* contains two reports by A. Moreau on two forms of road-tarring apparatus, due to MM. J. Lassailly and J. Vinsonneau respectively. The tar has to be extracted from the barrels, warmed to a temperature sufficient to reduce its viscosity and remove water, and applied to the road as uniformly and as rapidly as possible. In the first apparatus of M. J. Lassailly all these operations are carried out by steam, and require a minimum of skilled control. The Vinsonneau apparatus warms the tar to 80° C. by a thermosiphon heated by a petrol burner, and distributes it by means of compressed air. The cost of superficial tarring by either process is from 8 to 10 centimes per square metre treated.

A SECOND revised edition of the valuable little book on "Butter-making on the Farm," by Mr. C. W. Walker-Tisdale and Mr. T. R. Robinson, has just been issued by the publisher, Mr. J. North, Office of the *Dairy World and British Dairy Farmer*. The original work was favourably reviewed in NATURE of February 12, 1903, and the revised edition should secure for it many new readers. The subject-matter has been brought up-to-date by revision and additions. The price of the book remains 1s. net.

ERRATUM.—March 24, p. 104, col. 1, line 36, for "9 grams" read "6 grams."

OUR ASTRONOMICAL COLUMN.

ASTRONOMICAL OCCURRENCES IN APRIL:—

April 1. 8h. 29m. Minimum of Algol (β Persei).
11. Venus, apparent diameter 28·3".
16. 16h. 0m. Saturn in conjunction with the Sun.
20–21. Epoch of April meteoric shower (Lyrids).
21. 10h. 10m. Minimum of Algol (β Persei).
23. 4h. 0m. Venus at greatest elongation (46° 13' W.).
25. 5h. 10m. to 7h. 31m. Transit of Jupiter's Satellite III. (Ganymede).
30. Mercury visible as an evening star situated amongst the Pleiades.

THE SPECTRUM OF COMET 1910*a*.—In addition to the objective-prism spectra, already mentioned in these columns, MM. Deslandres and Idrac secured some spectra of comet 1910*a* with a slit-spectroscope of great light-gathering power, which they describe in No. 11 of the *Comptes rendus*.

The series of bands usually ascribed to hydrocarbons and cyanogen, respectively, are fully represented in these spectra, with the exception of the band at λ 460. The hydrocarbon bands of the comet's spectrum exhibit different intensities to the similar bands photographed in the laboratory, with the carbon arc or the Bunsen flame as the light-source, the most refrangible of the blue bands being the most intense. An "unknown" band at λ 402·1 is similar to one which M. Deslandres found in the spectrum of Morehouse's comet, which Prof. Fowler has since traced to some carbon compound at very low pressures, but other "unknown" bands seen in the earlier spectra are not shown in that of comet 1910*a*.

HALLEY'S COMET IN JAPANESE RECORDS.—Some exceedingly interesting extracts from Japanese records, probably referring to early observations of Halley's comet, are communicated to No. 420 of the *Observatory* (March, p. 129) by Mr. K. Hirayama, of the Tokyo Observatory. They include accounts of comets which appeared in the years A.D. 684, 837, 912, 989, 1066, 1145, and 1222, and generally give some details as to the direction and appearance of the object. The details for the apparition of 1145 are especially full, the observer remarking on the peculiarity that the comet itself should remain bright after the disappearance of its tail; this he explains, four days later, as possibly due to the presence of the moon when the observation was made. The time of perihelion—as calculated by Messrs. Cowell and Crommelin—agrees with the time at which the comet was observed in the year A.D. 912.

METEORIC ASTRONOMY.—Anyone interested in the observations of meteors, and those amateurs who, without the benefit of elaborate equipment, are seeking a field where observations may become both interesting and useful, should read Mr. Denning's article, on the progress of meteoric astronomy, in the current number of *Science Progress* (No. 15, p. 444). The writer therein gives a brief outline of the ideas concerning, and the observations of, meteors, and summarises the chief events in meteoric phenomena since 1798.

The Lyrids, Perseids, and Leonids are especially described, and the association between comets and meteors is concisely discussed. Mr. Denning also mentions that some showers persist, more or less actively, for months, and instances are not wanting where radiants continued in active play all the year round, the apparent position of the radiant remaining practically constant. He also makes it obvious that the study of meteoric phenomena needs recruits; the field is a large one, the observations are comparatively simple, and the results important. So far, photography has played but a small part, and there is ample scope for useful work by those possessing suitable cameras and a fair amount of persistent patience.

STARS WITH VARIABLE RADIAL VELOCITIES.—A number of observations of stars which have been found to exhibit variable radial velocities are published by Messrs. Campbell, Albrecht, and Wright in No. 173 of the Lick Observatory Bulletins. Among the twenty northern stars discussed may be noted κ Persei, α Ursæ Majoris, ρ Leonis, ο, ν, τ and ε Draconis, η Lyræ, σ Cygni, θ Cephei, and ζ Capricorni. Six southern stars, observed by the Chile observers, have also shown a variation of velocity in the line of sight; they are δ Canis Majoris, β Crucis, η Centauri, α Lupi, γ Apodis, and ν Scorpii.

In the same bulletin Dr. Curtis announces that the definitive reductions of the spectrograms of κ Centauri confirm the variation of that star's radial velocity. Dr. Campbell notes that thirteen years' observations of Procyon show that the radial velocities do not appear to have varied appreciably in a manner to accord with a period of forty years, more or less; they seem to harmonise with Dr. Auwers's conclusion that the orbit plane of the system is approximately tangent to the celestial sphere. There is, however, a suggestion of a secondary variation of the radial velocity, of very small amplitude and a period of about seven years, for the confirmation of which further observations are necessary.

The minimum of the radial velocity of the binary system in the triple system of Polaris is shown, by the Lick observations, to have been passed, and the velocity of the centre of mass of that system appears to be increasing rapidly. Therefore velocity observations of the bright component of the Polaris system during the next few months promise to have unusual weight in the determination of the period of the third member of the system around the centre of mass of the whole system.

PRECAUTIONS NECESSARY IN PHOTOGRAPHIC PHOTOMETRY.—Under this title Mr. Parkhurst publishes a paper in the *Astrophysical Journal* (vol. xxxi., No. 1, p. 15) which contains a number of hints useful to all workers in astronomical photography.

During the work on photographic photometry, which has been carried on for a number of years at the Yerkes Observatory, it was found that the measures were affected by a number of errors introduced by differences in the method of development, by lack of uniformity in the photographic film, and by many other causes. These errors have now been fully investigated, and the results of the investigations are given in the present paper, illustrated by numerous curves. As an example of the results, it may be noted that at one point of the "developer curves" the density given by "pyro" introduces a difference equal to 0·3 mag. from that given by rodinal, whilst in the case of hydroquinone the resulting difference in magnitude amounts to 0·9. The necessity for rigidly controlling the time of development, the temperature of the developer, and other variables is just as forcibly shown by other curves.

OBSERVATIONS OF SATELLITES.—No. 172 of the Lick Observatory Bulletin contains the measures, made by Prof. Aitken during the years 1906–9, of the satellites of Mars, Saturn, and Uranus. In the case of Mars the positions of the satellites are referred to the planet's limb, but for Uranus and Saturn each satellite is referred to another satellite, except in the case of Titania. The positions given in the tables are uncorrected, except for differential refraction.

Photographs of Jupiter's eighth satellite were obtained at Greenwich on January 19 and February 11, and the resulting positions, showing fair agreement with the ephemeris, are published in No. 4393 of the *Astronomische Nachrichten*.

RESEARCHES ON ALLOYS.[1]

THE report referred to below was presented to a meeting of the Institution of Mechanical Engineers on January 21. The report itself is a memoir of some 175 pages and sixteen plates, and embodies the results of researches carried out during a period of two and a half years at the National Physical Laboratory. These researches constitute a continuation of the previous work of Messrs. Carpenter and Edwards on the alloys of copper and aluminium as recorded in the eighth report to the Alloys Research Committee. Owing to the magnitude of the task which would have been involved in undertaking a complete study of a ternary system of alloys, the authors at the outset decided to limit their investigations to those regions of the system

[1] Ninth Report to the Alloys Research Committee of the Institution of Mechanical Engineers, by Dr. Walter Rosenhain and Mr. F. C. A. H. Lantsberry, on "The Properties of Some Alloys of Copper, Aluminium and Manganese."

where results of practical interest were to be anticipated. As regards the heavy alloys, consisting principally of copper, important results were to be sought only in alloys containing more than 85 per cent. of copper, while at the aluminium end of the series only alloys containing more than 95 per cent. of aluminium could be expected to yield results of practical value. The study of the constitution of the alloys has, however, been pushed beyond these limits in order to render the data obtained over the "useful" range more intelligible.

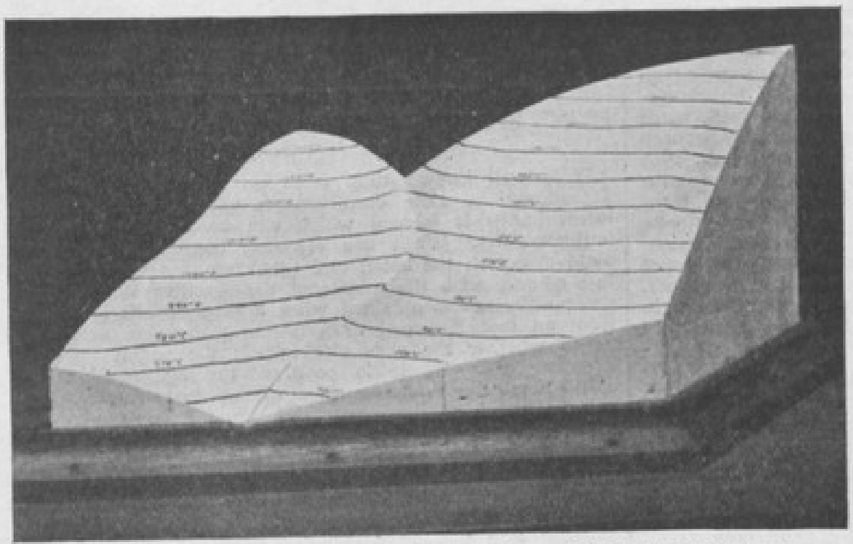

FIG. 1.—Model of the liquidus surface of ternary alloys of copper with aluminum and manganese.

The report therefore includes a model of the "liquidus surface" of the ternary system over a range of alloys containing less than 11 per cent. of aluminium and less than 10 per cent. of manganese. This model is constructed on the well-known principle of trilinear coordinates, in which the range of compositions of ternary alloys is represented by an equilateral triangle; the liquidus surface is constructed by erecting a vertical ordinate representing the temperature of initial freezing, on the points representing the alloys examined. A photograph of the model, upon which contour lines representing each 10° C. have been drawn, is reproduced in Fig. 1. The well-marked minimum of the copper-aluminium series, occurring at a concentration of about 8½ per cent. aluminium, is continued into the ternary system in the form of a valley in the liquidus surface, and microscopic examination has shown that alloys to the right of this valley (*i.e.* nearer the copper side) are homogeneous, while those to the left are duplex. A comparison of the model with the mechanical properties of the alloys further indicates that in the ternary alloys, just as in the binary copper-aluminium series, the presence of the second phase to the left of the minimum renders the alloys stiffer, stronger, and less ductile.

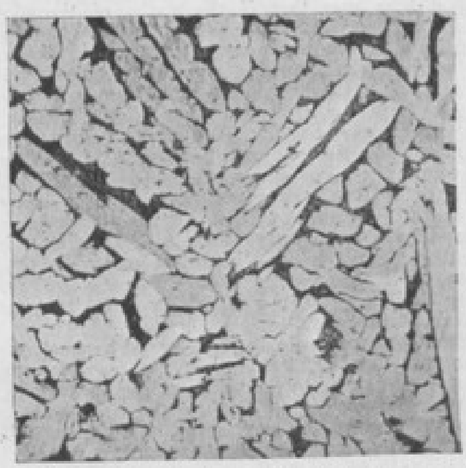

FIG. 2.—Micro-structure of alloy as cast in sand.

Throughout the range covered by this model (which represents the data obtained from more than 100 different alloys) no new phase resulting from the presence of manganese can be detected. This result is of special interest, because some of the alloys included in this group, viz. those lying towards the left-hand corner of the model, are distinctly magnetic, their permeability increasing towards the extreme left-hand corner of the figure. If, therefore, the magnetic properties of these alloys (which approach the type of some of the well-known Heussler alloys) are due to the existence of a magnetic metallic compound, this compound must be soluble in either or both the phases found in these alloys.

A photomicrograph typical of the structure of alloys in this region is reproduced from Fig. 75 of the report (Fig. 2), representing the structure of an alloy containing 8·56 per cent. of aluminium, 4·77 per cent. of manganese, and 86·67 per cent. of copper in the sand-cast condition. The effects of heat-treatment on the micro-structure of these alloys are very marked. Thus Fig. 122 of the report (Fig. 3) shows the structure of another alloy of this type after annealing at 900° C., both these photographs being taken at the same magnification (150 diameters). Quenching the same alloy from 900° C. produces a totally different structure, reproduced from Fig. 129 of the report (Fig. 4), and this change renders the alloy hard and brittle.

As regards alloys at the light end of the series, the introduction of manganese is found to give rise to the formation of a definite compound, Al_3Mn, which tends to render the alloys hard and brittle. If present in large proportions, this compound appears to undergo a more or less gradual change, which leads to the spontaneous disintegration of the alloys containing it; an ingot consisting of 65 per cent. aluminium and 35 per cent. manganese is a hard, metallic mass when first cooled, but falls to a fine crystalline powder in the course of six or eight hours, and this process appears to be independent of oxidation.

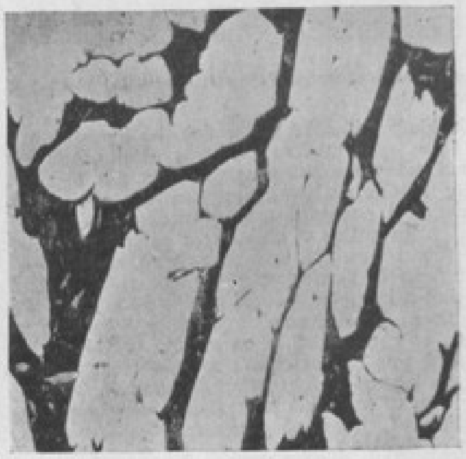

FIG. 3.—Micro-structure of alloy after long annealing at 900° C.

In the "useful" light alloys of the ternary system, however, such disintegration does not occur.

The mechanical properties found in the best of the alloys of the ternary system are remarkable. The authors suggest that alloys of copper with aluminium alone are generally contaminated with a certain proportion of alumina, formed when the aluminium is added to molten copper containing more or less oxygen; by the previous addition of manganese, which acts as a reducing agent, the formation of this alumina is inhibited and the properties of the resulting alloys are improved. It has been found that a small percentage of manganese renders the alloys more ductile without reducing their ultimate strength, while larger proportions of manganese increase the strength but lessen the ductility of the alloys. Sand-castings giving an ultimate strength of 36 tons per sq. inch with an elongation of 22·5 per cent. on 2 inches have been obtained, while in the form of rolled bars the best of the heavy ternary alloys reach an ultimate strength of 43 tons per sq. inch with 22 per cent. elongation. Perhaps the most remarkable result, however, is that obtained with one of these alloys in the hard-drawn condition, where an ultimate strength of 52 tons per sq. inch with 10 per cent. elongation has been obtained. This is probably the strongest alloy known, containing more than 80 per cent. of copper. The remarkable properties of this alloy led Dr. Rosenhain to take up the challenge recently thrown out by Sir Gerard

FIG. 4.—Micro-structure of alloy after quenching from 900° C.

Muntz in his presidential address to the Institute of Metals, to the effect that modern scientific attainments in metallurgy had not yet enabled us to produce a bronze cutting tool. By further cold-working one of these hard-drawn alloys under a powerful testing machine, a material was produced from which a chisel was ground, and with this it was found possible to incise hard stone or—with a different form of cutting edge—to cut wood so easily and cleanly that a lead-pencil could readily be sharpened with it.

A result of considerable importance has been obtained by means of abrasion tests on certain of these alloys; the test applied was one of simple wear against hardened steel rollers, the loss of weight being determined. It was found that the best of these alloys possess a very great resistance to abrasion of this kind far surpassing even the harder varieties of steel. This property, together with their very great strength and the fact that they can be machined and finished very readily, should render them of special value for the construction of scientific instruments, particularly for those parts where much mechanical wear has to be met. These alloys also possess a very great power of resisting corrosion, both by fresh- and sea-water, while tests made upon them at temperatures up to 500° C. indicate that they retain their strength up to 300° C.; these results suggest the possibility of their employment for the blades of steam-turbines working with superheated steam.

As regards the light alloys, containing more than 95 per cent. aluminium, the properties of the ternary alloys are not markedly superior to those of the aluminium-copper alloys, except, perhaps, that in the form of chill-castings a higher tensile strength has been obtained. The presence of manganese, however, appears to protect these alloys from corrosion to a marked extent, the specimens of these alloys exposed to sea-water, for instance, becoming coated with a black patina consisting largely of oxide of manganese. Specimens of some of these alloys have retained their original brightness after more than two years' exposure in the laboratory cupboards, thus indicating a decided superiority over pure aluminium, which has hitherto been regarded as decidedly superior to its alloys in regard to corrosion.

ELECTRICAL DISCHARGES OVER PHOTOGRAPHIC PLATES.

IT is well known that when an electric discharge is allowed to take place over a photographic plate a latent image is formed which can be developed in the ordinary way. When one electrode consists of a metal plate placed at the back, and the other is a wire brought into contact with the middle of the sensitive side, a very great difference is obtained when a single spark is passed from an induction coil or Wimshurst machine according as to whether the wire electrode is the positive or the negative one. If it is positive, the figure—which is called a positive figure—consists of numerous ramifications suggesting meandering streams, while if it is negative the main lines in the figure change their directions very abruptly, and are terminated by expansions suggesting fans or palm-leaves. In each case the discharge may or may not pass to the edge of the plate; if it does, the corresponding line is very broad, with a finer, well-defined, intenser line passing midway through it. Such figures were obtained first by Mr. J. Brown, of Belfast, and have since been repeated by many experimentalists. Experiments made by Prof. J. A. McClelland and Mr. Campbell Swinton seem to render certain that the latent image is due to the luminosity of the discharge, and not to a direct electrical action.

I have recently made experiments with the object of extending our knowledge with respect to the formation of these figures, and the results have been communicated to the Röntgen Society, appearing in the society's journal for January.

My first idea was that if the paths of the discharge represent moving electricity they would be seriously modified in a perpendicular magnetic field. However, the greater part of the figure is apparently quite unchanged in such a field; the only evidence of change is in the trunk discharges that flow over the edge of the plate. These become still broader, the fine pilot spark, however, remaining apparently unshifted, and forming a sharp boundary to the trunk discharge along one edge. The direction of lateral shift is that corresponding to a wire carrying a current from the positive to the negative electrode.

A blast of air produces the same kind of effect. Indeed, if several radiating trunk discharges occur on the same plate, the effect in a perpendicular magnetic field is much the same as if a cyclonic blast of air had circulated over the surface of the plate.

The fine tracery lines in the fan-shaped expansions which terminate the lines of discharge in the case of a negative spark are very similar to the paths of the separate portions of an exploded projectile. Such paths are easily plotted by superposing a radial component of velocity, following any assumed law, upon the initial translatory velocity of the unexploded projectile. Owing to this similarity it is suggested that these tracery lines represent the actual paths of single ions or of simple groups of ions in the electrical field.

When the discharge takes place in a partial vacuum very considerable changes occur. In particular, as the exhaustion proceeds a new phenomenon appears, which reaches its most marked stage at about 17 cm. pressure. This is somewhat difficult to describe, and is shown in Fig. 1.

The wire terminal touched the plate nearly at its centre. Besides the trunk discharge ascending the plate are seen two discharges in the shape of narrow triangles the bases of which are close to a *dark space* separating the glow surrounding them from a central rosette. The pointed ends are close to a *second dark space*, and are terminated by tuft discharges. These appearances show only imperfectly in the reproduction, but are exceedingly definite on the original negatives. The remarkable feature is the very sharp and well-defined edges which these triangles possess. It is suggested that there may be a connection

FIG. 1.—Discharge at a pressure of about 17 cm.

between these and the phenomena of *electrostriction*, but so far no definite theory has been formulated. In very many cases there is visible a *dark continuation* of these squirts on the side of their bases, which suggests that the effects seen represent a part only of the electrical effects taking place. The shape of the squirts suggests that in one region they experience a pressure from the disturbance which creates the luminous glow through which they stretch, and that this pressure is replaced by a tension as soon as the second dark space is passed. The luminous

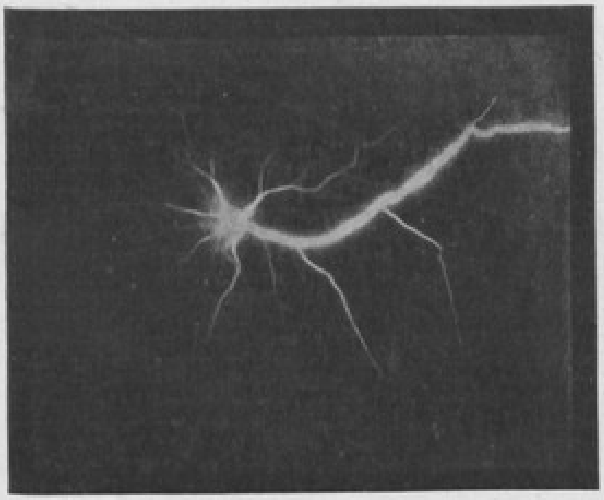

FIG. 2.—Negative figure in coal-gas at ordinary pressure.

glow bordered by the two dark spaces suggests a single stria as seen in a vacuum tube; the phenomena of ionisation in the two cases may be similar.

When the gas in which the plate is immersed is changed the effects obtained change also. Each gas produces a negative figure characteristic of itself. *The triangular squirts above referred to are given by air alone.* The negative figure in coal-gas at ordinary pressure is shown in Fig. 2. The figure in nitrogen is also very singular. It is noteworthy that the positive figures in various gases present very few differences from each other.

When a small metallic triangle placed on the sensitive film forms the electrode, some striking differences are obtained according as it is made the positive or the negative terminal. When it is positive the ramifications start in the main from the corners (Fig. 3); on the other hand,

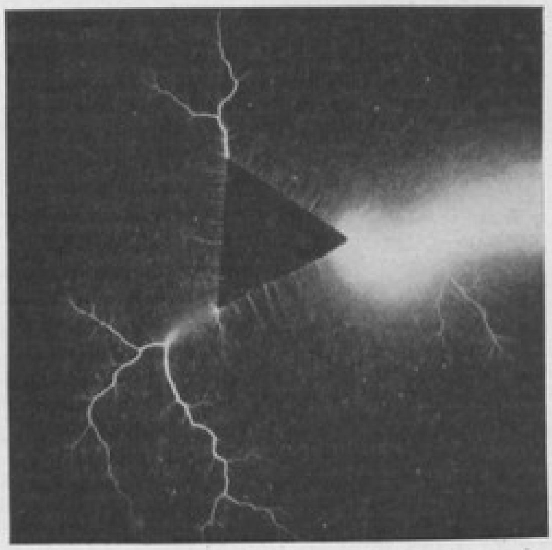

FIG. 3.—Positive ramifications from corners of triangular electrode.

when it is negative a preference is shown to leave at right angles to the edges (Fig. 4).

Throughout these descriptions it has been assumed that the discharges *leave* the electrode. It must be admitted that at present there is no proof that the streamers do not represent currents that advance *toward* the electrodes.

For some further details, as well as for numerous other

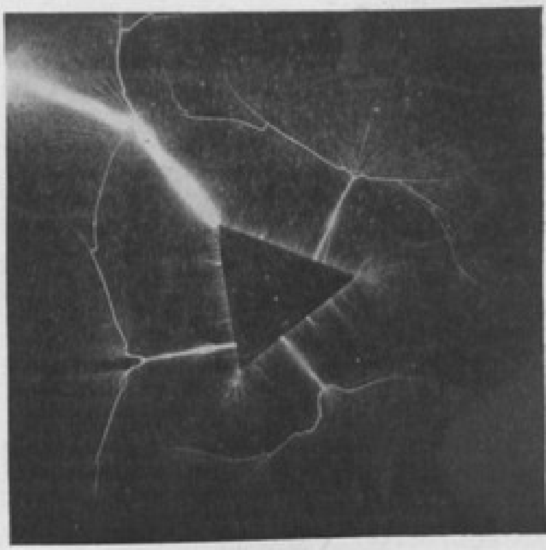

FIG. 4.—Negative streamers exhibiting a preference to leave the triangle at right angles to edges.

reproductions, reference must be made to the paper in the Journal of the Röntgen Society from which the figures illustrating this article have been selected. Perhaps it should be added that these are photographic positives from the original photographic negatives.

ALFRED W. PORTER.

FORESTRY.[1]

THE present half-yearly issue of the Transactions of the Royal Scottish Arboricultural Society contains a considerable number of useful and interesting articles on forestry and woodcraft generally. An article entitled "The Duty of the State as Regards Afforestation" shows, in a striking summary of the opinions expressed on the subject by landowners, foresters, and men of science, that all are agreed that the State should encourage extended afforestation. The article indicates very clearly what the State can and should do with this object in view.

In "Afforestation and Local Taxation" Sir Kenneth Mackenzie, Bart., president of the society, shows the disastrous results to the local taxpayer which might follow indiscriminate afforestation in large, continuous blocks on land compulsorily purchased by the State. The State could do a great deal to encourage afforestation by private owners by removing the burdens which at present deter many from extending their plantations. In the words of the author:—"There is a premium offered at present against planting—as long as an owner occupies his land with sheep he only pays rates on three-eighths of its valuation. If he fences and plants it, he has to pay rates on the full value appearing in the Valuation Roll."

"The Sitka Spruce as a Tree for Hill Planting and General Afforestation" (with plate), by Mr. Crozier, Durris, is the most important article which has yet appeared regarding the sylvicultural characters and capabilities of this important conifer.

"Vegetable Remains from the Site of the Roman Military Station at Newstead, Melrose," is an article which will appeal to historians and antiquarians. Samples of deposits from the pits and trenches of the Roman station were examined by Mr. H. F. Tagg, of the Royal Botanic Garden, Edinburgh. The numerous twigs and branches examined belonged to some seven different species of trees which have always been considered indigenous. There was no evidence to show the presence in Britain, at the period of the Roman occupation of this station, of species of exceptional interest.

Mr. W. Mackenzie, Forester, Novar, contributes an article entitled "Underplanted Larch Plantations at Novar." The sylvicultural methods adopted to combat the ravages of the larch canker fungus are clearly and succinctly described.

"Continental Notes—Germany," by Mr. B. Ribbentrop, with figures, gives a review of the recent sylvicultural developments in that country, while Mr. A. G. Hobart-Hampden deals in a similar manner with French sylviculture.

The society's excursion to the forests of Bavaria, which took place last August, is interestingly described by Sir Andrew N. Agnew, Bart. In "Notes and Queries" are included many topics of great value to sylviculturists, and the "Reviews and Notices of Books" will bring them in touch with the recent literature on the subject.

[1] Transactions of the Royal Scottish Arboricultural Society, vol. xxiii., part i. (January, 1910).

WORK OF A LOCAL SCIENTIFIC SOCIETY.

THE value of the work accomplished by local scientific societies is, perhaps, not always given adequate recognition. On what may be described as the educational side, such societies create and foster interest in the world of nature; and out of this comes the desire to investigate parts of the field of science. A report, programme, and presidential address received recently from the North London Natural History Society provide evidence of the well-directed activity and progressive spirit which should be characteristic of a society that desires to extend a knowledge of science and promote its progress. The society is particularly to be congratulated upon its research committees, which are concerned, among other matters, with the flora, lepidoptera, and birds of the local district. This district covers an area within twenty miles of St. Paul's, and is subdivided into twelve sections for recording.

As instances of the valuable work which these committees accomplish, we mention a few points in the annual report for 1908—that for 1909 not yet being available. It appears that the adventitious flora of the district is spreading widely, while, as might be expected, the native flora is diminishing. Twenty species were recorded for the first time in 1908, making the total of 684 species for the district. Six of these were aliens, and eight were new records for the outlying salt marshes of West Thurrock. Six additions were made to the list of Lepidoptera, bringing the total up to 542. The ornithological research committee, which was inaugurated in 1908, records 110 different species of birds, of which seventy-nine were then known to nest within the district. Two members of the biological research committee, Messrs. L. B. Prout and A. Bacot, have carried out a research on inheritance in *Acidalia virgularia*, and a paper on the results of their investigation was communicated to the Royal Society in February of last year.

It is clear, therefore, that the members are actively engaged in the extension and advancement of scientific knowledge. We congratulate the society upon the keenness and energy of its members and committees, both of which are worthy of emulation by other local scientific societies. The society has just taken rooms in Salisbury House, Finsbury Circus, for its meeting-place, library, and collections. The annual subscription is only five shillings a year, being kept purposely low in order to place the advantages offered within the reach of everyone. It is to be hoped many new and faithful observers will thus be brought within the scientific field through the instrumentality of the society. Subjoined is the main part of the presidential address delivered before the society on January 11 by Mr. Louis B. Prout.

Let us have done with the days of a nearly stationary membership of about seventy, and an average attendance of perhaps a score or less; let us individually use every endeavour to attract to our society all the nature-lovers with whom we come into contact, whether they aspire to be called "naturalists" or not; let us remember that no one who lives within reach of London at all can now plead the *inaccessibility* of our meeting rooms as an excuse for holding aloof, and that, although our local researches will continue to justify our title of the "North London Natural History Society," yet there is nothing whatever to prevent our drawing upon South London just as extensively as upon North London for our membership. It is proverbial that nothing succeeds like success, and if only the next few months witness anything like the accession of new members which the new facilities make feasible, the future of our society should be well assured.

I have directed attention more than once to the love of *facts* which has characterised the early career of most of those naturalists who have become the most famous for their *theories*. The pioneers of evolution—Darwin, Wallace, and Bates—were all careful and accurate recorders at a time when most "mere collectors," at least in entomology, no more thought of labelling every specimen with locality and other details of information than the philatelist of labelling every stamp with the date of purchase and the name of the dealer from whom it was obtained. The two hobbies were very nearly on a par. The collecting was, without reservation, an end in itself, and if the entomologist had any advantage over his brother collector, it was only in that he was developing a somewhat more æsthetic taste, and probably—unless he, too, collected solely in auction-rooms and similar localities—a somewhat healthier body. Science and all branches of research were equally beyond the mental horizon of both; and how could it matter when or where a specimen was obtained, unless it might be from the mercenary motive of knowing how to obtain more? I do not say that the outlook of the average collector has radically changed; I do not even say that I wish it radically to change. I have no patience with the lordly being who speaks and writes disparagingly, or even contemptuously, of the "mere collector," and forgets that he only theorises because it amuses him to do so, just as the other only collects with like intent; but I think most have now been educated up to that point where they know that there *is* value in facts, and I believe that the majority of these are willing to "take themselves seriously" to the extent of observing and recording those facts; and if there are any listening to me who have *not* yet realised these things, I would urge them from henceforth to bear their part in this movement, which may result in issues more far-reaching than any of us can at the moment conceive. Let me repeat that it is not necessary for every nature-lover, nor for every collector, to become a man of science; yet everyone may become in some measure a contributor to science.

When do the facts observed, or believed to have been observed, become *data*? Not when they are thrown away loosely into the chambers of memory, to be brought out again for use a few years later, clogged with the dust of time, or metamorphosed by long yet unsuspected contact with some subtle subconsciousness with which they ought to have had no affinity. No; the memory, however excellent, is not a safe repository for facts which are to be used as data; as soon as possible they ought to be reduced to writing. For it is impossible to overestimate the importance of absolute accuracy as a basis for all scientific generalisations.

I have often been impressed with the thought of the dependence of the greatest statisticians on the humblest recorders. Most of us have had questions addressed to us by Prof. Karl Pearson on simple questions of family statistics; and the entomologists have been asked to furnish to the Evolution Committee of the Royal Society certain data regarding percentages of black and of white moths among their favourites. These are but random examples which occur to me of what is constantly going on in the world of to-day; and yet on the faithfulness of the replies to such questions may well hang the entire development of the infant science of eugenics, the whole welfare, and perhaps ultimately the very continuance, of the human race. Fortunately, I believe—and one may hold this belief without a very over-exalted estimate of the average integrity of mankind—the danger of wilful perversion of facts which are to be used as data is extremely small. No doubt there are romancers here and there, and a de Rougemont or a Dr. Cook may set back the clock for a moment or two on occasions; but men such as these have generally some motive of self-interest behind their romancing, and I do not think there is any large army of hoaxers for hoaxing's sake.

Although, however, there is very little to fear from wilful deceivers of their fellows, there is very much to fear from unconscious self-deceivers. It is true that we have little to depend upon, whether in nature-study or in scientific research, but the evidence of our senses; but it is equally true that we must not allow ourselves to be deluded by our senses. I have on other occasions urged that the cardinal virtue of a naturalist is fidelity to his own observations, but he must make very sure that they *are* observations, and not imaginings. It is a perfectly well-known fact that even careful and experienced men of science have sometimes been led astray by certain psychological processes, and have seen things which it has afterwards been proved to demonstration were not, and could not have been, present for them to see.

The subtle enemy which all observers and recorders have to fight is, I believe, named by psychologists "suggestion," or, more particularly, "auto-suggestion." All of us know, and yet few of us give the knowledge its due weight in dealing with the analysis of our observations, that whatever is present as a mental background is ever liable to colour the newly arriving impressions from without. If something which we see, falls in naturally with our expectations, that is, if its incidence on the mind causes no sense of jarring, we assume that it is correctly observed, and make no attempt at verification; if, on the other hand, it conflicts with our expectation—in other words, with past experiences or general habits of thought —we are sceptical, and demand a repetition of the observation before acknowledging that our senses have not deceived us. Now is there not really a great deal to be said in favour of a diametrically opposite course? Should we not be more suspicious of the expected when it is observed, and more trustful of the unexpected? I need scarcely add that I do not mean this to be the universal principle of life; we should have more than enough to do if, every time we entered our homes, we made it a duty to investigate whether the familiar faces and objects with which we met—and had been expecting to meet—might not in reality be the phantasms of our own brain! I am referring solely to phenomena which are under observation or investigation for furnishing scientific data; it is in these that we are too apt to accept the expected, perhaps also too apt to discredit the abnormal.

A plain and evident observation, made under no preconceived notion that it was about to be observed, may, in a normal state of health, be noted down as a fact, and thenceforth relied upon. If a member of our ornithological or Lepidoptera committee observes a bird or a moth with which he is well acquainted, he is entitled to make and to use the "record," which should be given full credence. Of course, there may be an error—infallibility is not an over-common attribute of man—and it is always satisfactory if two or three can make the observation simultaneously, or in such a way as to confirm one another, or if, as with our botanical committee, a specimen can be obtained as a voucher; but no good purpose is served by constant suspicion of data of this kind unless the recorder has proved himself untrustworthy. On the other hand, the observer himself should be the first to desire every possible verification, especially in cases of intricacy or difficulty of observation, such as in most microscopic work, or where he has any reason to suspect that "the wish is father to the thought." In all such cases a fact should not be considered as established until it has been verified two or three times, and under the most favourable conditions obtainable.

The most difficult questions of all have been left until last, and I really do not feel competent to give either an adequate answer. What facts or data are worth recording? And what steps should the recorder take to place them at the disposal of the specialist who could use them? In regard to the first question, I would say that, ideally, almost everything is worth recording; but, practically, life is too short, nature too long. While we are staying to record something commonplace, or already well known, we may be missing valuable opportunities of turning our attention to something more important. A retentive memory should be cultivated, so that we may know, to some extent, what has already been established by ourselves or others; and we shall then find that the most casual passing attention will suffice to accumulate any supplementary testimony that may be needed. For the rest, I think we ought to work upon the principle that a few things thoroughly observed and confirmed will form from us a worthier contribution to the sum total of science than a hundred half-observed and half-guessed at. As to the second question, To what use should the recorder put his data? I touched upon this in my former address, but there are great difficulties in the way of the application of sound methods, and the ideal arrangements are as yet far off. A society like ours ought to have a research committee in every possible field of nature-study, besides one or two committees for coordination of work along different lines—organisation, biology, topographical knowledge, bibliography—besides a sort of clearing-house for miscellaneous information; then (and not until then, I fear) it will be possible for observers rightly to place their data, and though much will be handed in which leads no further, there will also be much solid material for the rearing of the noblest edifices in the future of natural-history research.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—The Hebdomadal council has appointed Prof. Poulton, F.R.S., Dr. Dixey, fellow of Wadham College, and Dr. Malcolm Burr, New College, as representatives of the University at the International Congress of Entomology to be held at Brussels in August next.

WE learn from *Science* that Columbia University has received an anonymous gift of 70,000*l.* for the erection of a building for the faculty of philosophy. The University has also received anonymously 3000*l.* for work in agricultural education. From the same source we gather that a zoological laboratory is to be erected at the University of Pennsylvania, at a cost of about 50,000*l.* In making the announcement recently, Provost Harrison stated that it would be "the most complete biological laboratory yet erected." By the will of Mrs. Mary A. Richardson, Tufts College is to receive 8000*l*. for fellowships.

By the will of the late Prof. Hilary Bauerman, the sum of 500*l*. is left to the Memorial Fund of the Iron and Steel Institute. The residue of his property (about 15,000*l*.) is left, subject to a life interest, upon trust to be applied by his trustees in the encouragement of the study of mineralogical science at the Royal School of Mines, by means of lectures on subjects of prominent or educational interest at the time, but which are not included in the ordinary list of subjects taught, and for making grants to enable students to make special investigations or to enable them to travel for the better pursuance of their studies.

In addition to the regular courses of instruction given by Profs. Sedgwick and MacBride and by Mr. Dobell at the Royal College of Science, a series of special courses on important departments of zoology are being delivered by lecturers, each as specialist in his particular branch. A course on heredity and variation, by Mr. A. D. Darbishire, has just been completed, and two other courses of great interest will commence shortly. One of these courses will be delivered by Dr. E. J. Allen, director of the Plymouth Marine Biological Station, and will treat of marine biology, with special reference to its bearing on fishery problems as well as on oceanic science. The question of the feasibility of the artificial rearing of marine animals of economic importance will also be dealt with. An opportunity will thus be afforded to the student of obtaining in a condensed form the results of a lifetime devoted to this form of research. The lectures will be delivered on Tuesdays, Wednesdays, and Fridays at 2 p.m., commencing April 10, and will be accompanied by practical work, for the benefit of which salt-water aquaria have been installed in the Royal College of Science, and are now in successful operation; at the conclusion of the course in London the practical part of the course will be continued at the Marine Laboratory, Plymouth, during July, for those desirous of prosecuting the study further. Another of these courses, treating of organs of embryonic and fœtal nutrition, is in charge of Mr. Richard Assheton, and will be held on Tuesdays and Thursdays at 5 p.m., beginning on Tuesday, April 19. This course will deal with the comparative anatomy and physiology of the placenta in a comprehensive way, taking into consideration all cases of trophic connection between mother and offspring throughout the Vertebrata, and tracing thus the fully developed placenta of the highest forms from its first beginning. An account of the œstrus cycle will also be given, and the question of the influence of the internal secretions of the generative organs on the organism will also be dealt with. The course will be accompanied by demonstrations and practical work. Such a course has never before been given in London, and it should enable a medical student to grasp the meaning and physiology of the placenta in a way unattainable to those who have only had acquaintance with the human type of the organ.

In an address at the recent annual banquet of the American Chemical Society, Dr. R. C. Maclaurin, president of the Massachusetts Institute of Technology, referred to the necessity for keeping industry in the closest possible touch with science. In the course of his remarks, he said:—"The awful example, the standing warning in this respect, is the case of England. There a few years ago was celebrated the fiftieth anniversary of an English chemist's epoch-making discovery of mauve, and yet the jubilee in honour of this man of science was the occasion of the funeral oration of the colour industry in his own country. This deplorable result was brought about entirely by two things that are closely related:—first, the failure to keep industry in close touch with science, and, second, the impatience of the manufacturer and his narrowness as a self-styled 'practical' man. The practical Englishman is too apt to be impatient of the slow processes of research. He wants to be compensated in hard cash, and at once. The German, on the other hand, has learned to be no less practical, but he has retained the traditions of a race of idealists plodding patiently and surely to success. But the field of *industrial* chemistry is not the only one in which the times are critical and exciting. That is equally true of the pure science itself. I hope my own predilection for physics does not mislead me into thinking that the most conspicuous development of chemistry during the past quarter of a century has been on the physical side; but, in any case, there can be no question that the artificial boundaries between physics and chemistry are being rapidly removed, and, of course, it is well to have it frequently brought home to us that all such boundaries are purely artificial. One point suggested by recent experiences is that we should pay more serious attention than we usually do to the logic of science, and have as clear ideas as possible as to what we are really aiming at, as to what we can reasonably expect to do and not to do. It seems unfortunate that men of science are still so much scared by the bogey of metaphysics. What we have to be afraid of is not metaphysics, but bad metaphysics, and it is difficult to accept the simple faith of many a man of science that his metaphysics is to be preferred to any other brand merely because it is either unconscious or naive. A little quiet thought and study should at least have the good effect of enabling us to preserve our calm when things seem to be tumbling down. We should realise, perhaps, that a science like chemistry is, above all else, a work of art, and that concepts like atoms, energy, and the like are not much more than pigments with which we paint our pictures. The next generation may find new pigments or mix the old ones differently."

SOCIETIES AND ACADEMIES.

London.

Geological Society, March 9—Prof. W. W. Watts, F.R.S., president, in the chair.—E. E. L. **Dixon** and A. **Vaughan**: The Carboniferous succession in Gower (Glamorganshire). The succession in three districts in Gower is described. With the description of the lithological sequence are notes on some breccia-like limestones, on "lagoon-phases," and the origin of radiolarian cherts. From the faunal sequence it is concluded that many zones are characterised by the same assemblages as in the Bristol area. The lithological sequence shows (1) that over the area the depth of the Carboniferous sea underwent a cycle of change during Lower Avonian time, the initial deepening being followed by shallowing up to the top of the lower part, C_1, of the Syringothyris zone, deposited almost at sea-level; (2) that a cycle marked the ensuing period up to the top of the Seminula zone; (3) that a cycle took place in the Dibunophyllum zone, the latter reaching the surface; and (4) that a fourth cycle characterised the Posidonomya zone. A comparison of the sequences and thicknesses in the districts shows that the axis on which the movement during the first cycle hinged was different in direction from the axis during the second cycle. The bearing of these movements on the question of the delimitation of the divisions of the Avonian is then discussed. It is suggested that the base of the upper part, C_2, of the Syringothyris zone should form the base of the Upper Avonian. The base of C_2 in at least two localities is connected, faunally, with the zones below, whereas the fauna of the main mass of C_2 passes into S_1 without appreciable change other than the introduction of Lithostrotion. Probably the break between the Lower and the Upper Avonian should be taken at a level within C rather than at the base of the Seminula zone.

Physical Society, March 11.—Prof. H. L. Callendar, F.R.S., president, in the chair.—Dr. W. H. **Eccles**: Coherers. A method of investigating detectors is developed with special reference to the relations between the energy given to the detector in the form of electrical vibrations and the energy delivered by the detector, as direct current, to the circuit of the indicating instrument. The author puts forward the hypothesis that the properties of an oxide coherer may arise solely from the temperature variations caused in the minute mass of oxide at the contact by the electrical oscillations and by the applied E.M.F. He examines the hypothesis mathematically, and shows that most of the phenomena recorded above can in this way be accounted for as perfectly as the present state of the measurements permits.—G. C. **Simpson**: Earth-air electric currents. The paper describes a method for recording automatically the electrical current which passes from the earth into the air during periods of fine weather. A large plate (17 metres²) was placed in the open as near to the ground as was consistent with efficient insulation; this was then connected to an insulated vessel, from which water issued through an orifice surrounded by an earth-connected cylinder. The water as it dropped from the insulated vessel carried away, by the well-known "collector" action, all the charge which the exposed plate received, and the latter remained at zero potential. The charged water drops were collected in a vessel connected to a self-registering electrometer, which was earth-connected for an instant at the end of every two minutes. The paper describes the sources of error and the method of determining the value of the earth-air current and of the conductivity of the air from the records of the electrometer.—Dr. B. D. **Steele**: An automatic Toepler pump designed to collect the gas from the apparatus being exhausted.

Zoological Society, March 15.—Mr. E. T. Newton, F.R.S., in the chair.—T. **Goodey**: A contribution to the skeletal anatomy of the fish *Chlamydoselachus anguineus*, Gar. The author dealt with the anatomy of the axial and appendicular skeleton, paying particular attention to the structure of the notochord. He stated that the notochord in this fish had generally been regarded as unconstricted except at the extreme anterior extremity, but that he had ascertained the presence of well-developed, calcified cyclospondylic centra at the anterior end of the column, and of calcified cyclospondylic centra of two sizes in the main caudal region.—W. R. **Ogilvie-Grant**: Additional notes on the birds of Hainan. The notes were based on a small collection of Hainan birds recently forwarded to the Zoological Society by Mr. Robert Douglas, of Shanghai, and, at the suggestion of Dr. Chalmers Mitchell, F.R.S., presented to the Natural History Museum. The collection contained several species of great interest, and two were described as new, namely, *Tephrodornis hainanus* and *Pitta douglasi*. Among the rarities, attention was directed to the remarkable magpie (*Temnurus niger*), with its curious truncate tail-feathers, the beautiful green jay (*Cissa katsumatae*), recently described by the Hon. Walter Rothschild, and a bulbul (*Pycnonotus sinensis*), not hitherto recorded from the island.—Dr. Einar **Lönnberg**: The variation of the sea-elephants.

Royal Meteorological Society, March 16.—Mr. H. Mellish, president, in the chair.—Captain H. G. **Lyons**: Climatic influences in Egypt and the Sudan. From early times the ancient Greeks recognised the marked difference between the climate of the Mediterranean and that of Africa, and Aristotle indicated correctly the rains of Ethiopia as the cause of the annual flood of the Nile. Travellers have supplemented our knowledge from time to time, but only within the last ten years has a network of meteorological stations given precision to our views and furnished a basis for further investigations. The comparatively low relief of the country, which lies as a vast land area in low latitudes, combined with the effect of the north-easterly trade winds which sweep over it, produce the hot and dry conditions which are so characteristic of north-eastern Africa. Modified somewhat in the north by the warm waters of the Mediterranean, and in the south by the rains of the monsoon in summer, the highest temperatures and most arid conditions are reached between Wadi Halfa and Dongola, where northerly winds, clear skies, and a great range of temperature prevail throughout the year. The important rains are those falling in Uganda, the southern plains of the Sudan, and on the tableland of Abyssinia, since they not only provide the whole supply of the Nile and its tributaries, but largely control their regimen. Fed by the south-easterly air currents blowing in from the Indian Ocean, these monsoon rains supply the equatorial lakes and the tributaries of the Nile; but it is the Abyssinian tableland, with its heavy summer rainfall, which is most effective, since it furnishes the whole of the Nile flood and enables the Nile to maintain itself through 1500 miles of desert. As the sole source of the flood, the variation of these rains directly determines the abundance or deficiency of Egypt's supply, so that this climatic problem is of immense importance. Hardly less important in these days of intensive cultivation of cotton is the study of the winter storms which occasionally break in the Sudan and Abyssinia, raising the level of the rivers and increasing the supply of the Nile appreciably at a time when the normal supply is inadequate. The climate of the region not only influences the water supply, but the great range of temperature rapidly disintegrates the rocks, and the wind removes the finer portion of the material. In this way the deserts are being constantly modified, and vast ranges of sand dunes are piled up. The distribution of vegetation is very markedly influenced both by the moisture and by the physical character of the country.

Linnean Society, March 17.—Dr. D. H. Scott, F.R.S., president, in the chair.—E. P. **Stebbing**: The life-history of *Chermes himalayensis* on the spruce (*Picea Morinda*) and silver fir (*Abies Webbiana*) of the N.W. Himalaya. The life-histories of the European species of Chermes, *C. abietis* and *C. viridis*, have been studied by Blochmann and L. Dreyfus in Germany, Cholodkovsky in Russia, and more recently by E. R. Burdon, of Cambridge. It is now well known that *C. viridis* has alternating series of generations upon the spruce and larch. The discovery that a species of Chermes formed galls on the spruce in the Himalaya was first reported by A. Smythies, of the Indian Foreign Service, in 1892. These were considered by the late Mr. Buckton to be *Chermes abietis*. Investigations commenced by the author in May, 1901, and carried on intermittently up to July, 1909, have led to the discovery that this Chermes, although an undescribed species, has a life-history somewhat similar to the European species of the genus, having series of agamic generations alternating between the spruce and silver fir (which grow together in mixture in the western Himalaya), with a sexual generation occurring but once a year, in the autumn, on the spruce. The paper shows that the Himalayan insect passes through similar generations to its European congeners, to which the names Fundatrices, Alatæ, Colonici, Sexuparæ, and Sexuales have already been given by European investigators. The periods at which these generations are to be found upon the trees in the Himalaya differ considerably, however, from the European ones, and are apparently chiefly governed by the appearance of the monsoon early in July in this region.—R. S. **Bagnall**: A contribution towards our knowledge of the neotropical Thysanoptera.

Institution of Mining and Metallurgy, March 17.—Mr. Edgar Taylor, president, in the chair.—W. A. **MacLeod**: The surface condenser in mining power plant. The author conducted a number of tests on the winding engines of a mine with which he was connected, the results of which were embodied in this paper, together with a vast amount of other information concerning the relative consumptions and efficiencies of condensing and non-condensing engines. He found that the employment of condensers was distinctly beneficial in both respects, even under the intermittent conditions attaching to most mining power plants, and the results of his investigations have enabled him to determine with some exactness the leading features to be emphasised in the laying down of a condensing plant suitable for work of a more or less intermittent nature, as in the case of winding engines.

CAMBRIDGE.

Philosophical Society, February 21.—Prof. Seward, vice-president, in the chair.—Prof. **Punnett**: Mimicry in Ceylon Rhopalocera, with some notes on the enemies of butterflies.—A. R. **Brown**: The Andaman Islands. Some of the features of the physical anthropology and the social life of the aborigines of the Andaman Islands were briefly described. The extremely primitive characteristics of the Andamanese are to be attributed to their long isolation from all other races and peoples, and the stability of population.—T. G. **Edwards**: The procession and pupation of the larva of *Cnethocampa pinivora*. The processionary habits of the larva of this moth—which is abundant in the pine woods of the neighbourhood of Bordeaux—were first studied by Réaumur in 1736, and again by Fabre in the latter half of the last century. Both these writers describe the life-history and habits of the insect in considerable detail. The present paper, which is a summary of observations made during a fortnight spent at Arcachon, is an attempt to supplement these accounts by supplying information on certain points which still remain obscure. The procession is one of single file, the whole moving along a silken thread which is commenced by the leader and added to by all the larvæ in succession. The author found that though any larva could function as leader, yet the leader was capable of taking a real initiative in cases, such as the selection of a path, burrowing for pupation, &c., the satellites following him whether influenced by the same stimuli or not. The "circulating mass" is a formation which the larvæ frequently adopt when on the march. It consists in an assemblage of larvæ moving among each other, the mass, as a whole, remaining stationary. A "circulating mass" is always formed before the larvæ burrow for pupation. The leader appears to start the process of formation by assuming a zig-zag mode of progression, which is followed by the satellites. Though the order is completely broken up within the mass, yet it was found in every case observed that the leader of the procession which was re-formed from it was identical with that of the original procession. This was ascertained by marking the larvæ by means of fine sand or flour scattered over the dorsal papillæ. Pupation occurs beneath the ground, and is complete about nineteen days after burrowing. The burrowing is a collective process, in which all the larvæ within a circulating mass take part.—Prof. W. **Burnside**: Double sixes. —H. **Bateman**: The solution of a system of differential equations occurring in the theory of radio-active transformations.—Dr. **Young**: The change of order of integration in an improper repeated integral.—R. T. **Beatty**: The production of kathode particles by homogeneous Röntgen radiations. The kathode particles produced when these radiations pass through a thin silver leaf are absorbed by air, so that their coefficient of absorption is a linear function of the coefficient of absorption in aluminium of the exciting radiations. The total energy of the kathode particles set free in the leaf is proportional to the absorption by the leaf of the radiations. The range of these particles in hydrogen relative to that in air increases with the speed of the particles from 5 to 8. The total ionisation produced by any bundle of these kathode particles when totally absorbed in hydrogen is the same when air replaces hydrogen.—Sir J. J. **Thomson**: The scattering of rapidly moving electrified particles by matter, and its application to the determination of the number of corpuscles in the atoms of the various elements. An expression for the scattering of a pencil of rapidly moving electrified particles is found by the following method. First calculate the average deflection of the direction of motion of an electrified particle when passing through an atom which is assumed to consist of a large number of corpuscles placed in a sphere of uniform positive electrification; the expectation of any angular deflection after a large number of particles have passed through a large number of atoms can then be found by the theory of probability. In this way it is shown that the average angular deflection when a pencil of rapidly moving particles passes through a thin plate of thickness t is

$$\frac{e^2}{mv^2}\sqrt{\pi . N . \left(64N_0+\frac{\pi^2}{16}N_0^2\right)t},$$

where v is the velocity of the particle, e the charge, and m the mass of the particle, N the number of atoms in unit volume of the plate, and N_0 the number of corpuscles in each atom in the plate; hence if we measure the scattering of such a pencil we can determine the value of N_0.

MANCHESTER.

Literary and Philosophical Society, February 22.—Mr. Francis Jones, president, in the chair.—A. **Brothers**: Halley's comet as seen in 1835, compared with Donati's in 1858. Good drawings or sketches of Halley's comet seem to be rare. Sir John Herschel saw the comet at the Cape of Good Hope, and from his sketches it was not very conspicuous. Struve in 1835 gives a sketch which shows it to have been bright, probably when it was near the sun, but as a brilliant object it must have been very inferior to several which were seen during the nineteenth century. The author has a distinct recollection of seeing the comet in 1835. The object certainly was not so bright as Struve shows it to have been, and still greatly inferior to Donati's comet. It is generally spoken of now as likely to appear as a brilliant object, but the author points out that there is little evidence for the assertion.—Dr. H. F. **Coward**: The inflammability of gas-mixtures. Hydrogen and oxygen mixtures were shown to be capable of inflammation at a much lower pressure than had been imagined previously if the igniting spark were produced in the most suitable manner. Minima for sparks of various nature with electrodes of various kinds were given.

March 8.—Mr. Francis Jones, president, in the chair.—D. M. S. **Watson**: Upper Liassic Reptilia, part ii., the Sauropterygia of the Whitby Museum. The chief Plesiosaur in the Whitby Museum is the type-specimen of *Plesiosaurus propinquus*, Blake. This is re-described in the present paper. It is a member of the large-headed group of Plesiosaurs, but differs considerably from other Liassic species, such as "*Thaumatosaurus*" *megacephalus* (Stutchbury) and *Rhomaleosaurus cramptoni* (Baily and Carte). It may possibly be necessary to found a new genus for the species. The other remains consist of small groups of vertebræ, one set of which, containing fifteen cervicals of *Sthenarosaurus dawkinsi* (Watson), may be Owen's type-specimen of *Plesiosaurus coelospondylus*.—Sir W. H. **Bailey**: Mr. Myring's recent discoveries of prehistoric pottery in Peru. Mr. Hewitt Myring, a mineralogist and mine owner, visited the Chincuna Valley in Peru, and explored some sand ridges with the view of finding something in the Inca graves. The valley, however, had been used as a cemetery, not by the Incas, but by the Chimus, who preceded them in the occupation of the country, and the work of exploration brought to light several hundred pieces of pottery of great variety and interest. The pieces are well preserved, owing to the absence of rain and the consequent dryness of the soil. This pottery, some of which recalls that of China, India, and Egypt, shows great care, skill, and art, especially in the modelling of the human face, and is remarkable in that no two pieces are alike, a fact which demonstrates, the author thinks, the intellectual liberty and original genius of these early craftsmen. The age of the pottery is not known, and may be several thousand years. About one-third of the pieces discovered have been secured for the British Museum, and it is hoped that shortly some portion of the collection may find a permanent home in Manchester.

EDINBURGH.

Royal Society, February 7.—Dr. J. Burgess, vice-president, in the chair.—Prof. Cossar **Ewart**: The short-tailed domestic sheep.—Principal **Laurie**: Electromotive force of cells with a single salt and two solvents. The paper gave some preliminary results with cells in which potassium iodide was the salt and water and alcohol the solvents, as, for example, the relation of the electromotive force to the strengths of the solutions, the variation with temperature, and the connected thermal properties, &c. In co-ordinating the curious results obtained, the author brought forward some suggestive views as to the distribution of molecular energy in the two solutions.—Prof. F. G. **Baily**: A stereoscopic optical illusion. When two objects are nearly in the line of vision, so that when one is clearly focussed the other appears as two images, one on each side of it, this clearly focussed object appears to be, under certain conditions, distinctly nearer to the observer than it really is. The phenomenon was noticed accidentally, and a careful study showed that it occurred only for a certain range of distances. It was evidently a physiological effect. —Drs. E. P. **Cathcart**, J. **Gray**, and A. **Black**: A new form of respitory respiratory calorimeter for physiological purposes. The temperature of the entering and issuing air was measured by platinum thermometers or thermoelectric junctions, the whole being under perfect automatic control, so that the exact amount of heat generated within the calorimeter could be estimated.—Dr. T. **Muir**: The theory of persymmetric determinants in the historic order of development up to 1860, and the theory of bigradients in the historical order of development up to 1860.

February 21.—Dr. Traquair, F.R.S., vice-president, in the chair.—J. **Murray**: Scientific work of the British Antarctic Expedition of 1907–9. Moraines were traced on the sides of Mount Erebus to a height of 1100 feet. At the same time, there are abundant evidences of recent elevation of the land to the extent of several hundred feet, so that it cannot be said that glaciers ever stood so high as the moraines now stand. The tabular Antarctic icebergs appear to consist of compressed snow, not ice. They float very high, the depth of the substance below water being just about equal to the height above. By the fortunate re-discovery of a depôt laid down by Captain Scott on known bearings six years previously, the average rate of travel of the Great Ice Barrier at its western edge was found to be, on the average, about 500 yards per annum. The accumulation of drift on the Barrier surface was measured at the same place, and averaged just above 1 foot per annum of compressed snow. With these somewhat rough data as basis, it was estimated that the snow cliff, which to a height of 200 feet forms the face of the stratum at sea-level, must have originated some fifty miles to the south some 200 years ago. Yet under this long-continued weight of accumulated snow the material is not transformed into ice. The original glacier ice depressed beneath the accumulating snow seems to have been corroded away below sea-level, as the whole barrier moved outwards from the land valleys. Some important conclusions may be expected to result from a study of the rocks collected, especially if the discovery of coal and fossil wood should lead to the determination of their geological horizon. Of the optical phenomena observed, one of the most interesting was the projection, as long tapering dark bars through the air, of the shadows of mountain peaks. Under certain conditions the observer saw the shadow of Erebus cast on to Mount Lister, appearing as a circular arc reaching a height of 30° or more above the horizon. The observer was looking transversely to the direction of the shadow. Aurora displays were very frequent. They seemed to be dominated by the mass of Ross Island, and frequently circled Mount Erebus. The chief fact of biological interest was the abundant development of microscopic fauna and flora in the shallow lakes. The microfauna survives from year to year frozen in the ice without suffering injury. The lakes attain a temperature as high as 60° F. in summer, and in winter may go down to −40°. In some deeper lakes, which do not thaw in ordinary summers, many live animals were found at the bottom under 15 feet of ice. These must have been frozen for years. It was shown by experiments that they can endure being heated when in the dry condition almost to the boiling point and cooled to −108° F., a range of 300°. As regards the vital phenomena exhibited, there was a striking contrast between the fresh-water animals and those living in the sea not many yards away. These perform all their vital functions several degrees below the freezing point of fresh water, and are killed if the temperature either rises or falls one or two degrees. The rotifer fauna of the Antarctic lakes, which alone has been fully worked up, is very limited in numbers, and presents distinct peculiarities. Of the sixteen species recognised, five are at present unknown elsewhere, and many of the others differ from the usual types. These facts point to long isolation and difficulty of access to the region.

DUBLIN.

Royal Irish Academy, February 28.—Dr. F. A. Tarleton, president, in the chair.—J. J. **Simpson**: A revision of the Juncellid group of the Gorgonellidæ. According to the author, the Juncellid group of the family of flexible corals (Gorgonellidæ) comprises the genera Juncella, Ellisella, Scirpearia, Scirpearella, Ctenocella, and Nicella. Having had special facilities for studying these forms in the living condition when taking part in some of the cruises of the Indian Government steamer *Investigator*, the author has been able to add a good deal to the knowledge of their mode of life, structure, and distribution of which in some cases our information was very deficient. He directs special attention to the great taxonomic importance of the canal system and the character and variety in type of the minute spicules. A full description of the family Gorgonellidæ and its genera, with new diagnoses, conclude this contribution to our knowledge of the flexible corals.—J. **Adams** and G. H. **Pethybridge**: A census catalogue of Irish fungi. This paper contains an historical account of the previous work which has been done on the Irish fungi and Myxomycetes from the first published records in 1726 up to the present day, together with a complete bibliography of the subject. It also contains a complete list of all the fungi hitherto recorded as occurring in Ireland, together with a few hitherto unrecorded species. The arrangement follows that adopted in Engler and Prantl's "Pflanzenfamilien" in the main, and the distribution of each species in the four main provinces and in twelve sub-provinces is indicated. The total number of species, including Myxomycetes, recorded is 1464, which probably represents far from the actual number of species occurring in Ireland, seeing that the province of Connaught is at present almost virgin ground so far as these plants are concerned.—J. **Adams**: A list of synonyms of Irish algæ, with some additional records and observations. The greater part of the paper consists of a list of the names under which Irish species of algæ were originally published, and of the modern names of the species to which they are considered to be equivalent either in whole or in part. Some additional records have been brought together, numbering rather more than a hundred species. There is a revised census of species, a list of errata occurring in the "Synopsis of Irish Algæ" published in 1908, and some additional bibliographical records.

PARIS.

Academy of Sciences, March 21.—M. Émile Picard in the chair.—M. **Metchnikoff**: Experiments in typhoid fever. Hitherto it has been impossible to make satisfactory experiments with this disease on animals owing to the invariable fatality, but by making the attempt of inoculating a chimpanzee with typhoid matter from infected excreta, many interesting observations have been obtained, which are described.—J. **Guillaume**: Observations made on the sun at the Lyons Observatory during 1909.—A. **Chatelet**: A transformation of continuous arithmetic fractions.—A. **Cotton** and H. **Mouton**: The magnetic and electric bi-refractivity of aromatic liquids and the theory of molecular orientation.—Ch. **Maurain**: Variation with temperature of the magnetic properties of iron in a weak magnetic field.—M. **Robin**: The phenomenon of the extinction of sound in iron.—Jean **Meunier**: The laws of combustion.—J. **Ville** and W. **Mestrezat**: The hydrofluoric hydrolysis of cellulose.—L. **Blaringhem**: An unstable variety of Nigella, *Nigella damascena cristata*, obtained after a mutilation.—L. **Moreau** and E. **Vinet**: The use of lead arsenate in vine culture. Among other observations, it was definitely shown that the lead arsenate did not make its appearance subsequently in the wine.—M. Maurice **Holderer**: The filtration of diastases.—M. **Doyon**: The normal secretion in the liver of a substance preventing coagulation of the blood.—Jean **Giaja**: The isolation of a biose sugar derived from amygdalin.—MM. **Lagriffone** and **Roger**: Malta fever in France.—L. **Lindet**: The raising of flour in baking.—Ph. **Glangeaud**: The architecture of the central part of the Monts du Forez.—Marcellin **Boule**: Some vertebral fossils from the district south of Tunis.—M. **Nouailhac-Pioch** and Edmond **Maillet**: The rise of the Seine in January-February, 1910.—B. **Galitzine**: The determination of the epicentre of an earth tremor, from the data provided by one seismic station.

CALCUTTA.

Asiatic Society of Bengal, March 2.—I. H. **Burkill**: Notes on the pollination of flowers in India, note No. 7. A few observations made in the Central Provinces and Berar. The notes were made in the Central Provinces and Berar, chiefly in the Melghat. In the Melghat at the end of the rains, flower-visiting insects are rare. *Megachile albifrons* was observed to be a regular visitor to cotton-flowers in north-eastern Buldana. As cotton has been asserted to be self-pollinated constantly in western India, and as some of the agricultural departments have been endeavouring to improve the plant on the assumption that races ought consequently to continue pure though grown by the side of other races, the observation has interest in directing attention to the necessity of relying on artificial pollination in breeding experiments. *Apis dorsata* was observed to work in the dawn and dusk on the flowers of *Dalbergia Sissoo* at Nagpur.—I. H. **Burkill**: Note on the spreading of *Croton sparsiflorus*, Morung, along the Assam-Bengal Railway. This introduced plant has reached Lumding and Gauhati by means of the Assam-Bengal Railway, along which it is to be found in several places between them and Chittagong. Chittagong is the port whereby, doubtless, it entered India.

GÖTTINGEN.

Royal Society of Sciences.—The *Nachrichten* (physico-mathematical section), part iv. for 1909, contains the following memoirs communicated to the society:—

July 3.—G. **Angenheister**: Cloud observations in Samoa.

November 20.—Rudolf H. **Weber**: Asymmetric and symmetric tensors.—The late K. **Zoeppritz** and L. **Geiger**: Seismic waves, iii., calculation of path and velocity of precursive waves; Poisson's constant in the interior of the earth.

December 4.—Researches from the Göttingen University chemical laboratory, xxii. Remarks on the terpinene question.

The "Business Communications," part ii. for 1909, include the text of the address presented to the University of Cambridge at the Darwin centenary, and a discourse by G. **Berthold** on organisation, morphogenesis, and metamorphosis in plants.

DIARY OF SOCIETIES.

FRIDAY, APRIL 1.

GEOLOGISTS' ASSOCIATION, at 8.—An Account of the District to be Visited at Whitsuntide (the Isle of Purbeck and Bournemouth): H. W. Monckton and F. Hovenden.

MONDAY, APRIL 4.

SOCIETY OF ENGINEERS, at 7.30.—Moulmein Waterworks: P. G. Scott.

ARISTOTELIAN SOCIETY, at 8.—Bergson's Theory of Instinct: H. Wildon Carr.

SOCIETY OF CHEMICAL INDUSTRY, at 8.—The India Rubber Industry: Dr. P. Schidrowitz.

VICTORIA INSTITUTE, at 4.30.—Darwinism and Malthus: Rev. J. White.

TUESDAY, APRIL 5.

ROYAL INSTITUTION, at 3.—The Modern Development of the Problem of Alcoholic Fermentation: Dr. A. Harden, F.R.S.

ZOOLOGICAL SOCIETY, at 8.30.—On the Alimentary Tract of Certain Birds, and on the Mesenteric Relations of the Intestinal Loops: F. E. Beddard, F.R.S.—The Caudal Fin of the Teleostomi: R. H. Whitehouse.—Some Notes on Tasmanian Frogs: T. M. S. English.

INSTITUTION OF CIVIL ENGINEERS, at 8.—The New Clyde Bridge of the Caledonian Railway at Glasgow: D. A. Matheson.—The Queen Alexandra Bridge over the River Wear, Sunderland: F. C. Buscarlet and A. Hunter.

FARADAY SOCIETY, at 8.—The Nature of the Action of Dyeing: W. P. Dreaper.—The Electrical Theory of Dyeing: Prof. W. W. Haldane Gee and W. Harrison.

WEDNESDAY, APRIL 6.

SOCIETY OF PUBLIC ANALYSTS, at 8.—A Note on the Composition of the Milk yielded from Cows on a Pasture manured with Potash and Phosphates: J. Golding and S. G. Paine.—Note on the Influence of Solvents on the Drying of Linseed Oil: W. E. F. Powney.—An Improved Method for the Estimation of Titanium: A. Gemmell.—Extraneous Mineral Matter in Rice: F. W. Richardson.

ENTOMOLOGICAL SOCIETY, at 8.—On the Behaviour of Coleoptera during Floods: N. H. Joy.

THURSDAY, APRIL 7.

ROYAL INSTITUTION, at 3.—The Himalayan Region: Dr. Tom G. Longstaff.

LINNEAN SOCIETY, at 8.—Elm-seedlings showing Mendelian Results: A. Henry.—On the Foraminifera and Ostracoda from Soundings, chiefly deep-water, collected round Funafuti by H.M.S. *Penguin*: F. Chapman.

RÖNTGEN SOCIETY, at 8.15.—Some methods of using the Alternating Current Mains for Röntgen Ray Work: Dr. G. B. Batten.

INSTITUTION OF ELECTRICAL ENGINEERS, at 8.—The Progress of Electric Braking on the Glasgow Corporation Tramways: A. Gerrard.

FRIDAY, APRIL 8.

ROYAL INSTITUTION, at 9.—Lowell Observatory: Photographs of the Planets: Prof. Percival Lowell.

INSTITUTION OF CIVIL ENGINEERS, at 8.—The Reconstruction and Extension of Egremont Ferry Pier: G. H. Hodgson and H. M. Gell.

PHYSICAL SOCIETY, at 8.—An Experimental Demonstration of the Loading of Artificial Telephone Cables: B. S. Cohen.—Further Tests of Brittle Materials: W. A. Scoble.

ROYAL ASTRONOMICAL SOCIETY, at 5.

SATURDAY, APRIL 9.

ROYAL INSTITUTION, at 3.—Bells, Carillons and Chimes: W. W. Starmer.

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Meteorological Instructions for the use of inexperienced Observers resident abroad.

The Council of the Meteorological Society invite the cooperation of those who may be willing and qualified to furnish the meteorological elements of places, in the neighbourhood of which no adequate observations have hitherto been made.

They are prepared to publish tabular statements of the accompanying form, on receiving not only the necessary materials in a shape ready for publication, but also copies of the limited observations hereafter alluded to, and of subsequent calculations. These are intended to be retained in the Library of the Society, and are required as permanent records, to give evidence of the sufficiency of the data whence the printed results have been obtained, and to afford opportunity of investigating such anomalies as may at any future time-call for inquiry.

Place.				
Lat.	Long.		Elev^{n}	
	Mean temp.	Monthly range.	Rain, &c.	Period. winds.
Jan. Feb. Mar. April May June July Aug. Sept. Oct. Nov. Dec.				
Year				
No. of years' observation...........				
Hours and mode of observation...				

GALTON/2/13/3/15

The following instructions have been framed to facilitate the labours of those who have little leisure and experience in conducting meteorological observations, and show the minimum of effort with which trustworthy results can be obtained.

In the desire of lightening the expenses of their accepted contributors and of ensuring the trustworthiness of the instruments they employ, the Council of the Society will be happy to forward, through one of their Secretaries, a simple outfit of the necessary thermometers and rain-gauges, verified and packed in readiness for immediate shipment and use, on receiving a prepayment of £2 10*s*. to meet their cost. The cost is liable to vary within small limits, according to the varying charges of the makers and the introduction of new classes of instruments, but it will in no case exceed the above-mentioned sum, and the surplus, whatever it may be, will be returned. The instruments included in the outfit are 1 maximum and 1 minimum thermometer; 1 ordinary thermometer, with a range adapted to the proposed station; 1 rain-gauge. The case or cases that contain them will be about 10 inches long, and 4 to 5 inches wide and deep. Their weight will be about 2 lbs., and they will travel securely as an ordinary package.

Geographical Position.

Latitude, Longitude, and Elevation above the sea-level.—The authorities whence these have been taken, or the method by which the observer has determined them for himself, must be stated.

Observations on Heat.

1. *To Expose Thermometers.*—The instruments must be placed in a carefully selected position, or all their results will be vitiated. Choose an airy place, where there is continuous, dense, and ample shade. There set up a box of not less than 2 feet in height, width, and depth. It must be constructed precisely on the principle of an ordinary meat-safe; that is to say, it must be roofed (and better still, double roofed) from the rain, and have perforated sides, whether of gauze, trellis-work, or Venetian blinds, through which the air may pass with perfect freedom. It must be fixed on a stand or be suspended 4 feet above the ground. The thermometers should be hung on supports placed in the middle of the box, except where otherwise mentioned in the 1st method, § 3.

2. *Monthly Mean Temperatures.*—The average of the daily means,

taken by one of the methods described in the next paragraph during an entire month, gives the monthly mean. If occasionally a day or a month be dropped, a gap must be left in the record and no attempt be made to fill it.

3. *Daily Mean Temperatures.*

1st Method: This is the more accurate, but requires observations to be made *twice* in each day.

Procure a jar or box, of not less than 8 inches in length, width, and depth; fill it with dry sand, and set it in a properly exposed box (§ 1). Place a thermometer upright in the middle of the sand, with its bulb buried from 3 to 4 inches below its surface. Note its readings twice a day, at intervals of twelve hours, say at 9 A.M. and 9 P.M.; the mean of these readings may be accepted as the daily mean.

2nd Method: By observations made *once* in each day.

Hang a maximum and a minimum thermometer on supports, as described in § 1, and note their readings once daily, either in the morning or in the afternoon, and readjust the indexes. The mean of the maximum and minimum usually differs from the mean temperature of the day by less than half a degree; but occasionally (as at Barnaul in Central Asia) the difference exceeds $1\frac{1}{2}°$. The liability to a constant error of this amount is too serious to be passed over without investigation, especially as the approximate correction due to each month can be readily ascertained by making occasional use of the 1st method as a standard of comparison. When the year's work is completed, it will be easy to estimate the corrections due to the several months, and to apply them to the monthly means obtained by this 2nd method.

4. *Monthly Range* is the difference between the lowest and highest readings during the month.

5. *Yearly Means*, whether of *temperature* or of *range*, are the averages of the monthly means.

"The enclosure of a maximum and minimum self-registering thermometer in a large cask of dry sand, which might be opened and read off twice a year, would also probably afford a very accurate mean result."—*Sir John Herschell.*

Rain, Snow, and Dew.

6. These must be measured by a Gauge, which should be placed on the ground or on a low stand in an exposed situation. The relation of the units of length and weight is such that the tenth of an

inch of rain falling into a vessel whose mouth is a circular area of about two inches and nine-tenths in diameter (1·4467 inch radius) will weigh an ounce (Troy). Every medicine-chest contains a fluid ounce (Troy) measure; and, failing this, it will suffice to mark the space occupied in a small vessel by 480 drops of water, whose weight is one fluid ounce. A properly made rain-gauge and graduated measure is, however, preferable to any makeshift.

Wind.

7. Practised observers rarely use a weathercock, but watch the way the clouds (when any) are drifting. These are far steadier in their course than anything driven by the surface currents of wind. For the requirement of the tabular statement now desired, it will be sufficient to note the prevalence of periodical weather.

The Meridian, or True North and South Line, is obtainable as follows:—Set a straight stick or pole upright in a plot of level ground, by the help of a plummet. Loop a string to it, and, with the base of the pole as a centre and the string as a radius, scratch a circle on the ground of such a size that the shadow of the pole shall fall somewhat within it at noon. Keep a sufficient watch on the shadow of the pole to ensure your being present when its top enters the circle in the forenoon and leaves it in the afternoon. Mark the places where it cuts it. Lastly, bisect the interval between the two marks, and drive in a peg to note the place permanently. The pole and the peg will be in the same Meridian Line. It is best to draw three or four concentric circles, and to drive the peg in the line that corresponds most closely with the mean of the several independent determinations.

If the compass be used, allowance must of course be made for the variation of the needle.

Foot-notes.

The *No. of years' observations* would be printed thus:—

3 years; viz. 1860, 1861, and 1862–3.

The *Hours and mode of observation* would be printed thus:—

9 A.M. and 9 P.M., or max. and min. read in forenoon (as the case might be).

The description should be recorded more in detail in the MSS., where the spelling should be unabbreviated.


STEREOSCOPIC MAPS,
Photographed from Models of Mountainous Countries; illustrating a memoir, read March 13, 1865, before the Royal Geographical Society.
By FRANCIS GALTON, Esq. F.R.S. and R. CAMERON GALTON, Esq.
ISLAND OF ST. PAUL,
Photographed in 4 Parts from a very beautiful, Austrian Model in Bronze. This specimen shows the method in which Stereoscopic Maps may be combined.

18/

F. G.
Theory of Heredity

THE CONTEMPORARY REVIEW

TENTH YEAR.

DECEMBER, 1875.

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blind man; a man who cannot write, is like a deaf and dumb man. Are those the men whom England wants to rear?

Once show to the people of England what is right, and they will do it. Is England a poorer country than Germany, Denmark, Sweden, Russia, or Italy? If all these countries tax themselves to the uttermost for compulsory and gratuitous education, is England to say, I cannot afford it? When slavery was to be abolished, did England count the cost? When, more lately, the army was to be relieved from the stigma of purchase, did Parliament shrink from paying the bill? Whatever the cost, sooner or later, the schools will have to be redeemed. England, in time of war, can bear an income-tax of eighteenpence, and call it a flea-bite; the duties of peace, of peace granted to this country by a kind Providence, are as sacred as the duties of war; and if Englishmen have once made up their mind that National Education is a national duty, they will think as little of repudiating that national duty as of repudiating the national debt.

It may take some years before all this is realized; but the higher your ideal of National Education the better. A man without ideals is a poor creature; a nation without national ideals is poorer still.

I hear it often said that England should do for national education what Germany has done; what Italy is doing. No; that is not enough. We have done our best in Germany, but our best is but poor work. Our difficulties are enormous. Who is to pay for schools and schoolmasters, such as they ought to be? The soil of the greater part of Germany is poor, and therefore the country will never be rich. Besides, we may do what we like, we shall always live between two Symplegades—between France on one side, and Russia on the other; and we shall always have to spend our best energies in self-defence. There is the strongest feeling among the statesmen of Germany that the greatest efforts will have to be made for improving our national education: only what we want for it is, what we are not likely to get, a long peace, and a Bismarck and Moltke rolled up into one Minister of Public Instruction. In England you have everything, and there is no reason why your national education should not be as much ahead of that of Germany, as the education of Germany is of that of China. You have money, you have peace, you have public spirit, and you have, what is best of all, practical religion—I mean you still do a thing, however much you may dislike it, because you believe it is the will of God. Well, then, invest your money, utilize your peace, rouse your public spirit, and convince the world that one-half, three-fourths, nine-tenths of real practical religion is—Education, National Education, Compulsory, and, it may be, Gratuitous Education.

F. MAX MÜLLER.

A THEORY OF HEREDITY.

MR. DARWIN stated, in the year 1868, in the preface to his theory of Pangenesis,* that "every one appears to admit that the body consists of a multitude of 'organic units,' each of which possesses its own proper attributes, and is to a certain extent independent of all others;" and it may be safely asserted that the general expression of biological opinion since that date has been emphatically the same. We may therefore rest assured that the hypothesis of organic units, and all that such an hypothesis implies, must lie at the foundation of the science of heredity. It remains to determine further particulars; we have to examine how far the details of such theories as are based upon the hypothesis of organic units are correct, and to consider how their deficiencies may be supplied.

The facts for which a complete theory of heredity must account may conveniently be divided into two groups; the one refers to those congenital peculiarities that were also congenital in one or more ancestors, the other to congenital peculiarities that were not congenital in any of the ancestors, but were acquired by one or more of them during their lifetime, through change in the conditions under which they lived; as of climate, food, disease, mutilation, or habit.

The first of these two groups is of predominant importance in respect to the number of well-ascertained facts that it contains,

* Darwin: "Variation of Plants and Animals under Domestication," ii. 370.

many of which it is possible to explain, in a broad and general way, by more than one theory based on the hypothesis of organic units. The second group includes much questionable evidence, usually difficult of verification, and which, as I shall endeavour to show, does not, for the most part, justify the conclusion commonly derived from it. In this paper I divide the general theory of heredity into two parts, corresponding respectively to these two groups. The first stands by itself, the second is wholly supplementary and subordinate to it.

No theory of heredity has been enunciated with more clearness and fulness than that of Mr. Darwin's Pangenesis, and the preparatory statement to that theory contains the most elaborate epitome that exists of the many varieties of facts for which a complete theory of heredity must account. What I have now to say is largely based on the arguments and considerations brought forward by Mr. Darwin in support of it; nevertheless the conclusions in this paper will be seen to differ essentially from his own. Pangenesis appears more especially framed to account for the cases which fall in the second of the above-mentioned groups, which are of minor and often doubtful import; and it will be seen that I accept that theory with modification, for the supplementary and subordinate part of a complete theory of heredity, but by no means for the primary and more important part.

Before proceeding, I beg permission to use, in a special sense, the short word "stirp," derived from the Latin *stirpes*, a root, to express the sum-total of the germs, gemmules, or whatever they may be called, which are to be found, according to every theory of organic units, in the newly fertilized ovum—that is, in the earliest pre-embryonic stage—from which time it receives nothing further from its parents, not even from its mother, than mere nutriment. (It is hardly necessary to remind the reader that not a drop of blood from the mother penetrates into the vessels of the embryo, but that the two circulations are wholly distinct; the placenta to which the embryo is attached, and with which it is in vascular connection, being itself nourished from the mother by mere imbibition.) This word "stirp," which I shall venture to use, is equally applicable to the contents of buds, and will be found very convenient, and cannot apparently lead to misapprehension.

The whole of the stirp, together with much of mere nutriment, is packed into a space not exceeding the size of the head of a pin, for that is the size of the newly fertilized ovum, which, curiously enough, is the same in all mammalia. It is evident that direct observation can tell us nothing concerning the form and behaviour of such minute objects as the germs of which the stirp is composed; they would be far beyond the ken of the microscopist, even if their separate actions upon light were different. But even this is not the case, for the fertilized ovum is almost homogeneous in colour. Cells and their contents are, to biologists looking at them through microscopes, much what mail-bags and the heaps of letters poured out of them are to those who gaze through the glass windows of a post-office. Such persons may draw various valuable conclusions as to the postal communications generally, but they cannot read a single word of what the letters contain. All that we may learn concerning the constituents of the stirp must be through inference, and not by direct observation; we are therefore forced to theorize.

We will begin with a statement of the four postulates that seem to be almost necessarily implied by any hypothesis of organic units, and which are included in that of Pangenesis. The first is, that each of the enormous number of quasi-independent units of which the body consists, has a separate origin, or germ. The second is, that the stirp contains a host of germs, much greater in number and variety than the organic units of the structure that is about to be derived from them; so that comparatively few germs achieve development. Thirdly, the germs that are not developed, retain their vitality; they propagate themselves while still in a latent state, and they contribute to form the stirps of the offspring. Fourthly, organization wholly depends on the mutual affinities and repulsions of the separate germs; first in their stirpal, and subsequently during all the processes of development.

Proofs of the reasonableness of these postulates are especially to be found in the arguments of Mr. Darwin: that there is at least a fair ground to believe in their reasonableness, may be shown in a cursory manner. Thus, the independent origin of the several parts of the body may be argued from the separate inheritance of their peculiarities. If a child has its father's eyes and its mother's mouth, these two features must have had a separate origin. Now, it is observed that peculiarities, even of a microscopic kind, are transmissible by inheritance, and therefore it may be concluded that the minutest parts of the body have separate origins. That the stirp contains a much greater variety of germs than achieve development, is proved by the fact that a person is capable of transmitting ancestral peculiarities to his children, that he did not himself possess. Everything that reached him from his ancestors must have been packed in his stirp; therefore his stirp contained not only such peculiarities as were developed in his own bodily structure, but also those numerous other ancestral peculiarities of which he was personally destitute, but which he bequeathed to one or more of his descendants. Therefore every stirp must be held to contain a great variety of germs in addition to those that may achieve development in the person who grows out of that stirp. It further follows that these residual germs retain their vitality, and contribute to form the stirp of the descendants, as will be explained more fully further on. The fourth and last postulate, that organization wholly depends on the mutual affinities of the separate organic units, commends itself to acceptation by the simplicity and sufficiency of what is asked; much of what I have to say in this paper will testify to this. We should also bear in mind that the alternative hypothesis of a general plastic force resembles that of other mystic conceptions current in the early stages of many branches of physical science, all of which yielded to molecular views, as knowledge increased. The science of heredity is still in an early stage, and analogy disposes us to expect that its course will be similar to that of its predecessors. The possibility of such minute objects as the germs possessing sufficient delicacy of perception to ensure that each of so enormous a variety of them should find its place, was illustrated by Mr. Darwin through the delicate perception of the pollen grains of the different species of plants. He says:* "About 10,000 compositæ exist, and there can be no doubt that if the pollen of all the species could be simultaneously or successively placed on the stigma of any one species, this one would elect, with unerring certainty, its own pollen." The partial failures in the action of these affinities are most instructive, as where a mark of any kind on the skin is transmitted by inheritance in an altered situation, to a neighbouring or to an homologous part. Having stated thus much by way of preface, we may now proceed freely.

Much wonder is expressed by physiologists at the apparent fact that none of the higher races admit of being long carried on by any system of unisexual parentage; but that deterioration, apparently due to a deficiency of some of the structural elements, gradually sets in, and the race ultimately perishes. It appears to be a universal law, that a system of double parentage is a very important, some think an essential requirement, for the indefinite maintenance of any race whose organization is complex; and I would submit that the differentiation of a race into two sexes is the result, and not the cause, of this requirement. In the lowest forms of organized life double parentage exists, but sex apparently does not, because it appears that any two cells may conjugate and combine their contents within a single cell; these forms are also capable of easy unisexual multiplication by self-division or by budding. Proceeding higher in the scale of life the sexual differentiation becomes increasingly marked, and unisexual propagation is of rarer occurrence. At length we reach the stage where the differentiation of sex is complete, and the power of unisexual propagation is wholly lost.

* "Variation of Plants and Animals under Domestication," ii. 380.

Now the especial necessity of a system of double parentage in complex organizations is the immediate consequence of a theory of organic units and germs. Let us fix our attention upon any one definite series of unisexual descents, and follow out its history: suppose we select, cut off, and plant the second bud, then after it has grown to maturity we similarly take the second of its buds, and so on consecutively. At each successive stage there is always a chance of some one or more of the various species of germs in the stirp dying out, or being omitted; and of course when they are gone they are lost for ever, and are irreplaceable by others. From time to time this chance must fall unfavourably, and will cause a deficiency in some of the structural elements, and a consequent deterioration of the race. If the loss be vital it will of course be extinguished at once; but on the more favourable supposition, the race will linger on, submitting to successive decrements in its constituent elements, until the accumulation of small losses becomes fatal. What is true for the series of second buds in our example, is of course equally true for any system we please to specify, and therefore it would be generally true in the experience of gardeners and others.* Exactly the same argument applies to every other unisexual process, all of which lead to deterioration and final extinction. On the other hand, when there are two parents, the chance deficiency in the contribution from either of them, of any particular species of germ, will be supplied by the other. No doubt, cases will rarely occur in which the same species of germ is absent from the contribution of both, and a very small proportion of the families will thereby perish. But what if they do become extinct? The remaining families are perfectly sound, or tend to become so in the next generation, and they fill up, only too easily, the gap. Thus we see that in any specified course of unisexual generation, every line of descent is doomed to extinction, sooner or later; but that in bisexual, only a very small proportion of families become extinct, or even temporarily suffer, from the cause we are considering, while the great majority do not suffer a whit, and the remainder tend to become rehabilitated. Again, as the stirp whence the child sprang can be only half the size of the combined stirps of his two parents, it follows that one half of his possible heritage must have been suppressed. This implies a sharp struggle for

* It would not, however, be always true in a free state of nature, where the weakly plants would be supplanted by those that remained sound. Here we have to consider, on the one hand, the growing chance against the deterioration of any single line of descent, and on the other, the growing number of all possible lines of descent. They both proceed in a geometrical ratio; and if the ratio of the latter exceeded that of the former, extinction need not take place. But this would become impossible after a certain degree of complexity had been reached, because with growing complexity, the chance of deterioration must increase, while the fecundity (see H. Spencer's Biology; vol. i., "Multiplication") must diminish.

place among the competing germs, and the success, as we may infer, of the fittest half of their numerous varieties.

The limitation of space in the stirp requires a limitation not only of the varieties of each species of germ, but also of the number of individuals in each variety. The knowledge of such a cause is helpful, and appears to be needed, in accounting for the not very large number of subdivisions in which peculiarities are transmitted. I am not considering cases of the slow loss of some characteristic of a race which proceeds by minute gradations, and may be ascribed, at least in part, to an alteration in the quality of the germs, nor am I speaking of cases where it is clear that one of two alternative qualities has overpowered the other, but of instances where they appear equipotent and capable of merging together. Thus, in the gradual breeding-out of negro blood, we may find the colour of a mulatto the half, and that of a quadroon one quarter that of his black ancestors; but as we proceed further, the subdivision is very irregular, and does not continue indefinitely in the geometrical series of one-eighth, one-sixteenth, and so on, but it is usually present very obviously, or not at all, until it entirely disappears. There are, of course, far more gradations in compound results, as in an expression of the face, because any one of its elementary causes may be present or absent; and as the number of possible combinations or alternatives, among even a few elements, is very great, there must be room for a large number of grades between the complete inheritance of the expression and its total extinction.

It is certain, from the rapidity of the visible changes in the substance of the newly fertilized ovum, that the germs in the stirp are in eager and restless pursuit of new positions of organic equilibrium, due, as we may suppose, to the unequal rates of development of some of the better nourished germs. We see that segregations occur as much as aggregations, and it is reasonable to suppose that repulsions concur with affinities in producing them. We know nothing yet of the nature of these repulsions and affinities, but it seems hardly possible to account for the whole state of affairs on the hypothesis of a purely step-by-step development like that proposed in Pangenesis, where B follows A, and C follows B, and so on. It is difficult to suppose the mutual influences of the germs to be limited to lines like those which attach the blood corpuscules face to face in long rouleaux when coagulation begins; neither can we suppose them limited to planes, like those which govern the harmonious groupings of the flora and fauna on the face of a land left in a state of nature; but we ought rather to expect them to act on many sides, in a space of three dimensions, just as the personal likings and dislikings of an individual in a flying swarm may be supposed to determine the position that he occupies in it. Each germ has many neighbours:

a sphere surrounded by other spheres of equal sizes, like a cannon ball in the middle of a heap of them, when they are piled in the most compact form, is in immediate contact with no less than twelve others. We may therefore feel assured that the germs must be affected by numerous forces on all sides, varying with their change of place, and that they must fall into many positions of temporary and transient equilibrium, and undergo a long period of restless unsettlement, before they severally attain the positions for which they are finally best suited. However ignorant we may be at present of the character of these affinities and repulsions, or of what Mr. Herbert Spencer calls their polarities in his instructive chapters in the first volume of his "Principles of Biology," a conviction of their existence is sufficient to afford general notions of what must be their mode of action, and enables us to illustrate its necessary consequences by many familiar experiences. Chief among these are the events of political life, such as the struggle for place and power, election and representation. For example, we know that the primary cells divide and subdivide, and we may justly compare each successive segmentation to the division of a political assemblage into parties, having, thenceforward, different attributes. Or, again, we may compare the stirp to a nation, and the germs that achieve development to its foremost men, who succeed in becoming its representatives; lastly, we may compare the qualities of the person whose structure is composed of the developed germs, to the political characteristics of the house of representatives of the nation. These are not idle metaphors, but strict analogies; they will be found to bear consideration, and to be worthy of being pursued, as they give a much-needed clearness to our views on heredity.

The great dissimilarity frequently observed between brothers or sisters is easily to be accounted for, and it may be well illustrated by a political metaphor. On the one hand, the stirps must be nearly alike, because the germs are simple organisms, and all such organisms breed true to their kind; on the other hand, we have very different structures developed out of these stirps. The well-known uncertainties of political elections, and their causes, afford a strict analogy and explanation of this. We have abundant experience that when a constituency is very varied, trifling circumstances will change the balance of parties, and therefore, although there may be little real variation in the electoral body, the character of its political choice at successive elections may change abruptly. A uniform constituency will always have representatives of a uniform type; and this precisely corresponds to what occurs in animals of pure breed, whose stirp contains only one or a very few varieties of each species of germ, and whose offspring always resemble their parents and one another. The more mongrel the breed, the greater is the variety of the offspring. The dissimilarity not unfrequently found between twins of the same sex is more marked than that between ordinary brothers and sisters, notwithstanding that the embryonic conditions were closely similar. The subject is a very curious one, and requires the following explanation. I had occasion to make many inquiries into the resemblances of twins, whence it appeared that among well-formed "true" twins,* so to speak, namely those who, up to the time of their birth, were enclosed in the same membrane, and had therefore been developed out of two germinal spots in the same ovum, there are two groups of cases that contrast strangely with one another, and there are but few intermediate cases. In the larger of these groups, the twins are exceedingly alike in body and mind, also in their growth, illnesses, and decay, and their resemblance is not unfrequently such as to justify the somewhat startling incidents referring to twins that are to be found in many works of fiction. In the smaller group, which contains perhaps one-fourth as many cases as the larger, the twins are absolutely unlike; so much so, that they have occasionally been described as "complementary" the one to the other; the one having what the other lacked. What can be the reason that, out of identically the same primary stirp, either two absolutely dissimilar persons can be developed, or else two closely similar ones; while the intermediate cases are so rare, that they may be considered due to quite another and more common contingency, namely, that in which the twins are not produced out of the same ovum, but from separate ova? The answer I suggest is as follows:—As regards the similarity of true twins, there can be little difficulty; we should expect, on statistical grounds, that the two halves of any assemblage of germs would be much alike. The secondary stirps of the twins being in this case alike, and the circumstances of their development being almost identical, the results must be closely similar. As regards the dissimilarity, we might expect that if there had happened to be a sufficient delay before the commencement of the division of the primary stirp to allow its germs to arrange themselves somewhat according to their affinities, the two halves would be strongly contrasted. In the case of an ordinary single birth, the germ (to make the illustration less complex, I will not say the germs) of each species

* For some general results of these inquiries, see *Fraser's Magazine*, Nov., 1875. I had twenty cases of strong dissimilarity in twins, and in all the cases, the twins were of the same sex. Now, it appears to be a rule without exception that what I have above termed "true" twins are of the same sex. Such twins are by no means uncommon; Spaeth's estimate of their frequency, as compared to that of twin births generally, is as high as 25 per cent., and I understand that his observations rank among the very best; however, the estimates of other observers are much lower. Hence there is much probability that my cases of strong dissimilarity were usually, if not invariably cases of true twins. But I have no direct evidence one way or the other.

that achieves development may be compared to the one representative of a body of electors, each of whom has a single vote. In the case of twins, we may suppose each elector still to have only a single vote, but that two representatives are elected. Let us now suppose one of the political parties slightly to predominate; then, if the electoral body be divided by some accidental line, the same party would predominate in each division; and if the election were conducted on that principle, the two representatives would certainly be men of the same predominant party. But if the electoral body acted as a whole, it would be impossible for the predominating party to return more than one candidate, and the two representatives would be men of opposite politics.

That part of the stirp which has become developed has been supposed (I believe universally) to be the chief agent in maintaining the progeny of germs. It is certainly an essential condition in the theory of Pangenesis, as the name of that theory testifies; where each separate cell in its nascent state is supposed to throw off germs which circulate freely in the body along with others which had been hereditarily transmitted, and which aggregate themselves owing to their mutual affinities, and so form the sexual elements. For my own part, while acknowledging that there exists undeniable evidence of the existence of this power, which will be discussed when we come to the second group of cases, I shall endeavour to show that it can, at the most, be effective in a very minute degree. The germs that become developed into structure, are relatively too few to exert much hereditary influence, and when fully developed they would be passive and sterile. I argue, that as fertility resides somewhere, it must have been vested in the non-developed residue of the stirp, or rather in its progeny and representatives (whatever, or however numerous, they may be) at the time when the individual has reached adult life.

The hypothesis that the developed germs are relatively few and sterile agrees singularly well with many classes of fact. Thus it explains why, although hereditary resemblance is the general rule, the offspring is frequently deficient in the very peculiarity for which the parent was exceptionally remarkable. We can easily understand that the dominant characters in the stirp will, on the whole, be faithfully represented by the structure of the person who is developed out of it; but if the personal structure be a faithful representative of the dominant germs, it must be an over-favourable representative of the germs generally, and therefore, *à fortiori*, of the undeveloped residue; nay, in extreme cases, the person may be absolutely misrepresentative of the residue, the accidental richness of the sterile sample in some particular valuable variety of germ, having drained the fertile residue of every germ of that variety. The possibility of this occurrence is the more credible, since, as we have already seen, the number of germs of each variety cannot be very large. Experience testifies to the fact that children of men of extraordinary genius have not unfrequently been singularly deficient in ability, and this condition has been especially remarked in instances where the man of genius was himself the offspring of a mediocre ancestry; therefore where, according to the above theory, the number of valuable germs were few, and all of them were used up and rendered sterile in the structure of his own person.

The steady tendency to deterioration in exceptional characters is likewise shown by the avowed difficulty, among breeders, of maintaining the characters of any valuable variety that has been produced by accident (that is, by some happy combination of a number of unknown variable causes).

Another result of the best elements of the stirp being rendered sterile is the strong tendency to deterioration in the transmission of every exceptionally gifted race. That this is a universal tendency among races in a state of nature, is proved by the fact that existing races are only kept at their present level by the severe action of selection. If they were left unpruned even for a single generation, the weaker members would survive, and the average quality of the race would necessarily diminish.

Again, the sterility of the developed elements of the stirp explains the fact of certain diseases skipping one or more generations, if the further very reasonable postulates are granted, that the germs of those diseases are both prolific and gregarious. Thus, nearly all the gout molecules in the stirp whence A sprang might, owing to their gregarious nature, become developed in the person of A, and so be rendered sterile; the small fertile residue in his stirp would be insufficient to supply that of his son B with enough gout germs to dominate and achieve development in the person of B, consequently they would be husbanded; then, owing to their prolific character, they would so multiply in a latent form in the structure of B, as to ensure transmission in sufficient numbers to the stirp of C the son, or D the grandson, to enable them to achieve development in the person of C or D, just as they had done in that of A; and so the cycle would be repeated.

The conclusion from what has thus far been said is amply confirmed by observation; it is:—(1.) That the contents of the stirp must segregate into septs, or divisions, and that these septs must subdivide again and again, just as a large political party may repeatedly subdivide itself into different factions. (2.) That the dominant germs in each successive sept are those that achieve development. (3.) That it is the residual germs and their progeny that form the sexual elements or buds.

No process of subdivision like this could be expected to be carried on with perfect accuracy; no political party was ever split with such clean precision into two political septs, that none of the A party were included in the ranks of B, and *vice versâ*. We must therefore feel assured that germs of many alien species would be included in each successive sept. Also, we may reasonably suppose that the structure of the developed germs must afford many convenient places for the lodgment and sustenance of these alien germs; consequently, representatives of all parts of the residue of the stirp would be found dispersed all over the body. Lastly, we cannot but expect that these alien germs, when they thrive and multiply, would somewhat transgress the bounds of the cell or cell-interspace in which their progenitors had lodged, knowing that even so large an object as a blood-corpuscule will occasionally find its way through the unruptured wall of a capillary vessel. This is a very different supposition to that of the free circulation of gemmules in Pangenesis, yet it seems to have the merits of that theory (so far as the group of cases are concerned which we are now considering, namely, the inheritance of qualities that were congenital in the ancestry), and at the same time to be free from the many objections that are urged against it. These are as follows:—On physical grounds, we cannot understand how colloid bodies, such as the Pangenetic gemmules must be, could pass freely through membranes. Moreover, if they did, the paternal gemmules in the body of the unborn child would diffuse themselves equally over the body of the child and that of its mother; consequently there would be very few remaining in the body of the child, while, on the other hand, there would be an invasion of maternal gemmules. The result of this would be, that the child would transmit its maternal peculiarities far more than its paternal ones; in other words, people would resemble their maternal grandmothers very much more than their other grandparents, which is not at all the case. That the gemmules are not contained in the blood-vessels circulating with the blood, is proved by my own experiments, in which I largely transfused the blood of an alien species of rabbit into the blood-vessels of male and female silver-grey rabbits, from which I afterwards bred. I repeated this process for three generations, and found not the slightest sign of any deterioration in the purity of the silver-grey breed.*

Again, a free circulation of the gemmules, such as Pangenesis supposes, would cause various events to be extremely common,

* The experiments on the first generation were published Proc. Royal Society, 1871, p. 393, to which see Mr. Darwin's remarks in *Nature*, 1871, p. 502. I subsequently carried on the experiments with improved apparatus, and on an equally large scale, for two more generations.

whereas the supposition of a small transgression of their limits shows them to be possible, though infrequent; just as they actually are. I mean such cases as the zebra-marks on the foal out of a thoroughbred mare by a thoroughbred horse, owing to the former having once borne a mule to a zebra; the action of pollen on the tissues adjacent to the fertilized pistil of a different variety of plant. The distribution of the germs, by the agency I supposed, all over the body, would fully account for the replacement of a lost limb in the lower animals, and the reparation of simple tissues in the higher ones. It would much transcend my limits if I were to enter at length into these and kindred questions, but it is not necessary to do so, for it is sufficient to refer to Mr. Darwin's work already quoted, where they are most fully and carefully discussed, and to consider, while reading it, whether or no the theory I have proposed could, as I think it might, be substituted with advantage for that of Pangenesis. I must repeat, that I limit these remarks to the very large proportion of cases that fall into the first of the two groups in which I am discussing the facts of heredity. We will next proceed to consider those that fall into the second group.

The cases now before us are those in which characters created artificially in the person of the parents are transmitted by inheritance to their offspring. In considering these, we must be extremely careful not to confuse the effects of totally different processes.

We have thus far dealt with three agents—(1) the stirp, which is an organized aggregate of a host of germs; (2) the personal structure, developed out of a small portion of those germs; and (3) the sexual elements, generated by the residuum of the stirp. The cases before us are those which are supposed to prove that 2 reacts on 3—that is, the personal structure upon the sexual elements. The first and the largest class of the cases now before us refer to adaptivity of race. It is said that the structure of an animal changes when he is placed under changed conditions; that his offspring inherit some of that change; and that they vary still further on their own account in the same direction, and so on through successive generations, until a notable change in the congenital characteristics of the race has been effected. Hence, it is concluded, that a change in the personal structure has reacted on the sexual elements. For my part, I object to so general a conclusion, for the following reasons. It is universally admitted that the primary agents in the processes of growth, nutrition, and reproduction are the same, and that a true theory of heredity must so regard them. In other words, they are all due to the development of the same germinal matter, variously located. Consequently, where it is everywhere affected by the same conditions, it would be everywhere affected in the same way. One of the species of germs whence the hair sprang, that was induced to throw out a new variety in the cells nearest to the surface of the body under certain changed conditions of climate and food, might be expected to throw out a similar variety in the sexual elements at the same time. The essential changes would be collateral, although the moment when the changed germs received their development might be different. So far from the changed structure of the hair causing the germs in the sexual organs to vary, it may often happen that the latter are the first to show change. Thus the progeny of thick-fleeced sheep, newly imported into the tropics, have less wool than their parents. There is not a shadow of proof that the adaptivity of a race to changed conditions, *affecting all parts of the body alike*, is due to the reaction of changed personal structure upon the sexual elements. The fact that a drunkard will often have imbecile children, although his offspring previous to his taking to drink were healthy, is another instance of simultaneous action. The alcohol pervades his tissues, and, of course, affects the germinal matter in the sexual elements as much as it does that in his own structural cells, which have led to an alteration in the quality of his own nerves. Exactly the same must occur in the case of many constitutional diseases that have been acquired by long-continued irregular habits. There is not the shadow of a proof that the adaptivity of a race to changed conditions, which affect all parts of the body alike, is due to the action of changed structure upon the sexual elements. The case is different as regards conditions that have a local influence, but races are very slow in acquiring these, such as the callosities on the knees of animals who use them much.

Another class of evidence brought forward in proof of the inheritance of non-congenital peculiarities concerns mutilations. No doubt the industry of M. Prosper Lucas, and of many others, has brought together several curious cases; but the negative evidence, that is to say, the certainty of the non-inheritance of mutilations in a vast number of cases (see Darwin: "Variation of Plants and Animals under Domestication," ii. 23), is so overpowering, that it may still be reasonable to look upon the former as more than a collection of coincidences. The earliest instance that I know of, that seems worthy of serious consideration, is that of Dr. Brown-Séquard's epileptic guinea-pigs, because it admits of verification; but this, if I understand his account rightly (Proceedings of Royal Society, x. 297), is open to some objection. It appears that Dr. Brown-Séquard found, during his researches into the cause of epilepsy, that, by a particular operation on the spinal cords of guinea-pigs, he could induce a convulsive disease very much like epilepsy. He operated upon many guinea-pigs, and kept them apparently apart from the rest of his stock, and noticed that their young were at times attacked with "epileptiform" convulsions, while the young of the rest of his stock never were; hence he concludes that the artificially induced epilepsy was transmitted hereditarily. My objection to this conclusion is, that if persons were brought up from childhood in a ward of epileptic patients, they would certainly acquire a tendency to epileptiform seizures by the mere effect of imitation. It is notorious that many an epileptic person has had his fits first brought on by witnessing the epileptic seizure of another. This however, may be an unfounded objection, due, as was remarked, to misapprehension of an experiment, whose details deserve a fuller description. It is much to be regretted, that two subsequent memoirs, read by Dr. Brown-Séquard at the British Association in 1870, do not appear to have been published; their titles only are to be found in its Journal (p. 134). But he has communicated a most important *résumé* of other results to the *Lancet* (Jan., 1875, p. 7) regarding the inheritance of purely physical effects, that were produced in the parent guinea-pigs by nerve mutilation, and their occurrence in the offspring, in the same order in which they had appeared in the parents.

A special cause may be assigned for the effects of disuse in causing hereditary atrophy; it has already been shown that all exceptionally developed organs tend to deteriorate; consequently those that are not protected by selection will dwindle. The level of muscular efficiency in the wing of a strongly flying bird is like the level of water in the leaky vessel of a Danaid, only secured to the race by constant effort, so to speak; let the effort be relaxed ever so little, and the level immediately falls.

In addition to much else that might be said in disparagement of evidence on which overmuch reliance has hitherto been put, we should recollect that it is hazardous to adduce the very gradual adaptation of a race to changed conditions as a proof that acquired habits are hereditarily transmitted, because when several generations elapse before any appreciable result can be observed, selection will have had many opportunities of operating. It is noticeable that a race is much longer in adapting itself to conditions that affect one part only of the body, than to those whose influence is more general; and this is just what we should have expected from the views already expressed. It is hard to find evidence of the power of the personal structure to react upon the sexual elements, that is not open to serious objection. That which appears the most trustworthy lies almost wholly in the direction of nerve changes, as shown by the inherited habits of tameness, pointing in dogs, and the results of Dr. Brown-Séquard.

The conclusion to be drawn from the foregoing arguments is, that we might almost reserve our belief that the structural cells can react on the sexual elements at all, and we may be confident that at the most they do so in a very faint degree; in other words, that acquired modifications are barely, if at all, *inherited*, in the correct sense of that word. If they were not heritable, then the second group of cases would vanish, and we should be absolved from all further trouble; if they exist, in however faint a degree, a complete theory of heredity must account for them. I propose, as already stated, to accept the supposition of their being faintly heritable, and to account for them by a modification of Pangenesis. Each cell may be supposed to throw off a few germs that find their way into the circulation, and have thereby a chance of occasionally finding their way to the sexual elements, and of becoming naturalized among them. In illustration of this process, we may recur to political metaphor, and imagine the stirp to be represented by some country, and the germs by its inhabitants. We know that, in every country, travellers from other nations occasionally find a place, which they can fill more suitably than at their own homes or elsewhere, and they become settlers. The population of the country may be as highly organized as it is needful to consider the sexual elements to be; every trade and profession may seem to be full; and yet the stranger obtains a lodgment, either through superiority or luck. He may displace one of the native-born inhabitants, or he may find an unoccupied corner which he can fill; anyhow, as a matter of fact, he becomes a permanent citizen.

The hypothesis of organic units enables us to specify with much clearness the curiously circuitous relation which connects the offspring with its parents.* The idea of its being one of direct descent, in the common acceptation of that vague phrase, is wholly untenable, and is the chief cause why most persons seem perplexed at the appearance of capriciousness in hereditary transmission. The stirp of the child may be considered to have descended directly from a part of the stirps of each of its parents, but then the personal structure of the child is no more than an imperfect representation of his own stirp, and the personal structure of each of the parents is no more than an imperfect representation of each of their own stirps. The political analogy to the common, but false, idea of the filial relationship is that which connects colonists to their parent nations: the relationship, according to the views in this memoir, is much more circuitous and feeble; it resembles that which connects the *representative government* of the colony with that of the parent nations. This, at least, is a first approximation:

* I endeavoured to explain this in a paper, Proc. Royal Soc., 1872, p. 394.

the second approximation consists in making allowance for the small power of transmitting acquired peculiarities; that is, for the reaction of the personal structure upon the sexual elements, and thereby upon the future stirp. This may be effected by supposing the governments of the parent states to have the power of nominating a certain proportion of the colonists.

It now remains to summarize briefly. I began by showing that certain postulates were admitted by most biologists, and that these gave a firm base whereon to develop a theory of heredity. By these, and by what appear to be their necessary consequences, I explained the object of double parentage, and therefore of sex. Then I dwelt on the restless movements of the germs in the stirp and the variety of their attractions and repulsions, and explained how it arose that brothers or sisters were often very dissimilar; also, on other grounds, why twins derived from the same primary stirp were either very much alike or extraordinarily contrasted (this being a fact that had resulted from inquiries of my own). Next, I argued that the developed part of the stirp was almost sterile, and that it was from the undeveloped residue that the sexual elements were derived. By this I explained the almost complete non-transmission of acquired modifications; also the occasional deficiency in the offspring, of qualities for which the parent had been exceptionally remarkable, and for certain diseases skipping alternate generations. The theory was proposed that the successive segmentations of the stirp were not perfectly clean and precise, but that each structure included many alien germs, whereby the progeny of all the contents of the residue of the stirp were distributed over the body. This accounted for much that Pangenesis over-accounted for, and was free from objections raised against the latter.

The assumed evidence that structural changes reacted on the sexual elements was then discussed, and it was pointed out that certain changes were really collateral which had been commonly thought to be effected by inheritance. Some of the evidence that structural changes might react on the sexual elements was, however, accepted, and to account for its existence, a modification of Pangenesis was adopted; each nascent cell being supposed to throw off germs which occasionally found their way into the circulation and obtained a lodgment in the already constituted sexual elements; this process being therefore independent of and subordinate to the causes which were supposed mainly to govern heredity. Finally, the exact relationship was defined, which connects the parents with their offspring.

FRANCIS GALTON.

SONGS AND LEGENDS OF MODERN GREECE.

THERE is a certain fresh earth-scent in early spring, and in the first flowers and opening leaves of the year, not unlike the "savour" of the people's songs in all countries. A child-like enjoyment in grief, an entire want of self-consciousness, or of looking at their own emotions from without, an utter abandonment of themselves to the feeling of the moment, is the distinctive feature of all "folk-lore." "Ils ne font rien parcequ'on les regarde, et ils ne s'abstiennent de rien parcequ'on les regarde," as Madame de Staël said of the Italians. The result is sometimes very touching, sometimes very painful and hideous, but always interesting, as a true picture of the mind of the time—there is no posing, no talking for effect, no disguise for good or evil—all is outspoken.

The Greek songs have preserved the characteristics of these early compositions down to our own days in a remarkable degree. Cut off from the rest of Europe, and the general advance of civilization by their subjection to the Turks, the national struggle against the oppressor was carried on in the most primitive fashion both in the mountains and at sea; a series of isolated hand-to-hand fights taking place in every village. A "cattle-lifter" combined his thieving with a patriotic resistance to his cruel tyrants: a "Klepht" (or robber) became a hero and a martyr in the eyes of his countrymen. Piracy was no more considered wrong than in the days of Ulysses: it was a praiseworthy spoiling of the Egyptians.

The Klepht songs were the refuge of the patriotic spirit of the people, and served to keep alive the feeling of nationality, even among those Greeks whom commerce and trade had scattered all over the world. There were songs for every event of life and for every season of the year; they may be rudely divided into three classes—"Poems of the imagination" (as Wordsworth has called a series of his own); "Domestic songs," comprising those on the festivals of the Church, and upon all social occasions such as deaths, births, and marriages; lastly, "Historical songs." Neither quantity nor rhyme is considered in these poems—accent alone is regarded; the "heroic verse" is, however, generally speaking, of fifteen syllables, divided into two parts, the first of eight, the second of seven; with an accent so placed as to end the first part with a dactyl, the second with an iambic. But nothing can be ruder than the structure of many lines; words are cut short to fit them into their places, grammar is disregarded, the sense often breaks off short, and must have been supplied by a gesture, or a word of explanation from the narrator; changes from the first to the third person, and back again, are made without any notice, according as it is supposed a more dramatic effect can be produced.

It must, never, however, be forgotten that such stories are not made to be read, but to be sung or told by itinerant minstrels, often blind, who made their way from door to door, after the fashion of the harpers in Scotland and Wales, or their far more illustrious prototype, Homer. They accompanied themselves on a sort of lyre of the ancient form, with five strings, sometimes degenerating to two, which were played on with a bow. Some of these rhapsodists only repeated the works of others, but the most renowned among them composed both the poem and the air to which it is sung, so that each new song was ushered into the world with an air invented especially for its own use. A celebrated old minstrel, John the Blind, who lived at the foot of Mount Ossa, at the end of the last century, was much sought after for the improvisations both in words and music which he was in the habit of producing on any subject which was given him.

The first collection of these songs was made by Fauriel in 1824; indeed, he may be almost said to have discovered their existence, certainly their poetic value; and publishing them, as he did, before the Declaration of Independence, his testimony is all the more valuable to the consideration in which the Klephts were held, and the patriotic feeling of Greece under their Ottoman oppressors.

The dates of the poems he thinks it impossible to fix, though he has discovered one in the Royal Library at Paris, which could not be later than 1640. A very large part of them have probably been sung for centuries, and altered again and again according to the taste of each succeeding age, as is the fate of all early ballads.

Since M. Fauriel's time, several popular collections have been published at Athens, and two volumes of them have been edited by Germans, accompanied by literal translations of not much merit, but full of curious and useful notes. Many of the songs are still extremely popular, but a great number are gradually dying out, even in the villages and hill districts.

One copy was found at a solitary monastery in the Morea, far up in the mountains, only to be reached by a narrow hill-path. A fine old ilex grew in the courtyard, on which hung the church bell; and the prior came forth in white, with a heavy hood, to do the honours of his house to a lady visitor, who could not be admitted into the cloister; to feed her with oranges and cream-cheese, and to show, for her instruction and amusement, that he had studied the songs of his country as well as his psalter.

The romantic or ideal division of the songs comes first. As of old in Greece, every river and fountain, every mountain, rock, and cavern, even every house, has its own particular genius, which watches over it with anxious care. The enduring remains of Paganism show through the thin disguise of that very dead form of Christianity, the Greek faith. The nymphs, naiads, lars, and lemurs appear in the shape of nixies and elves, and still inhabit their old haunts; the Moira, or Fate, still arranges the issues of life; the three Parcæ still pass through the awed city, though their office is now restricted to bringing the plague. One holds a great sheet of paper, the second a pair of scissors, the third a broom, for "writing down the names of the victims, cutting them off with the scissors, and sweeping them away." The small-pox is personified under the figure of a terrible woman, who is addressed, like the Furies, by a title of respect, to propitiate her, equivalent to the "Eumenides,"—the "Eulogia," she who must be well named. Charon, on the other hand, has preserved his name, but has changed his office: he escorts the dead to Hades, in the place of Mercury, or he is the porter, and keeps the gate, where, like Cerberus, he must be put to sleep by any mortal who would enter in. Still more often he has become the personification of Death itself—"His look is like the lightning, his face is like the fire, his shoulders like two hills, and his head a rock fortress; he makes the fields dark as he passes along." Sometimes he appears as a huge rider, on a black horse, accompanied with black dogs, sometimes as a black bird. His tent is described as either green or red, "but inside it is black, and the stoutest heart trembles at entering, for the tent-pegs are the arms of the Pallicari" (the "braves" among the "braves"), "the ropes are the tresses of beautiful maidens, and the stools are the heads of children."

One of the most touching of the shorter poems describes his

Sixty agas have I slain, and have burnt down their villages;
And as for the Turks and Albanians whom I left on the plain,
They were so many, beloved bird, that they could not be counted.
But now my turn is come for me to fall in battle." ' "

Gradually, as Greece achieved her freedom, the type degenerated, the Klepht ceased to be a patriot and became a brigand by no means of a superior class, without, however, losing his hold on the common people. A few years ago, a brigand held the whole country near Patras in fear, and when, at last, he was caught and imprisoned, he became the lion of the place; he belonged to a well-to-do family, was a handsome, agreeable man, and it was esteemed a great honour to dine with him in prison; he even piqued himself on the good wine and food that he gave, supplied him by his brother, a merchant in the town. At last the judge arrived to try him, and he thought it best to escape; a rumour was spread that he had been seen in the mountains, and all the town, the judge and court, and every soldier in the place rushed out to seek for him, some from curiosity, some to aid his escape; meantime he had been hid by the demarch or magistrate in his own cellars, who then, being much afraid of both the brigands and the "justices," joined the hue and cry. While the coast was clear the brigand, dressed as a gendarme, went off in a "speronare" to the mainland in Albania; there, in a little inn, he sat laughing and talking over his escape with some sailor friends, in his own patois, which he thought the innkeeper could not understand. The man, however, came forward, and gave him fair warning that he should tell the authorities, at the same time granting him some hours to escape, so that "justice" came in panting and tired and too late. Afterwards the brigand fell in love with a girl at Zante, and was discovered and watched as he came to and fro. They could not, however, succeed in catching him, when they put the poor girl and her mother in prison, where both soon after died. The man, out of heart and weary of life, was at last taken prisoner, and put into a wretched dungeon, where he also perished. His friends declared that he had been murdered by government, who retorted that he had put an end to himself. "To such base ends do" some heroic institutions "come at last!"

Not to end on so sad a note, a last song shall be given, which M. Fauriel says is the most ancient and the best in his collection, and commemorates an exploit like that of "Young Lochinvar." It is sung in Corfu and Cephalonia, and contains a number of words peculiar to the Archipelago and the maritime districts. It begins with telling how away in a far land a Klepht hears that his love is being constrained by a Turk to marry him:—

"As I was sitting at my marble table,
My black horse neighed, and my sword broke,
And I understood by my despair that they were marrying my love,

That they were betrothing her to another man, that they were crowning her with another.
I go and I fetch my black horses, the seventy and five.
'Which among my black horses, among my seventy and five,
Can with one flashing of his feet in the East arrive in the West?'
The black horses, as many as heard me, ran down with blood,
The mares who heard me dropped their foals;
But there was one old horse, very old he was, wounded forty times—
'I am old and ugly, and journeys do not longer suit me,
But for the love of my beautiful mistress, I will make a far journey,
Who fed me lovingly out of the hollow of her apron,
Who gave me drink lovingly out of the cup of her hand.'
Quickly he saddled the black horse, quickly he set forth.
'Wrap your dear head round with a turban of nine yards;
Do not act the prancing cavalier, or use your spurs,
For I should remember my youth and behave like a colt,
And sow thy brains upon nine acres of land.'
He gives a blow with his switch to the black horse and goes forty miles;
He gives a second and flies forty-five miles.

[Simply meaning that he goes a long way. The curious habit of putting the definite number to express the indefinite, in order to create a distinct idea of size or distance, is common in all the poems.]

And on the road, as he goes along, he prays to God—
'My God, grant that I find my father pruning in our vineyard.'
As a Christian he had spoken, as a Saint he was answered.
'Good morning, old man, whose is this vineyard?'
'It is the vineyard of sorrow and grief, it is the vine of my son John,
They are giving to-day another husband to his love;
They are blessing her with another, with another they are crowning her.'
'Oh, tell me, old man, shall I find them at the feast?'
'If thy horse is very swift, thou shalt find them still at table;
If thou hast only a good horse, thou shalt find them at the blessing.'
He gave a blow with his switch to the black horse, and springs forty miles;
He gives a second, and springs five and forty miles.
And on the road as he went he prayed to God—
'O my God, grant that I may find my mother watering our garden.'
As a Christian he had spoken, as a Saint was he answered.
He found also his mother watering in the garden—
'Good morning, good woman, to whom does this garden belong?'
'It is the garden of sorrow and misfortune, the garden of my son John;
They are giving another husband to his love;
They are blessing her with another, with another they are crowning her.'
He gives a blow with his switch to his horse and goes forty miles;
He gives a second and flies five and forty more.
The black horse began to neigh, and the bride has recognized it—
'Who is it that speaks to thee, O my bride, who is this who discourses to thee?'
'It is my eldest brother who brings my dowry.'
'If it is thy eldest brother, go out and give him to drink;
If it is thy first lover, I will go out myself, and I will kill him!'
'It is my eldest brother who is bringing my dowry.'
She takes a gold cup and goes out to give him to drink.
'Come to the right, my fair one, and give me to drink on the left.'
And the black horse knelt down, and the damsel is upon him.
He runs like the wind, and the Turks raise their muskets;
But they could not see the black horse, nor even his dust—
He who had a swift horse he saw the dust—
He who had only a good horse did not even see the dust." *

Now that Greece is apt to be very severely judged in the world's estimation, it is only right to remember the long agony she went through under Turkish oppression, and the many fine qualities displayed by her people in their resistance to the grinding tyranny of

* One touch to her hand, and one word to her ear,
When they reach'd the hall-door and the charger stood near.
So light to the croupe the fair lady he swung,
So light to the saddle before her he sprung.
"She is won! we are gone! over bank, bush, and scaur,
They'll have fleet horses that follow," quoth young Lochinvar.

[*From the* Proceedings of the Royal Society, No. 127, 1871.]

EXPERIMENTS IN PANGENESIS,

BY

BREEDING FROM RABBITS OF A PURE VARIETY,

INTO WHOSE CIRCULATION BLOOD TAKEN FROM OTHER VARIETIES HAD PREVIOUSLY BEEN LARGELY TRANSFUSED.

BY

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1871.

Darwin's provisional theory of Pangenesis claims our belief on the ground that it is the only theory which explains, by a single law, the numerous phenomena allied to simple reproduction, such as reversion, growth, and repair of injuries. On the other hand, its postulates are hypothetical and large, so that few naturalists seem willing to grant them. To myself, as a student of Heredity, it seemed of pressing importance that these postulates should be tested. If their truth could be established, the influence of Pangenesis on the study of heredity would be immense; if otherwise the negative conclusion would still be a positive gain.

It is necessary that I should briefly recapitulate the cardinal points of Mr. Darwin's theory. They are (1) that each of the myriad cells in every living body is, to a great extent, an independent organism; (2) that before it is developed, and in all stages of its development, it throws "gemmules" into the circulation, which live there and breed, each truly to its kind, by the process of self-division, and that, consequently, they swarm in the blood, in large numbers of each variety, and circulate freely with it; (3) that the sexual elements consist of organized groups of these gemmules; (4) that the development of certain of the gemmules in the offspring depends on their consecutive union, through their natural affinities, each attaching itself to its predecessor in a regular order of growth; (5) that gemmules of innumerable varieties may be transmitted for an enormous number of generations without being developed into cells, but always ready to become so, as shown by the almost insuperable tendency to feral reversion, in domesticated animals.

It follows from this, and from the general tenor of Mr. Darwin's reasoning and illustrations, that two animals, to outward appearance of the same pure variety, one of which has mongrel ancestry and the other has not, differ solely in the constitution of their blood, so far as concerns those points on which outward appearance depends. The one has none but gemmules of the pure variety circulating in his veins, and will breed true to his kind; the other, although only the pure variety of skin-gemmules happens to have been developed in his own skin, has abundance of mongrel gemmules in his blood, and will be apt to breed mongrels. It also follows from this that the main stream of heredity must flow in a far smaller volume from the developed parental cells, of which there is only one of each variety, than from the free gemmules circulating with the blood, of which there is a large number of each variety. If a parental developed cell bred faster than a free gemmule, an influx of new immigrants would gradually supplant the indigenous gemmules; under which supposition, a rabbit which, at the age of six months, produced young which reverted to ancestral peculiarities, would, when five years old, breed truly to his individual peculiarities; but of this there is no evidence whatever.

Under Mr. Darwin's theory, the gemmules in each individual must therefore be looked upon as entozoa of his blood, and, so far as the problems of heredity are concerned, the body need be looked upon as little more than a case which encloses them, built up through the development of some of their number. Its influence upon them can be only such as would account for the very minute effects of use or disuse of parts, and of acquired mental habits being transmitted hereditarily.

It occurred to me, when considering these theories, that the truth of Pangenesis admitted of a direct and certain test. I knew that the operation of transfusion of blood had been frequently practised with success on men as well as animals, and that it was not a cruel operation—that not only had it been used in midwifery practice, but that large quantities of saline water had been injected into the veins of patients suffering under cholera. I therefore determined to inject alien blood into the circulation of pure varieties of animals (of course, under the influence of anæsthetics), and to breed from them, and to note whether their offspring did or did not show signs of mongrelism. If Pangenesis were true, according to the interpretation which I have put upon it, the results would be startling in their novelty, and of no small practical use; for it would become possible to modify varieties of animals, by introducing slight dashes of new blood, in ways important to breeders. Thus, supposing a small infusion of bull-dog blood was wanted in a breed of greyhounds, this, or any more complicated admixture, might be effected (possibly by operating through the umbilical cord of a newly born animal) in a single generation.

I have now made experiments of transfusion and cross circulation on a large scale in rabbits, and have arrived at definite results, negativing, in my opinion, beyond all doubt, the truth of the doctrine of Pangenesis.

The course of my experiments was as follows:—Towards the end of 1869, I wrote to Dr. Sclater, the Secretary of the Zoological Society, explaining what I proposed to do, and asking if I might be allowed to keep my rabbits in some unused part of the Gardens, because I had no accommodation for them in my own house, and I was also anxious to obtain the skilled advice of Mr. Bartlett, the Superintendent of the Gardens, as to their breed and the value of my results. I further asked to be permitted to avail myself of the services of their then Prosector, Dr. Murie, to make the operations, whose skill and long experience in minute dissection is well known. I have warmly to thank Dr. Sclater for the large assistance he has rendered to me, in granting all I asked, to the full, and more than to the full; and I have especially to express my obligations to the laborious and kind aid given to me by Dr. Murie, at real inconvenience to himself, for he had little leisure to spare. The whole of the operations of transfusion into the jugular vein were performed by him, with the help of Mr. Oscar Fraser, then Assistant Prosector, and now appointed Osteologist to the Museum at Calcutta, I doing no more than preparing the blood derived from the supply-animal, performing the actual injection, and taking notes. The final series of operations, consisting of cross-circulation between the carotid arteries of two varieties of rabbits, took place after Dr. Murie had ceased to be Prosector. They were performed by Mr. Oscar Fraser in a most skilful manner, though he and I were still further indebted, on more than one occasion, to Dr.

Murie's advice and assistance. My part in this series was limited to inserting and tying the canulæ, to making the cross-connexions, to recording the quality of the pulse through the exposed arteries, and making the other necessary notes.

The breed of rabbits which I endeavoured to mongrelize was the "Silver-grey." I did so by infusing blood into their circulation, which I had previously drawn from other sorts of rabbits, such as I could, from time to time, most readily procure. I need hardly describe Silver-grey rabbits with minuteness. They are peculiar in appearance, owing to the intimate mixture of black and grey hairs with which they are covered. They are never blotched, except in the one peculiar way I shall shortly describe; and they have never lop ears. They are born quite black, and their hair begins to turn grey when a few weeks old. The variations to which the breed is liable, and which might at first be thought due to mongrelism, are white tips to the nose and feet, and also a thin white streak down the forehead. But these variations lead to no uncertainty, especially as the white streak lessens or disappears, and the white tips become less marked, as the animal grows up. Another variation is much more peculiar: it is the tendency of some breeds to throw "Himalayas," or white rabbits with black tips. From first to last I have not been troubled with white Himalayas; but in one of the two breeds which I have used, and which I keep carefully separated from each other, there is a tendency to throw "sandy" Himalayas. One of these was born a few days after I received the animals, before any operation had been made upon them, and put me on my guard. A similar one has been born since an operation. Bearing these few well-marked exceptions in mind, the Silver-grey rabbit is excellently adapted for breeding-experiments. If it is crossed with other rabbits, the offspring betray mongrelism in the highest degree, because any blotch of white or of colour, which is not "Himalayan," is almost certainly due to mongrelism; and so also is any decided change in the shape of the ears.

I shall speak in this memoir of litters connected with twenty silver-grey rabbits, of which twelve are does and eight are bucks; and eighteen of them have been submitted to one or two of three sorts of operations. These consisted of:—

(1) Moderate transfusion of partially defibrinized blood. The silver-grey was bled as much as he could easily bear; that was to about an ounce, a quantity which bears the same proportion to the weight of his body (say 76 oz.) that 2 lbs. bears to the weight of the body of a man (say 154 lbs.); and the same amount of partially defibrinized blood, taken from a killed animal of another variety, was thrown in in its place. The blood was obtained from a yellow, common grey, or black and white rabbit, killed by dividing the throat, and received in a warmed basin, where it was stirred with a split stick to remove part of the fibrine. Then it was filtered through linen into a measuring-glass, and thence drawn up with a syringe, graduated into drachms; and the quantity injected was noted.

(2) The second set of operations consisted in a large transfusion of wholly

defibrinized blood, which I procured by whipping it up thoroughly with a whisk of rice-straw; and, in order to procure sufficient blood, I had on one occasion to kill three rabbits. I alternately bled the silver-grey and injected, until in some cases a total of more than 3 ounces had been taken out and the same quantity, wholly defibrinized, had been thrown in. This proportion corresponds to more than 6 lbs. of blood in the case of a man.

(3) The third operation consisted in establishing a system of cross-circulation between the carotid artery of a silver-grey and that of a common rabbit. It was effected on the same principle as that described by Addison and Morgan (Essay on Operation of Poisonous Agents upon the Living Body. Longman & Co., 1829), but with more delicate apparatus and for a much longer period. The rabbits were placed breast to breast, in each other's arms, so that their throats could be brought close together. A carotid of each was then exposed; the circulation in each vessel was temporarily stopped, above and below, by spring holders; the vessels were divided, and short canulæ, whose bores were larger than the bore of the artery in its normal state, were pressed into the mechanically distended mouths of the arteries; the canulæ were connected cross-wise; the four spring holders were released, and the carotid of either animal poured its blood direct into the other. The operation was complicated, owing to the number of instruments employed; but I suspended them from strings running over notched bars, with buttons as counterpoises, and so avoided entanglement. These operations were exceedingly successful; the pulse bounded through the canulæ with full force; and though, in most cases, it began to fall off after ten minutes or so, and I was obliged to replace the holders, disconnect the canulæ, extract the clot from inside them with a miniature corkscrew, reconnect the canulæ, and reestablish the cross-flow two, three, or more times in the course of a single operation, yet on two occasions the flow was uninterrupted from beginning to end. The buck rabbit, which I indicate by the letter O, was $37\frac{1}{2}$ minutes in the most free cross-circulation imaginable with his "blood-mate," a large yellow rabbit. There is no mistaking the quality of the circulation in a bared artery; for, when the flow is perfectly free, the pulse throbs and bounds between the finger and thumb with a rush, of which the pulse at the human wrist, felt in the ordinary way, gives an imperfect conception.

These, then, are the three sorts of operations which I have performed on the rabbits; it is convenient that I should distinguish them by letters. I will therefore call the operation of simply bleeding once, and then injecting, by the letter u; that of repeated bleedings and repeated injections by the letter w; and that of cross-circulation by the letter x.

In none of these operations did I use any chemical means to determine the degree to which the blood was changed; for I did not venture to compromise my chances of success by so severe a measure; but I adopted the following method of calculation instead:—

I calculate the change of blood effected by transfusion, or by cross-circulation, upon moderate suppositions as to the three following matters:—

(1) The quantity of blood in a rabbit of known weight.

(2) The time which elapses before each unit of incoming blood is well mixed up with that already in the animal's body.

(3) The time occupied by the flow, through either carotid, of a volume of blood equal to the whole contents of the circulation.

As regards 1, the quantity of blood in an animal's body does not admit, by any known method, of being accurately determined. I am content to take the modern rough estimate, that it amounts to one-tenth of its total weight. If any should consider this too little, and prefer the largest estimate, viz. that in Valentin's 'Repertorium,' vol. iii. (1838), p. 281, where it is given for a rabbit as one part in every 6·2 of the entire weight, he will find the part of my argument which is based on transfusion to be weakened, but not overthrown, while that which relies on cross-circulation is not sensibly affected.

As regards 2, the actual conditions are exceedingly complex; but we may evade their difficulty by adopting a limiting value. It is clear that when only a brief interval elapses before each unit of newly infused blood is mixed with that already in circulation, the quality of the blood which, at the moment of infusion into one of the cut ends of the artery or vein, is flowing out of the other, will be more alienized than if the interval were longer. It follows that the blood of the two animals will intermix more slowly when the interval is brief than when it is long. Now I propose to adopt an extreme supposition, and to consider them to mix *instantaneously*. The results I shall thereby obtain will necessarily be less favourable to change than the reality, and will protect me from the charge of exaggerating the completeness of intermixture.

As regards 3, I estimate the flow of blood through either carotid to be such that the volume which passes through it in ten minutes equals the whole volume of blood in the body. This is a liberal estimate; but I could afford to make it twice or even thrice as liberal, without prejudice to my conclusions.

Upon the foregoing data the following Table has been constructed. The formulæ are:—Let the blood in the Silver-grey be called a, and let its volume be V, and let the quantity u of alien blood be thrown in at each injection, then the quantity of blood a remaining in the Silver-grey's circulation, after n injections,

$$=\mathrm{V}\left(1-\frac{u}{\mathrm{V}}\right)^{n}.$$

If the successive injections be numerous and small, so as to be equivalent to a continuous flow, then, after w of alien blood has passed in, the formula becomes $\mathrm{V}.e^{-\frac{w}{\mathrm{V}}}$.

A comparison of the numerical results from these two formulæ shows that no sensible difference is made if (within practicable limits) few and large, or many and small, injections are made, the total quantity injected being the same.

In cross-circulation the general formula is this :—If V′ be the volume of blood in the other rabbit, after *w* of alien blood has passed through either canula, the quantity of blood *a* remaining in the Silver-grey exceeds*

$$\frac{V}{V+V'}\left\{V+V'e^{-\left(\frac{1}{V}+\frac{1}{V'}\right)w}\right\}. \text{ This becomes } \frac{V}{2}\left\{1+e^{-\frac{2w}{V}}\right\} \text{ when } V=V';$$

also, when V′ is infinite, it gives the formula already mentioned for injection by a continuous flow of purely alien blood.

TABLE I.

(Contents of circulation of Silver-grey Rabbit = 100.)

Quantity of blood infused.	Maximum percentage of original blood remaining after: Successive injections of purely alien blood, each $=\frac{100}{12}$. Number of injections.		Continuous flow of purely alien blood.	Cross-circulation. Rabbits of equal size.	Cross-circulation. Blood-mate $\frac{1}{10}$ larger than the Silver-grey.	Period, in minutes, during which the continuous flow through each carotid has lasted.
25	3	77	78	80	80	$2\frac{1}{2}$
50	6	59	61	68	68	5
75	9	46	47	61	60	$7\frac{1}{2}$
100	12	35	37	56	55	10
125	15	27	29	54	52	$12\frac{1}{2}$
150	18	21	22	52	51	15
175	21	16	17	51	50	$17\frac{1}{2}$
200	24	12	14	51	49	20
300	36	4	5	50	48	30
400	48	1	2	50	48	40
infinite	infinite	0	0	50	48	infinite.

I now give a list (Table II.) of the rabbits to which, or to whose blood-mates, I shall have to refer. Every necessary particular will be found in the Table :—the weight of the rabbits; the estimated weight of blood in their veins; the operations performed on them, whether *u*, *w*, or *x*; the particulars of those several operations; the estimated percentage of alien blood that was substituted for their natural blood; and lastly, the colour, size, and breed of their blood-mates.

* I am indebted to Mr. George Darwin for this formula.

TABLE II.

Silver-grey Does.	Weight of rabbit.	Estimated weight of blood.	Nature of operation*.	Drachms infused, and period of cross-circulation.	Percentage of alienized blood.	Colour &c. of blood-mate.
	lbs. oz.	drachms.				
A	5 9	79	*u*	9	11	Common grey and white.
B	5 13	82	*u*	10	12	Yellow, large.
			x	10 min. perfect, 15 or 20 very good.	50, or more.	Common grey.
C	5 8	78	*u*	9·5	12	Albino, large.
D	5 4	75	*u*	8·5	12	Himalaya.
E	4 9	58	*u*	8	14	Common grey.
			x	13 min. good, 14 poor.	50, about	Common grey.
F	4 13	61	*u*	7·7	10	Black and white, large.
G	4 11	60	*w*	25·5, in 6 injections.	35	Grey and black, speckled.
			x	31 min. good, total.	75	Common grey.
H	...	...	*x*	15 min. perfect, 15 very good.	50	Common grey.
I†	...	...	*x*	16 min. perfect, not much more.	nearly 50	Common grey and white.
J†	...	...	*x*	35 min. perfect.	...	Yellow, brown mouth (? Himalaya).
S	...	...	*x*	too unsuccessful to be worth counting.	? any.	Angora, fawn and white.
T	...	...	None.	......	None.	......
Bucks.						
K	4 14	62	*u*	9	14	Yellow, brown mouth.
			w	14, in 4 injections, total.	32	Yellow and white.
L	4 13	61	*u*	7	11	Common grey.
M	4 0	51	*u*	7	14	Black and white.
			w	24·5, in 6 injections, total.	45	3 black and white in succession.
N	4 9	58	*u*	7·5	13	Angora, grey and white, red eyes.
			w	16·5, in 4 injections, total.	34	Yellow.
O (son of C (*u*) by K (*u*))	...	...	*x*	37½ min. perfect.	50	Yellow.
P†	...	...	*x*	25 to 30 min. perfect.	50	Common grey.
Q†	...	...	*x*	15 min. perfect, 15 very good.	50	Yellow and white.
	...	...	*x*	25 min. pretty good.	50	Common grey and white.

* *Note* (to 4th column).—*u* means simple transfusion, by one copious bleeding, and then injecting; *w* means compound transfusion by successive bleedings and successive injections; *x* means cross-circulation.

† These rabbits belong to a breed liable to throw "Sandy" Himalayas.

TABLE III.

Litters subsequent to first transfusion. Both parents Silver-greys. Average proportion of alienized blood in either parent $= \frac{1}{8}$; therefore in young $\frac{1}{8}$ also.

Out of	By	Number and character of litters.
A	K	4 true Silver-greys.
A	M	5 ditto, but 1 had a white foot to above knee.
B	K	5 true Silver-greys.
C	K	6 ditto.
D	K	4 ditto.
E	L	6 ditto.
		30 all true Silver-greys, except possibly one instance.

Litters subsequent to second transfusion of buck. Both parents Silver-greys. Average proportion of alienized blood in young about $\frac{1}{4}$.

Out of	By	Number and character of litters.
A	M	6 true Silver-greys.

Litters subsequent to cross-circulation of buck only, the does being 0 or *u*. Both parents Silver-greys. Average proportion of blood in young between $\frac{1}{4}$ and $\frac{1}{3}$.

Out of	By	Number and character of litters.
S	O	5 true Silver-greys.
C	O	5 ditto.
T	O	3 ditto.
		13 all Silver-greys.

Litters subsequent to cross-circulation of both parents (Silver-greys). Average proportion of alienized blood in young fully $\frac{1}{2}$.

Out of	By	Number and character of litters.
B	O	3 true Silver-greys.
H	O	7 ditto.
H	O	7 ditto.
I*	P*	6 ditto.
J*	Q*	6 ditto, all but one, a sandy Himalaya.
J*	P*	8 true Silver-greys.
		37 36 Silver-greys, 1 Himalaya.

* These rabbits belong to a breed liable to throw "Sandy" Himalayas.

Litters subsequent to cross-circulation of both parents (common rabbits).
Average proportion of alienized blood in young a little less than $\frac{1}{2}$.

Out of blood-mate to	By blood-mate to	Number and character of litters.
E	R	8 none Silver-grey, all like father or mother.
E	Q*	5 ditto.
G	O	9 ditto.
I*	Q*	8 ditto.
J*	Q*	8 ditto.
		38 none Silver-greys.

In another list (Table III.) I give particulars of all the litters I have obtained from these rabbits, classified according to the operations which the parents had previously undergone.

I will now summarize the results. In the first instance I obtained five does (A, B, C, D, and E) and three bucks (K, L, and M) which had undergone the operation which I call *u*, and which had in consequence about $\frac{1}{8}$ of their blood alienized. I bred from these †, partly to see if I had produced any effect by the little I had done, and chiefly to obtain a stock of young rabbits which would be born with $\frac{1}{8}$ of alien gemmules in their veins, and which, when operated upon themselves, would produce descendants having nearly $\frac{1}{4}$ alienized blood (the exact proportion is $1-(1-\frac{1}{8})^2=\frac{15}{64}$). I obtained thirty young ones in six litters; and they were all true silver-greys, except, possibly, in one instance (out of the doe A (*u*) by the buck M (*u*)), where one, of a litter of five, had a white fore leg, the white extending to above the knee-joint. This white leg gave me great hopes that Pangenesis would turn out to be true, though it might easily be accounted for by other causes; for my stock were sickly (both those on which I had not operated and those on which I had suffering severely from a skin disease), and it was natural under those circumstances of ill health that more white than usual should appear in the young.

Having, then, had experience in transfusion, and feeling myself capable of managing a more complicated operation without confusion, I began the series which I call *w*. I left my old lot of does untouched, but obtained one new doe (G(*w*)), which had undergone the last operation, and three bucks (K (*u*, *w*), M (*u*, *w*), N (*u*, *w*)) which had undergone both operations, *u* and *w*. On endeavouring to breed from them, the result was unexpected, they appeared to have become sterile. The bucks were as eager as possible for the does; but the latter proving indifferent, I was unable to testify to their union having taken place; so I left them in pairs, in the same hutch, for periods of three days at a time. Attempts were made in this

* These rabbits belong to a breed liable to throw "Sandy" Himalayas.

† I always allowed the bucks to run for awhile with waste does before commencing the breeding-experiments, that all old reproductive material might be got rid of.

way, to breed from them in seven instances; and five of them were utter failures. One case was quite successful; and that, fortunately, was of the same pair (A (u) and M (u, w)) which, under the u operation, had bred the white-footed young one. This time, the offspring (six in number) were pure silver-greys. The last case was unfortunate. The doe (E (u)) had been once sterile to its partner (N (u, w)), and she had been put again in the same hutch with him for a short period, but was thought not to have taken him. She was shortly afterwards submitted to the operation x. From this she had nearly recovered when she brought forth an aborted litter and died. I was absent from town at the time; but Mr. Fraser, who examined them, wrote to say he fully believed that some were pied; if so, it must have been under the influence of the cross-circulation. But I have little faith in the appearance of the skin of naked, immature rabbits; for I have noticed that difference of transparency, and the colour of underlying tissues, give fallacious indications.

My results thus far came to this, viz. that by injecting defibrinized blood I had produced no other effect than temporary sterility. If the sterility were due to this cause alone, my results admitted of being interpreted in a sense favourable to Pangenesis, because I had deprived the rabbits of a large part of that very component of the blood on which the restoration of tissues depends, and therefore of that part in which, according to Pangenesis, the reproductive elements might be expected to reside. I had injected alien corpuscles but not alien gemmules. The possible success of the white foot, in my first litters, was not contradicted by the absence of any thing of the sort in my second set, because the additional blood I had thrown in was completely defibrinized. It was essential to the solution of the problem, that blood in its natural state should be injected; and I thought the most convenient way of doing so was by establishing cross-circulation between the carotids. If the results were affirmative to the truth of Pangenesis, then my first experiments would not be thrown away; for (supposing them to be confirmed by larger experience) they would prove that the reproductive elements lay in the fibrine. But if cross-circulation gave a negative reply, it would be clear that the white foot was an accident of no importance to the theory of Pangenesis, and that the sterility need not be ascribed to the loss of hereditary gemmules, but to abnormal health, due to defibrinization and perhaps to other causes also.

My operations of cross-circulation (which I call x) put me in possession of three excellent silver-grey bucks, four excellent silver-grey does, and one doe whose operation was not successful enough for me to care to count it. One of my x does (B) had already undergone the operation u, and I had another of my old lot (C (u)), which I left untouched. There were also three common rabbits, bucks, which were blood-mates to silver-greys, and four common rabbits, does, also blood-mates of silver-greys. From this large stock I have bred eighty-eight rabbits in thirteen litters, and in no single case has there been any evidence of alteration of breed. There has been one instance of a sandy Himalaya; but the owner of this breed assures me they are liable to throw them, and, as a matter of fact, as I have already stated, one of the does he sent me, did litter and throw one a few days after she reached me. The conclusion from this large series of experiments is not to be avoided, that the doctrine of Pangenesis, pure and simple, as I have interpreted it, is incorrect.

Let us consider what were the alternatives before us. It seems *à priori* that, if the reproductive elements do not depend on the body and blood together, they must reside either in the solid structure of the gland, whence they are set free by an ordinary process of growth, the blood merely affording nutriment to that growth, or else that they reside in the blood itself. My experiments show that they are not independent residents in the blood, in the way that Pangenesis asserts; but they prove nothing against the possibility of their being temporary inhabitants of it, given off by existing cells, either in a fully developed state or else in one so rudimentary that we could only ascertain their existence by inference. In this latter case, the transfused gemmules would have perished, just like the blood-corpuscles, long before the period had elapsed when the animals had recovered from the operations.

I trust that those who may verify my results will turn their attention to the latter possibility, and will try to get the male rabbits to couple immediately, and on successive days, after they have been operated on. This might be accomplished if there were does at hand ready to take them; because it often happens that when the rabbits are released from the operating-table, they are little, if at all, dashed in their spirits; they play, sniff about, are ready to fight, and, I have no doubt, to couple. Whether after their wounds had begun to inflame, they would still take to the does, I cannot say; but they sometimes remain so brisk, that it is probable that in those cases they would do so. If this experiment succeeded, it would partly confirm the very doubtful case of the pied young of the doe which died after an operation of cross-circulation (which, however, further implies that though the ovum was detached, it was still possible for the mother gemmules to influence it), and it would prove that the reproductive elements were drawn from the blood, but that they had only a transient existence in it, and were continually renewed by fresh arrivals derived from the framework of the body. It would be exceedingly instructive, supposing the experiment to give affirmative results, to notice the gradually waning powers of producing mongrel offspring.

APPENDIX I.

It is important that I should give details of the operations of cross-circulation. I may mention that, having to deal with many rabbits, I distinguished them permanently by tattooing bold Roman numerals in the inside of their ears.

I. Experiments of cross-circulation on one buck and two does, pure silver-greys, of a breed obtained from Mr. E. Royds, of Greenhill, Rochdale, the same breed as that on which all my *u* and *w* experiments had been made.

Oct. 19, 1870.—*Silver-grey buck,* O, out of doe A (*u*) by M (*u*), and therefore own brother to the white-footed young one, a small rabbit, just six months old. His blood-mate was a

Yellow buck, lop-eared, white throat, probably one-fifth heavier than the silver-grey. I avoided unnecessary weighing, because it frightens the animals, and tends to interfere with the final success. At $12^h\ 30^m$ I made cross-circulation; flow was perfect; $12^h\ 35^m$, continued perfect; $12^h\ 40^m$, perfect, but yellow to silver-grey perhaps the stronger; $12^h\ 44^m$, ditto; $12^h\ 50^m$, perfect both ways; $12^h\ 55^m$, ditto; 1^h, ditto; $1^h\ 5^m$, ditto; $1^h\ 7\frac{1}{2}^m$, ditto. I then stopped and tied up. I tested the flow with a small and delicate but very simple pulse-meter on all these occasions, not liking to interfere overmuch with my fingers. I, however, used them at the commencement, at $12^h\ 50^m$, and at $1^h\ 5^m$.

Oct. 20, 1870.—*Silver-grey doe,* B (*u*), a fine large animal; her blood-mate was a *Common large grey lop-eared doe,* about one-tenth heavier than the silver-grey.

1^h, cross-circulation established, apparently perfect; I mean the throbbing of the canula and artery were obvious; $1^h\ 6^m$, felt and found the flow quite good; $1^h\ 12^m$, common to silver-grey quite good, *vice versâ* poor; $1^h\ 15^m$, ditto; I disconnected and cleaned and removed clots and reconnected. This I repeated several times; there was still much trouble in maintaining a proper flow from silver to common grey, but common to silver was always good. The operation continued till $1^h\ 40^m$; then I disconnected; and as the silver-grey had received too much, I let her bleed to 4 drachms.

Oct. 27, 1870.—*Silver-grey doe,* H, moderate size; her blood-mate was a *Common large grey doe,* certainly more than a tenth heavier than the silver-grey. There was some trouble with her, as the carotid was abnormal, and three offshoots from it had to be tied before the canula could be inserted.

$12^h\ 48^m$, cross-circulation established, perfect pulse, but silver to common the fullest; $12^h\ 53^m$, perfect; 1^h, silver to common perfect, *vice versâ* rather poor; $1^h\ 2^m$, ditto; $1^h\ 7^m$, common to silver stopped; I disconnected and cleaned and reconnected, and by $1^h\ 12^m$ had reestablished perfect cross-circulation; at $1^h\ 30^m$ I had stopped silver to common and made common to silver better; got five minutes good flow, then repeated cleanings and got three minutes more. My estimate at the close of the operation was that the silver-grey gave blood freely for thirty-five minutes, and received it freely for about the same time.

II. Experiments of cross-circulation on two bucks and two does of a silver-grey breed, reputed pure, and looking well-bred animals, but liable to show russet marks. They were procured of Mr. Vipan, of March, Cambridgeshire, and are of the same breed as those on which Mr. Bartlett made his well-known experiments about the production of Himalayas (Proc. Zool. Soc. 1861). They are liable to throw "Sandy Himalayas," as I found myself, as Mr. Bartlett also found, and as Mr. Vipan informs me is the case. I distinguish this breed by asterisks (*).

Oct. 6, 1870.—*Silver-grey buck,* P*, moderate size; his blood-mate was a *Common grey buck,* with some russet on his back and white on his belly; he was the larger of the two animals.

12ʰ 50ᵐ, cross-circulation established, perfect; 12ʰ 55ᵐ, ditto, but silver to common, I think, a trifle the stronger; 12ʰ 59ᵐ, ditto; 1ʰ 5ᵐ, common to silver very faint. I stopped them and cleaned out twice and successively; 1ʰ 15ᵐ, good, but common to silver was the least good; 1ʰ 25ᵐ, disconnected. My estimate was that there had been an equivalent to fully twenty-five minutes, and perhaps thirty minutes, of capital flow both ways.

Oct. 7, 1870.—*Silver-grey buck,* Q*, moderate size; his blood-mate was a *Yellow buck, white belly, large.*

11ʰ 40ᵐ, cross-circulation established; 11ʰ 45ᵐ, quite good; 11ʰ 50ᵐ, good but not perfect; 11ʰ 55ᵐ good; 12ʰ both stopped. Then I made several disconnexions and cleanings, and obtained short periods of success; at 12ʰ 35ᵐ I finally stopped. My estimate was thirty minutes' good running: the silver-grey received more than his share; there was a slip in the operation, and five drachms of blood were lost between the rabbits; so I did not care to let the silver-grey bleed more.

Oct. 6, 1870.—*Silver-grey doe,* I*, moderate size; her blood-mate was a *Common grey doe, large.*

3ʰ 40ᵐ, cross-circulation was established; 3ʰ 44ᵐ, excellent; 3ʰ 50ᵐ, excellent; 3ʰ 55ᵐ, excellent; shortly after, something was twisted or otherwise went wrong, and both stopped. I had a good deal of trouble and but little further success. Ten drachms of blood was lost between the rabbits (partly by leakage of the canulæ).

Oct. 7, 1871.—*Silver-grey doe,* J*, moderate size; her blood-mate was a *Yellow doe, dark about mouth,* and also of moderate size. I afterwards became convinced she was simply a sandy Himalaya.

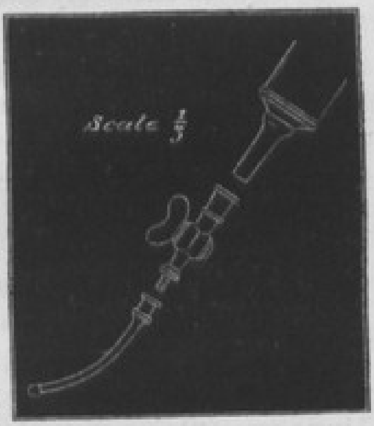
At 2ʰ 5ᵐ established cross-circulation; 2ʰ 13ᵐ, quite good; 2ʰ 20ᵐ, excellent; 2ʰ 25ᵐ, excellent; 2ʰ 30ᵐ, ditto; 2ʰ 35ᵐ, ditto; 2ʰ 40ᵐ, ditto, then disconnected. An accident occurred at the end, by which the silver-grey lost four drachms of blood.

APPENDIX II.

Description of the method of performing the operations.

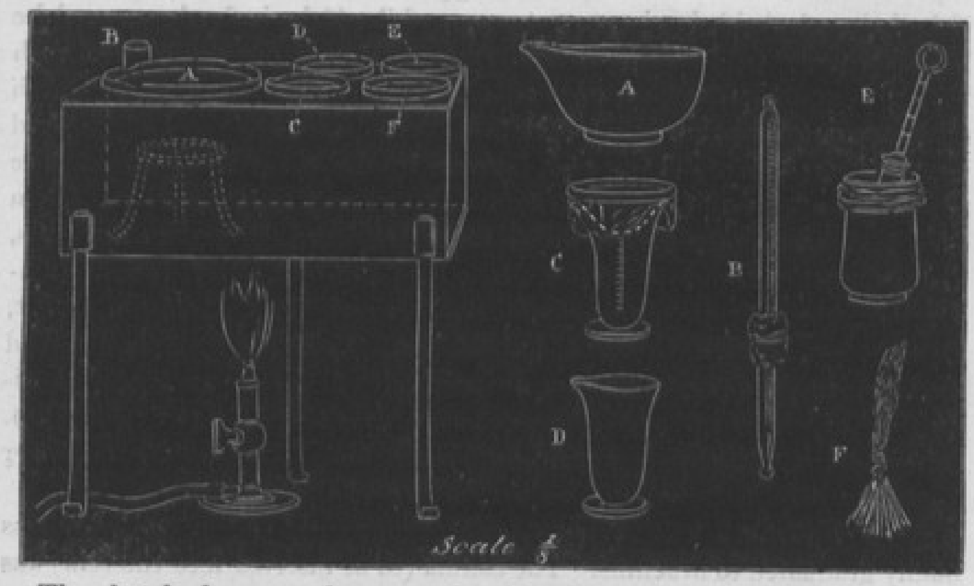
It is essential to a fair chance of success that the operator should have a large and thriving stock of full-grown rabbits. They cannot be procured at will in the market; and young ones are so timid and tender that they are not fit to be operated on. The next essential point is an operating-table, with ample and proper apparatus for holding the rabbits easily but rigidly. It is most improper to subject a helpless animal to an operation without taking every precaution for its success, so as to minimize the necessity for operating. The chief hindrances to success are, entanglement of instruments, or the breaking loose of blood-vessels, both owing to an unexpected start; also an animal will struggle violently, and become terrified if he is loosely held, hoping to get away, whilst if he is firmly secured he lies as though magnetized, without signs of fear or discomfort, and with his pulse and breathing perfectly normal. I regret extremely that, although I took pains to inquire, I did not at first hear of Czermak's recently devised apparatus for holding the head. I began by the old plan of putting the animals in a bag and holding them, which was very unsatisfactory. Then I devised a plan of my own, which was good, but inferior to Czermak's, and I therefore abstain from describing it. The latter, with recent modifications, can now be obtained at Mr. Hawkesley's, 4 Blenheim Street, Bond Street, London, to whom, I should say, I have been greatly indebted for the care and thought he gave to successive and very numerous modifications of my instruments (far more numerous than I care to describe). A drawing of Czermak's apparatus will be found in the 'Berichte der K. Sächs. Gesellschaft der Wissenschaften zu Leipzig,' 1867, p. 212.

For injections, I used a five-drachm ebonite syringe, whose stem was boldly graduated to drachms. The canula (to be inserted into the vein) was screwed into a light stopcock. This was filled with water, which, so long as the cock was closed, did not run out for want of a vent-hole. When it was thrust in the vein and the vein was tied round it, I held the syringe full of blood near the open end of the stopcock, drove out all air by allowing a few drops of blood to fall into its mouth, then pushed its nozzle firmly in, opened the cock and began to inject, steadily and slowly, at the rate of about one drachm in twenty seconds. When the syringe was emptied, I turned the stopcock, withdrew it, rapidly filled it, emptied it and again filled it with warm water, and returning to the canula with the same precautions as before, I threw in about $\frac{1}{4}$ drachm, to wash the blood out of the canula and adjacent vein. I do not think I lost more than three (or perhaps four) rabbits by injecting air, although the removals and replacements of the syringe were very numerous, often ten times in a single operation of the *w* kind.



My apparatus consisted of a zinc warm-water bath, represented on the left of the diagram (p. 408); the vessels drawn to the right of it fitted into holes in its lid, as indicated by the letters. A is the basin to catch the supply blood;

it was whipped up by the whisk F; then poured into C, which consists of a short funnel with muslin below, resting in the top of a glass measure; when the blood had strained through, the funnel and muslin were set on the top of D, to get them out of the way and, at the same time, to keep them warm for future use; B is the thermometer; E is a spill-case full of water to contain the syringe. In addition to these, I required a large slop-pail, a jug of hot, and another of cold water.

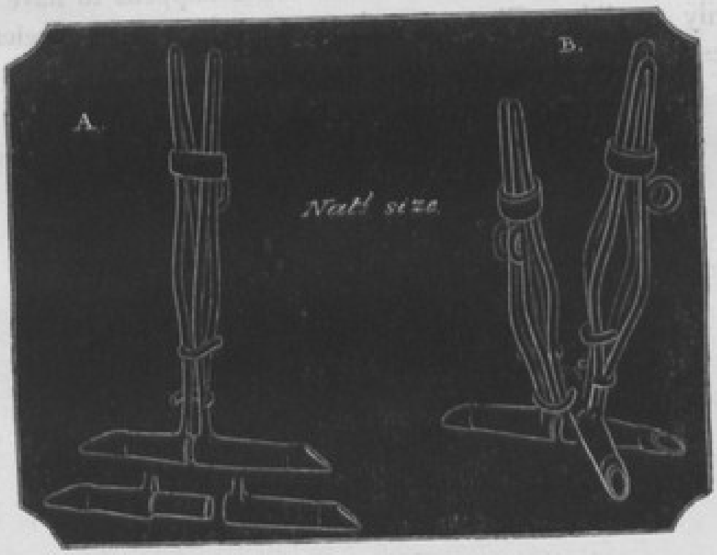


The sketch shows my latest outfit of basins and warm water for injecting. It was not perfected until I had nearly finished the experiments. Scrupulous cleanliness is requisite, and great orderliness; for the hazard lies, not in the performance of one difficult operation, but in making a mistake in some one of a great many easy operations. The course of an operation was as follows:—(1) secure the animal, (2) remove fur from neck, (3) anæsthetics, (4) expose jugular, (5) cut a slit in it and let the animal bleed as much as he can easily bear, about six drachms, (6) stop the flow with gentle pressure by spring forceps; the animal was then left for a minute while (7) Dr. Murie and Mr. Fraser divided the throat of the supply-rabbit, I catching the blood in a warmed basin and whipping it up, to defibrinize it, as it fell. I continued doing this while Dr. Murie was (8) inserting the canula; and when he was nearly ready he called to me, and I (9) filtered the blood, noting its amount, as a guide to what I had to dispose of, (10) drew up a syringe full, (11) injected a convenient number of drachms or half drachms, indicated by the graduations on the syringe-handle, (12) returned the overplus to the glass of supply-blood, (13) cleansed syringe and injected water, (14) let the rabbit bleed three or four drachms,—and then recommenced the series. I have not re-inserted in this description before (11) and (13) what I previously described about turning the stopcock &c.; nor have I spoken of the continual jotting down of notes in my case-book.

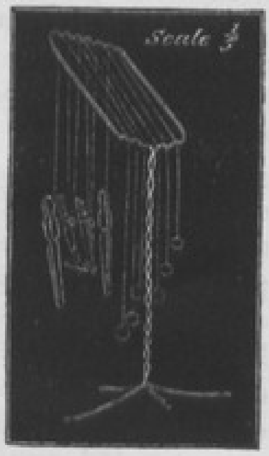
At the end of all, the vein was *tied.* It was, no doubt, the surest plan to avoid future hemorrhage, especially as the blood was defibrinized; but the rabbits were apt to suffer from phlebitis, and I lost some thereby.

Owing to the extreme rapidity and stiffness of the coagulation of rabbit's blood, it is quite easy to estimate the quantity that may have been spilt on the operating-table. It has simply to be sponged into a measuring-glass.

Cross-circulation would be a very easy operation in animals whose carotids were even a trifle larger than those of silver-grey rabbits; but it is difficult with these, because the smallest canula which can be used with propriety can only just be forced into the largest of them. It is no use operating with small canulæ; in every case, a layer of fibrine is sure to line the tube; if the bore is small this layer chokes it, while a layer of equal thickness in a larger tube leaves a free central passage. I found canulæ $\frac{1}{20}$ inch in diameter of bore were worthless; those I used were $\frac{1}{13}$ inch. If I were to operate again, I should not use silver-grey rabbits, on account of their smallness, but "Belgian hare" rabbits. When the canulæ are brought home together, the wire hooks, shown in the sketch, secure them; but I also slipped an India-rubber band over the tips of their handles. The cut ends of the artery were held open and stretched out by a pair of delicate curved forceps (a suggestion due to Dr. Murie), and the



canula was pressed in (the shape of its mouth was the result of many trials and modifications), and a ligature was put on. In the diagram, A represents one pair of canulæ, both opened and closed. B shows their position at the time of crossed circulation. It will be observed that each artery requires four pieces of apparatus, viz. two spring forceps to stop the blood, and two canulæ. Thus, when the throats were brought close together, to connect the arteries cross-wise, there were no less than eight separate pieces at work in a deep hollow, close together, and attached to delicate arteries, none of which could be permitted to twist or interfere with each other. I append a reduced sketch of one of the two frameworks over which, as previously described, I suspended these instruments, with attached counterpoises, and so avoided all confusion. Both pair of canulæ and two pair of forceps are here represented; they might be so arranged; but it is better to divide the instruments, equally, between the two frames.



For removing clogs from the canulæ, I tried a great many plans, none with as much success as I could wish. I have, however, been able to extract clots from the artery itself, a good quarter of an inch beyond the canulæ, with a wire whose end had been cut with a file into a delicate solid corkscrew. I washed out the canulæ, before reconnecting, with a thin stream of water sent through the quill of a small bird, which I had fastened, by help of a short India-rubber tube, to my syringe.

The wounds require careful dressing, just like those of a man. The rabbits bear the operations wonderfully well, and appear to suffer little or no pain when the influence of the anæsthetics happens to have left them temporarily sensible. They are often quite frisky when released, and sometimes look as though nothing whatever unusual had happened to them, all through the time of their recovery.

17

Experiments of Pangenesis

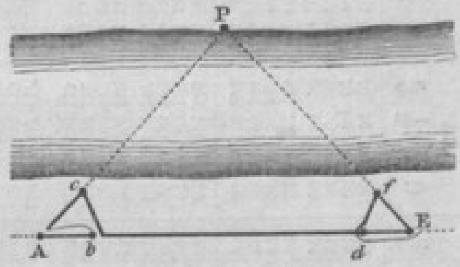
TABLE FOR ROUGH TRIANGULATION WITHOUT THE USUAL INSTRUMENTS AND WITHOUT CALCULATION.

A traveller may ascertain the breadth of a river, or that of a valley, or the distance of any object on either side of his line of march, by taking about 60 additional paces and by making a single reference to the annexed table.

Suppose that he is travelling in the direction A E and desires to know the distance of some object, P, from A. Let him proceed as follows:—

Walk 10 paces from A towards E, (to *b*). Back to A. Ten paces towards P, (to *c*). From *c* to *b*, *counting the paces to the nearest quarter-pace*, (*c b* is the chord of the angle at A to radius 10).

Walk on towards E. The distance A E must be taken as 100. It need not be 100 *paces*, but may be 100 of any convenient unit of length, as feet, fathoms, minutes' walk, furlongs, &c.; or it may be some simple multiple of 100, as 200, 300, 500, 1000.



When 10 paces short of E (at *d*), mark the spot and walk on to E. Thence towards P, (to *f*). From *f* to *d*, *counting the paces to the nearest quarter-pace*, (*f d* is the chord of B to radius 10).

This completes the operation. Nothing has to be recollected but the values of *c b*, of A E, and of *f d*.

To find A P, enter the table with *c b* at the side and *f d* at the top.
" E P, " *f d* " *c b* "

The tabular number is the value of these quantities, *supposing* A E *to have been* 100. If it be 200, 300, 500, &c., the tabular number must be multiplied by 2, 3, 5, &c., as the case may be. Beyond this, there is no calculation. If A E has been taken in paces, the tabular number will be in paces also; if in minutes' walk, in minutes' walk, and so on.

The angles corresponding to the chords, viz. *c* A *b* (or P A E), for *c b*, and *f* E *d* (or P E D), for *f d*, are also given. Examples:—

c b	is	5	paces:	A E	100	paces;	*f d*	6½	paces;	then	A P=	66	;	E P=	52	:	*c* A *b*=	28° 58′	;	*f* E *d*=	37° 56′
"	"	"		"	300	"	"	"	"		"	198		"	156		"	"		"	"
"		5½	"	"	100	"	"	6½	"		"	65		"	56		"	31° 56′		"	"
"		10¾	"	"	100	"	"	8	"		"	79		"	98		"	65° 2′		"	47° 10′

If A E be a north and south line, the *bearing* of P from A, as represented by the angle *c* A *b*, is to be found by simply ascertaining the value of *c b*.

The most methodical way of making these measurements is to select some tuft of grass, or stone, or stick, that may happen to be lying on the ground, as the starting-point, A. Then, to mark *b* by placing any object there, or by planting a stick, which can be recovered on returning to *b* from *c*. D is to be marked in the same way. E requires no mark at all; neither does *c*, nor *f*.

Particular care must be taken to walk in a straight line from A to E. It will surprise most people, on looking back at their track, to see how curved it has been, and how far E *d* is from pointing truly towards A. It is always well to sight some distant object in a line with E, when walking towards it.

The triangle P A E must be contrived so that none of its angles are less than 30°, or the chords of the angles at A and E will not be found in the Table. These cases do not give reliable results, and have therefore been omitted.

Should a traveller have no Tables by him, he can always *protract* his measurements to a scale on a sheet of paper, or even on the ground, and so solve his problem. If real accuracy be aimed at, it is clear that careful measurements of the base and chords, combined with a sufficiently rigorous calculation, will give it.

March, 1860. FRANCIS GALTON.

NUMBER OF PACES IN CHORD **OPPOSITE** TO THE REQUIRED SIDE, (TO A RADIUS OF 10 PACES).

NUMBER OF PACES IN CHORD **ADJACENT** TO THE REQUIRED SIDE, (TO A RADIUS OF 10 PACES).

		5				6				7				8				9			
		0	1/4	1/2	3/4	0	1/4	1/2	3/4	0	1/4	1/2	3/4	0	1/4	1/2	3/4	0	1/4	1/2	3/4
5 0	28° 58'	57	59	60	62	64	65	67	68	70	71	73	74	75	77	78	80	81	82	84	85
5 1/4	30 26	56	58	60	61	63	64	66	68	69	71	72	74	75	77	78	80	81	82	84	85
5 1/2	31 56	55	57	59	61	62	64	65	67	69	70	72	73	74	76	78	79	81	82	84	85
5 3/4	33 30	54	56	58	60	62	63	65	67	69	70	71	72	74	76	77	79	80	82	84	85
6 0	34 56	54	56	57	59	61	63	64	66	68	69	71	72	74	76	77	79	80	82	84	85
6 1/4	36 26	53	55	57	59	60	62	64	66	67	69	71	72	74	75	77	79	80	82	84	86
6 1/2	37 56	53	54	56	58	60	62	63	65	67	69	70	72	74	75	77	79	80	82	84	86
6 3/4	39 28	52	54	56	57	59	61	63	65	66	68	70	72	73	75	77	78	80	82	84	86
7 0	41 0	52	53	55	57	59	61	63	65	66	68	70	71	73	75	77	79	81	82	85	86
7 1/4	42 30	51	53	55	57	58	60	62	64	66	68	70	71	73	75	77	79	81	83	85	87
7 1/2	44 4	51	53	55	57	58	60	62	64	66	68	70	71	73	75	77	79	81	83	85	87
7 3/4	45 36	50	52	54	56	58	60	62	64	66	68	70	72	74	75	77	79	81	84	86	88
8 0	47 10	50	52	54	56	58	60	62	64	66	68	70	72	74	76	78	79	82	84	86	88
8 1/4	48 44	50	52	54	56	58	60	62	64	66	68	70	72	74	76	78	80	82	85	87	89
8 1/2	50 20	49	51	53	55	57	59	61	63	65	68	70	72	74	76	78	80	83	85	88	90
8 3/4	51 54	49	51	53	55	57	59	61	63	65	68	70	72	74	76	79	81	83	86	88	91
9 0	53 30	49	51	53	55	57	59	61	64	66	68	70	72	75	77	79	82	84	87	89	92
9 1/4	55 6	49	51	53	55	57	59	61	64	66	68	70	73	75	77	79	82	85	88	90	93
9 1/2	56 4	49	51	53	55	57	59	62	64	66	68	71	73	76	77	81	83	86	89	91	94
9 3/4	58 21	48	51	53	55	57	59	62	64	66	69	71	74	76	79	81	84	87	90	92	95
10 0	60 0	48	51	53	55	57	60	62	65	67	69	72	74	77	79	82	85	88	91	94	96
10 1/4	61 40	48	51	53	55	57	60	62	65	67	70	72	75	77	80	82	86	89	92	95	98
10 1/2	63 21	48	51	53	56	58	60	63	65	68	70	73	76	78	81	84	87	90	93	97	100
10 3/4	65 2	49	51	53	56	58	61	63	66	68	71	74	76	79	82	85	88	91	95	98	102
11 0	66 44	49	51	53	56	58	61	64	66	69	72	74	77	80	83	86	89	93	97	100	104
11 1/4	68 24	49	51	54	57	59	61	64	67	70	72	75	78	81	84	88	91	95	98	102	106
11 1/2	70 12	49	52	54	57	59	62	65	67	70	73	76	79	83	86	89	92	97	100	105	109
11 3/4	71 58	49	52	55	57	60	63	66	69	71	74	77	81	84	87	91	95	99	102	107	112
12 0	73 46	50	52	55	58	60	63	66	69	72	75	79	82	85	89	93	97	101	105	110	115
12 1/4	75 34	50	53	55	58	61	64	67	70	73	76	80	83	87	91	95	99	103	108	113	118
12 1/2	77 22	50	53	56	59	62	65	68	71	75	78	81	85	89	93	98	102	106	111	117	122
12 3/4	79 10	51	54	57	60	63	66	69	72	76	79	83	87	91	96	100	105	110	115	120	127
13 0	81 6	52	55	57	61	64	67	70	73	77	81	85	89	93	99	103	108	113	119	125	131
13 1/4	82 58	52	55	58	62	65	68	72	75	79	83	87	92	96	101	106	111	117	123	130	136
13 1/2	84 56	53	56	59	62	66	70	73	77	81	85	90	95	99	104	109	115	121	128	135	143
13 3/4	86 52	54	57	60	64	67	71	74	79	83	88	92	97	102	107	114	120	126	133	141	150
14 0	88 52	55	58	62	65	69	73	77	81	85	90	95	100	106	112	118	125	132	140	148	158
14 1/4	90 54	56	60	63	67	71	75	79	84	88	93	98	104	110	116	123	130	137	147	158	
14 1/2	92 56	57	61	65	69	73	77	81	86	91	97	102	108	114	122	129	137	145	156		
14 3/4	95 2	58	62	66	70	75	80	84	89	95	100	106	112	120	127	136	145	156			
15 0	97 10	60	64	68	72	77	82	87	93	99	105	110	118	126	135	143	155				
15 1/4	99 4	62	66	70	75	80	85	91	97	103	109	117	125	134	143	154					
15 1/2	101 36	64	67	73	78	83	89	95	102	108	116	123	138	141	154						
15 3/4	103 54	66	71	75	81	87	93	100	107	115	123	132	141								
16 0	106 16	69	74	79	85	90	98	105	113	121	131	140	151								
16 1/4	108 42	72	77	84	90	96	104	112	121	131	140	151									
16 1/2	111 12	76	82	88	95	103	110	120	130	141											

		10				11				12				13				14			
		0	1/4	1/2	3/4	0	1/4	1/2	3/4	0	1/4	1/2	3/4	0	1/4	1/2	3/4	0	1/4	1/2	3/4
5 0	28° 58'	87	88	89	91	92	94	95	97	98	100	101	103	105	107	109	110	113	115	118	120
5 1/4	30 26	87	88	90	91	93	94	96	97	99	101	102	104	106	108	110	112	114	117	120	122
5 1/2	31 56	87	88	90	91	93	95	96	98	100	102	103	105	107	109	112	114	116	119	122	124
5 3/4	33 30	87	88	90	92	93	95	97	98	100	102	104	106	108	111	113	116	118	121	124	127
6 0	34 56	87	88	90	92	94	96	97	99	101	103	105	108	110	113	115	118	120	124	126	130
6 1/4	36 26	87	89	91	92	94	96	98	100	102	105	107	109	111	114	117	120	122	126	129	133
6 1/2	37 56	87	90	91	93	95	97	99	101	103	106	108	111	113	116	119	122	125	128	132	136
6 3/4	39 28	88	90	92	94	95	98	100	102	104	107	110	113	115	118	121	124	127	131	135	140
7 0	41 0	88	90	92	94	96	99	101	103	106	108	111	114	117	120	123	127	130	134	139	144
7 1/4	42 30	88	91	93	95	97	100	102	104	107	110	113	116	119	122	125	129	133	137	142	148
7 1/2	44 4	89	91	94	96	98	101	103	106	109	112	114	118	121	125	128	132	136	141	146	152
7 3/4	45 36	90	92	94	97	99	102	104	107	110	114	116	120	123	127	131	135	140	145	150	
8 0	47 10	91	93	95	98	101	103	106	109	112	115	118	122	126	130	134	139	144	149	156	
8 1/4	48 44	91	94	96	99	102	105	108	111	114	117	121	125	129	133	137	142	148	153		
8 1/2	50 20	92	95	98	100	103	106	109	112	116	119	123	127	132	136	141	147	153			
8 3/4	51 54	93	96	99	102	105	108	110	115	118	122	126	131	135	140	145	152				
9 0	53 30	94	97	100	103	106	109	113	117	121	125	129	134	139	144	150					
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TABLE

FOR

MEASUREMENT OF THE BREADTHS OF RIVERS, AND OTHER KINDS OF ROUGH TRIANGULATION, WITHOUT "INSTRUMENTS," AND WITHOUT CALCULATION.

For the purposes of Travellers.

By FRANCIS GALTON.

BEING A SIMPLIFICATION OF A METHOD LATELY PROPOSED BY COLONEL EVEREST.

(For explanation of its use, see the other side.)

21/ Domestication of animals.

GALTON/2/13/3/20 p.1a

Brookside, Hale, Altrincham,
Dec. 18th, 1895.

Dear Sir,

Accept my sincere thanks for permitting me to see the pamphlet which I now return.

If you reprint it I shall hope to obtain a copy, but the extracts I have made are sufficient for my present purpose.

In case it is reprinted one slight error on p. 7, where "Samuel" is written for "Nathan", might be corrected.

Apologising for my mistake in addressing you, I am

Yours Sincerely

J. Crowther Hirst.

Francis Galton, Esq., F.R.S.

THE FIRST STEPS

TOWARDS

THE DOMESTICATION OF ANIMALS.

By FRANCIS GALTON, F.R.S.

Reprinted from the Transactions of the Ethnological Society.

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 Port 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 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 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.

5

COMPOSITE PORTRAITS.

BY

FRANCIS GALTON, F.R.S.

LONDON:
HARRISON AND SONS, ST. MARTIN'S LANE,
Printers in Ordinary to Her Majesty.

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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

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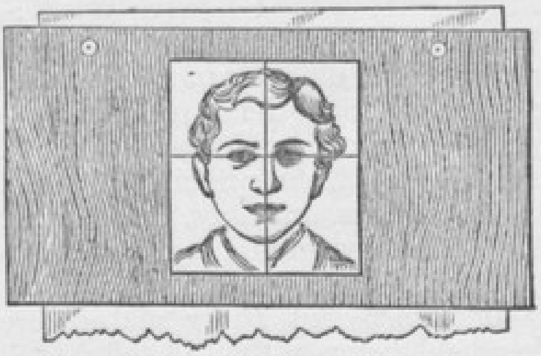
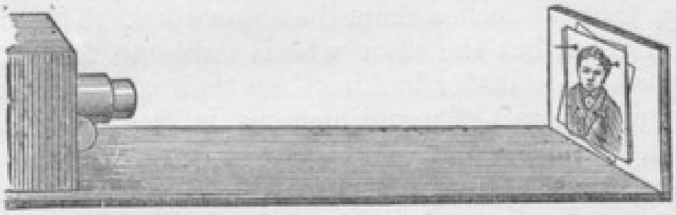
average features of any given group of men. These ideal faces have a surprising air of reality. Nobody who glanced at one of them for the first time, would doubt its being the likeness of a living person, yet, as I have said, it is no such thing; it is the portrait of a type and not of an individual.

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,

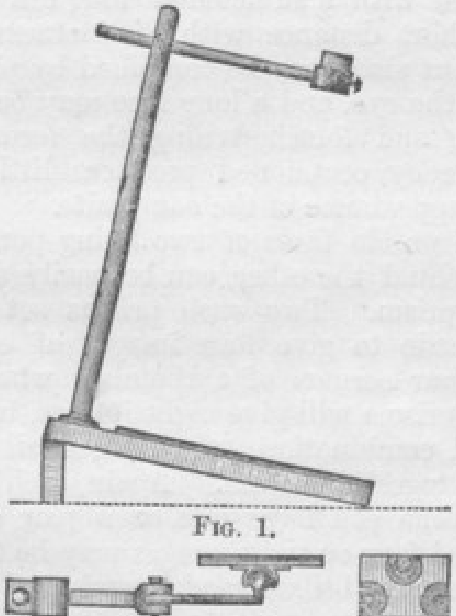


FIG. 1.

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.

Harrison and Sons, Printers in Ordinary to Her Majesty, St. Martin's Lane.

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For names & [illegible]

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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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

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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.

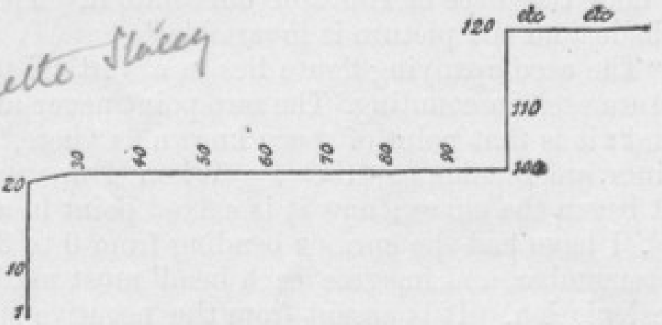
Last of all, I have taken advantage of the opportunity afforded by a meeting of this Society, to bring with me many gentlemen well known in the scientific world, who have this habit of seeing numerals in Forms, and whose diagrams are in the collection before you. Amongst them are Mr. G. Bidder, Q.C., 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.

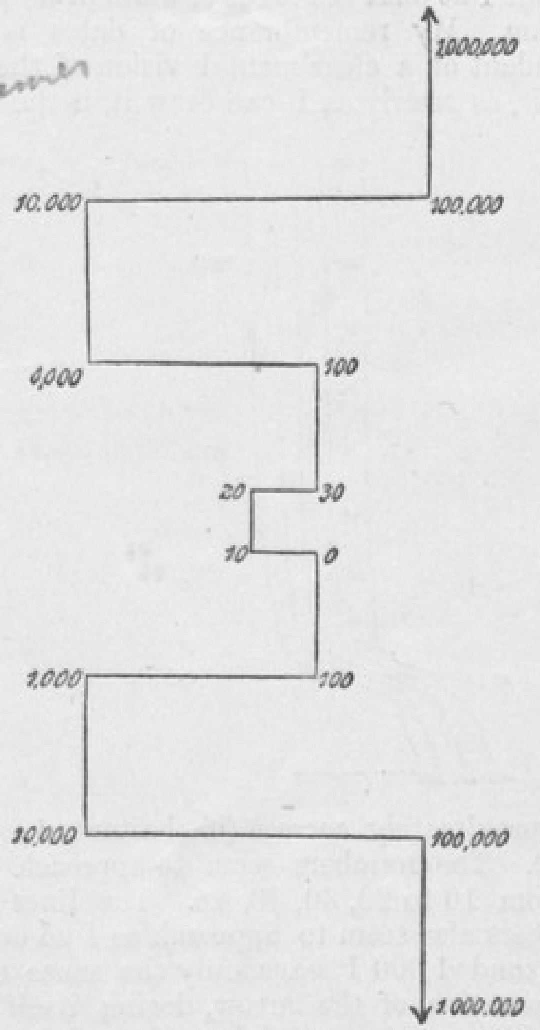
I give wood-cuts of some of these forms, and very brief descriptions of them extracted from the letters of my correspondents. (The wood-cuts have already appeared in "Nature."* Many other drawings on a smaller scale on two lithographed

J.S.
Jeanette Stacey



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.
Isabella J. Clemmer



* I am indebted to the courtesy of the publishers of "Nature" for the use of these woodcuts.

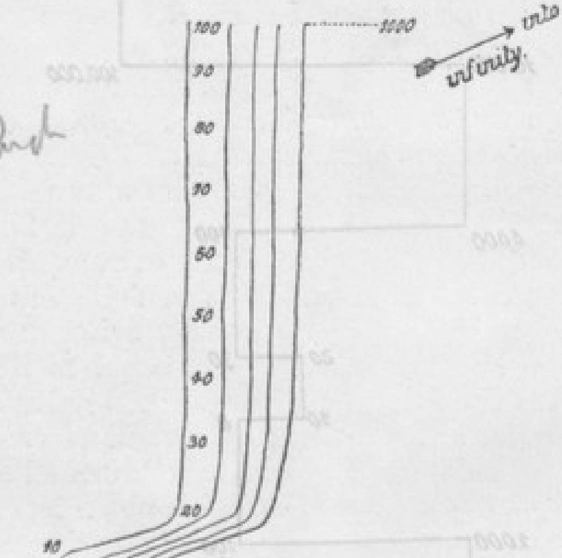
I.S. "The figures are about a quarter of an inch in length, and in ordinary type. They are black on a white ground. 200 generally take the place of 100 and obliterate it. There is no light or shade, and the picture is invariable."

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.

Theodore Mess
Communicated through
Dora Albright

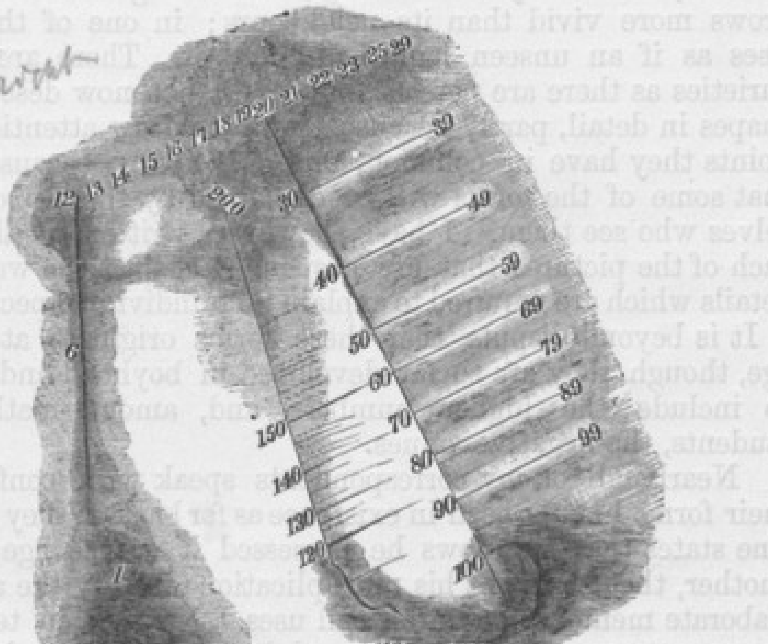


It is only approximately correct (if the term "correct" be at all applicable). The numbers seem to approach more closely as I ascend from 10 to 20, 30, 40, &c. The lines embracing a hundred numbers also seem to approach as I go on to 400, 500, to 1,000. Beyond 1,000 I have only the sense of an infinite line in the direction of the arrow, losing itself in darkness towards the millions. Any special number of thousands returns 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.

Dora Albright



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.

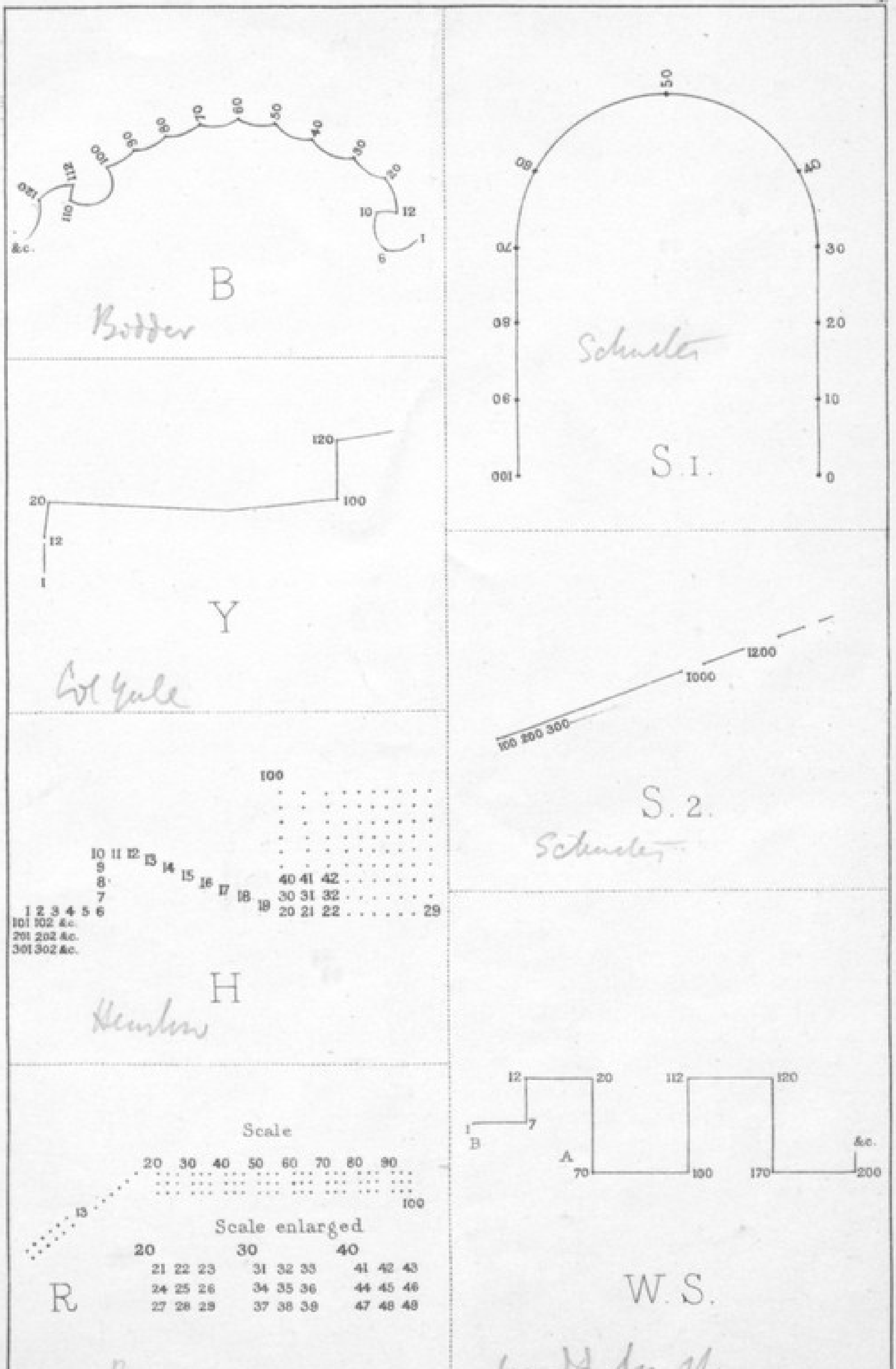
Brief Extracts from a few letters, with illustrations see Plate I (*the letter accompanying each illustration is the initial of the Correspondent.*)

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.

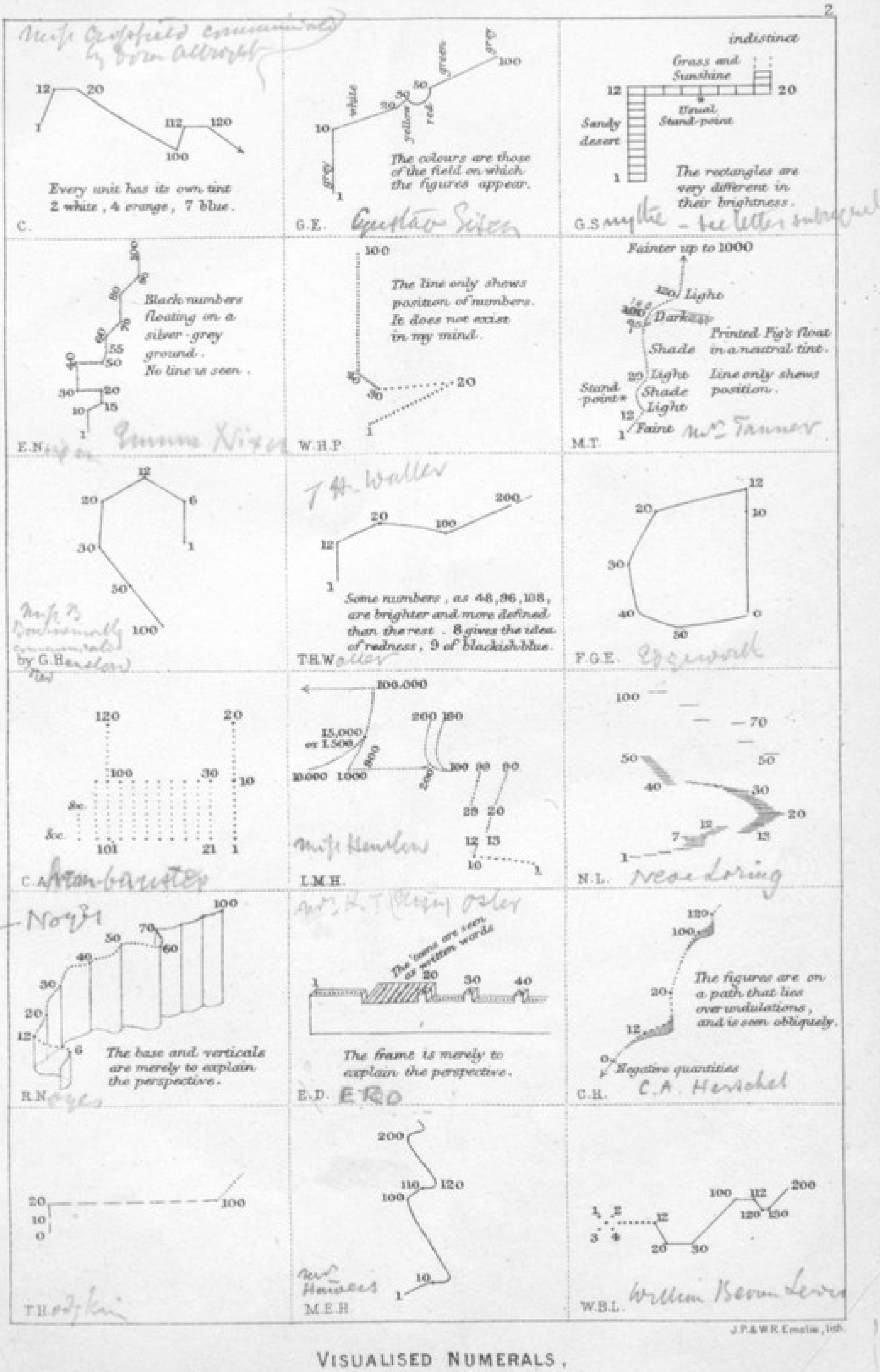
1



J.P. & W.R. Emslie, lith.

VISUALISED NUMERALS
BY FRANCIS GALTON, F.R.S.

writes letter that
he approves.



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.

Compare Lauder Brunton letter

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.

see Edgeworth

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.

Does it shew that children have had a visual number of mental lines of reference

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 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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For Nancy

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Uncorrected proof.]

On determining the heights and distances of Clouds by their reflexions in a low pool of water, and in a mercurial horizon. By FRANCIS GALTON, *M.A., F.R.S.*

The calm surface of a sheet of water may be made to serve the purpose of a huge mirror in a gigantic vertical range-finder, whereby a sufficiently large parallax may be obtained for the effective measurement of clouds. The observation of the heights and thicknesses of the different strata of clouds, and of their rates of movement, is at the present time perhaps the most promising, as it is the least explored branch of meteorology. As there are comparatively few places in England where the two conditions are found of a pool of water well screened from wind, and of a station situated many feet in height above it, the author hopes by the publication of this memoir to induce some qualified persons who have access to favourable stations, to interest themselves in the subject, and to make observations.

The necessary angles may be obtained with a sextant and mercurial horizon, but it is convenient, for reasons shortly to be explained, to have in addition a tripod stand, with a bar of wood across its top to support the mercurial trough, and some simple instrument for the rapid and rough measurement of altitudes. I have used the little pocket instrument sold by Casella, of Holborn Bars, London, called a 'pocket alt-azimuth,' and have employed Captain George's mercurial horizon on account of its steadiness and ease in manipulation.

The observer has to determine :—

1. The difference of level between the mercury and the pool of water (call it d).

2. The angle between the reflexions of a part of a cloud in the mercury and in the pool (call it p). This should be carefully measured.

3. The angle between the portion of the cloud and its reflexion in the *mercury* (call it $2\,a$). This may be roughly measured; its altitude a may most conveniently be taken at once by the pocket alt-azimuth or other instrument. The subjoined tables will then give the required result with great ease.

If p be not greater than 3°, and if n be the number of minutes of a degree in p, the error occasioned by writing $n \sin 1'$ for $\sin n'$, will never exceed six inches in a thousand feet, and may be disregarded. Other errors of similar unimportance, due to the eye not being close to the mercury, may also be ignored. Under these conditions, since $\log. \sin. 1' = 6{\cdot}46373$, it can be easily shown that

$$\text{distance of cloud} = \frac{d}{n} \times 6875{\cdot}5 \cos\,(a + p).$$

$$\text{vertical height of cloud} = \text{distance} \times \sin a.$$

The following table has been calculated for these values when $\frac{d}{n} = 1$. To use it, multiply the tabular numbers by d (the difference in feet between the level of the mercury and that of the pool) and divide by n (the number of minutes of a degree in the angle between the reflexion in the mercury and that in the pool). The result will be the distance, or height, as required in feet.

TABLE for calculating distances and height of clouds by their reflexions from a mercurial horizon, and from a pool of water at a lower level.

a = Altitude of cloud, (being half the sextant angle between the cloud and its reflexion as seen in the *mercury*, not pool).
p = Angle between the reflexion of the cloud in the mercury and that in the pool.
d = Vertical height of mercury above pool.
n = Number of minutes of a degree in the angle p.

Then the distances and heights of clouds = tabular numbers $\times \frac{d}{n}$

$a + p$	Distance from Observer	Vertical Height of Cloud above Observer			
		$n=0$ (or $p=0°$)	$n=60$ (or $p=1°$)	$n=120$ (or $p=2°$)	$n=180$ (or $p=3°$)
10°	6771	1176	1059	942	825
15°	6641	1719	1607	1494	1381
20°	6461	2210	2103	1997	1889
25°	6231	2633	2534	2435	2334
30°	5954	2977	2886	2795	2703
35°	5632	3230	3149	3067	2985
40°	5267	3386	3314	3243	3170
45°	4862	3438	3377	3316	3253
50°	4419	3386	3335	3284	3232
55°	3944	3230	3198	3150	3108
60°	3438	2977	2947	2915	2883
65°	2906	2633	2612	2597	2566
70°	2352	2210	2195	2180	2165

The observation of the angle between the two reflexions is perfectly easy with a full-sized sextant, if the trough of mercury be so propped up that the reflexion from the pool can be viewed *underneath* the trough. For this purpose I use a tripod stand with a bar of rough wood, say 18 inches long, 3 wide, and 2 thick, secured horizontally across its top. I lay the mercurial horizon on one of its projecting ends, and between a few studs that have been driven in the bar to prevent its accidentally slipping off. The edge of the bar is bevelled, and its thickness is reduced at the place where the mercury trough is set. Then the observation is taken, just as any other sextant observation would be. The reflexion from the mercury falls upon the index-glass, and that from the pool is viewed directly through the object-glass below the trough and its supporting bar.

Unless the sextant be a full-sized one, this operation cannot be effected, because the index-glass will not stand high enough above the line of sight to catch the reflexion from the mercury. It will simply reflect the side of the trough.

If there be no tripod stand, and it becomes necessary to lay the trough on the ground, an observation can still be made, but in an inconvenient fashion. The sextant will have to be held topsy-turvy, that the brighter reflexion of the cloud from the mercury, and not the feebler one from the pool, should be that which falls on its index-glass. The angle read will be negative; it will be what is commonly called an 'off' angle. A small sextant may be used in this manner, because the rim of the trough is narrow that intervenes between the further edge of the mercury and the objects seen beyond and over it.

The most convenient method of measuring the rate of movement of clouds, after the height of the cloud plane has been once determined, is to watch the movements of a patch nearly overhead, and passing away from the zenith, as seen reflected in the mercury, and measuring its angle of depression (= its altitude) with some simple and suitable instrument, such as the pocket alt-azimuth already mentioned. Two measurements, a_1 and a_2 are taken, as well as the intervening time, t seconds, whence we obtain rate of movement = height of cloud × (cotan α_2 − cotan α_1 in seconds.

When the water is almost wholly calm, I find that 2′ of error is the utmost that need be feared. If quite calm 1′ would be ample to make allowance for in a set of three or four observations. Now suppose we wish that our determination shall never be more than, say, 10 per cent. in error, we can easily find from the tables what the minimum height of the station must be in any given case, to secure this result. In the first instance we should require a parallax of 10′ and in the second of 20′. This is obtained by an elevation of 10 or 20 feet as the case may be, when the height of the clouds in feet corresponds to the tabular numbers; that is, when it is between 2000 and 3000 feet. At 100 or 200 feet elevation, clouds of ten times that height could be observed with equal accuracy. Numerous stations exist whence mountain tarns can be seen lying at a much lower level than this, and where even the highest cirrus could be measured with satisfactory precision.

Useful regular work might be done by a meteorologist whose station was at a height of even 50 feet above a pool, supposing it to be so well sheltered from the wind as to frequently afford perfectly good reflexions with, say, 1′ maximum error. Very shallow water is much stiller than deep water, as waves cannot be propagated over it; thus we may often see wonderfully good reflexions in road-side splashes and puddles, in the intervals between puffs of wind. The most stagnant air is in the middle of a high and broad plantation, where there is also plenty of dense underwood. Detached puddles of water in broad ruts would be the best equivalent for a pool. As regards the size of the pool, if we let fall a perpendicular k from the mercury trough to the level of the water, the utmost portion of the surface of the pool that can be used with effect extends between the distances of about $\frac{1}{2}k$ and $4k$ from the base of the perpendicular. The angles of depression would be then from 64° to 14° about, or say, a range of 50°. The usual limits would be from k to $3k$, or between 45° and 18°, being a range of 27°.

A 3

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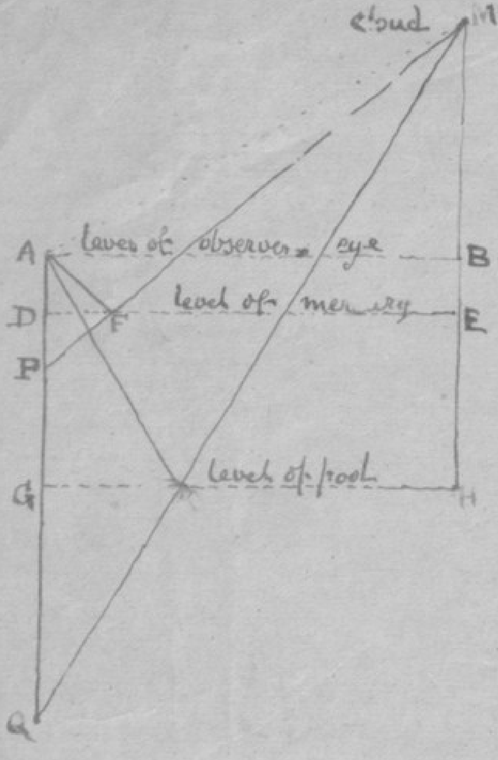
f.3r 41 a

If a = difference of level of the mercury and the pool
n = number of minutes of a degree in the angle between the two reflexions
then Distance $d = \frac{a}{n} \times$ tabular numbers.

Angle of depression of ray reflected from the mercury	Distance from Observer	Height above Observer	Horizontal distance from Observer
5°	6849	597	6823
10	6771	1176	6668
15	6641	1719	6415
20	6461	2210	6071
25	6231	2633	5647
30	5954	2977	5157
35	5632	3230	4614
40	5257	3386	4036
45	4862	3438	3438
50	4419	3386	2841
55	3944	3230	2262
60	3438	2977	1719
65	2906	2633	1228
70	2352	2210	804
75	1775	1714	459
80	1193	1176	207
85	599	597	52

f.3v



Given:- DG, FAK, and BAF

$FAK = FAG - KAG = FMH - KMH$
$= FMK$ (or PMQ).

$QPM = 90^\circ + MFE = 90^\circ + BAF$.

$PQM = 180^\circ - \{(90^\circ + BAF) + PMQ\}$
$= 90^\circ - \{BAF + FAK\}$

$PQ = 2AG - AP = 2\{AG - AD\} = 2DG$.

$$\frac{PM}{\sin PQM} = \frac{PQ}{\sin PMQ}, \quad PM = 2DG \frac{\cos(BAF + FAK)}{\sin FAK}.$$

let n = number of minutes of a degree in FAK
then as FAK is small we may write $n \sin 1'$ for $\sin FAK$
and $\cos BAF$ for $\cos\{BAF + FAK\}$

PM (which is very nearly = AM) $= \frac{DG}{n} \cdot \frac{2}{\sin 1'} \cdot \cos BAF$

height of cloud $= PM \cdot \sin BAF$

horizontal dist^n^ of cloud $= PM \cdot \cos BAF$.

In the tabular values, DG and n are each taken $= 1$.

41 b

Proof.]

The Equipment of Exploring Expeditions Now and Fifty Years Ago.
By Francis Galton, *F.R.S.*

The equipment of a modern exploring expedition differs in many respects now from what it was in or about the year 1830, with the general result of increased efficiency and rapidity of execution. The standard instruments—namely, the theodolite, the sextant, the chronometer, and the azimuth compass—have not received any great improvements in the interval, and the best of those made in 1830 would be valued now. But they are made more handy and portable than they were, and at much lower cost for equal degrees of excellence. The modern water-tight cover, with the keyless winding arrangement of travellers' watches, is a great boon to them. The mercurial horizon, without which the sextant on land is almost useless for astronomical purposes, has been transformed from a lumbering trough shielded from the wind by a heavy glazed screen, which was difficult to fill and did not admit of the observation of low angles, into a very compact contrivance by Captain George, which is filled by tilting, and is sheltered from the wind by a piece of glass floating on the mercury. The liability to errors introduced by this arrangement is much smaller than might have been expected, and travellers speak highly of its merits. The appliances for measuring elevation above the sea level have been greatly improved. The old method was to carry a mountain barometer, which, from the weight of the mercury and the fragility of the glass that contained it, was rarely carried far without breakage. Since then the aneroid has been invented, and the appliances connected with the boiling-point thermometer have been greatly improved. A traveller provided with these very portable instruments can use the aneroid for everyday purposes, checking its change of index error from time to time by boiling-point observations. Even the mercurial barometer has been rendered a comparatively portable instrument. The tubes are packed empty, and they are filled when required by Captain George's method, which in moderately careful hands is found to give good results. The enclosed thermometer for deep-sea observations is a recently invented instrument absolutely essential to accuracy.

The art of exploring ocean depths and performing what has been called Thallasography, has been immensely improved, owing to the requirements of submarine telegraphy and of such scientific expeditions as that of the 'Challenger.' Sir W. Thomson's method of sounding at the depth of many fathoms without checking the ship's course is in full use, but Dr. Siemens' bathometer has not yet been made practically serviceable.

The accuracy of thermometric graduation has been greatly increased by the verifications afforded by the Kew Observatory, which is the child of the British Association, established, and for a long time maintained, by a yearly grant from its funds, but now supported by the endowment of Mr. Gassiot. The errors in thermometers occasionally furnished even by the best makers in 1830, were such as would not be tolerated now. The verifications of Kew are extended to other instruments, and the influence of the observatory for good is firmly established and appears to be yearly increasing. Of the other appliances for geographical travellers—such as scales for plotting, metal pens which were invented since 1830, and that admirable recent contrivance the stylographic pen—it is unnecessary to speak in detail. The binocular opera-glass is, practically speaking, a new instrument, and its merits as a night-glass were first found out long after 1830. The lunar tables of the 'Nautical Almanack' have been greatly improved of late years, for in 1830 their predictions of the place of the moon could not have been trusted as they now are for delicate determinations of longitude. Lastly, the means of instruction in the use of geographical instruments is at length afforded by the Geographical Society, who have erected a small observatory on the roof of their premises, where instruction is given on moderate terms to intending travellers.

The modern equipment of travellers as regards dress has been greatly improved by the general use of flannel, which is a most important preservative of health, but was neglected half a century ago. Thus, at much more recent times than 1830, the hardy Swiss guides had a horror of what they called a *coup-d'air,* or a chill on the mountain top, when they were hot and perspiring; and no wonder, as they then all wore linen shirts next the skin. The modern loose form of dress, the shooting boots and easy overcoat, are a vast improvement on the pinched costumes of 1830. The derivation of the word paletôt conveys a history. The first warm and convenient coats used in England were reproductions long subsequent to 1830 of those used by sailors in rough weather under the name of 'pilot coats' (and were sometimes, for the sake of shortness, called 'p. coats' or 'pea-coats'). They quickly became the fashion, were copied and made more elegantly by the French, who adapted our name of 'pilot coat' to their own pronunciation of paletôt, and so we received it back from them. India-rubber and gutta-percha adaptations to articles of dress and manufactures generally date from a little subsequent to 1830; they are invaluable for many purposes to exploring expeditions. The form of tent has been greatly improved. Portable mackintosh and other boats are comparatively recent contrivances, and have done good service. Lucifer-matches had been invented, but only very recently, in 1830.

The equipment of a travelling party as regards packsaddles has been improved, chiefly through Australian experiences, where, moreover, the camel has been introduced as a beast of burden, with more success than the tamed elephant in Africa. The art of sledge travelling has been vastly improved by the skilful cutting down of all superfluous weight, enabling travellers to drag more food, and so to journey a larger number of days from their depôts.

As regards food, the tinned meats, compressed vegetables, and condensed milk, which are invaluable during the first days of travel before the expedition has settled into regular ways, are all late inventions, and the merits of lime-juice are now far better understood than they were fifty years ago.

The *personnel* of a travelling party is decidedly improved. Whatever may be the state of the physique of the lower orders of the population, there can be no doubt that the upper orders are physically better developed than they were. They are, as I have good reason to believe, in the absence of direct measurements, taller; they achieve greater feats in running, leaping, walking, and other athletic performances than their grandfathers did. They lead healthier lives from the discontinuance of the heavy eating and hard drinking of old days, from the better aired sleeping rooms, the existence of proper means of washing, and the seaside or Continental summer vacation.

The greatest benefit of all to travellers is the modern rapidity and ease with which distant parts of the world are now reached. In 1830 it required 70 days sailing from England to reach the Cape of Good Hope, 120 days (in the SW. monsoon) to reach Bombay, and 130 days to Sydney. It was 40 days' sail to New York, 42 to Jamaica, 56 to Rio, and 110 to Valparaiso. The length of time that the post now takes from London to these places is as follows: Cape Town 21 days, Bombay 18 days, Sydney 43 days, New York 10 days, Jamaica 18 days, Rio 21 days, Valparaiso 39 days; the average increase of speed being more than three-fold. There is scarcely any important part of the world that cannot now be reached in two months from London; even the Antipodes are only 7 weeks' journey. This facility of communication is accompanied by a corresponding spread of commerce, and travellers can now easily refit themselves at distant points. It has recently occurred to the Geographical Society to have had to meet bills drawn upon her Majesty's consul at Zanzibar by a traveller in their employ, for which he had been furnished with goods by Arab traders at Nyangwé on the Upper Congo, as well as at places in Central Africa which had never before been visited by a white man.

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57/ Family likeness in Stature
Corrected Copy
reproduce Diagram
p. 55 K.P

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FROM THE

PROCEEDINGS OF THE ROYAL SOCIETY.

815.

[*From the* PROCEEDINGS OF THE ROYAL SOCIETY, No. 242, 1886.]

"Family Likeness in Stature." By FRANCIS GALTON, F.R.S. With an Appendix by J. D. HAMILTON DICKSON, Fellow and Tutor of St. Peter's College, Cambridge. Received January 1, 1886.

I propose to express by formulæ the relations ~~that subsists~~ between the statures of specified men and those of their kinsmen in any given degree, and to explain the processes through which ~~family peculiarities of stature gradually diminish, until~~ in every remote degree of kinship ~~the~~ groups of kinsmen become undistinguishable from ~~a~~ groups of men selected out of the general population at random. ~~I shall determine~~ The constants in my formulæ ~~referring to kinship~~ are determined with a useful

b

degree of precision, and ~~These constants~~ may provisionally and with some reservation be held applicable to other human ~~peculiarities~~ qualities, than stature, ~~while~~ The formulæ themselves are, I presume, applicable to every one-dimensioned ~~faculty~~ quality that all men possess in some degree, but ~~that~~ different men ~~possess~~ in different degrees.

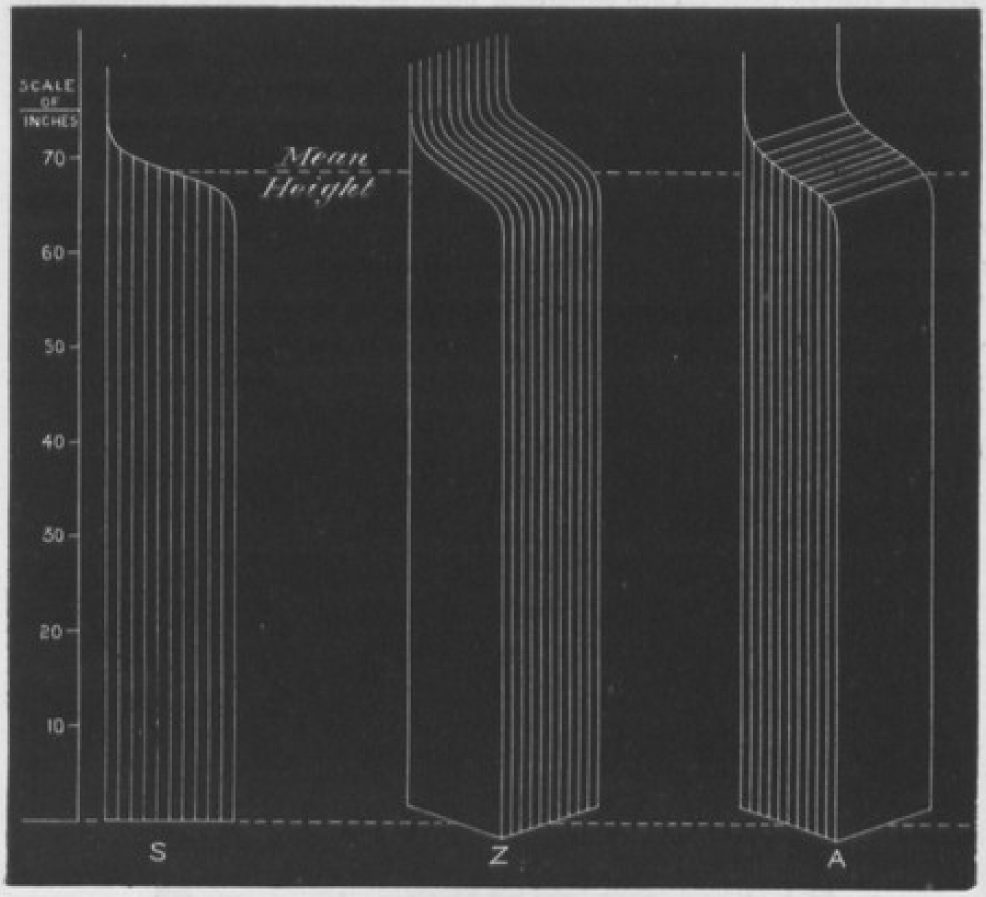
I selected stature for the subject of this inquiry, for because of reasons fully set forth in two recent publications,* which dealt with one small portion of the ground covered by the present memoir, and from which it will be convenient that I should make as I proceed occasional short extracts, in order to complete the present argument and to save cross-reference. The reasons that combine to render stature an excellent subject for hereditary inquiry are, briefly, the ease and frequency of its measurement, its constancy during adult life, its inconsiderable influence on the death-rate, its dependence on a multiplicity of separate elements, and other points that I shall dwell on as I proceed, namely, the ease with which female statures are transmuted to their male equivalents, and so enabled to be treated on equal terms with male statures, the tendency of the parental statures to blend in inheritance, and the disregard of stature in marriage selection.

Such from Regression towards mediocrity p. 249 & p. 252

Stature-schemes.—It is an axiom of statistics that large samples taken at random out of the same drawn groups population at random are statistically similar, and in such inquiries as these present which do not aim at minute accuracy, they may be considered identical. Thus the statures in every group, say of 1000 male adults, when distributed in order of their magnitudes at equal distances apart and in a row, will form almost identical figures; it being only towards either end of the long row that irregularities will begin to show themselves. These are unimportant ~~in~~ the present inquiry which deals with the bulk of the population and I disregard them. The Diagram S, fig. 1, shows the outline of such a group of statures. It is drawn to scale, each of the statures being supposed to have been represented by a vertical line of proportionate length, standing on a horizontal base, the lines being at equal distances apart, and the whole system being compressed into the space between two termini, which ~~may be~~ are set at ~~any~~ a convenient distance asunder. The vertical lines in the figure ~~do not indicate these statures, but they~~ are divisions, ten in number, between each of which 100 stature lines are supposed to be compressed. The first and last stature will not touch the termini, but will be removed from them by a half-interval. As it will be convenient to assign a name to this figure, I ~~will~~ call it a

* (1.) "Presidential Address to the Anthropological Section of the British Association in 1885." (2.) "Regression towards Mediocrity in Hereditary Stature." "Journ. Anthrop. Institute," 1885, p 246. The latter is a reprint of that portion of the former with which I am now concerned, together with some additional matter; it contains tables and diagrams, and should be referred to in preference.

Fig. 1.



× Definition

"stature-scheme." The numerous cases near mediocrity that differ little from one another, cause the middle portion of the curved upper boundary of the stature-scheme to ~~assume~~ be a gentle, ~~slope~~, which increases rapidly towards either end, where the increasing rareness of more and more exceptional cases causes ~~that~~ the boundary ~~line~~ of the stature scheme to slope upwards, ~~as an asymptote to one of the termini, and downwards as an asymptote to the other.~~

Now suppose that instead of compressing 1000 statures between the termini, I had compressed 1000 × 1000, or a million of them, the stature-scheme would be unaltered, except that such small irregularities as might have been previously seen would become smoothed. The height of the middlemost or med~~ian~~ stature-line would remain the same as before, and so would the heights of the lines standing at each quarter, each tenth, and at every other ~~proportionate~~ distance between the termini. // ~~Or again,~~ instead of arranging the lines in a single scheme, we ~~might~~ arrange them in a thousand schemes, which as we have seen, would be practically identical in shape, and we ~~may~~ place thes~~e~~ ~~schemes~~ side by side, as is done in Z, fig. 1, ~~forming~~ a

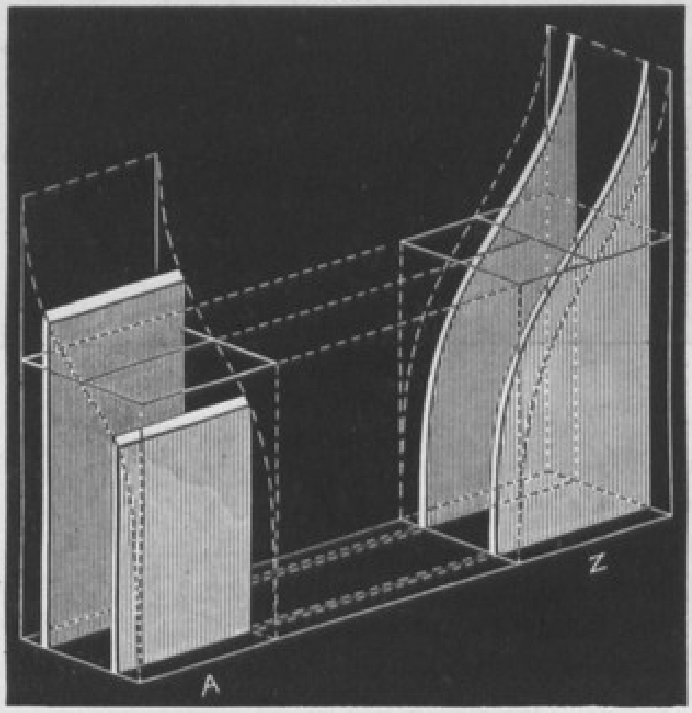
b 2 they would form

The composition of our squadron may be viewed in two ways, either as in Z where it is

"squadron" numbering 1000 statures each way, the whole standing upon a square base. ~~Our squadron may be considered as~~ made up of *ranks* (parallel to the plane of *zx*) ~~as in Z~~, or of *files* (parallel to the plane of *zy*) as in A. The ranks, ~~as we have seen~~, are ~~all similar~~ stature-schemes; the files are ~~all~~ rectangles ~~which have the same breadths~~ but are of dissimilar heights.

It is now easy to give a general idea, ~~to be developed as we proceed~~, of the way ~~in which~~ any large sample of a population ~~gives rise to a group of distant~~ kinsmen in any given degree, ~~who are statistically (in all respects except numbers) undistinguishable as regards their statures from themselves~~. I ~~must~~ suppose for convenience of ~~explanation~~, that tall, short, and mediocre men are equally fertile ~~(which~~ is not, however, strictly the case, the tall ~~being~~ somewhat less fertile than the short*), and then on referring to fig. 2, the

FIG. 2.



fortunes of the distant descendants of two of the rectangular files of squadron A will be seen traced.

~~As the number of kinsmen, in any remote degree we please to specify, of the men in each of the two files is about the same; I take 1000 of them in each case.~~ Again, as the stature-schemes of ~~those~~ kinsmen are identical with those of ~~equal numbers of men taken at random, as~~ samples of the general population, it follows that they

* Oddly enough, the shortest couple on my list have the largest family, namely, sixteen children, of whom fourteen were measured.

of the men corresponding to any one of the rectangular files, shown in Fig 2 A

~~will be identical with one another~~. Every ~~other~~ rectangular file in A being similarly represented, a complete squadron Z of the kinsmen of A will be ~~is~~ produced. ~~It is obvious, then, that~~ The squadrons A and Z are identical (in shape) that ~~and~~ as the ranks of Z have proceeded from the files of A, ~~the result is that~~ the two squadrons will stand at right angles to one another. (Z is identical with A after it has been rotated through 90 degrees) The upper surface of A was curved in rank, but was horizontal in file; that of Z will be ~~is~~ curved in file, but ~~is~~ horizontal in rank.

Kinsmen in any ~~near~~ degree~~s~~ are represented by squadrons of intermediate form. The~~se~~ squadrons will not have lost the whole of the curvature in rank (that is peculiar to) ~~of~~ A, nor will they have acquired the whole of the curvature in file (that is peculiar to) ~~of~~ Z. Consequently they will be curved moderately in both ways.* Also ~~it will be found that~~ the intersection of their surfaces by the horizontal plane of median height forms in each case an approximately straight line that assumes different and increasing inclinations, in the successive squadrons of intermediate shape between A and Z. These lines are indicated by straight lines on the squares below the squadrons in fig. 4, which represent the square bases upon which the ~~squadrons stand.~~

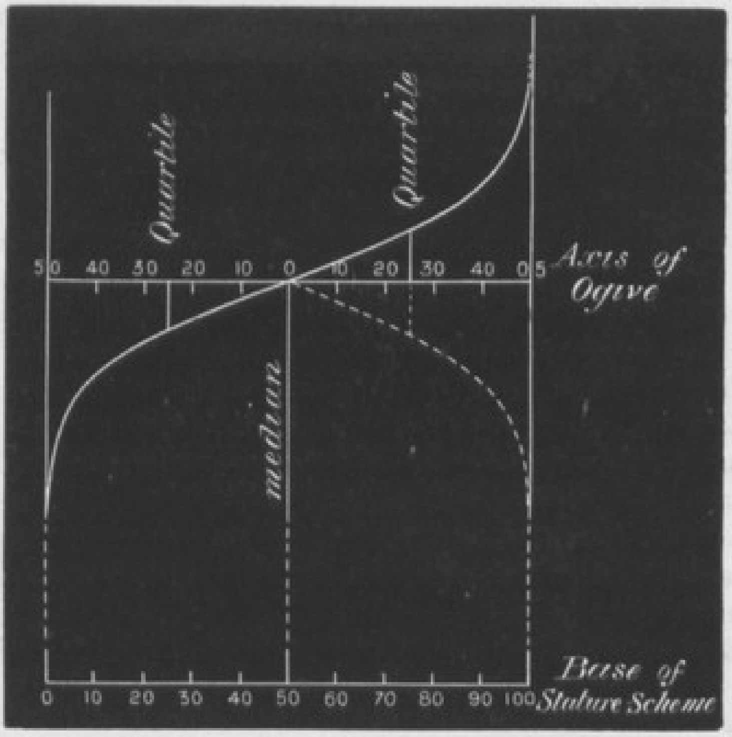
they will be supposed on the same ~~rotated~~ through some angle less than 90 degrees

I ~~shall now~~ show how these curves in rank and file should be treated, ~~But~~ Before ~~doing so~~, it is necessary to remark that female adult stature (I speak throughout of adults) may be safely transmuted to its male equivalent by multiplying it by a constant constant, which as regards my data is 1·08. After this has been done, the transmuted female statures may be treated on equal terms with the male statures, and the word "men" or other masculine term will include both sexes, unless otherwise stated distinctly. This procedure (supposed to have been in every case) ~~is~~ adopted in the ~~present memoir.~~ following calculations.

It is now generally recognised that the statures in every ordinary population are distributed in approximate conformity with what might have been inferred, if it were known that their variations were governed by such conditions as those upon which the exponential law of frequency of error is based. Therefore the upper boundary of the stature-scheme is approximately a curve (I have call it an "ogive") that admits of mathematical expression. The abscissæ of the normal ogive (fig. 3) are values of the probability integral $\frac{1}{\sqrt{\pi}}\int_0^t e^{-t^2}dt$, and the ordinates are the corresponding values of t. These are given in column A of Table I. Column B contains the same values divided by 0·477, by which means they are expressed in units of the probable error. I find it convenient to call the ordinates to an ogive (drawn

* A plaster model of one of these intermediate forms was exhibited at the meeting by Mr. J D. H. Dickson, who stated that his recent mathematical investigation of the properties of their surfaces, had shown that no strictly straight line could be drawn upon them. —F. G.

FIG. 3.



Their abcissæ are measured along the axis as in Fig 3, starting from the middle of the ogive as 0 the two terminals being at ± 50. It is also convenient to

from its axis) by the name of "deviates," ~~and~~ to describe either of the two symmetrical deviates of the normal ogive that stand at ± 25° by the name of "quartile deviate," or, more briefly, "quartile." I ~~also~~ give this name to the mean length of the upper and lower quartile, In those ogives which are drawn from observed data, and which are not strictly symmetrical, The numerical value of the quartile is identical with that of the well-known but here inappropriate term of "probable error."

Construction of Stature-Schemes and of Ogives from Observations.— The method of drawing an ogive from observations of stature is as follows. The observations (see Tables III, IV, and V, and compare with VI and VII) are sorted into grades, such as ". . . cases of 60 inches and under 61," ". . . cases of 61 inches and under 62," &c. If we are constructing a stature-scheme, or if we only desire to obtain the median value of the series, we have to deal ~~consider~~ these values of inches, but if we only wish to ~~in~~ construct ~~ing no more than~~ an ogive, which is only the upper boundary of a stature-scheme, it suffices to treat ~~consider~~ them as successive grades differing by ~~of~~ 1 inch each, In this case ~~and~~ I reckon the first grade not as 0, but as 1, as ~~This~~ has been done in column A, Table VI, for the sake of treating different groups on a uniform plan. The number of cases in each these grades are then summed from the beginning, and the sums up to each grade inclusive, are ~~is~~ written down, as shown

In speaking of class-places it is most convenient to use a different division such as is shown at the base of the stature scheme Fig 3. Here the abcissæ run from 0 to 100, the former ~~[illegible]~~ at the terminal on the side of the scheme where the statures are shortest. In this chapter I have no occasion to speak of class-places after the next paragraph.

on the supposition that

in column B in Table VI. The percentage values of these, ~~taking~~ the total number of observations is 100, are written in column C. A series is there obtained which shows how many per cent. of the statures fall short of the parting value that separates each pair of adjacent grades. Thus if n per cent. of the statures fall within the first r grades, that is to say, are less than the value of the rth parting line, then $100-n$ per cent. of them will exceed that value. Consequently, if the observations are read off and recorded to the utmost nicety, r will be the value of the ordinate representing the stature which has to be erected on a base line at n per cent. of its length from one of its ends. ~~In short,~~ A base line of any convenient length ~~has~~ is to be divided into 100 parts, and an ordinate of a length proportionate to r erected at the division n. But As observations are never read off ~~and recorded~~ with perfect accuracy, a correction has ~~have~~ to be applied ~~according to the circumstances of the particular case~~, whenever we are drawing a stature-scheme, but it is not necessary when we are drawing ~~and not~~ merely an ogive. This is as follows: If the records are kept to the nearest mth part of an inch, the phrase "exceeding r inches" would really mean exceeding ~~$r+1/m$~~ $r-\frac{1}{m}$ inches." This then is the true parting value corresponding to the nominal r. ~~In drawing ogives, and not stature-schemes, this correction may of course be disregarded~~. Having erected ordinates corresponding to each value of r, their tops are connected by straight lines forming a polygonal boundary that approximates to the curvature of an ogive, and would become one if it were corrected with a free hand, or otherwise smoothed. The centre of the ogive lies at the intersection of the curve with the ordinate drawn from the base at the fiftieth ~~division~~ class-place, and the horizontal axis of the ogive runs through that point of intersection (see fig. 3).

A half-ogive, whose ordinates are the mean lengths of the symmetrically disposed ordinates of the complete ogive, is constructed on the same general principles, but more simply, because the base from which it is plotted coincides with the axis of the ogive, and the graduations run alike, viz., from 0° to 50°, the signs + ~~and~~ - being omitted.

In Table VII, the entries in the first lines of each of the three groups it contains, are ~~the lengths of the ordinates that have been measured from the bases of ogives~~ values graphically obtained ~~constructed~~ from the data in Table VI. The abscissæ corresponding to the measured ordinates, are in every case the same fractional lengths of the bases. The entries in the second lines are the differences between these several ordinates and the median ordinate; they are, therefore, the deviates. The entries in the third lines are the negative deviates written under the corresponding positive ones. The entries in the fourth lines are the means of the values of the positive and negative deviates, disregarding their signs.

re-written

Comparison of Ogives.—The ogive being drawn according to the observations, its axis is divided into 100 parts, the fiftieth division being reckoned as 0°, then the deviates standing at the ± graduations of 10°, 20°, 25°, 30°, 40°, and 45° are measured. The mean of each pair of lengths, not regarding signs, has then to be divided by the mean lengths of the deviates at ± 25°, that is by the quartile deviate, and so is made to yield a series that is directly comparable with column B in Table I. The closeness with which ~~it~~ the series of observed values conforms to th~~at~~e standard series, is the test of the closeness with which the observations conform to the law of frequency of error.

Table II effects this comparison for all the series that I ~~have to~~ deal with in the present paper. The values in the Table are entirely unsmoothed, except in two named instances, being taken from measurements made to the above-mentioned polygonal boundary. I thought it best to give these interpolated values in this, their rudest form, leaving it to be understood that with ~~perfectly~~ some legitimate correction the accordance would become still closer. I do not carry the comparison beyond 45°, partly because my cases are not numerous enough to admit of a fair comparison being made in those extreme instances, where too adjacent class-places differ sensibly, and chiefly because I am well aware that conformity with the normal series is not to be expected towards the end of any observed series. I am content to deal with nine-tenths of the observations, namely, those between 0° and 45° with the half ogive, and to pay little heed to the remaining tenth, between 45° and 50°. It will be seen that the conformity of more than one half of each series is closer than to the first decimal place, and that in absolute measurement it is closer than to one-tenth of an inch.

Arithmetic and Geometric Means.—I use throughout this inquiry the ordinary law of frequency of error, which being based on the assumption of entire ignorance of the conditions of variability, necessarily proceeds on the hypothesis that *plus* and *minus* deviations of equal amounts are equally probable. In the present subject of discussion our ignorance is not so complete; there ~~is good reason~~ are grounds to suppose that those *plus* and *minus* deviations, of which the probability is equal, are so connected together that the ratio (and not the difference) between the lower observed measurement and the truth is equal to that between the truth and the upper observed measurement. My reasons for this supposition were explained some years ago, and were accompanied by a memoir by Mr. Donald Macalister, showing how the law of frequency of error would be modified if based on the geometric, instead of on the arithmetic mean.* Though in the present instance the former process is undoubtedly the more correct of the two, the smallness of the error ~~here~~ introduced by using the well known law, is so insignificant ~~that it is~~ as not to be worth regarding. Thus the mean stature of the population is about 68·3 inches, and the quartile of the stature-scheme (the probable error) is 1·7 inch, or only about one-fortieth of its amount, and the

* "Proc. Roy. Soc.," vol. 29 (1879), pp. 365, 367.

The logarithms of the observed values are dealt with, instead of the actual values, and the results when they have been re-transmuted, are those that it is desired to obtain. ~~as the~~

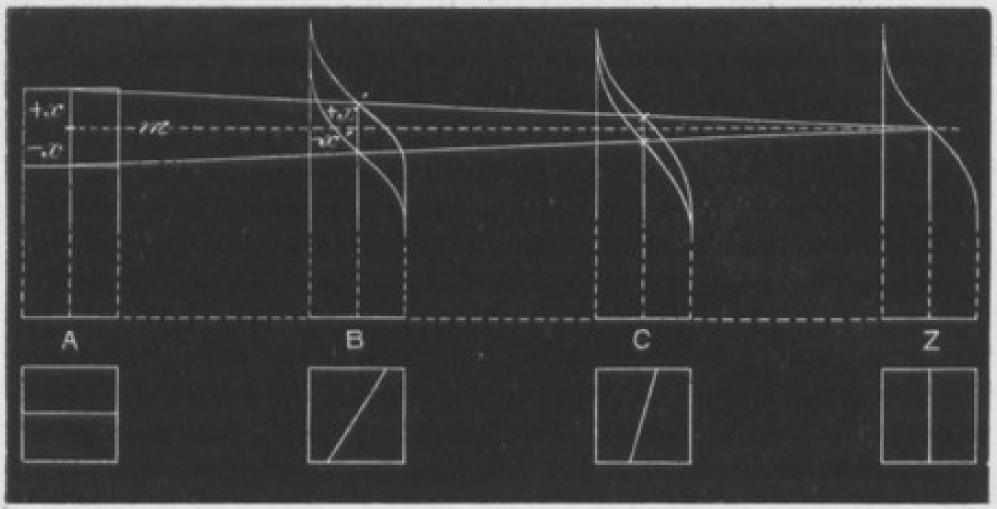
If the average stature of the population is P and the stature of a particular person is P ± x, and if that of a particular unknown kinsman of his in any degree with him found to have the stature P ± x'

necessarily

difference between $40^2/39$ and 41 is that between about 41·025 and 41·000, or only about 6 per thousand.

Regression.—It is a universal rule that the unknown kinsman in any degree of any specified ~~man~~ person, is probably more mediocre than ~~he~~ that person. Let the relationship be what it may, it is safe to wager that ~~the~~ unknown kinsman ~~of a person whose stature is $68\frac{1}{4} \pm x$ inches~~, is of some height ~~$68\frac{1}{4}$~~ P $\pm x'$ inches, where x' is less than x. The reason of this can be shown to be due to the combined effect of two causes: (1) the statistical constancy during successive generations of the statures of the same population who live under, generally speaking, uniform conditions; (2) to the ~~reasonable~~ presumption that a sample of the original population and a sample of ~~their~~ the kinsmen in any specified degree ~~are statistically~~ have similar ~~in the distribution of their statures~~ stature schemes. To fix the ideas, let us take ~~an example, namely, that of the relation between men and their nephews:—(a.)~~ A sample of men, and a sample of the nephews of those men, are presumed to be statistically alike in stature, that is to say, their mean heights and their quartile deviates of height will be of the same value. I will call the value of this quartile p. (b.) Each family of nephews affords a series of statures that are distributed above and below the common mean of them. They are deviations from a central family value, or, as we may phrase it, from a nepotal centre, and it will be found as we proceed (it results from what appears in Tables III, IV, and V) that these deviations are in conformity with the law of error, and that the quartile values (probable errors) of these systems of deviations, which we will call f, are practically uniform, whatever the value of the central nepotal family stature may be. (c.) It will be found, as it is reasonable enough to anticipate, that the system of nepotal centres is distributed above and below the median stature of the population, in conformity with the law of frequency of error, and with a quartile value that we will call d. It follows from (a) that we possess data for an equation between p, f, and d, which, from a well-known property of the law of error, assumes the form $d^2+f^2=p^2$. Now the unknown nephew is more likely to be of the stature of his nepotal centre than any other stature that can be named. But the system of statures of nepotal centres is more concentrated than that of the general population (d^2 is less than p^2). That is to say, the unknown nephew is likely to be more mediocre than the known man of whom he is the nephew. What I shall have to show is expressed in fig. 4, where A and Z are side views of squadrons such as A and Z in fig. 2. [They are drawn shorter than the stature-schemes in fig. 1, and therefore out of scale, to save space, which is an unimportant change, as it is only the variation in the ogives we are now concerned about.] Let m represent the level of mediocrity above the ground, $m+x$ and $m-x$ the heights of any two rectangular files in

FIG. 4.



the squadron of known men. We have seen that x becomes 0 in remote degrees of kinship, and I shall show that in intermediate degrees the value of x'/x is constant for all statures in the same degree of kinship. This fraction is what I call the ratio of regression, and I designate it by w. Consequently the above formula becomes $w^2p^2+f^2=p^2$, which is universally applicable to all degrees of kinship between man and man, so long as the statistics of height of the population remain unchanged.

Hence in the squadrons, the curvature in rank is an ogive with the quartile value of wp, and in file with one having the quartile value of f, these two values being connected by the above formula. If the squadron is resolved into its elements, and those elements are redistributed into an ordinary stature-scheme, the quartile of the latter will be p.

Another way of explaining the universal tendency to regression may be followed by showing that this tendency necessarily exists in each of the three primary relationships, fraternal, filial, and parental, and therefore in all derivative kinships. Fraternal regression may be ascribed to the compromise of two conflicting tendencies on the part of the unknown brother, the one to resemble the given man, the other to resemble the mean of the race, in other words to be mediocre. It will be seen that this compromise results in a probable fraternal stature that is expressed by the formulæ $(p^2-b^2)/p^2$, in which b is a constant as well as p, therefore the ratio of fraternal regression is also a constant. Filial regression is due (as I explained more fully than I need do here, in the publications alluded to in the second paragraph) to the concurrence of atavism with the tendency to resemble the parent. The remote ancestry in any mixed population resembles, as has been already said, any sample taken at random out of that population, therefore their mean stature is mediocre; consequently the parental peculiarities are transmitted in a diluted amount. Parental regression is shown to be the necessary converse of filial regression by mathematical considerations, kindly investigated for me by Mr. Dickson, in the Appendix to this memoir in Problem 1. It is easy in a general way to see that this would be the case, but I find it not easy otherwise to prove it. Still less would it be easy to prove the connexion between filial and mid-parental regression, which depend on considerations that are thoroughly investigated in the Appendix.

Data.—I will now describe the data from which I obtain my conclusions. They consist of two sets of practically independent observations, though they do in some small degree overlap.

(1.) Special observations. These concern variation in height among brothers. I circulated cards of inquiry among trusted correspondents, stating that I wanted records of the heights of brothers who are more than 24 and less than 60 years of age; not necessarily of all the brothers of the same family, but of as many of them as could be easily and accurately measured, the height of even two brothers being acceptable. If more than one set of brothers were entered on the same card, the entries were of course to be kept separate. The back of the card was ruled vertically in three parallel columns: (*a*) family name of each set of brothers; (*b*) order of birth in each set; (*c*) height, without shoes, in feet and inches. A place was reserved at the bottom for the name and address of the sender. The circle of inquiry widened, and I closed it when I had obtained returns of 295 families, containing in the aggregate 783 brothers.

I look upon these returns as quite as trustworthy as any such returns are likely to be. They bear every internal test that I can apply to them very satisfactorily. They are commonly recorded to quarter and half inches.

(2.) R.F.F. data. By this abbreviation I refer to the Records of Family Faculties that I obtained in the summer of 1884, in reply to an offer of prizes. I have been able to extract from these the heights of 205 couples of parents, with those of an aggregate of 930 adult children of both sexes. I have transmuted all the female heights to their male equivalents, and have treated them thus transmuted on equal terms with the measurement of males, except where otherwise expressed. These data have by no means the precision of the special observations. There is in many cases considerable doubt whether the measurements refer to the height with the shoes on or off; many entries are, I fear, only estimates, and the heights are commonly given only to the nearest inch. Still, speaking from a knowledge of many of the contributors, I am satisfied that a fair share of these returns are undoubtedly careful and thoroughly trustworthy, so that I have reason to place confidence in mean results. They bear those internal tests that I apply to them better than I should have expected, and when taken in connexion with and checked by the special data, and used with statistical caution, they have proved very valuable to me.

I have discussed these materials in a great variety of ways to guard myself against rash conclusions, but I shall not present more than three primary tables, which contain sufficient materials for determining the constants of the formulæ to be used.

The first of them (Table III) refers to the children of what I call "mid-parents" of various statures. A mid-parent is the imaginary mean of the two parents, after the female measurements have been transmuted to their male equivalents, so that a mid-parent of 70 inches in height refers to a couple whose mean stature under the above reservations is 70 inches. I have given data in the "Journ. Anthrop. Inst." (*loc. cit.*) to show that we need not regard differences in stature between the parents, inasmuch as the distribution of heights among the children proves to be statistically the same, so long as the mid-parentages are alike, whether the two parents are the same or of different statures. This blending of paternal and maternal qualities in the stature of the offspring is one great advantage in selecting stature as a subject for the present inquiry.

General Population.—(1.) Its variability. The value of the quartile deviate in the population ogive (that is to say, the probable error) may be deduced from the bottom lines of any one of the three Tables III, IV, and V. Those in III and IV refer to data that are in part but by no means wholly the same, that of V refers to almost totally distinct data. The work is shown in Tables VI and VII; in the former the ordinates are calculated whence the ogive is drawn, in the latter I have given the values of the measured ordinates at the same points along its axis as those to which the ordinates given in Table I refer. The values of the quartile that I obtain in this way from the three cases are 1·65, 1·7, and 1·7. I should say that the more careful treatment that I originally adopted happened to make the first of these values also 1·7, so I have no hesitation in accepting 1·7 as the proper value of p for all my data.

(2.) Variability of system of mid-parents. I have published data in the memoir already alluded to, to show that marriage selection takes small account of stature, which is another great merit in stature as a subject for this inquiry. Some further proof of this may be got by comparing the variability of the system of mid-parents with that of the general population. If the married couples had paired together regardless of stature, their mean heights would be elements of a statistical system identical with one in which the pairs had been selected at random. In this latter case the quartile value of the system of mid-parents would be $1/\sqrt{2}\,.\,p=1{\cdot}21$ inch. Now, I find the quartile of the series of the mid-parental system obtained from the two columns in Table III, that are headed respectively "Heights of the mid-parents" and "Total number of mid-parents," to be 1·19 inches,* which is an unexpectedly exact accordance.

(3.) Median Stature. I obtain the values 68·2, *68·5*, 68·4, from the three series mentioned above, but the middle value, printed in *italics*, is a smoothed value. This is one of the only two smoothed values in the whole work, and has been justifiably corrected because the one ordinate that happens to accord closely with the median is out of harmony with all the rest of the curve. This fortuitous discrepancy amounts to more than 0·15 inch. It does not affect the quartile value, because neither the upper nor the lower quartile is touched, and, therefore, the half-interquartile remains unchanged. It must be recollected that the series in question refers to R.F.F. brothers, which are a somewhat conditioned selection from the general R.F.F. population, and could not be expected to afford as regular an ogive as that made from observations of men selected from the population at hazard. It is undoubtedly in this group that the least accuracy was to have been expected.

Mean Ratios of Regression in the Primary Degrees of Kinship.—(1.) From the stature of mid-parents of the same height, to the mean of the statures of all their children. I have already (*loc. cit.*) published the conclusions to which I arrived about this, but it is necessary to enter here into detail. The data are contained in Table III, where each line exhibits the distribution of stature among the children of all the mid-parents in my list, who were of the stature that forms the argument to that line. The median stature in each successive line is the mean stature of all the children, and is given at the side in the column headed "Medians." Their values are graphically represented in fig. 5. It will be there seen that these value are disposed about a straight line. If the median statures of the children had been the same as those of their mid-parents this line would have accorded with the line AB, which, from the construction of the table, is inclined at an angle of 45° to the line "Mean Stature of Population," which represents the level of mediocrity. However, it does not do this, but its position is inclined at a smaller angle, θ, such that

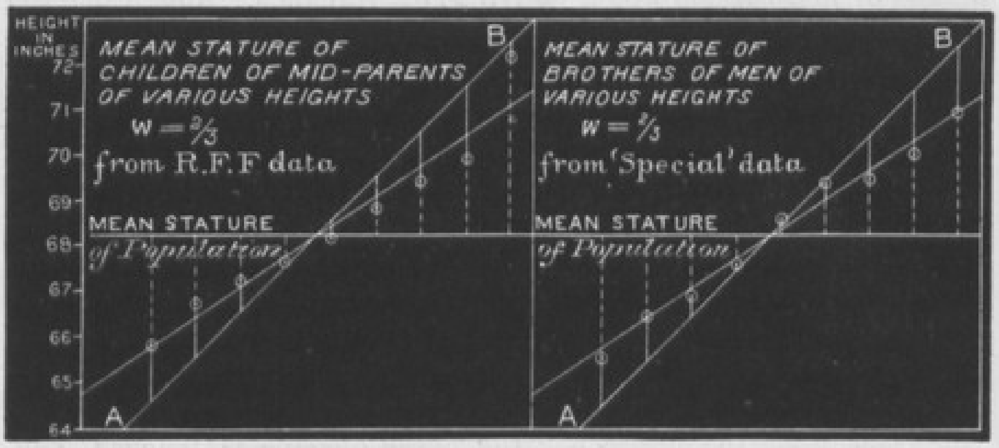
$$\tan\theta : \tan 45 \;::\; 2:3.$$

This gives us the ratio of regression ($=w$) in the present case; and, therefore, in the notation I adopt $w=\frac{2}{3}$.

(2.) From the stature of men of the same height, to the mean of the statures of all their children. We have just seen that when both

* In all my measurements the second decimal is only approximately correct.

Figs. 5 and 6.

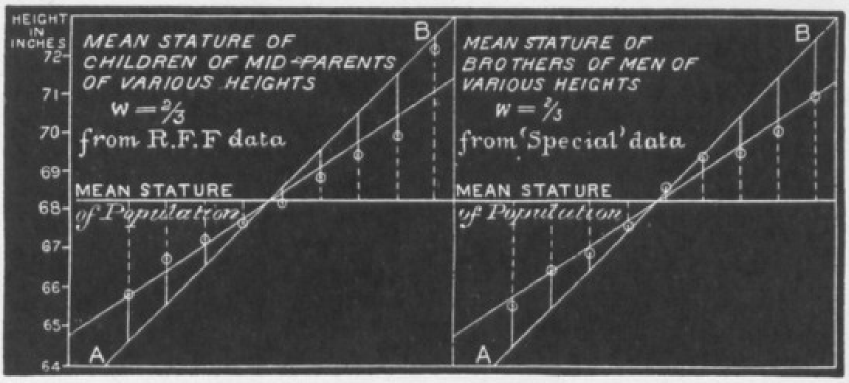


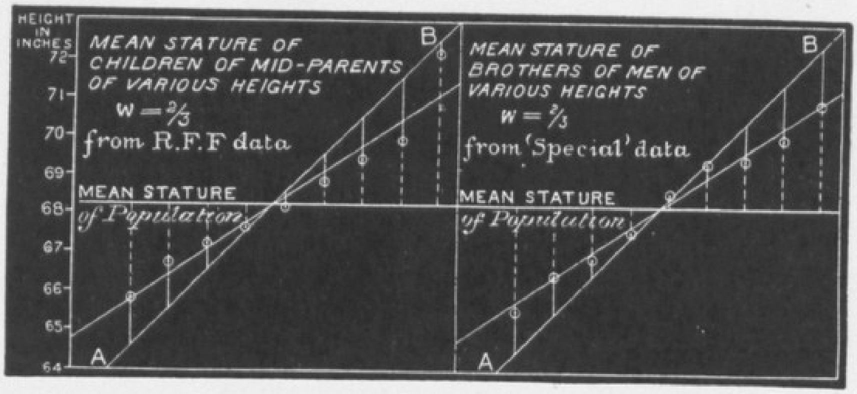
parents have a deviate of $\pm x$, the mean of the deviates of all of their family centres will be $\pm\frac{2}{3}x$. It follows that if one parent only has that deviate $\pm x$, and if the stature of the other parent is unknown, and, therefore, on the average, mediocre, the mean of the statures of their children will be half the above amount, or $\frac{1}{3}$. I cannot test this conclusion very satisfactorily by direct observation, for my data are barely numerous enough to enable me to deal even with the mid-parentages. They are consequently insufficient to deal with a question involving the additional large uncertainty of the stature of one of the parents. I have, however, tabulated the data, but do not think it worth while to give them. They yield a ratio of regression of 0·40 instead of 0·33 as above. I disregard it, and adopt the latter, namely, $w=\frac{1}{3}$.

(3). From the stature of men of the same height to the mean of the statures of their mid-parents. By treating the vertical columns of Table III in the same way as we have just dealt with the horizontal lines, we obtain results of the same general form as in the last paragraph but one, though of different values.

Taking the height of a group of men of the same stature (viz., the "Adult Children") as given in the line that forms the heading to the table, we find the median stature of all their mid-parents, whence I deduce in this case $w=\frac{1}{3}$. The apparent paradox that the same table should give results by no means converse in their values for converse degrees of kinship, will be more conveniently examined later on.

(4.) From the stature of men of the same height to the mean of the statures of all their brothers. In seeking for this I shall at first confine myself to the more accurate special data, reserving to the end a comparison between their results and those derived from the R.F.F. The entries in the column headed "medians" in Table V are


HEIGHT IN INCHES
72
71
70
69
68
67
66
65
64
MEAN STATURE OF CHILDREN OF MID-PARENTS OF VARIOUS HEIGHTS
W = 2/3
from R.F.F data
MEAN STATURE of Population
A
B
MEAN STATURE OF BROTHERS OF MEN OF VARIOUS HEIGHTS
W = 2/3
from 'Special' data
MEAN STATURE of Population
A
B


HEIGHT IN INCHES
72
71
70
69
68
67
66
65
64
MEAN STATURE OF CHILDREN OF MID-PARENTS OF VARIOUS HEIGHTS
W = 2/3
from R.F.F data
MEAN STATURE of Population
A
B
MEAN STATURE OF BROTHERS OF MEN OF VARIOUS HEIGHTS
W = 2/3
from 'Special' data
MEAN STATURE of Population
A
B

graphically represented in fig. 6, whence I deduce the value of $w=\frac{2}{3}$.

Variability of Statures of "Co-kinsmen" about their common mean Value.—By "co-kinsmen" I desire to express the group distributed in any one line of Tables III, IV, V, or of other tables constructed on a like principle. They are the kinsmen in a specified degree, not of a single person, but of a group of like persons, who probably differ both in ancestry and nurture. For example, the persons to whom the entries opposite 68·5 in Table III refer are not brothers, but they are what I call "co-fraternals," or from another point of view, "co-filials," namely, the children of numerous mid-parentages, differing variously in their antecedents, and alike only in their personal statures.

Co-filial Variability.—It appears from Table III that the mean of the quartiles derived from the successive lines, and which I designate by f, is 1·5 inch; also that the quartiles are of nearly the same value in all of the lines, allowance being made for statistical irregularities. A protraction on a large sheet of the individual observations in their several exact places, gave the result that the quartile was a trifle larger for the children of tall mid-parentages than for those of short ones. This justifies what was said some time back about the use of the geometric mean; it also justifies the neglect here of the method founded upon it, on the ground that it would lead to only an insignificant improvement in the results.

We have now obtained the values of the three constants in the general equation $w^2p^2+f^2=p^2$, when it is used to express the relation between mid-parentages and cofilials. Thus the quartile of the population being $p=1·7$, it was shown both by observation and by calculation, that the quartile of the mid-parental system was $1/\sqrt{2}\,.\,p$, or 1·21. It was also shown that the ratio of regression in that case was $\frac{2}{3}$, consequently the general equation becomes $(\frac{2}{3}\times 1·21)^2+(1·5)^2=(1·7)^2$, or $0·64+2·25=2·89$, which is an exact accordance, satisfactorily cross-testing the various independent estimates.

Converse Ratios of Regression.—We are now sufficiently advanced to be able to examine more closely the apparent paradox that the ratio of regression from the stature of mid-parents of the same height to the mean of the statures of their sons should be $\frac{2}{3}$, while that of men of the same stature to the mean of the statures of their several mid-parents should be, not the numerical converse of this, but $\frac{1}{3}$. We may look upon the entries in Table III as the values of (vertical) ordinates in z to be erected upon it at the points where those entries lie, and which are specified by the arguments of "heights of mid-parents" written along the side, as values of ordinates in y, and of "heights of adult children" written along the top, as values of ordinates in x. The smoothed result would form a curved surface of frequency. I accordingly smoothed the table by writing at each intersection of the lines that separated the vertical columns with those that separated the horizontal lines, the sums of the four adjacent entries. Then I drew lines with a free hand through all entries, or interpolations between entries, that were of the same value. These lines formed a concentric series of elliptical figures, passing through values of z that diminished, going outwards. Their common centre at which z was the greatest, and which therefore was the portion of maximum frequency, lay at the point where both x and y were of the same value of $68\frac{1}{4}$ inches, that is, of the value of the mean stature of the population. The line in which the major axes of the ellipses lay was inclined nearer to the axis of x than that of y. It was evident from the construction that the median value of the entries, whether in each line or in each column of the table, must lie at the point where that line or column was touched by the projection of one of these ellipses. It was easy also to believe that the equation to the surface of frequency and the lines of loci of the above-mentioned points of contact, admitted of mathematical expression. Also that the problem to be solved might be expressed in a form that had no reference to heredity. In such a form I submitted it to Mr. J. Hamilton Dickson, who very kindly undertook its solution, which appears as an Appendix to this paper, and which helps in various ways to test and confirm the approximate and uncertain conclusions suggested by the statistical treatment of the observations themselves. I shall make frequent use of his mathematical results, both in respect to this problem and to another one (also given in the Appendix), in the course of my further remarks.

As regards the present subject of the connexion between the regression in direct and in converse kinships, it appears that it wholly depends on the relation between the quartiles of the two series of "arguments," and is expressed by the formula $c^2w = p^2w'$. In this case $c^2 = (1{\cdot}21)^2 = 1{\cdot}46$, and $p^2 = 2{\cdot}89$; also $w = \frac{2}{3}$; therefore $w' = \frac{1}{3}$ nearly.

It will be observed that in all cases of converse kinship, from man to man—as from man to brother, and conversely; from man to nephew, and conversely; from father to son, and conversely; $c = p$, therefore in these the ratio of regression is the same in the converse as in the direct kinship.

Brotherly Variability.—The size of human families is much too small to admit of the quartile of brotherly variability which we will call b being determined ~~in the same way as that of the population, namely, by finding the quartiles in~~ directly from any single famil~~ies~~y, but there are four indirect ways of finding it~~s value, which I will call b~~.

fraternities and cousins

(1.) A collection of differences (see Table VIII) between the statures of individual brothers, in ~~families~~ of n brothers, and the mean of all the n statures in the same ~~family~~, ~~gives a quartile value, which I will~~

fraternity

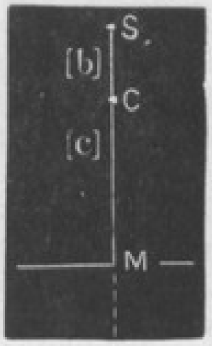
a quartile value d may be deduced from a call d, ~~whence b may be deduced as follows~~:—Suppose an exceedingly large ~~family~~ (theoretically infinitely large) ~~of brothers; their~~ quartile would be b. Then if we select from it, at random, numerous groups of n brothers in each, ~~the means~~ of the mid-deviates of the several groups would form a series whose quartile is $1/\sqrt{n} \times b$. Hence b is compounded of this value and of d; that is to say,

$$b^2 = d^2 + 1/n \times b^2 \quad \text{or} \quad b^2 = \frac{n}{n-1} d^2.$$

I treated in this way four groups of families, in which the values of n were 4, 5, 6, and 7 respectively, as shown in Table VIII, whence I obtained for b the four values of 1·01, 1·01, 1·20, and 1·08, whose mean is 1·07.

(2.) Let c be the quartile of a series of brotherly centres whose quartile is unknown and has to be determined, and that the statures of the individual brothers diverge from their several family centres $C_1 C_2$ . . ., with a quartile b, the whole group of brothers thus forming a sample of the ordinary population; consequently $c^2 = p^2 - b^2$. Now in fig. 7, MS represents the deviate in stature of a group of like persons

FIG. 7.



who are not brothers, and MC represents the mean of the mid-deviates of their respective families of brothers. It can be shown (see Appendix, Problem 2) that if the position of C varies with respect to M with a quartile $= \sqrt{p^2 - b^2}$, and if S varies with respect to C with a quartile $= b$, then, when S only is observed, the most probable value of CM is such that $\frac{\text{CM}}{\text{SM}} (=w) = \frac{p^2 - b^2}{p^2}$,

$$\text{or } b^2 = p^2(1 - w).$$

Substituting 1·7 for p, and $\frac{2}{3}$ for w,

$$b = 0·98 \text{ inch.}$$

(3.) It can also be shown (see Appendix, Problem 2) that the variability of particular mid-brotherly deviates, $C_1 C_2$ . . ., about C, the

c

mean of all them, is such that its quartile $= \frac{cb}{\sqrt{(c^2+b^2)}}$. Now the distribution of values in each line of Table V, whose quartile $= f$, is due to the combination of two variables. The one is the variability of $C_1C_2\ldots$, about C; the other is the variability of the individual brothers in each family, about C_1, C_2, &c., respectively. Therefore $f^2 = \frac{c^2b^2}{c^2+b^2}+b^2$. Substituting for c^2 its value p^2-b^2, we obtain

$$b^2=p(p-\sqrt{(p^2-f^2)}).$$

The observed value of f in Table V is 1·24, whence we obtain $b=1{\cdot}10$.

(4.) Pairs of brothers may be taken at random, and the differences noted between their statures; then under the following reservation, as regards the differences to be taken, we should expect the observed quartile of the differences to be $=\sqrt{2}\times b$. The reservation is, that only as many differences should be taken out of each family as are independent. A family of n brothers admits of $n\,.\,n-1/2$ possible pairs, but no more than $n-1$ of these are independent and only these should be taken. I did not appreciate this necessity at first, and selected pairs of brothers on an arbitrary system, which had at all events the merit of not taking more than four pairs of differences from any family, however numerous. It was faulty in taking three differences instead of only two from a family of three, and four differences instead of only three from a family of four, and therefore giving an increased weight to those families, but in other respects the system was hardly objectionable. On the whole the introduced error would be so slight as scarcely to make it worth while now to go over the work again. By the system adopted I found a quartile value of 1·55, which divided by $\sqrt{2}$ gives $b=1{\cdot}10$ inch.

Thus far we have dealt with the special data only. The less trustworthy R.F.F. give larger values of b. An epitome of all the results appears in the following table.

	Values of b obtained by different methods and from different data.	
	Specials.	R.F.F.
(1.) From families	1·07	1·38
(2.) From w (Tables V and IV)	0·98	1·31
(3.) From f (Tables V and IV)	1·10	1·14
(4.) From pairs of brothers	1·10	1·35
Mean	1·06	

The R.F.F. results refer to brothers only and not to transmuted sisters, except in method (2), where the paucity of the data compelled me to include them. I should point out that the data used in these four methods differ. In (1) I did not use families under four. In (2) and (3) I did not use large families. In (4) the method of selection was as we have seen, again different. This makes the accordance of the results still more gratifying. I gather from the above that we may securely consider the value of b to be less than 1·10, and allowing for some want of precision in the special data, the very convenient value of 1·0 inch may reasonably be adopted.

We are now able to deal completely with the distribution of statures in every degree of kinship of the kinsmen of those whose statures we know, but whose ancestral statures we are ignorant of or do not take into account. We are, in short, able to construct tables on the form of III, IV, and V, for every degree of kinship, and to reconstruct those tables in a way that shall be free from irregularities. The fraternal relation as distinguished from the co-fraternal has also been clearly explained.

In constructing a table of the form of III, IV, and V, we first find the value of w for the degree of kinship in question, thence we deduce f by means of the general equation $w^2p^2+f^2=p^2$ (p is supposed to be known, or for the general purpose of comparing the relative nearness of different degrees of kinship as tested by family likeness in stature, it may be taken as unity). The entries to be made in the several lines are then to be calculated from the ordinary tables of the "probability integral."

As an example of the first part of the process, suppose we are constructing a table of men and their nephews. A nephew is the son of a brother, therefore in his case we have $w=\frac{1}{3}\times\frac{2}{3}=\frac{2}{9}$; and $f=p\sqrt{(1-w^2)},=1{\cdot}66$.

Form of Data for calculating Tables of Distribution of Stature among Kinsmen.

From any group of persons of the same height, to their kinsmen as below.	Mean regression w.	Quartile of individual variability, $f(=p\times\sqrt{(1-w^2)})$.
Mid-parents	2/3 = .67	1·27
Brothers	2/3 = .67	1·27
Fathers or sons	1/3 = 33	1·60
Uncles or nephews	2/9 = 22	1·66
Grandfathers or grandsons	1/9 = 11	1·69

Trustworthiness of the Constants.—There is difficulty in correcting the results obtained solely from the R.F.F. data, by help of the knowledge of their general inaccuracy as compared with the special data. The reason is that this inaccuracy cannot be ascribed to an uncertainty of equal $\pm$ amount in every entry, such as might be due to a doubt of "shoes off" or "shoes on." If it were so, the quartile deviate of the R.F.F. would be greater than that of the specials, whereas it proves to be the same. It is likely that the inaccuracy is a result of the uncertainty above mentioned, which would increase the value of the quartile deviate, combined with a tendency on the part of my correspondents to record medium statures when they were in doubt, and which would reduce the quartile deviate. What the effect of all this might be on the value of w in Table IV, which is a datum of primary importance, I am not prepared to say, except that it cannot be great. While sincerely desirous of obtaining a revised value of w from new and more accurate data, the provisional value I have adopted may be accepted as quite accurate enough for the present.

Separate Contribution of each Ancestor to the Heritage of the Child.—I here insert a short extract from my paper in the "Journ. Anthrop. Inst.," with slight revision, as this memoir would be incomplete without it.

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. But though nothing is known, something is implied, and this must be eliminated before we can learn what the parental bequest, pure and simple, may amount to. Let the deviate of the mid-parent be x (including the sign), then the implied deviate of the mid-grandparent will be $\frac{1}{3}x$, of the mid-ancestor in the next generation $\frac{1}{9}x$, and so on. Hence the sum of the deviates of all the mid-generations that contribute to the heritage of the offspring is $x(1+\frac{1}{3}+\frac{1}{9}+\&c.)=x\frac{3}{2}$.

Do they contribute on equal terms, or otherwise? I have not sufficient data to yield a direct reply, and must, therefore, try the effects of limiting suppositions. First, suppose the generations to contribute in proportion to the values of their respective mid-deviates; then as an accumulation of ancestral deviates whose sum amounts to $x\frac{3}{2}$, yields an effective heritage of only $x\frac{2}{3}$, it follows that each piece of heritable property must be reduced, as it were, by a succession 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 proportionate diminutions, the property being taxed afresh in each transmission to $1/r$ of its amount, so that the effective heritage would be—

$$x\left(\frac{1}{r}+\frac{1}{3r^2}+\frac{1}{3^2r^3}+-\right)=x\left(\frac{3}{3r-1}\right)$$

and this must, as before, be equal to $x\frac{2}{3}$, whence $\dfrac{1}{r}=\dfrac{6}{11}$.

A third possible supposition of the mid-ancestral deviate in any one remote generation contributing more than would be done by an equal mid-parental deviate, is notoriously incorrect. Thus the descendants of "pedigree wheat" in the (say) twentieth generation show no sign of the remarkable size of their mid-ancestors in that degree, but the offspring in the first generation do so unmistakably.

The results of our only two valid limiting suppositions are therefore (1) that the mid-parental deviate, pure and simple, influences the offspring to $\frac{3}{5}$ 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.

[I do not propose here to discuss the reason why the effective heritage of the child should be less than the accumulated deviates of his ancestors. It is obviously connected with considerations that bear on stability of type.]

Pure breed.—In a perfectly pure breed, maintained during an indefinitely long period by careful selection, w would become $=0$, and the value of b would be changed, but apparently only a little. Call its new value β. It may be roughly estimated as follows. In mixed breeds the value of b includes the probable uncertainty of the implied value of the contributions inherited from the mid-grandparents, and from the mid-ancestry of each preceding generation. This can be but a trifle. Suppose the quartile of the uncertainty in the implied stature of each grandparent to be even as much as 1·7 inch (we need not wait to discuss its precise value), then the quartile of the uncertainty as regards the implied mid-grandparental stature would be $1/\sqrt{4}\times$ that amount, or say 0·8. The proportion of this, which would on the average be transmitted to the child, would be only $\frac{1}{4}$ as much, or 0·2. From all the higher ancestry put together, the contribution would be much less than this, and we may disregard it. The result then is $b^2=\beta^2+0{\cdot}04$. Taking $b=1{\cdot}07$, this gives $\beta=1{\cdot}05$ inch.

Probable Stature of the Child when the Statures of several of his Kinsmen are known.—First we have to add their several contributions as assessed in the last paragraph but one, and to these we have to add whatever else may be implied. A just estimate of the latter requires the solution of a very complex problem. Thus:—a tall son has a short father; this piece of knowledge makes us suspect that the mother was tall, and we should do wrong to set down her unknown stature as mediocre. Our revised estimate would be further modified if we knew the stature of one of her brothers, and so on. Moreover, the general equation $w^2p^2+f^2=p^2$ may cease to hold good. The possible problems are evidently very various and complicated, I do not propose to speak further about them now. It is some consolation to know that in the commoner questions of hereditary interest, the genealogy is fully known for two generations, and that the average influence of the preceding ones is small.

In conclusion, it must be borne in mind that I have spoken throughout of heredity in respect to a quality that blends freely in inheritance. I reserve for a future inquiry (as yet incomplete) the inheritance of a quality that refuses to blend freely, namely, the colour of the eyes. These may be looked upon as extreme cases, between which all ordinary phenomena of heredity lie.

Appendix. By J. D. Hamilton Dickson.

Problem 1.

A point P is capable of moving along a straight line P′OP, making an angle $\tan^{-1}\frac{2}{3}$ with the axis of y, which is drawn through O the mean position of P; the probable error of the projection of P on Oy is 1·22 inch: another point p, whose mean position at any time is P, is capable of moving from P parallel to the axis of x (rectangular co-ordinates) with a probable error of 1·50 inch. To discuss the "surface of frequency" of p.

1. Expressing the "surface of frequency" by an equation in x, y, z, the exponent, with its sign changed, of the exponential which appears in the value of z in the equation of the surface is, save as to a factor,

$$\frac{y^2}{(1{\cdot}22)^2}+\frac{(3x-2y)^2}{9(1{\cdot}50)^2}\quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (1)$$

hence all sections of the "surface of frequency" by planes parallel to the plane of xy are ellipses, whose equations may be written in the form,

$$\frac{y^2}{(1{\cdot}22)^2}+\frac{(3x-2y)^2}{9(1{\cdot}50)^2}=\text{C, a constant}\quad . \quad . \quad . \quad . \qquad (2)$$

2. Tangents to these ellipses parallel to the axis of y are found, by differentiating (2) and putting the coefficient of dy equal to zero, to meet the ellipses on the line,

$$\left.\begin{aligned} &\frac{y}{(1{\cdot}22)^2}-2\frac{3x-2y}{9(1{\cdot}50)^2}=0,\\ \text{that is}\qquad &\frac{y}{x}=\frac{\dfrac{6}{9(1{\cdot}50)^2}}{\dfrac{1}{(1{\cdot}22)^2}+\dfrac{4}{9(1{\cdot}50)^2}}=\frac{6}{17{\cdot}6} \end{aligned}\right\}\quad . \quad . \quad . \quad . \quad . \qquad (3)$$

or, approximately, on the line $y=\frac{1}{3}x$. Let this be the line OM.

From the nature of conjugate diameters, and because P is the mean position of p, it is evident that tangents to these ellipses parallel to the axis of x meet them on the line $x=\frac{2}{3}y$, viz., on OP.

3. Sections of the "surface of frequency" parallel to the plane of xz, are, from the nature of the question, evidently curves of frequency with a probable error 1·50, and the locus of their vertices lies in the plane zOP.

Sections of the same surface parallel to the plane of yz are got from the exponential factor (1) by making x constant. The result is simplified by taking the origin on the line OM. Thus putting $x=x_1$, and $y=y_1+y'$, where by (3)

$$\frac{y_1}{(1{\cdot}22)^2}-2\frac{3x_1-2y_1}{9(1{\cdot}50)^2}=0$$

the exponential takes the form

$$\left\{\frac{1}{(1{\cdot}22)^2}+\frac{4}{9(1{\cdot}50)^2}\right\}y'^2+\left\{\frac{y_1^2}{(1{\cdot}22)^2}+\frac{(3x_1-2y_1)^2}{9(1{\cdot}50)^2}\right\}\quad . \quad . \quad (4)$$

whence, if e be the probable error of this section,

$$\left.\begin{aligned}\frac{1}{e^2}&=\frac{1}{(1{\cdot}22)^2}+\frac{4}{9(1{\cdot}50)^2}\\ \text{or [on referring to (3)]}\quad e&=1{\cdot}50\sqrt{\frac{9}{17{\cdot}6}}\end{aligned}\right\}\quad . \quad . \quad . \quad . \quad . \quad (5)$$

that is, the probable error of sections parallel to the plane of yz is nearly $\frac{1}{\sqrt{2}}$ times that of those parallel to the plane of xz, and the locus of their vertices lies in the plane zOM.

It is important to notice that all sections parallel to the same coordinate plane have the same probable error.

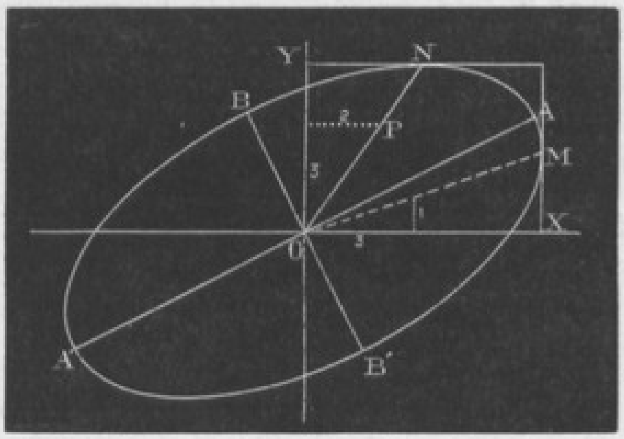
4. The ellipses (2) when referred to their principal axes become, after some arithmetical simplification,

$$\frac{x'^2}{20{\cdot}68}+\frac{y'^2}{5{\cdot}92}=\text{constant,}\quad . \quad . \quad . \quad . \quad . \quad (6)$$

the major axis being inclined to the axis of x at an angle whose tangent is 0·5014. [In the approximate case the ellipses are $\frac{x'^2}{7}+\frac{y'^2}{2}=$ const., and the major axis is inclined to the axis of x at an angle $\tan^{-1}\frac{1}{2}$.]

5. The question may be solved in general terms by putting YON$=\theta$, XOM$=\phi$, and replacing the probable errors 1·22 and 1·50 by a and b respectively: then the ellipses (2) are

FIG. 8.



$$\frac{y^2}{a^2}+\frac{(x-y\tan\theta)^2}{b^2}=\mathrm{C}, \quad \ldots \ldots \quad (7)$$

equation (3) becomes

$$\left.\begin{aligned}&\frac{y}{a^2}-\tan\theta\frac{x-y\tan\theta}{b^2}=0\\ \text{or}\quad &\frac{y}{x}=\tan\phi=\frac{a^2\tan\theta}{b^2+a^2\tan^2\theta}\end{aligned}\right\} \quad \ldots \ldots \quad (8)$$

and (5) becomes

$$\frac{1}{e^2}=\frac{1}{a^2}+\frac{\tan^2\theta}{b^2} \quad \ldots \ldots \quad (9)$$

whence

$$\frac{\tan\phi}{\tan\theta}=\frac{e^2}{b^2} \quad \ldots \ldots \quad (10)$$

If c be the probable error of the projection of p's whole motion on the plane of xz, then

$$c^2=a^2\tan^2\theta+b^2,$$

which is independent of the distance of p's line of motion from the axis of x. Hence also

$$\frac{\tan\phi}{\tan\theta}=\frac{a^2}{c^2} \quad \ldots \ldots \quad (11)$$

Problem 2.

An index q moves under some restraint up and down a bar AQB, its mean position for any given position of the bar being Q; the bar, always carrying the index with it, moves under some restraint up and down a fixed frame YMY′, the mean position of Q being M: the movements of the index relatively to the bar and of the bar relatively to the frame being quite independent. For any given observed position of q, required the most probable position of Q (which cannot be observed); it being known that the probable error of q relatively to

Q in all positions is b, and that of Q relatively to M is c. The ordinary law of error is to be assumed.

If in any one observation, $MQ=x$, $Qq=y$, then the law of error requires

$$\frac{x^2}{c^2}+\frac{y^2}{b^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

to be a minimum, subject to the condition

$$x+y=a, \text{ a constant.}$$

Hence we have at once, to determine the most probable values of x', y',

$$\frac{x'}{c^2}=\frac{y'}{b^2}=\frac{a}{b^2+c^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

and the most probable position of Q, measured from M, when q's observed distance from M is a, is

$$\frac{c^2}{b^2+c^2}a.$$

It also follows at once that the probable error v of Q (which may be obtained by substituting $a-x$ for y in (12)) is given by

$$\frac{1}{v^2}=\frac{1}{c^2}+\frac{1}{b^2}, \text{ or } v=\frac{bc}{\sqrt{b^2+c^2}} \quad . \quad . \quad . \quad . \quad . \quad (14)$$

which, it is important to notice, is the same for all values of a.

Throughout this discussion the technical term "probable error" has been used; it may in every instance be replaced by Mr. Galton's very apt name "quartile," in which case the results of these problems may be read in conjunction with Mr. Galton's papers.

Table I.

Ogive, or Normal Curve of Distribution of Error.

Abscissæ reckoned from 0° to ± 50° (value of the probability integral).		Corresponding ordinates (or deviates).	
		Value of the deviate when modulus = 1, A.	Value of deviate reduced proportionately to quartile = 1, B.
	10	0·179	0·38
	20	0·371	0·78
Quartile	25	0·477	1·00
	30	0·595	1·25
	40	0·906	1·90
	45	1·163	2·44

Table II.

Comparison of observed Ogives with the Normal.

	Abscissæ of the half-ogive.						Value of the unit in inches.
	10.	20.	25.	30.	40.	45.	
Normal ogive, from Table I....	0·38	0·78	1·00	1·25	1·90	2·44	1·00
General population, R.F.F.....	0·33	0·74	1·00	1·23	2·06	2·62	1·7
Population of brothers, R.F.F..	0·36	0·78	1·00	1·41	1·95	2·12	1·7
" " specials.	0·38	0·79	1·00	1·25	1·92	2·46	1·7
Mid-parentages..............	0·35	0·79	1·00	1·28	2·12	2·78	1·2
Brothers in random pairs, R.F.F.	0·47	0·84	1·00	1·29	2·11	2·64	1·4
" " specials.	0·42	0·78	1·00	1·25	1·88	2·44	1·4

Note.—The second decimal is only approximate.

Table III (R.F.F. Data).

Number of Adult Children of various Statures born of 205 Mid-parents of various Statures.
(All Female Heights have been multiplied by 1·08).

Height 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.

Table IV (R.F.F. Data).

Relative number of Brothers of various Heights to Men of various Heights, Families of Six Brothers and upwards being excluded.

Heights of the men in inches.	Heights of their brothers in inches.														Total cases.	Medians.
	Below 61·7	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 73·7		
Above 73·7	..	..	..	..	1	..	1	..	1	4	3	3	3	2	18	
73·2	1	..	..	..	..	..	1	1	2	1	3	4	..	3	16	
72·2	1	..	1	2	1	1	..	8	6	8	11	5	4	3	51	70·3
71·2	..	..	..	4	4	4	9	11	15	12	8	11	3	3	84	69·3
70·2	1	..	2	4	3	7	6	12	25	18	11	8	1	3	101	69·3
69·2	..	..	4	6	13	12	18	29	29	24	15	6	2	1	159	68·6
68·2	1	..	..	3	6	7	15	16	29	12	11	8	1	..	109	69·9
67·2	1	..	4	3	8	14	21	15	19	6	9	..	1	1	102	67·7
66·2	..	..	1	7	10	12	14	7	12	7	4	1	..	..	75	67·2
65·2	..	1	1	4	13	9	8	6	13	3	4	1	..	1	64	67·2
64·2	..	1	..	6	4	7	3	3	6	4	4	2	..	..	40	67·3
63·2	..	..	..	..	1	1	4	..	4	2	..	1	..	..	13	
62·2	..	..	..	..	1	..	..	..	..	..	..	..	..	..	1	
Below 61·7	..	..	..	..	..	..	1	1	..	1	..	1	1	..	5	
..	5	2	13	39	65	74	101	109	161	102	83	51	16	17	838	

Table V (Special Data).

Relative number of Brothers of various Heights to Men of various Heights, Families of Five Brothers and upwards being excluded.

Heights of the men in inches.	Heights of their brothers in inches.													Total cases.	Medians.
	Below 63	63·5	64·5	65·5	66·5	67·5	68·5	69·5	70·5	71·5	72·5	73·5	Above 74		
74 and above	1	1	..	..	..	..	..	1	1	..	5	3	12	24	
73·5........	..	..	..	..	..	1	3	4	8	3	3	2	3	27	
72·5........	..	..	..	..	1	1	6	5	9	9	8	3	5	47	71·1
71·5........	..	1	..	1	2	8	11	18	14	20	9	4	..	88	70·2
70·5........	..	..	1	1	7	19	30	45	36	14	9	8	1	171	69·6
69·5........	..	1	2	1	11	20	36	55	44	17	5	4	2	198	69·5
68·5........	..	1	5	9	18	38	46	36	30	11	6	3	..	203	68·7
67·5........	2	4	8	26	35	38	38	20	18	8	1	1	..	199	67·7
66·5........	4	3	10	33	28	35	20	12	7	2	1	..	..	155	67·0
65·5........	3	3	15	18	33	36	8	2	1	1	..	..	..	110	66·5
64·5........	3	8	12	15	10	8	5	2	1	..	..	..	..	64	65·6
63·5........	5	2	8	3	3	4	1	1	..	1	..	..	1	20	
Below 63	5	5	3	3	4	2	..	..	..	..	..	..	1	23	
Totals	23	29	64	110	152	200	204	201	169	86	47	28	25	1329	

Table VI.

Construction of Ogives from Observations.

(The Statures are here distributed in successive grades ~~of~~ differing by one inch ~~each~~.)

The number of the grade.	General population. (R.F.F.)			Population of brothers. (R.F.F.)			Population of brothers. (Specials.)		
	Number of cases in each grade.	Sums from the beginning.	Per cents.	Number of cases in each grade.	Sums from the beginning.	Per cents.	Number of cases in each grade.	Sums from the beginning.	Per cents.
	A.	B.	C.	A.	B.	C.	A.	B.	C.
1........	5	5	0·5	5	5	0·6	23	23	1·7
2........	7	12	1·3	2	7	0·7	29	52	3·9
3........	32	44	4·8	13	20	2·4	64	116	8·7
4........	59	103	11·1	39	59	7·1	110	226	16·9
5........	48	151	16·3	65	124	14·8	152	378	28·2
6........	117	268	28·9	74	198	23·7	200	578	43·1
7........	138	406	43·8	101	299	35·7	204	782	58·4
8........	120	526	63·1	109	408	48·7	201	983	73·4
9........	167	693	74·7	161	569	68·0	169	1152	86·0
10........	99	792	85·4	102	671	80·2	86	1238	92·4
11........	64	856	92·2	83	754	90·1	47	1285	95·9
12........	41	897	96·7	51	805	96·2	28	1313	98·8
13........	17	914	98·5	16	821	98·1	25	1338	100·0
14........	14	928	100·0	17	838	100·0	..	..	..

Table VII.

Measurement of Ogives.

drawn from · each of the three

set h

The Entries are Ordinates to the Curves constructed from Table VI, ~~at~~ points which are situated in ~~every~~ case, at the same fractional divisions, ~~either of their bases as in the first lines of each of the three groups, or~~ of their axes ~~as in the other lines.~~

Abscissæ, reckoned from the middle of the ogive, in percentages of the length of its axis.	−45	−40	−30	−25	−20	−10	0	+10	+20	+25	+30	+40	+45
General population (R.F.F.) ……	2·85	3·75	5·27	5·68	6·10	6·75	7·32	7·85	8·60	9·05	9·50	10·67	11·65
	4·47	3·57	1·95	1·64	1·22	0·57	0·00	0·53	1·28	1·73	2·18	3·35	4·33
							0·00	0·57	1·22	1·64	1·95	3·57	4·47
						Means……………	0·00	0·55	1·25	1·68	2·07	3·46	4·40
Population of brothers (R.F.F.) ..	3·55	4·38	5·60	6·12	6·54	7·35	7·95*	8·59	9·17	9·63	10·00	11·00	11·80
	4·40	3·57	2·35	1·73	1·41	0·60	0·00	0·64	1·22	1·68	2·05	3·05	3·85
							0·00	0·60	1·41	1·73	2·35	3·57	4·40
						Means……………	0·00	0·62	1·32	1·70	2·40	3·31	3·61
Population of brothers (specials) …	2·45	3·15	4·28	4·72	5·12	5·76	6·45	7·10	7·77	8·10	8·52	9·65	10·75
	4·00	3·30	2·17	1·73	1·33	0·69	0·00	0·55	1·32	1·65	2·07	3·20	4·30
							0·00	0·69	1·33	1·73	2·17	3·30	4·00
						Means……………	0·00	0·64	1·33	1·69	2·12	3·25	4·15

* The values are unsmoothed with this one exception. Its correction in no way affects the line headed "means."

Table VIII. (Special Data.)

Number of cases in which the Stature of individual Brothers was found to deviate to various amounts from the Mean Stature of their respective families.

Number of brothers in each family	4	5	6	7
Number of families	39	23	8	6
Amount of deviation.	Number of cases.	Number of cases.	Number of cases.	Number of cases.
Under 1 inch	88	62	20	21
1 and under 2	49	30	18	14
2 and under 3	15	17	5	6
3 and under 4	4	3	3	1
4 and above	..	3	2	..

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PRESIDENTIAL ADDRESS

TO THE

ANTHROPOLOGICAL INSTITUTE OF GREAT BRITAIN AND IRELAND,

JANUARY 26th, 1886.

BY

FRANCIS GALTON, ESQ., M.A., F.R.S.

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PRESIDENTIAL ADDRESS *to the* ANTHROPOLOGICAL INSTITUTE, *January* 26*th*, 1886, *by* FRANCIS GALTON, F.R.S.

It will perhaps be recollected that, at the meeting last autumn of the British Association in Aberdeen, I chose for my Presidential Address to the Anthropological Section a portion of the wide subject of "Hereditary Stature." My inquiries were at that time advanced only to a certain stage, but they have since been completed up to a well-defined resting-place, and it is to their principal net results that I shall ask your attention to-night.

I am, happily, released from any necessity of fatiguing you with details, or of imposing on myself the almost impossible task of explaining a great deal of technical work in popular language, because all these details have just been laid before the Royal Society, and will in due course appear in their *Proceedings.* They deal with ideas that are perfectly simple in themselves, but many of which are new and most are unfamiliar, and therefore difficult to apprehend at once. My work also required to be tested and cross-tested by mathematical processes of a very technical kind, dependent in part on new problems, for the solution of which I have been greatly indebted to the friendly aid of Mr. J. D. Hamilton Dickson, Fellow and Tutor of St. Peter's College, Cambridge. I shall therefore quite disembarrass myself on the present occasion from the sense of any necessity of going far into explanations, referring those who wish thoroughly to understand the grounds upon which my results are based, to the forthcoming memoir in the *Proceedings* of the Royal Society, and to that amplified and illustrated extract from my Address at Aberdeen, accompanied by tabular data, which appeared among the "Miscellanea" of the *Journal* of this Institute last November.

The main problem I had in view was to solve the following question. Given a group of men, all of the same stature, whatever that stature may be,—it is required to be able to predict two facts regarding their brothers, their sons, their nephews, and their grandchildren, respectively, namely, *first*, what will be their average height; *secondly*, what will be the percentage of those kinsmen whose statures will range between any two heights we may please to specify:—as between 6 feet and 6 feet 1 inch, 6 feet 1 inch and 6 feet 2 inches.

The same problem admits of another rendering, because whatever is statistically *certain* in a large number is the *most probable* occurrence in a small one, so we may phrase it thus: Given a man of known stature, and ignoring every other fact, what will be the probable average height of his brothers, sons, nephews, grandchildren, &c., respectively, and what proportion of them will probably range between any two heights we please to specify?

I have solved this problem with completeness in a practical sense. No doubt my formulæ admit of extension to include influences of a minor kind, which I am content to disregard, and that more exact and copious observations may slightly correct the values of the constants I use; but I believe that for the general purposes of understanding the nearness of kinship in stature that subsists between relations in different degrees, the problem is solved.

It is needless to say that I look upon this inquiry into stature as a representative one. The peculiarities of stature are that the paternal and maternal contributions blend freely, and that selection, whether under the aspect of marriage selection or of the survival of the fittest, takes little account of it. My results are presumably true, with a few further reservations, of all qualities or faculties that possess these characteristics.

Average Statures.—The solution of the problem as regards the average height of the kinsmen proves to be almost absurdly simple, and not only so, but it is explained most easily by a working model that altogether supersedes the trouble of calculation. I exhibit one of these: it is a large card ruled with horizontal lines 1 inch apart, and numbered consecutively in feet and inches, the value of 5 feet 8 inches lying about half way up. A pin-hole is bored near the left-hand margin at a height corresponding to 5 feet $8\frac{1}{4}$ inches. A thread secured at the back of the card is passed through the hole; when it is stretched it serves as a pointer, moving in a circle with the pin-hole as a centre. Five vertical lines are drawn down the card at the following distances, measured horizontally from the pin-hole: 1 inch, 2 inches, 3 inches, 6 inches, 9 inches. For brevity I will call these lines I, II, III, VI, and IX respectively. This completes the instrument. To use it: Hold the stretched thread so that it cuts IX at the point where the reading of the horizontal lines corresponds to the stature of the given group. Then the point where the string cuts VI will show the average height of all their brothers; where it cuts III will be the average height of the sons; where it cuts II will be the average height of the nephews; and where it cuts I will be the average height of the grandchildren. These same divisions will serve for the converse kinships; VI, obviously so; III, son to a parent; II, nephew to an uncle; I, grandson to a grandfather. Another kinship can be got from VI, namely, that between "mid-parent" and son. By "mid-parental" height I mean the average of the two statures: (*a*) the height of the father, (*b*) the transmuted height of the mother. This process, I may say, is fully justified by the tables already printed in our *Journal*, to which I have referred. (It is a rather curious fact that the kinship between a given mid-parent and a son should appear from my statistics to be of exactly the same degree of nearness as that between a given man and his brother.) Lastly, if we transmute the stature of kinswomen to their male equivalents by multiplying them (in inches) by 1·08, or say, very roughly, by adding at the rate of 1 inch for every foot, the instrument will deal with them also.

You will notice that the construction of this instrument is based on the existence of what I call "regression" towards the level of mediocrity (which is 5 feet 8¼ inches), not only in the particular relationship of mid-parent to son, and which was the topic of my address at Aberdeen, but in every other degree of kinship as well. For every unit that the stature of any group of men of the same height deviates upwards or downwards from the level of mediocrity as above, their brothers will on the average deviate only two-thirds of a unit, their sons one-third, their nephews two-ninths, and their grandsons one-ninth. In remote degeees of kinship, the deviation will become zero; in other words, the distant kinsmen of the group will bear no closer likeness to them than is borne by any haphazard group of the general population.

The *rationale* of the regression from father to son is largely to be ascribed (as was fully explained in the Address) to the double source of the child's heritage. That heritage is derived partly from a remote and numerous ancestry, who are on the whole like any other sample of the past population, and therefore mediocre, and partly only from the persons of the parents. Hence the parental peculiarities are transmitted in a diluted form, and the child tends to resemble, not his parents, but an ideal ancestor who is always more mediocre than they. The *rationale* of the regression from a known man to his unknown brother is due to a compromise between two conflicting probabilities: the one that the unknown brother should differ little from the known man, the other that he should differ little from the mean of his race. The result can be mathematically shown to be a ratio of regression that is constant for all statures. The results of observation accord with, and are therefore confirmed by, this calculation.

Variability of Kinsmen above and below their Mean Stature.—Here the net result of a great deal of laborious work proves, as in the previous case, to be extremely simple, and to be very easily expressed by a working model. A set of four scales can be constructed, such as I exhibit, one appropriate to each of the lines, I, II, III, and VI, and suitable for any position on these lines. They are so divided that when the centres of the scales are brought opposite to the points crossed by the thread, in the way already explained, we shall see from the divisions on the scales what are the limits of stature between which successive batches of the kinsmen, each batch containing 10 per cent. of their whole number, will be included. Smaller divisions indicate the 5 per cent. limits, or even narrower ones. The extreme upper and extreme lower limits are perforce left indefinite. Each of the scales I give deals completely with 99 per cent. of the observations.

The principal divisions on the movable scales that are appropriate to the several lines VI, III, II, and I, are given in the Table.

Per-cents. of included statures.	Divisions, upwards and downwards, from centres of the scales; in inches. VI.	III.	II and I.
10	0·5	0·6	0·6
20	1·0	1·3	1·3
30	1·6	2·0	2·1
40	2·4	3·0	3·1
45	3·1	3·9	4·0
49·5	4·8	6·1	6·3

The divisions are supposed to be drawn at the distances there given, both upwards and downwards from the centres of the several scales, which have to be adjusted, by the help of the thread, to the average height of the kinsmen indicated in the several lines. The percentage of statures that will then fall between the centre of each scale and the several divisions in it is given in the first column of the table. Example:—In line VI, 40 per cent. will fall between the centre and a point 2·4 inches above it, 40 per cent. will fall between the centre and a point 2·4 inches below it; in other words 80 per cent. will fall within a distance of 2·4 inches from the centre. Similarly we see that $2 \times 49{\cdot}5$, or 99 per cent. will fall within 4·8 inches of the centre.

In respect to the principle on which these scales are constructed, observation has proved that every one of the many series with which I have dealt in my inquiry, conforms with satisfactory closeness to the "law of error." I have been able to avail myself of the peculiar properties of that law and of the well-known "probability integral" table, in making my calculations. A very large amount of cross-testing has been gone through, by comparing secondary data obtained through calculation with those given by direct observation, and the results have fully justified this course. It is impossible for me to explain what I allude to more minutely now, but much of this work is given, and more is indicated, in the forthcoming memoir to which I have referred.[1]

I know of scarcely anything so apt to impress the imagination as the wonderful form of cosmic order expressed by the "law of error." A savage, if he could understand it, would

[1] The following will be of help to those who desire a somewhat closer idea of the reasoning than I can give in a popular address:—

m = mean height of race = 68·25 inches.

$m \pm x$ = height of a known individual.

$m \pm x'$ = the probable height of an unknown kinsman in any given degree.

$\frac{x'}{x}$ (which I designate by w) = the ratio of mean regression: it is shown by direct observation to $= \frac{2}{3}$ both in the case of mid-parent to son, and of man to brother; it is inferred to be $\frac{1}{3}$ in the case of parent to son. It is upon these primary kinships that the rest depend.

The "probable" deviations ("errors") from the mean values of their respective systems are—

p = that of the general population = 1·70 inch.

b = that of any large family of brothers = 1·0 inch.

f = that of kinsmen from the mean value of $m \pm x'$.

Since a group of kinsmen in any degree may be considered as statistically worship it as a god. It reigns with serenity in complete self-effacement amidst the wildest confusion. The huger the mob and the greater the anarchy the more perfect is its sway. Let a large sample of chaotic elements be taken and marshalled in order of their magnitudes, and then, however wildly irregular they appeared, an unsuspected and most beautiful form of regularity proves to have been present all along. Arrange the statures side by side in order of their magnitudes, and the tops of the marshalled row will form a beautifully flowing curve of invariable proportions; each man will find, as it were, a pre-ordained niche, just of the right height to fit him, and if the class-places and statures of any two men in the row are known, the stature that will be found at every other class-place, except towards the extreme ends, can be predicted with much precision.

It will be seen from the large values of the ratios of regression how speedily all peculiarities that are possessed by any single individual to an exceptional extent, and which blend freely together with those of his or her spouse, tend to disappear. A breed of exceptional animals, rigorously selected and carefully isolated from admixture with others of the same race, would become shattered by even a brief period of opportunity to marry freely. It is only those breeds that blend imperfectly with others, and especially such of these as are at the same time prepotent, in the sense of being more frequently transmitted than their competitors, that seem to have a chance of maintaining themselves when marriages are not rigorously controlled—as indeed they never are, except by professional breeders. It is on these grounds that I hail the appearance of every new and valuable type as a fortunate and most necessary occurrence in the forward progress of evolution. The precise way in which a new type comes into existence is untraced, but we may well suppose that the different possibilities in the

identical with a sample of the general population, we get a general equation that connects f with w, namely, $w^2p^2+f^2=p^2$.

The ratio of regression in respect to brothers can be shown to depend on the equation $w=\frac{p^2-b^2}{p^2}=\frac{2}{3}$ nearly.

groupings of some such elements as those to which the theory of pangenesis refers, under the action of a multitude of petty causes that have no teleological significance, may always result in a slightly altered, and sometimes in a distinctly new and a fairly stable position of equilibrium, and which, like every other peculiarity, admits of hereditary transmission. The general idea of such a process is easy enough to grasp, and is analogous to many that we are familiar with, though the precise procedure is beyond our ken. As a matter of fact, we have experience of frequent instances of "sports" useful, harmful, and indifferent, and therefore presumably without teleological intent. They are also of various degrees of heritable stability. These form fresh centres, towards which some at least of the offspring have an evident tendency to revert. By refusing to blend freely with other forms, the most peculiar "sports" admit of being transmitted almost in their entirety with no less frequency than if they were not exceptional. Thus a grandchild, as we have seen, regresses on the average one-ninth. Suppose the grandfather's peculiarity refused to blend with those of the other grandparents, then the chance of his grandson inheriting that peculiarity in its entirety would be as one to nine; and, so far as the new type might be prepotent over the other possible heritages, so far would the chance of its reappearance be increased. On the other hand, if the peculiarity blends easily, and if it was exceptional in magnitude, the chance of inheriting it to its full extent would be extremely small.[1] The

[1] The chance that the stature of the son will at least rival the stature of the father is not uniform; it varies with the stature of the father. The following table shows the value of the probability in various cases. Columns A contain the height of the fathers; the columns B show how many *per cent.* of the sons will be of at least the same height as their fathers.

A.		B.	A.		B.	A.		B.
feet.	inches.	per cent.	feet.	inches.	per cent.	feet.	inches.	per cent.
5	$8\frac{1}{4}$	50	6	0	15	6	4	1·4
5	9	42	6	1	9	6	5	0·7
5	10	31	6	2	5	6	7	0·3
5	11	22	6	3	3			

probability (easily to be calculated for any given instance by the "probability integral" tables) might even be many thousand times smaller. I will give for an example a by no means extreme case. Suppose a large group of men, all of 6 feet 5 inches in height, the statures of whose wives are haphazard, then it can be shown that, on an average, out of every thousand of the sons not more than seven will rival or surpass the height of his father. This consideration is extremely important in its bearing on the origin of species. I feel the greatest difficulty in accounting for the establishment of a new breed in a state of freedom by slight and uncertain selective influences, unless there has been one or more abrupt changes of type, many of them perhaps very small, but leading firmly step by step, though it may be along a devious track, to the new form.

It will be of interest to trace the connection between what has been said about hereditary stature and its application to hereditary ability. Considerable differences have to be taken into account and allowed for. *First*, after making large allowances for the occasional glaring cases of inferiority on the part of the wife to her eminent husband, I adhere to the view I expressed long since as the result of much inquiry, historical and otherwise, that able men select those women for their wives who on the average are not mediocre women, and still less inferior women, but those who are decidedly above mediocrity. Therefore, so far as this point is concerned, the average regression in the son of an able man would be less than one-third. *Secondly*, very gifted men are usually of marked individuality, and consequently of a special type. Whenever this type is a stable one, it does not blend easily, but is transmitted almost unchanged, so that specimens of very distinct intellectual heredity frequently occur. *Thirdly*, there is the fact that men who leave their mark on the world are very often those who, being gifted and full of nervous power, are at the same time haunted and driven by a dominant idea, and are therefore within a measurable distance of insanity. This weakness will probably betray itself occasionally in disadvantageous forms among their descendants. Some of these will be eccentric, others feeble-minded, others nervous, and some may be downright lunatics.

It will clear our views about hereditary ability if we apply the knowledge gained by our inquiry to solve some hypothetical problem. It is on that ground that I offer the following one. Suppose that in some new country it is desired to institute an Upper House of Legislature consisting of life-peers, in which the hereditary principle shall be largely represented. The principle of insuring this being that (say) two-thirds of the members shall be elected out of a class who possess specified hereditary qualifications, the question is, What reasonable plan can be suggested of determining what those qualifications should be?

In framing an answer, we have to keep the following principles steadily in view:—(1) The hereditary qualifications derived from a single ancestor should not be transmitted to an indefinite succession of generations, but should lapse after, say, the grandchildren. (2) All sons and daughters should be considered as standing on an equal footing as regards the transmission of hereditary qualifications. (3) It is not only the sons and grandsons of ennobled persons who should be deemed to have hereditary qualifications, but also their brothers and sisters, and the children of these. (4) Men who earn distinction of a high but subordinate rank to that of the nobility, and whose wives had hereditary qualifications, should transmit those qualifications to their children. I calculate roughly and very doubtfully, because many things have to be considered, that there would be about twelve times as many persons hereditarily qualified to be candidates for election as there would be seats to fill. A considerable proportion of these would be nephews, whom I should be very sorry to omit, as they are twice as near in kinship as grandsons. One in twelve seems a reasonably severe election, quite enough to draft off the eccentric and incompetent, and not too severe to discourage the ambition of the rest. I have not the slightest doubt that such a selection out of a class of men who would be so rich in hereditary gifts of ability, would produce a senate at least as highly gifted by nature as could be derived by ordinary parliamentary election from the whole of the rest of the nation. They would be reared in family traditions of high public services. Their ambitions, shaped by the conditions under which hereditary qualifications could be secured, would be such as to encourage alliances with the gifted classes. They would be widely and closely connected with the people, and they would to all appearance—but who can speak with certainty of the effects of any paper constitution ?—form a vigorous and effective aristocracy.

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BY

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ADDRESS *delivered at the* ANNIVERSARY MEETING *of the* ANTHROPOLOGICAL INSTITUTE *of* GREAT BRITAIN *and* IRELAND, *January 25th*, 1887.

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

OUR Institute, as appears from the Report of the Council, and as I hope from your own observation also, continues to perform ts self-appointed task with usefulness, and to satisfy to the best of its opportunity the current needs of anthropological record and research.

It was formally incorporated under the Companies Acts on March 26th.

The year that has passed by has been eventful to it in many respects. The Institute has sustained, as in the course of nature it must do from time to time, the loss of valued members by death; it has also witnessed a considerable widening of the field of anthropological interest.

The nearest of our losses is through the death of our former judicious President, Mr. George Busk, distinguished in many lines, but in those which concern us, more especially as a craniologist. No one is better qualified to do justice to his labours in this special department of anthropology than his intimate friend Professor Flower, who at my request has kindly drawn up the notice of his life and works which will be found printed after this Address (p. 403).

Sir Arthur Phayre, G.C.M.G., was an administrator of high rank, who eminently devoted himself to the study of the men over whom he had to rule, and whose frequent memoirs, geographical and others, connected with Burmah, made him for many years the principal authority upon that country.

Through the death of Dr. Mann we miss a frequent attendant at many scientific meetings, who had been an eager exponent of South African ethnology for many years, and always ready to give or to obtain information for scientific inquirers on African subjects. In advanced age, though suffering from the severe bodily infirmities which ended in death, he superintended the arrangement of the Ethnological Collection of Natal in the late Colonial and Indian Exhibition, and almost, if not quite, his last public appearance was when reading a memoir upon them at one of our Conferences in that building.

These and other active and efficient members have been taken from us, while new and zealous men have joined our ranks, so the Institute as a whole lives and thrives like an organic body; each of us in his turn plays his part, then falls away, and another succeeds to his place.

I will in my further remarks on the past year refer not directly to our own proceedings, as they appear set forth in our Journal, under the careful and willing editorship of Mr. Rudler but to those instances of our action outside, with which members have less opportunity of becoming acquainted.

An extensive ethnological inquiry has been initiated by the Council of the Palestine Exploration Fund. They formed a Committee upon which I was appointed to serve on behalf of this Institute, to draw up a list of questions applicable to the various races inhabiting Syria, which are to be placed in the hands of the numerous persons who come within the sphere of their operations. Many of these have had medical instruction and are likely to prove competent observers. The task of doing this was ultimately placed mainly in the hands of Captain Conder, R.E., to be carried out upon the general lines laid down in the Anthropological "Notes and Queries," but of course they have been much modified to suit the special inquiry. The questions are now printed and will very shortly be distributed.

The Anthropological "Notes and Queries" to which I have just referred, are running out of print. They were drawn up by various members of our Institute, at the suggestion of and under the editorship of our then President, Colonel Lane Fox, now General Pitt-Rivers. They were published at the cost of the British Association, who at their last meeting constituted a Committee from among the former writers of the little volume, to consider the propriety of publishing a second and revised edition. The Association also made a small grant to cover initial expenses.

The British Association has further assisted the objects of our Institute in another way. It will be recollected by many that in the course of a discussion last spring that arose after the memoir read by Mr. Reginald Stuart Poole on the races portrayed in the ancient Egyptian monuments, that gentleman pointed out the urgent importance of obtaining photographs of all those sculptures and pictures that refer to persons of known races. He also suggested that Mr. Flinders Petrie might be induced to undertake the task of making them. Many of our members entered warmly into this view, and on application being made to the British Association a grant was made by that body to a Committee of which I was chairman, to carry this proposal into effect. The Committee has met and discussed the matter with Mr. Petrie, who was then in this country. A list of about 70 of the portraits that appear most desirable to photograph, was drawn up and carefully considered, and Mr. Petrie willingly undertook the labour of photographing them, so far as opportunity should permit. He is now in Egypt.

The Ethnographical Gallery at the British Museum was thrown open in April last after its re-arrangement in rooms left vacant through the removal of the Natural History collection to South Kensington. The adjoining Asiatic saloon, which contains specimens of Oriental art and objects illustrative of the Oriental religions, was opened at the same time. The collection now comprises that formed and bequeathed by Mr. Christy 20 years ago, which, for want of space in the Museum had remained for most of that time comparatively unknown to the public, and installed in his former private residence. The whole has been very largely extended and supplemented through the continued zealous efforts of Mr. Franks. The arrangement of the pre-historic section is being vigorously proceeded with and will probably be completed in the spring. It will include the collection of Canon Greenwell as well as the pre-historic portion of that of Mr. Christy. Greatly as the space allotted to the collection has been increased, and though it now occupies a magnificent suite of rooms, it is still seriously cramped in many of its sections. It is far from being as amply housed as those of Berlin and Vienna. Its area is too small for the legitimate requirements of a collection whose object is to explain the development of the faculties of mankind by specimens of their handiwork, beginning with those of pre-historic times and passing through successive and parallel stages of barbarism to the dawn of the higher modern civilization.

The anthropological collection presented by General Pitt-Rivers to the University of Oxford, is now nearly arranged by Professor Moseley in the building erected by the University to receive it. The ground floor will be thrown open to the public daily in the afternoon during the present term, and Dr. E. B. Tylor will lecture every Monday afternoon on the collections in the building. There is hope that the remainder of the room will be opened before the end of summer. It is gratifying to find that this magnificent collection excites much interest in the University, and is likely to be largely frequented.

Another great event of anthropological interest to us in the past year was the Colonial and Indian Exhibition, whose exhibits, so far as they concerned ethnology, were well brought into notice during the series of Conferences held by our Institute in the Conference Hall of that building. The subjects of the various Conferences will be found described in the Report of the Council and in the Journal of the Institute.

It has, moreover, led to the project of an Imperial Institute, that shall also serve as a memorial of the 50th year of Her Majesty's reign. Its principal function will be to bring us, who live in the mother country, into close and permanent touch with our fellow subjects of all varieties of race, creed, and mode of thought, who are spread over Her Majesty's dominions. It is a grand idea, which, if adequately carried into effect, will prove a noble achievement. Primarily the object of the Imperial Institute is to afford a centre of intelligence for commerce and emigration; but a busy mart and frequent meeting place for representatives of all the races in the British dominions cannot fail incidentally to become an important centre of anthropological intelligence. It is in reference to that aspect of the future Imperial Institute, which also in some degree characterised the past Colonial and Indian Exhibition, that I offer the following remarks.

I am not sure whether there is any need for me to allude at all to a proposal that has been publicly urged, that a prominent feature of the Imperial Institute should be an Ethnological Museum of the races in the British dominions. There is no reason, so far as I have heard, to suppose that a museum of this kind is likely to be included in the plan, but as a proposal for it has been and may again be brought forward, I think it is well to show reasons why so costly and large an adjunct would not be of first-rate importance to us. The British possessions are spread widely over the globe, but they do not by any means include representatives of all the races that inhabit it. It follows that an Ethnological Museum, limited to the handiwork of populations subject to the British rule, cannot have the same scientific importance and interest as such general ethnological collections as those at the British Museum and at Oxford, of which I have just spoken. There seems to be no very useful stage half way between a good local and a good general museum. The former exhausts the peculiarities of its district, the latter collates analogous objects from every district where they exist, and makes each help in interpreting the rest. It therefore seems to me undesirable to ask that a prominent feature of the future Imperial Institute should be an ethnological collection, limited to the particular group of races who happen now to fall within the British possessions; on the other hand, the desire of any colony to maintain a local museum of its own would, I conceive, receive warm encouragement from anthropologists.

In the hope that the proposed Imperial Institute will be a focus for anthropological reference and information, we ought cordially to wish it success. With its prospective libraries, with the opportunities it will afford of personal intercourse with colonists, and by the stimulus that it is capable of giving to a wide scientific co-operation, it may become a powerful agent in advancing anthropological knowledge and research.

The Colonial and Indian Exhibition brought forcibly to notice the rapid diminution in present and future importance of the barbarous races who inhabit the temperate regions of the world in which Europeans are now establishing themselves. Their peculiarities are losing present interest and are becoming historical and archaic, little to be taken into account in reckoning upon the future of those regions. They are to the new European lords of the soil of not much more consideration than the vegetation of the wilderness might be to the owner of a newly reclaimed and scientifically cultivated farm. The whole of the exhibits of native handiwork in the large courts occupied by Canada made so small a show that they could have been partly placed on an ordinary sized dinner table and partly hung up on the wall behind it.

In such colonies as these the anthropological interest of the future will become less and less concerned with the customs of the barbarous races who may still inhabit them, and more and more assimilated to that which we now take in the inhabitants of the United Kingdom. A vast deal remains to be done at home before this interest can even be moderately satisfied. It is but very lately that we have acquired a fairly exact knowledge of the most marked physical peculiarities of our countrymen; as to their mental characteristics they are almost untouched by the methods of strict scientific inquiry. Whatever concern we justly feel in taking stock from time to time of our race at home, and in discovering how far its quality is improved or deteriorated by locality, occupation, or other influences, that concern will be even more keenly felt in extending a similar inquiry to distant settlements of our race, where the differences of environment are greater than with us, and their effects are therefore less liable to be confused with those of concurrent and hidden influences. In astronomical language they will have a larger parallax, and therefore the errors of observation will be less liable to vitiate the results. We can be sure that whatever effort we may bestow upon inquiries into the vital statistics of the numerous communities of our race who are settled in diverse climates and under various circumstances, will be more effective in solving the problems of sociology than the same amount of effort limited to investigations in the mother country.

Here I will draw your attention to the very important aid to sociological research that is likely to be given by the International Statistical Institute which Sir Rawson W. Rawson has had the good fortune to succeed in establishing. It is a body of great administrative weight and influence. It consists of members and associates, limited to the number of 200, who are heads of official statistical bureaux in all parts of the world, of commissions and of societies, and others who have special statistical knowledge or qualifications. Its object is to introduce uniformity, as far as may be, in statistical returns, so as to make those of different countries mutually comparable, and to stimulate the interests of Governments and individuals in the study of social phenomena. This Institute as at present arranged, is to meet biennially. The present year will be that of its second meeting, and at Rome.

As regards India and the Colonies in which the native population is large and is likely to subsist, whether owing to its vitality being strong enough to hold its own against that of the whites in a fair field of competition, or because the white races cannot thrive and multiply in their climates, additional objects of anthropological research will abound. Each of the various native races call for as much study as our own, and the sociological problems that arise from the mixture of races introduce a further complexity. Moreover, they are problems not only of academic interest, but they are living conditions that statesmen have to face and deal with.

I must diverge for a moment to express the welcome we afford to the Anthropological Society newly established at Bombay, for the discussion of Indian topics. It seems to be supported on all sides by natives as well as Europeans, with the utmost cordiality. The first number of its publications reached me a few days ago, and judging from the variety of its contents and the originality of its papers, it seems likely to give valuable future aid to the advancement of our science. Also, I will take this opportunity of referring to another new Anthropological Society, that of Japan. It has already during the few months of its existence, published two numbers in the Japanese language, with some illustrations, and English tables of contents. The society has been instituted at a most propitious moment, when the traditions and usages of Old Japan remain in full memory, while the rapidly growing culture of New Japan has become sufficiently advanced to make their collection and study a matter of interest to the people. No doubt some of the more valuable papers in this journal will hereafter appear in one or other of the chief European languages. The curse of the Tower of Babel, in whatever sense we may employ the phrase, has long pressed heavily upon scientific men in Europe; the contemplation of the additional burden on our descendants of having possibly to learn Japanese, Russian, and Chinese as well as the western European languages can hardly be indulged in with equanimity.

The recent extraordinary spread and domination of the white races over the world is forcibly brought into notice by the various political treaties that have lately assigned vast regions in the Pacific Islands and in Africa to the protectorate of one or other of the great European Powers. It makes us again consider the often discussed problem whether any offshoots from European races are destined to take root and to naturalise themselves in the tropics, or whether the conditions of life in those climates are so prejudicial to their health, vigour, and fertility as to exclude the possibility of such an event.

It seems strange to say, after the experience of generations that we have had in India and elsewhere, that adequate data for the decisive answer to this question by appeal to past fact, do not appear to exist. Statisticians who have attempted the problem have commonly arrived at this conclusion. The paucity of available data is due to the habit of successful colonists to return to their homes in later life, and for their children, even if they settle in the land where they were born, to marry European wives, and so to import fresh blood. Besides this the field of inquiry is full of statistical complexities and pitfalls, so much so as to render it futile to attempt to fairly state and weigh such evidence as exists, on an occasion like the present. However, I am desirous to say something on the subject, and to bring to your notice two or three general considerations, that are not without importance in themselves, and which have an independent interest of their own.

The unsuitability of the tropics for European settlement is principally due to their heat and to their diseases. I will consider these separately.

As regards heat we should bear in mind the great and increasing power of man to control within doors the influence of the out-of-door temperature. It has been almost wholly exerted until very recent years in resisting cold, with the happy result that active industries are carried on under inclement skies throughout the year, irrespectively of season, and that a highly refined and artificial society exists in countries which without warming appliances could be inhabited only by rude races, half dormant during the winter. It is difficult to assign any limit in the direction of the poles at which civilisation is impossible on account of the incapacity of man to battle with the cold. That limit is certainly not reached at St. Petersburg nor at Archangel.

It has not been the practice until very recent times to produce cold on a large scale by artificial means. I do not speak of the cooling produced in dry air by the evaporation of water, nor of that produced by radiation into space from the surface of the ground when the air is very still and the sky perfectly clear; these are exceptional circumstances, and are absent in the countries where the oppression of a hot and humid atmosphere is most severely felt. But I mean such cooling as is produced on a large scale and of great intensity by one or other of the several forms of refrigerating machines worked by coal that are now used in the transport of frozen meat even from the Antipodes, and to preserve it for a long time in the same condition after its arrival in this country. It is reasonable to ask whether it might not be possible to alleviate the heat at least of sleeping rooms where there is no opening and shutting of doors, by some such process, and so to render the tropics more habitable to Europeans than they now are.

The idea is not new. It was, I believe, first broached by the late Mr. Siebe in his examination before the Commission of 1863 into the Sanitary State of the Army in India (pp. liv and 326), and now that his machine and those of many other inventors are largely employed and their use is rapidly extending, the same idea has again been occasionally brought forward. I would refer those who desire late intelligence about refrigerating machines to Mr. T. B. Lightfoot's admirable paper upon them in May last (1886) before the Institution of Mechanical Engineers, and to the discussion that followed. A previous memoir by the same author before the same society was read in 1881. I have, however, come across no calculations of the expense of cooling sleeping rooms in hot climates, so I have myself made a calculation for a single typical case which will afford a useful basis for hypotheses of what is or may hereafter be feasible.

In an occupied room when the purity of the air neither increases nor diminishes, the volume of outflowing air in each unit of time must contain just as much impurity as was being exhaled into the general body of air in the room during the same period. The greater the rate of outflow and replacement by fresh air the less the percentage of remaining impurity. Experience shows that an outflow of 1,200 cubic feet of air per man per hour, and a corresponding inflow will keep a room in these climates in fair condition; 1,500 would probably suffice in the tropics. This amount is, of course, independent of the size of the room, and it is that which is now allowed in barracks.[1]

The volume of air that actually passes through the lungs is comparatively insignificant, being only from 7 to 8 cubic feet per man per hour, or the one hundred and fiftieth part of the air needed for ventilation.

In supplying cold air under the supposed circumstances it must at the same time be dry air, else its mixture with the hot humid atmosphere would produce a cloud of vapour. The special case I will examine is that where it is required to supply air at 70° Fah., with a dew-point of 60°, when the temperature of the surrounding atmosphere is 90°, and the air is damp to saturation.

At a barometric pressure of 30 inches each cubic foot of the air to be supplied contains a weight of 514 grains troy of dry air associated with 5 grains of vapour. The same weight of dry air when raised to 90° and fully saturated will contain no less than 15 grains of vapour. Therefore for each cubic foot of supply, 10 grains weight of vapour in excess will have to be condensed into water, and to do this exhausts no less than $78\frac{1}{2}$ per cent. of the total cooling power that is required.

I find this total cooling power to be such that 68 grains of ice at 32° will effect it,[2] in other words that one ton of ice will supply air of the desired quality sufficient for one man, namely, 1,500 cubic feet per hour, for 127 hours, or during 16 nights of eight hours each.

There are some additional items of cooling to be effected, but

[1] *See* "Healthy Dwellings," by Captain Douglas Galton, C.B., F.R.S., 1880.

[2] The number of units of heat required—

(1) To melt 68 grains of ice at 32° F. into water of the same temperature and—

(2) To raise that water to 70°;

Are equal to the number of units of heat parted with—

(3) To condense 10 grains of vapour at 90° to water of the same temperature;

(4) To cool that water from 90° to 70°;

(5) To cool 514 grains of dry air to the same amount, and—

(6) To cool the 5 grains of vapour that are associated with it.

they are relatively insignificant in amount. About 530 grains of vapour per man per hour are given off from the lungs and skin, and all of this has to be condensed. But as we have already allowed for the condensation of ten times 1,500 or for 15,000 grains per hour, the additional demand in this respect is only one twenty-eighth part of that which has been already met.

Again, the volume of heated expired air is said in the tropics to be less than in these climates, and to be only 7 cubic feet per hour; its temperature will be say 98°. The additional demand for cooling somewhat less than this small quantity of dry air through 28°, is insignificant compared to the first charge which has already been met, of cooling 1,200 cubic feet through 20°.

Again, we may safely assume that the amount of warmth radiated from the surface of the body or carried away from it by heated currents is of relative insignificance, but I have no data to estimate it correctly.

We may fairly conclude that an additional 5 per cent. to the previously calculated quantity of ice would more than cover the demand for all these additional purposes.

We have lastly to consider the waste of ice owing to the invasion of heat through the walls and roof. Of course these would have to be made of very good non-conducting material, like the walls of refrigerating chambers.

Allowing for everything, it seems that a ton of machine-made ice, which can be produced at the prime cost of a very few shillings, might well serve to cool the sleeping room of one man for a fortnight. Artificially made ice can, as I learn on inquiry, be bought at the works at any time in London, if on a large scale, at 20*s.* a ton. It is carted, delivered, and stored for 30*s.* a ton.

The cooling of a sleeping room even by the costly method of artificially made ice would therefore be by no means a serious expense in comparison to other luxuries, and the details of successfully constructing a refrigerated sleeping room seem to present no serious difficulty and to involve no large cost. It is easy to imagine how the ice would have to be stacked as in an ice house, above the ceiling of the refrigerated chambers among air flues; and how the inlet pipe before entering the room might pass by the newly incoming warm air from the outside in order that the saturated and over-chilled air should yield some of its cold to it, and enter the room as a somewhat less cold but dry air. Whether a better and much cheaper way of cooling a sleeping chamber by compressed air or otherwise might not be employed, is another question into which I do not enter. Certainly experiment is desirable, for whenever the problem of artificially cooling bed chambers and dwelling rooms shall have been practically solved, one of the difficulties in the way of Englishmen naturalising themselves in the tropics will have been removed.

As regards the diseases of hot climates which severely affect most Europeans, experience has largely shown that tropical countries are much more habitable in established settlements than they were to travellers and to the earlier settlers who were destitute of wholesome comforts. Sir Bartle Frere laid much stress on this, and quoted striking instances of it in India, in his memoirs on Eastern Africa.

Sanitation has within very recent years improved the life rate of our soldiers in India, so much so that the proportion who die annually is stated to be only one-quarter as great as it was a few years ago, their death rate now lying between 15 and 17 per thousand, while before the Crimean War it was between 60 and 70 per thousand.

There is I presume little chance of mere acclimatisation producing much effect in a few generations, or of an acquired capacity of withstanding tropical disease being transmitted hereditarily to descendants. The successful settlement of tropical countries seems to depend on "accidental" varieties of our race being found able to thrive in them. There is a marked difference between the power of different Englishmen to withstand, for example, the effects of African climate. It has been a prominent feature among the successful explorers of that country that although they may frequently suffer from fever, it takes no permanent hold upon their constitution. It is clear that men possessing such natural peculiarities, have a far better chance than others of naturalising themselves and their descendants in tropical homes. There is therefore some hope of vigorous varieties of the English race being found able to establish themselves in our tropical possessions. The process would be effected least wastefully to life, through a step-by-step fashion; emigrants from families already thriving in subtropical countries being likely to include a much larger proportion of individuals capable of thriving in still hotter climates than those coming directly from England.

Much has recently been written on the difficulty of any rare accidental variety of animal or plant establishing itself, when it has unrestricted opportunity of intercrossing with the parent stock. It is urged that the peculiarity would be halved in each successive generation, and would very soon cease to be apparent in the descendants. It seems to me that this argument is sometimes pressed too far. It cannot be a general truth that characteristics blend, else, to take a conspicuous example, there would be a growing tendency in every mixed population for the eye-colour to become of a uniform hazel or brown gray tint, through the intermarriage of persons whose eye-colours differ widely. On the contrary, I have lately shown by a considerable body of statistics[1] that among the English, the proportions between the eye-colours, as sorted under seven headings, has not changed at all during four generations. The fact is that heritages are only partially liable to be blended together; partially they are mutually exclusive. No case of inheritance probably falls altogether under either of these opposed extreme conditions, but some approximate to one, and others to the other. I am not aware that the respective results of these two extreme conditions have yet been put forward quite as forcibly as they admit and deserve to be.

[1] "Family Likeness in Eye-Colour," "Proc. Royal Soc.," 1886.

I will explain what I mean by rude but sufficient illustrations. Let us suppose a black population with a single white individual in it, and endeavour to trace the tints of his descendants under each of the two ideal conditions of completely blending and of mutually exclusive heritages. We will reduce the problem to its simplest form by assuming that intermarriage with the parent stock is the rule, and that there is no change in the vitality or the fertility of the hybrid offspring. It will be best to begin by supposing each pair to leave just two children to succeed them. Let us, for illustration sake, imagine a large number of similar glasses, each intended to represent a single individual, and the tint of their contents to represent those of the persons to whom they severally refer. In illustrating the effect of perfectly blending inheritance we have merely to mix a glass full of black fluid with a glass full of white fluid and to pour the mixture into two other glasses which represent the two children. That mixture will be of course the same in both, and of a pure mulatto tint. Repeating the process with each of the two glasses we obtain four glasses all of quadroon tint, then eight of octoroon tint, and so on. All this is plain enough; but now let us take the case of mutually exclusive heritages. I will represent the tint of each individual by a cylinder that just fills the glass. There will be a large number of glasses each filled with a black cylinder and one with a white cylinder. We will now treat their contents in the same outward form as before. We mix, that is, we throw and shake together in a separate jar the contents of the two glasses, namely, a white cylinder and a black cylinder, and then fill two other glasses from out of the jar. The contents of these two glasses will not be mulatto, but one of them will be pure white and the other pure black. We repeat the process and obtain four grand children, one of whom will still be of unmixed white and the other three of unmixed black; we repeat it again and obtain eight grand-children, one of whom will be pure white and the other seven pure black, and so on for any number of generations, the one white cylinder appearing unchanged in every one of them

It would be tedious and of little profit to endeavour to modify this rude but distinct illustration so as to apply to families of varying numbers of children. In some cases the offspring would fail and the race of the white cylinder would come altogether to an end, in others it would be prolific and increase. In all cases the broad fact remains conspicuous that when heritages are mutually exclusive a rare variety may have numerous chances of establishing itself, one in each of many successive generations. Until it is wholly abolished, it will present itself again and again for competitive examination without diminution of vigour, and if it has natural advantages over the general population it has a corresponding number of chances of profiting by them. The conditions are far different with the heritages that blend. In these cases the peculiarity of one parent is diluted to half its amount in the very first generation, so that under the most favourable supposition of the offspring of that parent mating together and never mixing their blood with outsiders, and of not suffering from this close interbreeding, they would only be mulatto. No more than one-half of the original peculiarity of the one black parent could possibly become an established characteristic.

It is between these two extreme conditions that the facts of inheritance really lie. They might be roughly illustrated by supposing each of the glasses to contain neither a volume of fluid nor yet a single cylinder, but a moderate number of large beads partly strung together as on a broken necklace, from which some fall off each time it is handled; but I will not pursue this illustration further. Suffice it to conclude that the establishment of a somewhat rare variety as that of white men naturally suited to thrive and multiply in tropical climates, is not so great an improbability as those anticipate, who lay exclusive stress on the tendency of rare peculiarities to disappear in a very few generations, through free intermarriage with the ordinary members of the original stock.

Obituary Notice of the late Professor Busk.

By the death on the 10th of August last of Mr. George Busk the Institute has lost one of its oldest and most valued members.

He was born on the 12th of August, 1807, at St. Petersburg, being the second son of Mr. Robert Busk, an English merchant residing in that city. He early devoted himself to the study of surgery, entering as a pupil at the Medical School, which had at that time a considerable reputation, established in Aldersgate Street, near St. Bartholomew's Hospital. He became a member of the Royal College of Surgeons in 1830, and was elected an honorary fellow of that body in 1843. For many years he was Surgeon to the Seamen's Hospital established on board the Dreadnought, an old man-of-war moored off Greenwich, an office which he resigned in 1856. Although never in large practice. chiefly owing to the fortunate circumstance that he was not under the necessity of devoting himself to the drudgery of the prc fession, he acquired a considerable reputation as a scientific surgeon and made some important contributions to the advancement of surgical knowledge. It was, however, as a naturalist that he was best known to the world. His early predilection for microscopic research, and familiarity with the instrument at a time when it was in comparatively few hands, led him to select the lower forms of animal life, as the principal objects of his painstaking and accurate researches. The numerous memoirs which he published, especially upon the organization and classification of the polyzoa had already in 1856 made him so great a reputation that when in that year Sir Richard Owen resigned the Hunterian Professorship at the Royal College of Surgeons, which he had long held with great distinction, Mr. Busk was chosen by the Council of the College to succeed him. His strength, however, lay rather in investigation than in exposition, and his modest, retiring nature making public lecturing an uncongenial pursuit, after three years he resigned the chair. He did, however, admirable service to the college for many years, as a Member of the Council and of the Board of Examiners, and in 1871 was chosen to serve in its highest office, that of President. He was also an examiner in the University of London and the Army Medical Board; for many years Secretary to the Linnean Society, a member of the Council and Vice-President of the Royal Society, a Member of the Council and Vice-President of the Zoological Society, a Member of the Council of the Geological Society, Treasurer of the Royal Institution, a Member of the Senate of the University of London, Trustee of the Hunterian Museum, and one of the Governors of Charterhouse School. The number and variety of these appointments show the esteem in which his sound judgment, wide knowledge, excellent common sense, unwearied industry, and sterling integrity of character were held by his friends and colleagues.

For his numerous and varied researches in zoology, physiology, and comparative anatomy, the Royal Society in 1871 awarded to Mr. Busk a Royal medal, and he also received the Lyell and Wollaston medals from the Geological Society for his labours in palæontology, mainly the description of mammalian remains found in caves. It is, however, chiefly his work in connection with anthropology, a subject to which he devoted much of his time in the later years of his life, that must be spoken of here. He was elected a member of the old Ethnological Society in 1863, and soon after became one of its Vice-Presidents. In the negotiations connected with the fusion of that society with the Anthropological, which resulted in the formation of the present Institute in 1871, he took a considerable part. Of this body he was a Member of the Council from its foundation until the advance of illness about a year before his death compelled him to cease from attending. In 1873 he was elected President, an office which he served for two years with great advantage to the Institute, having been most assiduous in the discharge of its duties.

Mr. Busk's taste for anthropology appears to have been first roused by the opportunities for its study afforded by the seamen of the most varied races and nationalities who became patients at the Dreadnought Hospital; and a small collection of typical crania which he then formed, furnished the materials for commencing those investigations into the distinctive characters of the skulls of races, which will always be associated with his name. He was the first in this country who seriously attacked this difficult problem, and he expended a vast amount of careful observation and experiment in devising methods of measuring the external form and estimating the internal capacity of crania. Since he first took up this question, the science of craniometry has engaged the attention of numerous anatomists in all parts of the civilised world, and has made advances which naturally have left Busk's methods somewhat in the rear, but still the ingenuity of his modes of procedure, and the thoroughly scientific and conscientious spirit in which his investigations were carried on will never fail to meet their due recognition. A large work which he had for many years in hand, entitled "Crania typica," containing descriptions and carefully executed lithographic figures, either by his own hand or of that of one of his accomplished daughters, was never published; but the plates, as far as they were completed, have been deposited in the library of the Institute.

The following list of Mr. Busk's published memoirs in anthropological subjects will give some idea of the extent and scope of his researches in this branch of science.

1. "Observations on a Systematic Mode of Craniometry." "Trans. Ethnol. Soc.," I, 1861, p. 341.

2. Translation of Schaaffhausen, "On the Crania of the most Ancient Races of Man;" with remarks, and original figures, taken from a cast of the Neanderthal Cranium. "Nat. Hist. Review," 1861, pp. 155–176.

3. "Observations on some Skulls from Ceylon, said to be those of Veddahs." "Linn. Soc. Journ.," VI (Zool.), 1862, p 166.

4. (With Carpenter and Falconer). "An account of the proceedings of the late Conference held in France to enquire into the circumstances attending the reported discovery of a

Human Jaw in the gravel at Moulin-Quignon, near Abbeville; including the *Procès Verbaux* of the sittings of the Conference, with notes thereon." "Nat. Hist. Review," 1863, pp. 423–462.

5. "Note on the Skeleton found at Bennet Hill, Elgin." "Journ. Anthrop. Soc.," II, 1864, pp. 9, 10.

6. "On a very Ancient Human Cranium from Gibraltar." "Brit. Assoc. Rep.," XXXIV, 1864 (Sect.), pp. 91, 92.

7. "Account of the Discovery of a Human Skeleton beneath a bed of peat on the coast of Cheshire." "Trans. Ethnol. Soc.," IV, 1866, p. 101.

8. "Description of two Andamanese Skulls." "Trans. Ethnol. Soc.," IV, 1866, p. 205.

9. "Description of an Aino Skull." "Trans. Ethnol. Soc.," VI, 1868, pp. 109–111.

10. "Description of, and Remarks upon, an Ancient Calvaria from China, which has been supposed to be that of Confucius." "Journ. Ethnol. Soc.," II, 1870, p. 73.

11. "Supplementary Remarks to a note on an Ancient Chinese Calva." "Journ. Ethnol. Soc.," II, 1870, p. 156.

12. "Remarks on a Collection of Skulls from Rothwell, in Northamptonshire." "Proceedings Ethnol. Soc.," 1870, p. xci. [In "Journ. Anthrop. Inst.," I, 1872, Appendix.]

13. (With W. Boyd Dawkins). "On the Discovery of Platycnemic Men in Denbighshire." "Brit. Assoc. Rep.," XL, 1870 (Sect.), p. 148.

14. "Note on a ready method of Measuring the Cubic Capacity of Skulls." "Journ. Anthrop. Inst.," III, 1874, p. 200.

15. "Remarks on a Collection of 150 Ancient Peruvian Skulls, presented to the Anthropological Institute by T. J. Hutchinson." "Journ. Anthrop. Inst.," III, 1874, p. 86.

16. "Description of a Samoiede Skull in the Museum of the Royal College of Surgeons." "Journ. Anthrop. Inst.," III, 1874, p. 494.

17. "Notes on some Skulls from Palmyra, presented to the Institute by the late Mr. Cottesworth." "Journ. Anthrop. Inst.," IV, 1874, p. 366.

18. "Presidential Address to the Anthropological Institute." "Journ. Anthrop. Inst.," III, 1874, p. 499.

19. "Presidential Address to the Anthropological Institute." "Journ. Anthrop. Inst.," IV, 1875, p. 469.

20. "Notice of a Skull from Ashantee, and supposed to be that of a Chief or Superior Officer." "Journ. Anthrop. Inst.," IV, 1875, p. 62.

21. "Description of two Beothuc Skulls." "Journ. Anthrop. Inst.," V, 1876, p. 230.

22. "Notes on a Collection of Skulls from the Islands of Mallicollo and Vanikoro in the New Hebrides Group." "Journ. Anthrop. Inst.," VI, 1877, p. 200.

W. H. F.

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ADDRESS

DELIVERED AT THE

ANNIVERSARY MEETING

OF THE

ANTHROPOLOGICAL INSTITUTE

OF

GREAT BRITAIN AND IRELAND,

JANUARY 24TH, 1888.

BY

FRANCIS GALTON, F.R.S., President.

LONDON:
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Printers in Ordinary to Her Majesty.

1888.

ADDRESS *delivered at the* ANNIVERSARY MEETING *of the* ANTHROPOLOGICAL INSTITUTE *of* GREAT BRITAIN *and* IRELAND *January 24th*, 1888.

By FRANCIS GALTON, F.R.S., *President*.

ON behalf of this Institute, and sanctioned by their Council, I had the honour of delivering a short course of Lectures in December last, on *Heredity and Nurture*, at the South Kensington Museum. Their object was to test the reality of a supposed demand for information on such subjects, and so far as it was possible to judge from the results, there seemed to be a widely spread interest in the matter. It gives me pleasure to express my obligations to the Lords Commissioners of Education for the free use of their theatre, and to the many officers at South Kensington who aided in the various arrangements. Major Abney and General Festing exhibited in action their beautiful apparatus for testing the colour sense, which was described in the Bakerian Lecture before the Royal Society last year, and at the conclusion of each lecture Dr. Garson, Mr. Rudler, and Mr. Bloxam explained the working of the anthropometric instruments that were laid on side tables. Whether it be feasible for this Society hereafter to promote other lectures bearing on special topics in Heredity and Nurture, is a question on which I do not feel competent as yet to form an opinion, though I have no doubt that hopeful attempts to enlist popular interest in any branch of anthropology will always meet with your approval.

These lectures have led to at least one tangible result. I took the opportunity to reiterate my often expressed regret that no anthropometric laboratory existed in this country, at which children and adults of both sexes could at small cost have their faculties measured by the best methods known to science, and a record kept for their future use. I explained how difficult it would be to maintain such a laboratory, and to make it effective except under the shelter of some important institution, that was daily frequented by the class of persons likely to make use of it. Previously, I had applied for permission to erect such a laboratory at the South Kensington Museum, but the difficulties of a suitable position seemed insuperable. Thanks, however, to a recent suggestion of General Donnelly, and with his cordial aid, and also with that of General Festing, a successful application was made to Her Majesty's Commissioners of 1851 for a small portion of the Arcades, rent free, that adjoins the Western Galleries at South Kensington, containing the collection of scientific instruments, wherein to erect a wooden building for the laboratory. It will be connected with and have its only entrance from the gallery. The building has (at the time when I revise these pages) been completed under the obliging superintendence of General Festing, and is opened to the public, though as yet incompletely equipped. I append in a foot note a copy of the printed notice.[1] In one sense it is small, but it offers sufficient accommodation for the purpose immediately in view, which is little more than a development on

[1] Anthropometric laboratory for the measurement in various ways of human form and faculty. Entered from the Western Galleries containing the Science Collection of the South Kensington Museum.

This laboratory is established by Mr. Francis Galton for the following purposes :—

1. For the use of those who desire to be accurately measured in many ways, either to obtain timely warning of remediable faults in development, or to learn their powers.

2. For keeping a methodical register of the principal measurements of each person, of which he may at any future time obtain a copy under reasonable restrictions. His initials and date of birth will be entered in the register, but not his name. The names are indexed in a separate book.

3. For supplying information on the methods, practice, and uses of human measurement.

4. For anthropometric experiment and research, and for obtaining data for statistical discussion.

Charges for making the principal measurements :—Three pence each, to those who are already on the Register. Fourpence each, to those who are not :—One page of the Register will thenceforward be assigned to them, and a few extra measurements will be made, chiefly for future identification.

The Superintendent is charged with the control of the laboratory and with determining in each case, which, if any, of the extra measurements may be made, and under what conditions.

a more permanent basis of the anthropometric laboratory that I established in the International Health Exhibition of 1884, and at which nearly 10,000 persons were measured. I propose now to preserve copies of the records in such a form that the persons measured may always be able to refer to them so long as the laboratory exists. There will be one page of a folio register book assigned to each person in which the measurements made on successive occasions will be copied on successive lines, to show at a glance the personal development. No names will appear in the registers, but only initials and dates of birth; the names and the mothers' surnames will be entered in a separate book. There will be besides a brief list of questions, both personal and family, which the applicant for measurement will be invited to answer, one of them is whether the parents were first cousins. The copies of the measurements retained in the laboratory will be useful in two ways, the one as statistical documents, and the other as records always accessible under proper restrictions to the persons measured, or to their representatives. I conceive that this arrangement will facilitate the desirable, practice of keeping family records, because so far as members of any family may have been measured, it will be feasible, with their concurrence, to obtain copies of those measurements. I am by no means one of those who desire to confine anthropometry to the simpler physical data, but I wish to extend it as widely as the possibilities of measurements, however rough, may allow. Under judicious statistical treatment, rough measurements of many individuals are capable, as we all know, of yielding trustworthy results, and if we ascertain the degree of precision of our measurements, we can treat them individually on scientific principles, assigning to them their just weight, however small their precision may be. The off-hand measurements that can alone be made of a person who is only a few minutes under experiment, in respect to the delicacy of his senses, and of his reaction-times, are far better than none at all. They will at least serve to indicate such marked peculiarities as may merit more sustained examination.

The conditions of the laboratory admit only of measurements of the living person and in clothes, and we must make the best of these conditions. It would be undesirable to ask even that the shoes should be taken off. When persons of all ranks and of both sexes are admitted, and many operations have to be gone through in a brief time, it is necessary to measure those persons in their usual indoor clothing. Quite enough can be done under this restriction to furnish a record of the rate of growth and development of the young, and to yield statistical data of considerable value. We can at least record the eye colour; the length, breadth, and possibly the height of head; the stature in shoes less the thickness of the heel, the height above chair when sitting squarely in it, and the height of the knee above the ground; also the spread of the arms from finger tip to finger tip, the length of the middle finger, which is correlated with the length of the foot, and that from finger tip to elbow. These measurements give directly or inferentially the total stature and total arm-spread, and the respective lengths of the trunk and the two leg-bones; also the lengths of the upper and lower arm and of the middle finger. We also can easily and rapidly obtain the lung capacity, strength of squeeze with the right and left hand, keenness of sight with right and left eye, and the colour sense. More delicate apparatus will be at hand to be used occasionally, to test the remaining senses, the psycho-physical reactions, and such other physiological constants as may be found feasible and convenient to measure.

The curious memoir by M. Alphonse Bertillon in the "Annales de Démographie Internationale," republished as a pamphlet in 1881,[1] and the memoirs read at the International Penitentiary Congress at Rome in 1885,[2] by that gentlemen and by M. Louis Herbette, Director of the Penitentiary Department of the Interior,

[1] Une application pratique de l'anthropométrie sur un procédé d'identification, permettant de retrouver le nom d'un récidiviste au moyen de son seul signalement, &c. (G. Masson, Paris, 1881).

[2] "Les Signalements Anthropometriques." Conférence faite au Congrès Pénitentiare International de Rome (G. Masson, Paris, 1886).

and the very favourable remarks on M. Bertillon's methods by M. Paul Topinard, in the "Revue d'Anthropologie," of 1886, p. 607, and of 1887, p. 379,[3] suggest another use for an anthropometric laboratory. M. Bertillon showed that the various measurements of an individual might afford data of extraordinary value in deciding questions of identity. Ten or a dozen words easily transmissible by telegraph, could give a sufficiently exact description of a man to make it highly improbable that the same words would apply to any other out of many thousands of persons. The immediate object of M. Bertillon's method was to afford means of discovering whether an arrested person had been previously convicted. It is impossible for the French police to make effective search through the vast collection of photographs in their keeping, which is stated to have received an accession of 100,000 in number during the course of 10 years. He, therefore, suggested the plan of indexing prisoners according to their measurements, and this appears to be now done with considerable success. The service over which he presides is well installed and is in full work. The measurements chiefly relied upon were adopted after considerable preliminary experience and consideration in concert with M. Topinard, who speaks of M. Bertillon's method in the first of the passages above referred to, as "an ingenious system which experience has proved to be excellent, which I have seen in work, and have myself practised, and which I declare to answer its purpose perfectly." Independently of this application of anthropometry to rogues, it is clear that it may also be of service to honest men; I cannot do better than extract some phrases from M. Herbette's speech, as published in the French report of the Penitentiary Congress at Rome, already alluded to.

"S'élevant à des considérations d'ordre plus général encore et louant les heureux efforts de M. Bertillon, M. Herbette a montré comment cette constatation de la personnalité physique et de l'indéniable identité des individus arrivés à l'âge d'adulte,

[3] "Une visite à la Préfecture de Police au bureau des signalements anthropométriques" de M. Alphonse Bertillon.

doit répondre, dans la société moderne, aux besoins les plus réels, aux services les plus variés.

« Qu'il s'agisse de donner par exemple aux habitants d'une contrée, aux soldats d'une armée, aux voyageurs allant dans les pays les plus lointains, des notices ou cartes individuelles, des signes recognitifs permettant de déterminer et de prouver toujours quels ils sont ; qu'il s'agisse de compléter par des indications certaines les actes de l'état civil, d'empêcher toute erreur et toute substitution de personnes; qu'il s'agisse de consigner ces marques distinctives de l'individu dans les documents, titres, contrats, où sa personnalité doit être établie pour son intérêt, pour l'intérêt des tiers ou pour l'intérêt de l'État, le mode de signalement anthropométrique peut trouver sa place.

« Qu'il y ait certificat de vie, contrat d'assurance sur la vie ou parfois acte de décès à dresser, qu'il y ait à trouver, à certifier l'identité d'une personne aliénée ou grièvement blessée, ou défigurée, dont le corps aura été en partie détruit, ou sera devenu méconnaissable ou sera difficile à reconnaître, en cas de mort subite ou violente, à la suite d'un crime, d'un accident, d'un naufrage, d'un combat,—quelle ne sera pas l'utilité de tracer ces caractères invariables en chaque individu, infiniment variables d'un individu à l'autre, indélébiles au moins en partie, jusque dans la mort ?

« A plus forte raison aurait-on à s'en préoccuper s'il fallait faire reconnaître les gens à longue distance et à une longue durée d'intervalle, après que l'apparence extérieure, la physionomie, les traits et les habitudes physiques ont pu se modifier de façon naturelle ou artificielle, et cela sans déplacement ni frais, par simple échange de quelques notes ou chiffres à envoyer d'un pays à l'autre, d'un continent à l'autre, de manière à savoir aux États-Unis ce qu'est tel homme venu de France, et à établir si tel voyageur que l'on trouve à Rome est bien tel personnage qu'on a mesuré à Stockholm dix ans auparavant.

« En un mot, fixer la personnalité humaine, donner à chaque, être humain une identité, une individualité certaine, durable, invariable, toujours reconnaissable, et facilement démontrable, tel semble l'objet le plus large de la méthode nouvelle.

"On peut dire en conséquence que la portée du problème comme l'importance de la solution dépasse de beaucomp les limites de l'œuvre pénitentiaire et l'intérêt pourtant bien considérable de l'action pénale à exercer dans les diverses nations."

Whether all that was claimed for the power of M. Bertillon's system, on purely theoretical grounds and in his earlier publications can be sustained, may fairly be questioned; but there can be no doubt that a series of measurements must be of considerable service as supplementary evidence, either that a person is really the man he professes to be, or negatively that he is not the man for whom he is taken. In speaking of these matters it is impossible not to allude to the Tichborne trial, and the enormous waste of money, effort, and anxiety which might have been spared, had Roger Tichborne passed through an anthropometric laboratory before he went abroad. It would be a reasonable precaution for every person about to leave his country for a long time, having regard to the various accidents of good or ill-fortune, to be properly measured, and to leave a copy of his measurements in the safe keeping of an anthropometric laboratory.

It will doubtless be of interest to many if I should give here the principal details of M. Bertillon's system such as I have learnt partly from published memoirs, and partly from the obliging answers accompanied by useful illustrations that I have received from that gentleman in answer to my inquiries.

All the measurements and other remarks concerning each person are written opposite to printed headings, upon a thick card $5\frac{1}{2}$ inches square. The most convenient primary basis for classifying the cards is found to be not stature, but the head-length and the head-breadth, and in each case under the three-fold division of large, medium, and small. The limiting values of the measurements ranked as medium are so chosen that the number of large, medium, and small measurements shall be approximately equal. We thus obtain nine primary classes. Each of these is sub-divided according to a secondary classification of foot-lengths and of the middle finger-lengths of the left foot and left middle finger respectively, and as before under the threefold division of large, medium, and small. Thus there are nine secondary sub-divisions of each of the nine primary classes; that is eighty-one sub-divisions in all, to each of which is allotted a separate compartment in a large cabinet. Each of the cards is sorted into its appropriate compartment. The number of persons at present dealt with is such that there are an average of five hundred cards in each compartment. In each of the eighty-one compartments the cards are again sub-divided into nine tertiary groups by means of attached tickets, that project beyond the upper margin of the cards. They are of three different colours, according to whether the man is of large, medium, or small stature, and they are cut into different shapes, something on the plan of an A B C index, according to whether the person measured has a long, medium, or short arm, reckoning from the elbow to the middle finger tip of the left arm. Thus there are nine times eighty-one, or seven hundred and twenty-nine tertiary groups. There still remains the possibility of further sub-division on the same general principle.

It is found to be a rapid operation to scrutinise individually the small batch of cards to which this process of six successive sub-divisions, each with three categories, directs the search. It is also found that the cases are not so numerous as might be feared in which the nearness of the measurement to limiting values, makes it necessary to extend the search to many compartments, but on this point precise details are as yet wanting. There is also an absence of data from which the frequency of such cases might be theoretically inferred.

It appears to me that the problem of the easiest method of identification by measurements might be usefully furthered, if certain data existed which could be procured with little difficulty. Let us consider what it is with which we have to deal. It is the comparison of two fallible measures of a variable subject. The man who measures the subject in the first instance, is liable to error; the subject in the course of months or years is liable to vary; again, the second operator who measures him at the end of the period is liable to error. The data which we want for calculation are the "probable errors" of the two operators, whose compound effect could easily be formulated if they measured, say, a couple of hundred persons consecutively and independently. If there were three operators, A, B, and C, the series of differences between the measurements by A and B, by A and C, and by B and C, would enable us to easily disentangle the probable errors of each. Again we want more definite information than we as yet possess about the variability of the subject, after different intervals of time, and at different ages.

Another, and a very important question, is as to the degree in which the several bodily proportions that are measured may be looked upon as independent variables. The stature is related with the length of the foot, and with that of forearm, and we should expect a still closer relation to exist between any two of these taken together, and the third. We have yet to learn the proportion between the number of the elements measured, and their value for purposes of identification. The supposition that they may be treated as independent variables, which lies at the bottom of some of the earlier estimates, such as that in page 22 of the Conference at Rome, headed "Étendue infinie de la Classification," cannot be accepted as correct.

The whole subject of "Personal Identification and Description" forms an important chapter of anthropological research, and it is one on which I hope before long to be in a position to offer some views of my own.

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IDENTIFICATION BY FINGER-TIPS

EVERY one bears on his body a visible token of identity which has the unique value of persisting throughout his whole life. It apparently becomes fully defined some three months before his birth, and it remains unaltered after his death until the final stage of corruption. This token of identity lies in the system of ramification of the minute ridges that run across the palms of the hands and the soles of the feet, and it more especially resides in the scrolls or other patterns that the ridges form on the inner surfaces of the bulbs of the fingers. Attention will be directed almost exclusively to the latter in these pages, as they are amply sufficient in themselves for purposes of identification, while they are easy to print from and are conveniently isolated.

The utility of a sure means of identification cannot be doubted, if it admits of being easily applied to show either (1) that a man is the person he professes to be, or (2) that he is not the person whom he is suspected to be, or (3) that he is or is not included among the persons whose names and tokens are to be found in any given register. In criminal investigations the existence of such a method would settle questions of personation, of mistaken identity and of previous conviction. In the army and navy it would afford a sure means of convicting deserters and be a powerful deterrent from desertion. It would supply an invaluable adjunct to a severe passport system. It would be of continual good service in our tropical settlements, where the individual members of the swarms of dark and yellow-skinned races are mostly unable to sign their names and are otherwise hardly distinguishable by Europeans, and, whether they can write or not, are grossly addicted to personation and other varieties of fraudulent practice.

There remain other cases, that occur rarely, but when they do occur, are of sufficient importance to make it well worth the while of persons about to emigrate to take the small trouble of leaving their finger-prints behind them as a token of their identity. For in a large population like ours, whose members migrate to all quarters of the earth, the instances are numerous of men who, having left their homes in youth, find a difficulty on their return after many years, in proving claims to kinship and property. Or some alien scoundrel from foreign parts may assert himself to be the long-lost rightful claimant to an estate held in previous security by others on the supposition of his decease. Lastly, the important need often arises of performing the gruesome task of placing data on record that might afterwards serve to identify the unknown victim of an accident, as of the stranger who dies in hospital of a wound that left him speechless, of bodies washed up after a wreck, or of the other ghastly contents of a Morgue. If, then, a practical method could be devised which would be applicable to such cases as these, it would be of real value.

I shall in these pages describe one which I profess to be workable at once, even in its present comparatively crude form. I have no doubt that the experience of others would suggest improvements in details that have not as yet occurred to myself, though I have given a great deal of time to the subject and made a large number of experiments, and my own collection of analysed finger-prints now consists of many thousand specimens. I must now explain the nature of the markings of the fingers which appear in the prints about which I am writing. Then I must adduce evidence of the extraordinary persistency that is claimed on their behalf. Next I have to show the way of comparing two finger-prints in order to ascertain whether or no they were made by the same finger of the same person. After this I must describe how a pattern may be expressed by numerals with sufficient precision to sort it under its right heading. Lastly, I have to explain the best way of obtaining impressions from fingers, and to point out the professional persons who are well qualified and most likely to make it their business to take finger-prints and to preserve copies of them. Every one of this long list of requirements has to be fulfilled in a practical and efficient manner, otherwise there will be a weak link in the system as a whole, and it will fail to hold together. Finger-prints have been proposed over and over again before now as a means of identification, but no method of employing them has ever become definitely established, owing, as I believe, to failure in fulfilling these many requirements. No trustworthy evidence of their life-long persistence had ever been brought together and published, until by myself in a memoir read before the Royal Society some months ago (*Phil. Trans.*, 1891). No investigation had been made into what points are and are not suitable for comparison. No method of sorting patterns under heads had been brought forward that is comparable in its simplicity and exactitude with that which will be treated of here. It was communicated by me in a second short memoir read very lately before the Royal Society, and will soon appear in its Proceedings. Even the way by which finger-prints might be professionally made had not been thought out.

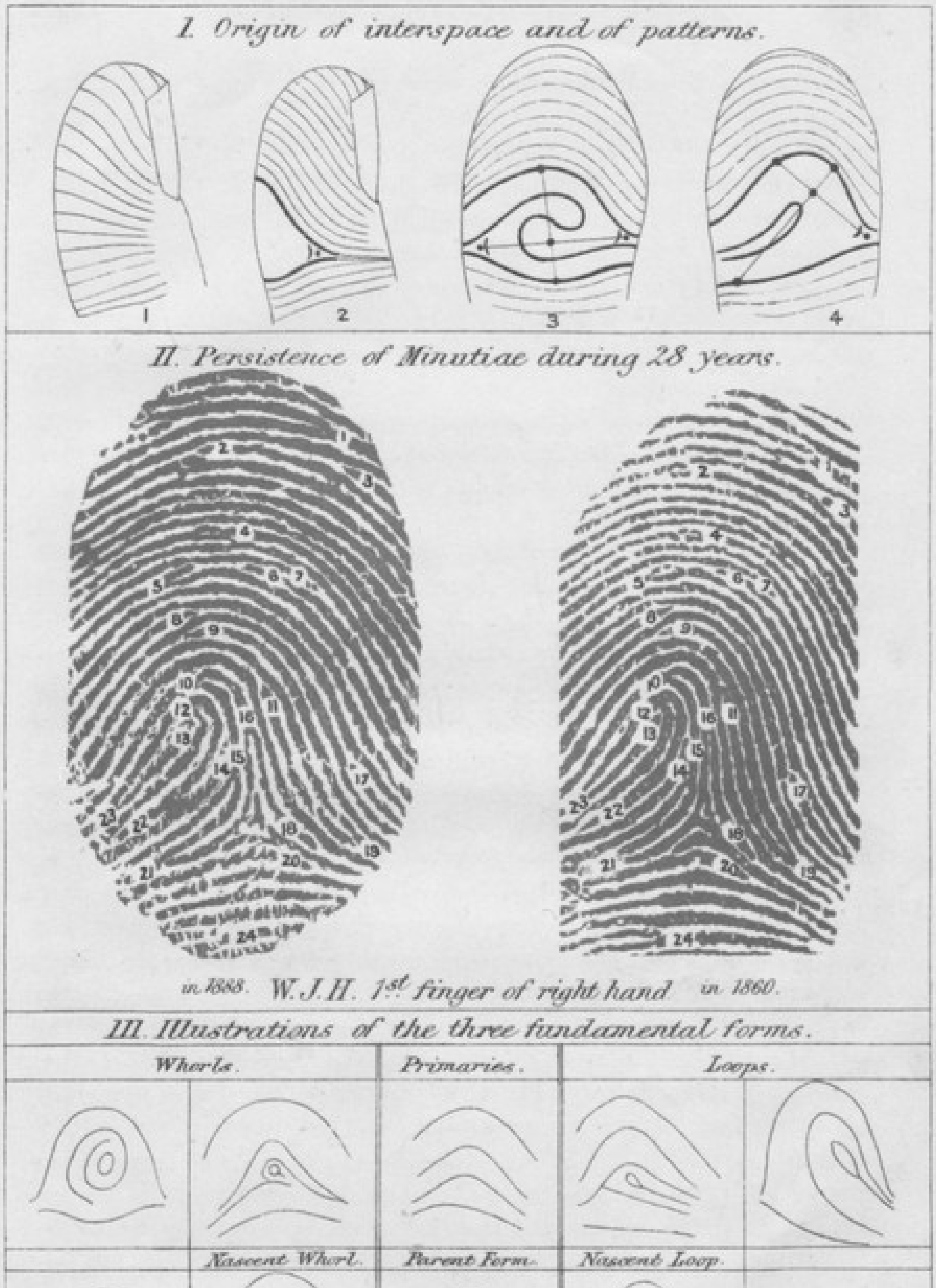
M. Alphonse Bertillon assures me that he does not use finger-prints in connection with his system of anthropometric identification which is now employed in the French criminal service. The often-repeated tale of its use in the prisons of China is baseless, so far as I can learn after repeated inquiries, or, if it is not entirely baseless, it certainly rests on a very limited foundation that I have not yet succeeded in discovering. The only person who has used the method on a large scale as a check against personation by natives, is Sir William J. Herschel, during the tenure of his magistracy in Bengal, which commenced between thirty and forty years ago. I am beyond measure indebted to the finger-prints collected by him there and subsequently, which have been minutely compared by myself with other finger-prints taken recently from the same persons. He has supplied me with all the material I possess for inquiring into the question of persistence, except one couplet, which consists of a set of impressions taken by a friend of mine from his own fingers seventeen years ago in sealing-wax and accidentally preserved, and of a similar set taken a few months since. Without the help of Sir W. J. Herschel I could not have planted my first step. Moreover, the quasi pocket apparatus that I employ for taking finger-prints is the same in its essentials as one that he recently devised and I copied.

The patterns (see Fig. II.) are formed by the convolutions of delicate ridges, each of which is seen to be studded with small holes, which are the open mouths of ducts issuing from perspiratory glands. As a rule the issues of all ducts are surrounded by slight elevations of the skin, but those on the inner surface of the hands and feet have the peculiarity of not being contained in separate elevations like craters in isolated cones, but of occurring along ridges, like the craters which stud the crest of some long mountain-chain. The ridges are based in a curious way, which I must not stop to describe, upon the subcutaneous papillæ in which the ultimate organs of touch are enclosed. The ridges seem to me to act in a somewhat analogous way to the whiskers of a dog or cat. A slight pressure at the end of a hair in the whisker causes a forcible pressure at the side of the sheath that holds it, which is easily felt. So the ridges engage themselves in the roughnesses of the surface that we explore by rubbing it with the fingers, as is our wont, and the result is to forcibly affect the organs of touch which lie below and to cause a sort of thrill, which varies according to the degree of roughness and enables us to discriminate it. We learn very little indeed of the nature of a surface by merely pressing the finger upon it; the ridges do not then come into play in the way I have described.

The reason why patterns exist in the bulbs of the fingers is to be found in the presence of the finger-nail. If it were not for the finger-nail, the ridges would run athwart the fingers up to its very tip, just as they do below. But the nail disturbs their parallelism, and squeezes them downwards at either side of the finger (Fig. I., 1, 2). Consequently the ridges that run close to the tip are greatly arched; those that successively follow are gradually less so, until in some cases (Fig. I., 1) the arch insensibly disappears about the level of the joint. Usually, however, the gradual transition from an arch to a straight line fails to be carried out, and there is a break in the sequence, and a consequent interspace (Fig. I., 2, 3, 4). The uppermost boundary of the interspace is formed by the lowermost arch, and its lowermost boundary is formed by the uppermost straight ridge. The same number of ducts exist within the interspace as are to be found elsewhere in an adjacent area of equal size, and their mouths require somehow to be supported and connected. This is done by an independent scroll-work of ridges, which forms the pattern (Figs. I. and II.).

Without a knowledge of these conditions, the pattern appears to be an intricate and undefined maze, as difficult to comprehend and to describe as are the ripple-marks on seasand. But as soon as the outline of the interspace is perceived, the confused effect suddenly disappears and is replaced by one of orderliness. The first thing that the eye should do in scrutinising a pattern is to satisfy itself generally as to this outline. The core is an untrustworthy guide.

The existence of an interspace implies the divergence of two adjacent ridges on one side at least of the finger, in order to embrace it. Just in front of the place where divergence begins, and before the sweep of the pattern is reached, there is usually a very short cross-ridge. Its effect is to complete the enclosure of a minute triangular plot, which affords a valuable position or rough point of reference. When there is a plot on either side of the finger, the line that connects them (Fig. I., 3) affords a base-line whereby the pattern may be oriented and the position of any point in it can be charted. If there be a plot on one side only (as in Fig. I., 4) the pattern has almost necessarily an axis which serves for orientation, and the pattern can still be charted, though on a different principle and in the way there shown. I shall not further pursue here the subject of charting. It is gone into at length in the memoir mentioned above, and is shown to lead to curious results which do not concern us now. What has been already said was merely to show the possibility of describing the position of any remarkable peculiarity by reference to the base-line and again to the outline of the interspace. The reason why I refrain here from making an exact use of the outline is that in such finger-prints as we are usually likely to deal with, the points of reference are often absent and can only be supplied inferentially. To ensure their being printed, the finger must be somewhat *rolled*, and not simply dabbed down as in the case of Fig. II. Although no exact use can be made in such cases of the outlines, an assurance of their existence and the possibility of roughly inferring the position of the


I. Origin of interspace and of patterns.
1
2
3
4
II. Persistence of Minutiae during 28 years.
in 1888. W. J. H. 1st finger of right hand in 1860.
III. Illustrations of the three fundamental forms.
Whorls.
Primaries.
Loops.
Nascent Whorl.
Parent Form.
Nascent Loop.

points of reference must always be borne in mind. The character of the pattern then becomes more clear, and it can be easily oriented. As soon as we are familiar with this way of viewing patterns, we rest satisfied that we have in all cases to deal with figures that are in reality sharply defined, and not with an undefined maze of ramifications and twists. I give a few specimen-outlines in Fig. III., to which I shall recur later on.

When a finger or a finger print is scrutinised under a lens, even of low power (I commonly use my eyeglasses as well as my spectacles, both of which are 12-inch focus, in place of a lens, putting them on together), it is seen to abound in minute peculiarities, due to the branchings of existing ridges and to the abrupt interpolations of new ones. It is in these minutiæ, as well as in the general character of the pattern, and *not* in the measured diameters of its outline, in which the extraordinary persistence resides on which I am about to speak. The pattern grows together with the finger, and its proportions vary with fatness or leanness, and are further deformed by usage, gout, and age, which make the hands of old people less sightly than those of young ones. But, though the pattern as a whole may become considerably altered in length or breadth, the number of ridges that concur in forming it, and their embranchments and other minutiæ, remain unchanged. So it is with the pattern on a piece of lace. The piece as a whole may be stretched in one way and shrunk in another, and its outline may be much changed; nevertheless, every one of the threads of which it was made, and every knot in each thread, can be easily traced. The stretchings and shrinkages draw adjacent threads slightly apart here and bring them closer together there, but those that were adjacent at the beginning remain so to the end. Not a stitch disappears, and not a stitch is added. Therefore, in speaking of the persistence of the marks on the finger, the phrase is intended to apply partly to the general character of the pattern but principally to the minutiæ, and not to the measure of its length, breadth, or other diameter, which are no more constant than the stature or any other ordinary anthropometric datum.

A small reservation will have to be made, but we must first show more clearly what these minutiæ are. The enlarged prints of the first finger of the right hand of Sir W. J. Herschel, made in 1888 and previously in 1860 (Fig. II.), will serve as a text. The originals of these prints were shown by me at a Friday evening lecture on 'Personal Identification' before the Royal Institution in 1888. An enlargement of them by a photographic printing process to double their size was printed both in *Nature* and in the *Transactions* of the Royal Institution. It came out well in sharp blacks and whites, so I selected it for a second double enlargement to illustrate the present pages, rather than any other of the couplets of original impressions of which I shall speak. Every one of the corresponding minutiæ now bears the same numeral in either print, and I have marked twenty-four of them altogether. Had space permitted I could have added a few more.

A new ridge is seen to be suddenly interpolated at 3, 7, 8, 9, 17, and elsewhere. An existing ridge is seen to bifurcate at 10, 15, 16, 24, and elsewhere. But, and here comes in the small reservation, an interpolation in the one may be represented by a fork in the other, as is seen clearly at 1 and in the small enclosures 4 and 5. In 4 the upper limb of the enclosure is a fork on the right side and open on the other, in the print of 1860, but not so in that of 1888; in 5 the difference between the two is still more marked. The reservation is that we must not be too particular about the apparent way in which a new ridge first arises. It may seem to be a fork or not, according to the depth of the printing, or owing to some minute alteration in the level of the ridge at its neck. The primary point is to assure ourselves of the place where a new ridge first makes its appearance; how it does so is a matter of secondary importance.

It is well worth while to carefully study these two prints, as they can tell much. We see that the lateral extension of the print made in 1888 is considerable, especially about the core, while every ridge which appears in the print of 1860 remains unchanged, and every peculiarity in each ridge remains unchanged also. The latter impression is also coarser and more worn than the earlier one. When searching for purposes of identification, a large number of prints that fall under the same general heading such as will be hereafter described, and that have a generally similar appearance, the quickest process is to fix on any one noticeable peculiarity in any one finger, such as 5 in Fig. II., and to confine the attention in the first instance solely to this, passing print after print successively under the lens to look for it, and taking a second test-point, such as 6, whenever the 5 test seems to be satisfied. A complete analysis can subsequently be made for satisfactory proof of identity.

I cannot in these pages adduce further evidence of the persistence of minutiæ, but must refer the reader to the memoir already mentioned, where he will find the photo-lithographs of eight couplets, including an equivalent to the present one, in which the second impression was made in 1890, not 1888. Those eight couplets yielded an aggregate of 296 points of comparison and *every one of them was found to hold good*. Since writing that memoir I have been able to examine many other couplets, and now possess those of one, two, or more fingers, and in some cases of the whole hand, of fifteen different persons. Among the couplets that I have analysed minutely, and usually after great enlargement, are the four right-hand fingers, and the ball of the thumb of the same person when he was a child of $2\frac{3}{4}$ years, and again when he was a boy of 15; the finger-prints of three persons when they were boys or girls, and again after about seven years in their early man or woman hood; the fingers of many persons when they were between the ages of 25 and 30, and again between those of 50 and 60. Lastly, the fingers of one man aged 63 and again when close upon 80. The total number of minutiæ thus compared amount to many hundreds, and in all this multitude I have found only one failure: it was in the first case, where the forked portion of one ridge in the child of $2\frac{3}{4}$ had become fused together by the time he was a boy of 15. At first, pains and patience were often required to thoroughly unravel the impressions, especially when they were partly blotted or imperfect, but the confidence that was soon acquired through experience greatly promoted the quickness of work.

For purposes of registration and reference, the finger-prints must be classified in some ready and sure way under their appropriate headings. After a great many trials, I have come decidedly to the conclusion that the most practical way is to base the method on a few easily recognised differences of pattern in each of the ten digits and not on many minute peculiarities in a single digit. The utility of the latter method is felt in a later stage and in the way already described. Almost every pattern can be sorted without hesitation under one of the three fundamental heads of Primaries, Whorls, and Loops (Fig. III.). Those few that cannot, may be judiciously forced to do so, just as the names of foreign places have somehow to be consistently expressed by the letters of the English alphabet. A system of expression has to be adopted and rendered generally intelligible by means of a small collection of standard instances. I will speak further on of transitional cases. We have thus far specified three fundamental distinctions, but every pattern that has an axis or a tail to it must be sloped, and there are only two possible slopes. The usual slope is from below upwards and inwards. The unusual or abnormal slope is from below upwards and outwards. This rule applies equally to both hands, either to the prints made from them or to the markings in the hands themselves. The words 'right' and 'left' are wholly inappropriate here. As each of the three fundamental patterns has two varieties, we thus obtain six possible headings, which are those that I use. A Primary is numbered 1 or 2 according to its slope, if any; a Whorl, 3 or 4; a Loop, 5 or 6. Abnormal slopes have the even numbers. Normal slopes and symmetrical patterns all have the odd numbers.

Again, for reasons of convenience, partly regarding the more limited number of digits that I use for hereditary and racial inquiries, I find it best by far not to enter the index-numbers appropriate to each digit in the order in which the digits lie, either in the print or in the hand, but as follows:—first, second, and third fingers of left hand; first, second, and third of right hand; thumb and little finger of left hand; thumb and little finger of right hand. The index-number of any pair of hands consequently takes a form such as

555, 353 ; 35, 35 ; which is my own index-number; and its print is sorted into the compartment that bears that heading.

The frequency varies greatly, with which different sequences of figures occur. I inserted a full analysis of 100 cases in the last of my two memoirs; the commonest sequence is that where all the fingers have plain loops; this has the index-number of *555, 555 ; 55, 55*. In the 100 cases there were seventy-one separate sequences each of which occurred only once; ten separate sequences each of which occurred only twice; one sequence occurred three times; and one, the above-mentioned loops, occurred six times. Hence we have

$$(71 \times 1) + (10 \times 2) + (1 \times 3) + (1 \times 6) = 100$$

The mere knowledge of an index-number that expresses the main characteristic of the pattern in each of the ten digits is therefore not to be despised. It may be compared to, though it is less exact than, the knowledge of a man's surname. If to this we add a somewhat more exact description of the several patterns as well as some one or two noticeable minutiæ on the principle mentioned a few pages back, we invest the token with considerable exactitude, which can quickly be turned into moral certainty by an extension of the process.

I do not find transitional cases to give difficulty. They raise the average number of references by about one-third, and not more. In saying so I do not speak at random, but after a great deal of experiment with movable catalogues. I have now acquired much facility in reading off the appropriate index-numbers from even bad prints. My assistant, Sergeant Randall, began to succeed after the first two or three days' trial. I am sure that the proposed method lies well within the powers of an ordinary clerk, supposing him to be properly instructed. He would be a little puzzled if he had no fuller guide than this brief paper affords.

It is easy to take good prints if the proper apparatus is at hand, but otherwise it is very difficult. I use—and Hawksley, the surgical instrument maker, 357 Oxford Street, now makes and sells—a little box three and a half inches square by seven and a half long, containing a slip of stout glass, a small and good printer's roller, a collapsible tube filled with *very fluid* printer's ink, a book of blank paper, and a phial of benzole and some rags to clean the fingers. A drop of ink is squeezed out of the tube on to the glass, and is spread very evenly and *very thinly* over it by the roller. Then the fingers are lightly pressed, first on the inked surface of the glass, and afterwards on smooth paper. Finally they are cleaned. With a quasi pocket apparatus of this kind, my assistant took, in one day, the impressions of the first three fingers of the right hands of no less than 336 school children. At my laboratory, now on the point of being re-established in the western gallery of the Science Collection in South Kensington, I used a larger apparatus, consisting of a copper plate 11 by 8 inches mounted on wood, with a printer's roller 5 inches long and 3 in diameter.

I have contrived a capital little folding-case of the size of a note-book, for occasional purposes. There are two zinc plates in it that are prevented by their frames from touching. These are blackened and will keep good for months if unopened. Only a few prints can, however, be taken from each without reblackening, but this is quickly done with the apparatus mentioned above.

The last part of our programme is to consider what professionals are likely to take to the occupation of finger-printing if a demand should arise for it, who are capable of doing it neatly and are at the same time everywhere accessible. I say the photographers. They are a class of men who are naturally gifted with dexterity of fingers, mechanical aptitudes, versatility, and some artistic taste. So far as they are engaged in portraiture, they already occupy themselves in supplying one means of identification; therefore the pursuit of another means of identification would in some sense lie within their present province. Photographers are also habituated to preserve registers and negatives of their photographs in an orderly way. Moreover they one and all crave for an extension of practice, as I judge from the letters I read in photographic newspapers. The photographers as a class would be well qualified to take finger prints neatly, which they would know how to mount artistically. They would also probably photograph the result. It is easy for them to try the process of finger printing. A piece of half-inch india-rubber tubing stretched over a wooden cylinder is a makeshift for a printer's roller that is not to be despised, and boiled or burnt linseed oil procurable at the oilman's, and mixed with a little fine soot that has collected on a plate held over a candle, makes a serviceable ink.

I look forward to a time when every convict shall have prints taken of his fingers by the prison photographer, at the beginning and end of his imprisonment, and a register made of them; when recruits for either service shall go through an analogous process; when the index-number of the hands shall usually be inserted in advertisements for persons who are lost or who cannot be identified, and when every youth who is about to leave his home for a long residence abroad, shall obtain prints of his fingers at the same time that the portrait is photographed, for his friends to retain as a memento.

FRANCIS GALTON.

FRONTIERS AND PROTECTORATES

It does not often occur to the home-keeping English citizen, who dwells securely behind his inviolable unchanging sea barriers, that the British Empire, in its largest sense, is largely surrounded by frontiers that are more movable, more debatable, and often no less exposed, than those of any other civilised State in the world. He knows the British Islands to be the citadel and treasury of a vast dominion; he does not always consider that this dominion has every kind of border, runs through almost every kind of country and climate, is confronted across its boundaries by neighbours of every sort and condition. Although on each Ash Wednesday the Anglican Church pronounces its annual curse upon the man who removes his neighbour's landmark, the Englishman has long been in the habit of pushing forward his own.

Now the landmarks of the national property are, of course, its frontiers; and I doubt whether many of us duly appreciate the continual widening of them that goes on, the processes by which the movement operates, its character and its consequences. The object of this paper is, first, to examine briefly that system of protectorates to which the incessant expansion of our territorial responsibilities is mainly due; secondly, to take a rapid survey of the frontiers of the British Empire on the Asiatic mainland; and, lastly, to make some remarks upon the general working and probable consequences of the system in other parts of the uncivilised world.

The system of protectorates has been practised from time immemorial as a method whereby the great conquering and commercial peoples masked, so to speak, their irresistible advance, and have regulated the centripetal attraction of a greater over lesser masses of territory. It was much used by the Romans, whose earlier relations with Asia and Africa were not unlike our own. The motives have been different—sometimes political, sometimes military, sometimes commercial; the consequences have been invariably the same. It is used politically as a convenient method of extending various degrees of power, of appropriating certain attributes of sovereignty, without affirming full jurisdiction. It has become the particular device whereby one powerful State forestalls another in the occupa-

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93

PHYSICAL INDEX

To 100 persons, based on their measures and finger prints.

(Set up in two parts as an experiment)

By FRANCIS GALTON, F.R.S., July, 1894.

PHYSICAL INDEX

To 100 persons, based on their measures and finger prints.
(Set up in two parts as an experiment).

By Francis Galton, F.R.S., July, 1894.

Part I. is based primarily on 5 measures, as in bertillonage; secondarily on finger prints.

Part II. Conversely.

The labels are printed from different arrangements of two sets of stereotype blocks, taken from the letterpress of a supposed criminal register in which the order of entry is that of registration.

The labels are also suitable for cutting up and pasting on cards, to serve as a card catalogue, each with one or more finger prints at its back for final identification.

A cabinet of 27 broad and shallow drawers measuring, over all, less than 12 inches in height and 4½ feet in width, would contain more than 100,000 of these small cards arranged as a catalogue.

Description of the Labels.

The first line of each label contains the title for alphabetical and numerical reference.

Its left portion expresses in an abbreviated form the results of the five measurements given in detail in the second line; its right portion defines the patterns (arch, loop or whorl) of the ten digits. The method of abbreviation is described further on.

The second line gives the actual measures (in millimetres) of 5 dimensions, in the following order:—(1) head length, (2) head breadth, (3) extreme breadth between cheek bones, (4) length of left cubit, (5) length of left middle finger.

The third line relates to peculiarities in the prints of the following 6 fingers, and in this order—fore, middle and ring fingers of the right hand; then those same fingers of the left hand.

The fourth line contains—initials, dates of birth and of measurement, and register number.

Abbreviations used in the Title.

A, L, W signify Arch, Loop, and Whorl; U and R are used in the forefingers only, for loops, according to their direction downwards, whether to the ulnar, or little finger side, or to the radial, or thumb side.

Drawing do

a, l, w signify short, medium, or long. The limits here used for the medium values are, for (1) 191 to 196 inclusive, (2) 150 to 155, (3) 129 to 136, (4) 450 to 464, (5) 113 to 116. Measures below these are reckoned as short, above these as long.

The further abbreviations are:—

1 = a a or AA, 2 = a l or A L, 3 = a w or AW;
4 = l a or LA, 5 = l l or L L, 6 = l w or LW;
7 = wa or WA, 8 = wl or WL, 9 = ww or WW.

Example—64 w U 5 A 5, 52; means, medium, long; medium, short; long.—ULL, ALL; LL, AL. The order of the measures has already been described, also that of the first six finger prints; the remaining four (as LL, AL) refer to the thumb and little finger of the right hand, then to those of the left.

These abbreviations may seem at first to be complex, but they are in reality simple. They are used not only to save space, but chiefly as a great relief to the eye and memory when making references.

Explanation of the Suffixes,

which are printed as letters and figures in the third lines of the labels

? used

a   is equivalent to "this pattern might also be called an arch."

b   partially burnt by fire or chemicals, or so spoilt by work as to leave granulations in the place of ridges.

C   a spiral whorl that does not narrow towards the core, but has blunted ends.

D and —

d   the finger is so damaged that a legible print can hardlybe made.

g   the whorl has a large core.

k   the inner part of the pattern is hooked, more or less.

ko   the whorl has a coil inside.

l   "this pattern might also be called a loop."

? use this

m   the pattern is minute, so small that two specimens of its characteristic portion would occupy less space than that covered by a single dabbed print.

o   well defined rings about the core of a whorl.

p   compound ; as when two loops lie one above the other.

q   a spiral whorl that narrowing towards the core, is twisted up to a point, or nearly so.

r   opening towards the radial, or thumb side.

s   opening towards both the radial and the ulnar sides. The whorls that do so are often traversed by a sinuous line of ridges.

t   tent-shaped ; commonly used with *a* ; thus *at* is a tented arch.

u   opening towards the ulnar or little finger side. As this is by far the most frequent direction it is not noted, but assumed, except under special circumstances.

v   invasion by a blunted system of ridges. Strictly speaking, they should proceed from and return to the contour of the pattern, but latitude is allowed when the v formation is very distinct.

w   "the pattern might also be called a whorl."

y   the core is eyed like a needle, or like the rim of a racquet.

z   the finger is deficient.

~~!~~   scarred by a cut or otherwise ; or it may be partly deficient.

†   badly scarred as above.

*   smashed.

:   In cases of Loops means

The figures express the number of ridges in a loop that lie between the outer and inner termini as defined below. The terminus from which the count begins is reckoned as o; the count proceeds in a straight line up to and including the other terminus.

Outer terminus—The contour of the loop may begin with (*a*) a ridge that splits into two or (*b*) by two adjacent ridges that suddenly diverge, in which case a dot usually lies in front of the place of divergence.

(*a*) The point of bifurcation is the terminus. When there is doubt between two or more bifurcations, the one to be selected is that which is nearest to, but *not less than*, a right angle.

(*b*) The dot or nearest ridge, in front of the divergence is the outer terminus.

Inner terminus—The core of the loop may consist of (*c*) an uneven or (*d*) an even number of ridges.

(*c*) The top of the central "rod" is the inner terminus.

(*d*) The further shoulder of the central "staple" is the inner terminus, the nearer shoulder counting as a separate ridge.

PART I.

This consists of four separate pages; in each page the labels are arranged in numerical and alphabetical order :—

(1) of the measures, as shewn by the left-hand portion of the title.

(2) of the finger prints, as shewn by the right-hand portion of the same title.

The subsequent entries aid in the differentiation of persons who bear the same title.

FRANCIS GALTON

Adult males, measured or remeasured April 16 to May 9, 1894.

13 l W9 W6, 99 185 149 126 470 115 — — — ‖ o vy — F.C.F. 1868-94 6544	51a W6 W5, 88 194 152 128 439 104 r — — ‖ ro — — H.G. 1867-94 560	65w U5 R5, 55 194 158 131 456 117 13v 14 v ‖ 7k — — H.R. 1850-94 3837
16a U5 U5, 55 189 147 130 465 109 13 11! -- ‖ 12 — — E.A.M. 1873-94 5339	53w R6 W6, 85 194 154 125 467 119 yw — — ‖ r — — A.A.B. McF. 1861-94 6512	71a R5 R5, 55 197 149 126 420 107 8 13 — ‖ 7 — — A.G.S. 1863-94 6509
16w W9 U9, 55 184 148 129 489 122 rm o g ‖ — — g M.W.T. 1872-94 6504	54a W5 U5, 58 196 155 135 435d 109d vg — — ‖ — — — F.C. 1857-94 6533	73 l R6 U5, 98 197 148 128 478 114 — — — ‖ 13 — — G.L.W. 1877-94 6546
24a U5 U5, 55 186 151 129 447 111 14 21 — ‖ 14 — — F.H.G. 1874-94 6537	54 l A2 R5, 85 191 152 135 433 113 — tr — ‖ 7 — — T.A.L. 1859-94 250	79w R6 A2, 85 199 145 137 527 130 — rk — ‖ r — — E.A.DuC. 1872-94 2290
26w X3 U6, 85 189 151 132 471 119 d y r ‖ 3 — y A.J.R. 1842-94 11	55w W6 W9, 98 194 153 131 458 119 — 18 0 ‖ s — — C.H. 1825-94 6551	86w R9 U5, 55 206 151 134 499 126 — ay y ‖ 8 — — W.B.D.E. 1867-94 148
39a W6 U5, 88 181 156 147 466 111 — — — ‖ — — — F.G. 1822-94 12	56a U6 U6, 65 196 151 133 440 109 9 — — ‖ 11! — — G.H. 1850-94 6543	89w U5 R5, 55 203 151 137 486 121 6 16 — ‖ 2a — y C.J.E. 1870-94 6547
41a W6 U5, 55 196 145 124 441 107 rm — m ‖ 12 — — G.E.W.B. 1874-94 4554	56 l W6 W9, 55 191 150 135 478 122 r — — ‖ s — — P.B.T. 1873-94 6505	94 l U6 R5, 55 199 156 130 436 114 1a — — ‖ 4 4 — W.J.N. 1873-94. 2827
44 l A5 U5, 85 192 148 134 447 115 2uy — — ‖ 5 — — P.J.H. 1874-94 4576	59w W5 R9, 88 192 150 139 467 118 — — y ‖ — — — A.E.L. 1866-94 6565	97a R6 R3, 88 201 165 143 440 112 3 3 — ‖ 3 2r — S.L. 1867-94 5508
48a R9 R6, 56 192 149 140 451 112 y — — ‖ — — — H.F.O. 1873-94 6558	63w U5 R6, 55 196 159 125 489 122 13 — — ‖ — ! g W.M.D.E. 1874-94 5181	99w U6 W5, 55 201 157 157 509 124 ! — y ‖ r 4 — W.S. 1824-94 6561
49w W6 W5, 56 195 149 139 473 119 — — — ‖ ro — y P.J.B. 1873-94 6568	65 l A2 A2, 55 192 163 135 461 115 — 1 — ‖ 2 — — A.W.A. 1841-94 3818	99w R5 A5, 55 200 159 155 474 117 19 — — ‖ t — — G.F.O'D. 1858-94 6536

Adult males, measured or remeasured May 9 to May 22, 1894.

15 l R5 R5, 85	59 w U6 U6, 55	86 w A2 A1, 22
188 147 133 462 115	194 154 142 483 121	199 150 132 470 118
— y! — ‖ ra — —	— — — ¦ — — —	— — 3 ¦ — ! —
J.P.J. 1869-94 6592	C.E.B. 1841-94 6596	R.M. 1857-94 6603
15 l U5 U6, 55	64 w U5 A5, 52	88 l WXL W5, 85
186 147 131 452 113	195 156 131 445 117	199 154 138 455 113
3 — — ¦ t — y	— — — ¦ u1 3 —	— d — ¦ r ! yv
C.H.C. 1865-94 6571	P.A.C. 1866-94 966	A.D. 1871-94 278
27 a U5 U6, 55	67 a R6 U5, 85	89 l R6 R5, 56
187 150 140 434 105	195 159 139 432 111	199 155 138 465 115
! — y ¦ ! — --	2 — y ¦ — — —	7 — ly ‖ — t —
J.S. 1866-94 6591	G.R. 1876-94 6560	J.P. 1864-94 6584
29 w W9 W9, 98	69 w R6 W6, 95	89 w R5 R5, 52
190 153 140 487 117	193 168 140 491 121	197 154 137 499 123
— m — ¦ — r! —	— — — ‖ p — —	2 — — ‖ 2 ! —
A.S. 1867-94 6539	W.H.F. 1831-94 136	F.F. 1853-94 6555
37 l W6 W5, 68	69 w U5 R5, 55	89 w R5 R5, 55
187 158 139 449 113	190 162 145 482 119	206 152 138 481 118
r — — ‖ r — vy	3 — v ‖ 5 — v	11k 2 y ‖ 2 — —
R.K. 1867-94 6588	E.G.F. 1863-94 6602	A.T.C. 1872-94 1243
41 a U6 W6, 65	69 w U5 U5, 85	89 w R6 W6, 65
194 146 127 437 110	191 161 139 470 122	200 154 141 501 123
v — — ‖ r — —	8 7 — ‖ 7 — —	— — r ‖ s ! y
H.B. 1872-94 6583	C.C.F.M. 1867-94 162	R.A. 1869-94 2214
47 a W6 W6, 88	69 w W5 W9, 58	89 w W9 W9, 99
195 148 140 446 111	195 156 142 492 119	204 154 144 488 125
o v — ‖ o wv —	— — — ‖ g — —	r — — ‖ r — —
G-A. 1839-94 6578	J.C.B. 1850-94 6595	E.S. 1870-94 6580
48 l U6 U6, 98	78 w W9 W9, 89	97 a U5 A2, 58
196 147 137 460 115	199 151 142 464 119	200 156 140 425 107
14 — — ‖ 11 ! — —	r r — ¦ r r —	8 — — ‖ — — —
C.T. 1840-94 4199	W.W.A. 1868-94 6606	A.M.B. 1848-94 967
58 a A6 R5, 88	79 l W6 W5, 55	98 l U6 U6, 55
196 153 138 454 111	203 149 137 479 116	198 157 145 454 116
y v y ¦ — k —	r — — ¦ r — y	10 — — ¦ 14 — v
G.K. 1862-94 6590	G.S.E. 1869-94 155	J.W. 1873-94 6569
59 w A3 A2, 22	82 a W6 W5, 55	99 w W6 W6, 55
195 150 148 486 122	198 151 127 452 111	204 160 142 500 129
— — ly ‖ — — y	r — — ‖ r — —	r — — ¦ ko — —
W.L.J. 1852-94 2591	W.E.R. 1873-94 6605	A.M.A. 1873-94 6589

Adult males, measured or remeasured May 22 to June 4, 1894.

11 a A 5 U 5, 55	56 w R 6 W 5, 85	87 a R 5 R 5, 55
190 145 130 435 108	196 153 134 480 130	203 153 141 443 108
1 — y! ‖ — — —	vy — — ‖ s — v	8 — — ‖ 3 — —
W.H. 1860-94 6641	B.C.P. 1872-94 6617	W.F.H. 1857-94 6628
12 l U 2 A 2, 55	59 a U 5 U 6, 98	87 a U 5 R 5, 55
181 138 122 457 115	196 152 139 455 110	197 152 138 439 110
3 — — ‖ — 2u —	— — yr ‖ y ! —	5 ! 6 ‖ — — —
R.M.W. 1871-94 5409	H.P.F. 1843-94 6651	J.J.G. 1864-94 6622
14 l U 6 U 5, 85	67 a U 6 U 2, 85	87 a W 9 W 9, 96
187 139 130 440 115	191 158 137 427 106	201 155 144 442 115
— — — ‖ — — —	Burn — y ‖ 3 1 —	r — — ‖ r s —
W.B.D. 1869-94 4407	J.W.E. 1874-94 6636	S.M. 1865-94 2899
14 l W 6 U 5, 98	74 a R 5 W 5, 55	87 l U 5 U 5, 52
188 148 132 445 115	198 149 133 434 110	198 155 140 442 113
r — — ‖ — — —	y — ! ‖ y a —	— — wy ‖ — 3 wy
O.P.M. 1872-94 5652	R.M. 1859-94 6627	W.J.J.L. 1854-94 6626
34 a U 5 R 5, 55	75 a U 5 U 5, 55	88 w A 5 W 5, 88
189 156 136 420 102	197 143 132 453 112	199 151 138 453 120
5 — — ‖ — — —	16 — y ‖ — — —	— — — ‖ Kor — y
E.H. 1869-94 6643	F.O'D.H. 1865-94 4687	H.D.M. 1870-94 6652
41 a R 5 R 5, 85	75 w U 6 R 6, 88	89 w W 9 W 9, 99
192 139 123 431 108	199 149 134 459 119	205 151 140 490 129
— 1a v ‖ — — —	! — — ‖ Kv — !	r s — ‖ r s r
H.P. 1866-94 6614	A.E.B. 1870-94 5560	H.A.P. 1870-94 3589
46 w A 5 U 5, 88	84 l A 5 R 5, 86	97 a R 9 W 9, 55
190 148 129 476 121	199 149 134 442 115	205 156 139 445 111
1r — — ‖ — — —	2r — — ‖ — — —	K r — ‖ s r s
C.O. 1870-94 6611	T.S.H. 1873-94 6635	J.C.M. 1842-94 6625
46 w W 6 W 8, 85	86 l W 9 W 9, 98	99 l U 5 A 6, 85
196 146 132 470 121	199 154 136 479 115	201 162 147 474 115
r rat — ‖ r r —	r — — ‖ r sl —	— — — ‖ t 3 —
H.W.C. 1872-94 6618	R.M. 1860-94 6648	J.A. 1872-94 6640
52 w W 9 W 9, 98	86 w A 6 W 5, 55	99 l W 9 W 8, 88
195 151 125 450 117	203 153 132 469 118	198 157 141 466 115
— — — ‖ g — —	r y — ‖ r — y	r ! — ‖ r! s y
F.A. 1827-94 6613	P.T. 1873-94 2166	O.T.M. 1863-94 6632
54 w R 9 W 6, 55	87 a R 5 R 2, 88	99 w R 8 R 5, 85
195 153 135 432 118	201 153 140 415 110	206 160 147 487 122
v ry — ‖ r — —	— — — ‖ — t! —	— — — ‖ — — —
H.V.R. 1860-94 6642	H.S.K. 1860-94 6615	A.J.H. 1871-94 6645

FRANCIS GALTON

Adult males, measured or remeasured June 4 to June 9, 1894.

14 a W 9 W 9, 58 185 142 133 430 111 r — — ‖ r s ! W.H.W. 1870-94 6662	17 a U 5 U 6, 55 184 147 143 440 110 — — y ‖ y – m H.R.F. 1837-94 6632	54 a A 1 A 2, 52 196 150 136 437 110 — — — ‖ — — 2a H.J.W. 1869-94 2301
57 a A 5 U 5, 55 196 153 137 438 108 t — — ‖ — — — E.L.R. 1864-94 6654	68 l U 5 A 5, 55 196 156 140 452 115 v6 — — ‖ ! — — W.G.G. 1849-94 3765	68 l W 6 U 5, 95 192 157 143 460 114 r — — ‖ — — y E.L.C. 1863-94 6665
69 a W 9 W 9, 99 191 161 142 469 111 — — — ‖ Coil s s O.F.R. 1874-94 4702	77 a W 9 W 9, 98 204 148 143 448 114 r r — ‖ r — — J.F. 1857-94 6660	85 l R 5 R 5, 55 198 155 134 458 115 16* — — ‖ v! — — A.G.M. 1868-94 6667
88 a U 6 W 5, 55 199 152 142 454 110 — — — ‖ — ! y E.P.T. 1859-94 6665		

PART II.

This consists of four pages in which the labels are arranged consecutively in alphabetical and numerical order:—

(1) of the finger prints, as shewn by the right-hand portion of their titles.

(2) of the measures as shown by the left-hand portion of the same titles.

The subsequent entries aid in the differentiation of persons who bear the same title.

PHYSICAL INDEX

TO 100 PERSONS,

Based primarily on their finger print, and secondly on their measures.

By FRANCIS GALTON, June, 1894

54 a A1 A2, 52
196 150 136 437 110
— — — || — — 2a
H.J.W. 1869-94 2301

86 w A2 A1, 22
199 150 132 470 118
— — 3 | — ! —
R.M. 1857-94 6603

65 l A2 A2, 55
192 163 135 461 115
— 1 — | 2 — —
A.W.A. 1841-94 3818

54 l A2 R5, 85
191 152 135 433 113
— tr — || 7 — —
T.A.L. 1859-94 250

59 w A3 A2, 22
195 150 148 486 122
— — ly || — — y
W.L.J. 1852-94 2591

84 l A5 R5, 86
199 149 134 442 115
2r — — || — — —
T.S.H. 1873-94 6635

11 a A5 U5, 55
190 145 130 435 108
1 — y! || — — —
W.H. 1860-94 6641

44 l A5 U5, 85
192 148 134 447 115
2uy — — | 5 — —
P.J.H. 1874-94 4576

46 w A5 U5, 88
190 148 129 476 121
1r — — || — — —
C.O. 1870-94 6611

57 a A5 U5, 55
196 153 137 438 108
t — — || — — —
E.L.R. 1864-94 6654

88 w A5 W5, 88
199 151 138 453 120
— — — | Kor — y
H.D.M. 1870-94 6652

58 a A6 R5, 88
196 153 138 454 111
y v y | — k —
G.K. 1862-94 6590

86 w A6 W5, 55
203 153 132 469 118
r y — || r — y
P.T. 1873-94 2166

R

99 w R5 A5, 55
200 159 155 474 117
19 — — | t — —
G.F.O'D. 1858-94 6536

87 a R5 R2, 88
201 153 140 415 110
— — — || — t! —
H.S.K. 1860-94 6615

89 w R5 R5, 52
197 154 137 499 123
2 — — || 2 ! —
F.F. 1853-94 6555

71 a R5 R5, 55
197 149 126 420 107
8 13 — || 7 — —
A.G.S. 1863-94 6509

74 a R5 W5, 55
198 149 133 434 110
y — ! || y a —
R.M. 1859-94 6627

85 l R5 R5, 55
198 155 134 458 115
16* — — || v! — —
A.G.M. 1868-94 6667

87 a R5 R5, 55
203 153 141 443 108
8 — — || 3 — —
W.F.H. 1857-94 6628

89 w R5 R5, 55
206 152 138 481 118
11k 2 y || 2 — —
A.T.C. 1872-94 1243

41 a R5 R5, 85
192 139 123 431 108
— 1a v | — — —
H.P. 1866-94 6614

15 l R5 R5, 85
188 147 133 462 115
— y! — || ra — —
J.P.J. 1869-94 6592

79 w R6 A2, 85
199 145 137 527 130
— rk — || r — —
E.A.DuC. 1872-94 2290

97 a R6 R3, 88
201 165 143 440 112
3 3 — | 3 2r —
S.L. 1867-94 5508

89 l R6 R5, 56
199 155 138 465 115
7 — ly || — t —
J.P. 1864-94 6584

67 a	R6 U5, 85	195 159 139 432 111	2 — y ‖ — — —	G.R. 1876-94 6560
73 l	R6 U5, 98	197 148 128 478 114	— — — ‖ 13 — —	G.L.W. 1877-94 6546
56 w	R6 W5, 85	196 153 134 480 130	vy — — ‖ s — v	B.C.P. 1872-94 6617
89 w	R6 W6, 65	200 154 141 501 123	— — r ‖ s ! y	R.A. 1869-94 2214
53 w	R6 W6, 85	194 154 125 467 119	yw — — ‖ r — —	A.A.B. McF. 1861-94 6512
69 w	R6 W6, 95	193 168 140 491 121	— — — ‖ p — —	W.H.F. 1831-94 136
99 w	R8 R5, 85	206 160 147 487 122	— — — ‖ — — —	A.J.H. 1871-94 6645
48 a	R9 R6, 56	192 149 140 451 112	y — — ‖ — — —	H.F.O. 1873-94 6558
86 w	R9 U5, 55	206 151 134 499 126	— ay y ‖ 8 — —	W.B.D.E. 1867-94 148
54 w	R9 W6, 55	195 153 135 432 118	v ry — ‖ r — —	H.V.R. 1860-94 6642
97 a	R9 W9, 55	205 156 139 445 111	K r — ‖ s r s	J.C.M. 1842-94 6625

U

12 l	U2 A2, 55	181 138 122 457 115	3 — — ‖ — 2u —	R.M.W. 1871-94 5409
97 a	U5 A2, 58	200 156 140 425 107	8 — — ‖ — — —	A.M.B. 1848-94 967
64 w	U5 A5, 52	195 156 131 445 117	— — — ‖ u1 3 —	P.A.C. 1866-94 966
68 l	U5 A5, 55	196 156 140 452 115	v6 — — ‖ ! — —	W.G.G. 1849-94 3765
99 l	U5 A6, 85	201 162 147 474 115	— — — ‖ t 3 —	J.A. 1872-94 6640
34 a	U5 R5, 55	189 156 136 420 102	5 — — ‖ — - —	E.H. 1869-94 6643
65 w	U5 R5, 55	194 158 131 456 117	13v 14 v ‖ 7k — —	H.R. 1850-94 3837
69 w	U5 R5, 55	190 162 145 487 119	3 — v ‖ 5 — v	E.G.F. 1863-94 6602
87 a	U5 R5, 55	197 152 138 439 110	5 ! 6 ‖ — — —	J.J.G. 1864-94 6622
89 w	U5 R5, 55	203 151 137 486 121	6 16 — ‖ 2a - y	C.J.E. 1870-94 6547
87 l	U5 U5, 52	198 155 140 442 113	— — wy ‖ — 3 wy	W.J.J.L. 1854-94 6626
16 a	U5 U5, 55	189 147 130 465 109	13 11! — ‖ 12 — —	E.A.M. 1873-94 5339
24 a	U5 U5, 55	186 151 129 447 111	14 21 — ‖ 14 — —	F.H.G. 1874-94 6537
75 a	U5 U5, 55	197 143 132 453 112	16 — y ‖ — — —	F.O'D.H. 1865-94 4687
69 w	U5 U5, 85	191 161 139 470 122	8 7 — ‖ 7 — —	C.C.F.M. 1867-94 162
15 l	U5 U6, 55	183 147 131 452 113	3 — — ‖ t — y	C.H.C. 1865-94 6571
17 a	U5 U6, 55	184 147 143 440 110	— — y ‖ y — m	H.R.F. 1837-94 6632
27 a	U5 U6, 55	187 150 140 434 105	! — y ‖ ! — —	J.S. 1866-94 6591

59 a U5 U6, 98
196 152 139 455 110
— — yr ‖ y ! —
H.P.F. 1843-94 6651

63 w U5 R6, 55
196 159 125 489 122
13 — — | — ! g
W.M.D.E. 1874-94 5181

94 l U6 R5, 55
199 156 130 436 114
1a — — | 4 4 —
W.J.N. 1873-94. 2827

75 w U6 R6, 88
199 149 134 453 119
! — — ‖ Kv — !
A.E.B. 1870-94 5560

67 a U6 U2, 85
191 158 137 427 106
Burn — y ‖ 3 1 —
J.W.E. 1874-94 6636

14 l U6 U5, 85
187 139 130 440 115
— — — ‖ — — —
W.B.D. 1869-94 4407

59 w U6 U6, 55
194 154 142 483 121
— — — | — — —
C.E.B. 1841-94 6596

98 l U6 U6, 55
198 157 145 454 116
10 — — | 14 — v
J.W. 1873-94 6569

56 a U6 U6, 65
196 151 133 440 109
9 — — | 11! — —
G.H. 1850-94 6543

48 l U6 U6, 98
196 147 137 460 115
14 — — ‖ 11 ! — —
C.T. 1840-94 4199

88 a U6 W5, 55
199 152 142 454 110
— — — ‖ — ! y
E.P.T. 1859-94 6665

99 w U6 W5, 55
201 157 157 509 124
! — y | r 4 —
W.S. 1824-94 6561

41a U6 W6, 65
194 146 127 437 110
v — — ‖ r — —
H.B. 1872-94 6583

W

59 w W5 R9, 88
192 150 139 467 118
— — y | — — —
A.E.L. 1866-94 6565

54 a W5 U5, 58
196 155 135 435d 109d
vg — — ‖ — — —
F.C. 1857-94 6533

69 w W5 W9, 58
195 156 142 492 119
— — — ‖ g — —
J.C.B. 1850-94 6595

41 a W6 U5, 55
196 145 124 441 107
rm — m | 12 — —
G.E.W.B. 1874-94 4554

39 a W6 U5, 88
181 156 147 466 111
— — — | — — —
F.G. 1822-94 12

68 l W6 U5, 95
192 157 143 460 114
r — — ‖ — — y
E.L.C. 1863-94 6665

14 l W6 U5, 98
188 148 132 445 115
r — — ‖ — — —
O.P.M. 1872-94 5652

79 l W6 W5, 55
203 149 137 479 116
r — — | r — y
G.S.E. 1869-94 155

82 a W6 W5, 55
198 151 127 452 111
r — — ‖ r — —
W.E.R. 1873-94 6605

49 w W6 W5, 56
195 149 139 473 119
— — — | ro — y
P.J.B. 1873-94 6568

37 l W6 W5, 68
187 158 139 449 113
r — — ‖ r — vy
R.K. 1867-94 6588

51 a W6 W5, 88
194 152 128 439 104
r — — ‖ ro — —
H.G. 1867-94 560

99 w W6 W6, 55
204 160 142 500 129
r — — | ko -- —
A.M.A. 1873-94 6589

47 a W6 W6, 88
195 148 140 446 111
o v — ‖ o wv —
G-A. 1839-94 6578

46 w W6 W8, 85
196 146 132 470 121
r rat — ‖ r r —
H.W.C. 1872-94 6618

56 l W6 W9, 55
191 150 135 478 122
r — — | s — —
P.B.T. 1873-94 6505

55 w	W 6 W 9, 98			
194	153	131	458	119
— 13	0	‖	s —	—
C.H. 1825-94				6551

16 w	W 9 U 9, 55			
184	148	129	489	122
rm o	g	‖	— —	g
M.W.T. 1872-94				6504

13 l	W 9 W 6, 99			
185	149	126	470	115
— —	—	‖	o vy	—
F.C.F. 1868-94				6544

99 l	W 9 W 8, 88			
198	157	141	466	115
r !	—	‖	r! s	y
O.T.M. 1863-94				6632

14 a	W 9 W 9, 58			
185	142	133	430	111
r —	—	‖	r s	!
W.H.W. 1870-94				6662

78 w	W 9 W 9, 89			
199	151	142	464	119
r r	—	‖	r r	—
W.W.A. 1868-94				6606

87 a	W 9 W 9, 96			
201	155	144	442	115
r —	—	‖	r ɔ	—
S.M. 1865-94				2899

23 w	W 9 W 9, 98			
190	153	140	487	117
— m	—	‖	— r!	—
A.S. 1867-94				6539

52 w	W 9 W 9, 98			
195	151	125	450	117
— —	—	‖	g —	—
F.A. 1827-94				6613

77 a	W 9 W 9, 98			
204	148	143	448	114
r r	—	‖	r —	—
J.F. 1857-94				6660

86 l	W 9 W 9, 98			
199	154	136	479	115
r —	—	‖	r sl	—
R.M. 1860-94				6648

69 a	W 9 W 9, 99			
191	161	142	460	111
¬ —	—	‖	Coil s	s
O.F.R. 1874-94				4702

89 w	W 9 W 9, 99			
204	154	144	488	125
r —	—	‖	r —	—
E.S. 1870-94				6580

89 w	W 9 W 9, 99			
205	151	140	490	129
r s	—	‖	r s	r
H.A.P. 1870-94				3589

Containing X D

88 l	WXL W 5, 85			
199	154	138	455	113
— d	—	‖	r !	yv
A.D. 1871-94				278

No 2

26 w	D X 3 U 6, 85			
189	151	132	471	119
d ɏ	r	‖	3 —	y
A.J.R. 1842-94				11

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With verbal corrections

From the PROCEEDINGS OF THE ROYAL SOCIETY, VOL. 55.

RESULTS DERIVED FROM THE NATALITY TABLE OF KŐRÖSI BY EMPLOYING THE METHOD OF CONTOURS OR ISOGENS.

BY

FRANCIS GALTON, F.R.S.

822

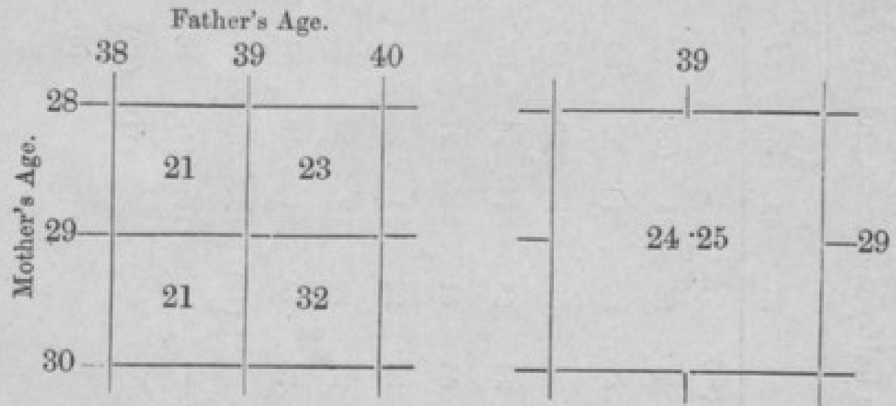
(It is Körösi throughout

[*From the* PROCEEDINGS OF THE ROYAL SOCIETY, Vol. 55.]

"Results derived from the Natality Table of Kőrösi by employing the Method of Contours or Isogens." By FRANCIS GALTON, F.R.S. Received January 12, 1894.

There are three variables in the statistics of natality. The age of the father is one, that of the mother is another, and the percental offspring of parents of those ages is the third. These three variables may be coordinated in the same way as that which is daily followed at meteorological offices in dealing with (1) the longitudes of the various stations, (2) their latitudes, and (3) the barometric height at each. After these data have been entered on a chart in their proper places, contours, known by the name of isobars, are drawn to show the lines of equal barometric pressure. In natality tables, the ages of the father and the mother take the place of the longitudes and latitudes in weather charts, and lines of similar birth rates, or as I would call them, "isogens," take the place of isobars.

Table I contains the means of each set of four adjacent entries as shown by the arrangement below, the left-hand diagram showing the four entries, and the right-hand one showing their mean. The



entries themselves were copied to the nearest integer from Kőrösi's tables. The means are recorded in Table I to the nearest integer only, subject to an allowance of correction not exceeding 0·30 for the sake of slight smoothing; thus 24·25, which would otherwise have been entered as 24, might be treated as if it were 24·25 + 0·30 = 24·55 and be entered as 25. Similarly 24·75 might be entered either as 25 or as 24. It will be seen by the right-hand diagram that the position of the mean corresponds to the first moment of the years shown at the side and top; therefore the interval to which the annual birth rate corresponds is made up of the half year before and after that epoch. , made been made

The means that are enclosed in brackets are those in which one or

b

Table I.—Annual Percentage of Births according to the Ages of the Father and Mother, derived from Kőrösi's Table of Natality at Budapest.

The tabular values refer to the half-year before and after the beginning of the year entered at the top and side.

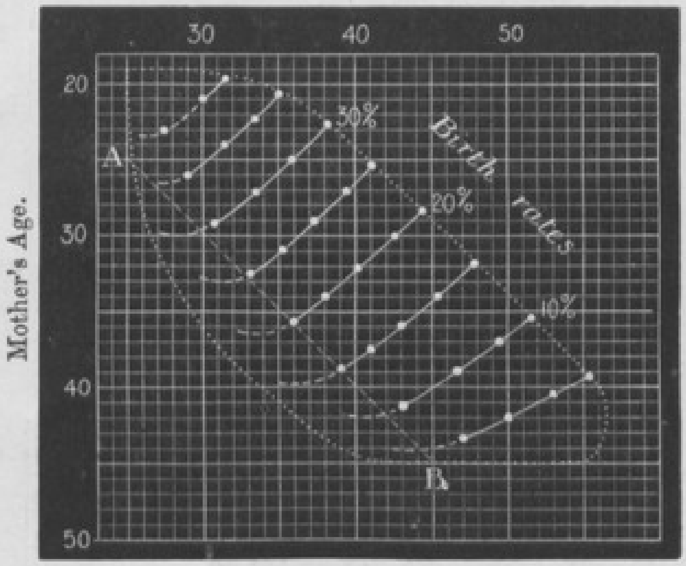
Age of the mother.	Age of the father (the even years are omitted). 25	27	29	31	33	35	37	39	41	43	45	47	49	51	53	55	57
19	(49)	46	(42)														
21	44	44	42	33	34	(36)											
23	43	42	41	35	35	32	30	(26)									
25	32	36	38	32	31	30	29	28	(25)								
27	31	32	36	33	31	26	27	25	19	(21)							
29		34	35	31	32	27	27	24	21	23	(17)						
31		24	26	22	24	26	24	21	20	18	17	16					
33			28	25	22	22	23	22	18	17	16	15	(12)				
35			24	19	21	19	21	29	16	15	13	10	12	(9)			
37				(13)	18	21	17	17	18	16	14	13	12	12	(13)		
39					(17)	(16)	16	15	15	15	14	10	9	9	9	(5)	
41						(11)	(12)	12	10	11	10	10	6	6	5	(3)	(5)
43							(6)	(7)	6	6	5	5	4	4	4	(3)	3
45									3	(4)	3	2	3	2	(2)	(1)	

more of the four squares from which they were derived was blank. They are, of course, less trustworthy than the rest; moreover, they may depend on less than 100 families.

The ages of married couples are distributed over only about one-half of the squares of Table I, as there are too few examples of other ages to be statistically available. This partial distribution is well seen in the following diagram of isogens, where a dotted outline encloses all the material that can be used with safety. The broken line AB corresponds to the instances in which both parents are of the same age. The chart is practically limited to marriages in which the wife is less than five years older, and less than seventeen years younger, than her husband.

Isogens.

Father's Age.



It will be noticed that the isogens run in nearly straight, diagonal, and equidistant lines across the greater part of the chart. If we omit six squares in the upper left-hand corner where there is no room for an isogen, we shall find these diagonal lines to cross 89 of the total number of 118 entries, or between eight and nine tenths of them. These peculiarities ~~This~~ indicate ~~s~~ the existence of a very ~~curious and~~ unexpected law of natality; it ~~which~~ is well brought out by Table II, which shows the values measured from the dots marked on the isogens. The dots have been taken at convenient places to serve as examples, one at the beginning, one at the end of the straight portion of each isogen, and at some other intervening places.

In Table II are given the ages of the father and mother that correspond to each of these dots.

The first curious law is ~~As~~ a consequence of the straightness and diagonal course of the isogens, namely that the *sums of the*

Table II. Values of the Isogens at the Dots.

Percentage of births in the year. A	Examples of the corresponding ages of the Mother. B	Examples of the corresponding ages of the Father. C	B + C.	Accepted mean of B + C.	A + B + C.
40	23 21 19½	27½ 30 31½	50½ 51 51	51	91
35	26 24 22 20½	29 31½ 33½ 35	55 55½ 55½ 55½	55½	90½
30	29½ 27 25 22½	30½ 33½ 35½ 38	60 60½ 60½ 60½	60½	90½
25	32½ 31 29 27 25½	33 35¼ 37¼ 39½ 41	65½ 66¼ 66¼ 66½ 66½	66½	91½
20	35¾ 34 32 30 28¼	35¾ 38 40 42½ 44¼	71½ 72 72 72½ 72½	72	92
15	39 37½ 36 34 31¾	39 41 43 45¼ 47¾	78 78½ 79 79¼ 79½	79	94
10	41¼ 39 37 35½	43 46½ 49½ 51½	84¼ 85½ 86½ 87	86	96
5	43½ 42 40½ 39½	47 50 53 55½	90½ 92 93½ 95	93	98

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ages of the parents, to which each point in the straight portion of the same isogen refers, are *constant*. The difference between the ages of the parents is of no account whatever in eight or nine tenths of the total number of marriages; it is only when the wife is (1) older than the husband or (2) when she approaches the limit of the child-bearing age, that this curious

In other words, the birth rate is chiefly determined by the joint ages of the father and mother.

law ceases to hold true. The connexion between it and the straightness of the isobar is easily understood from the equation to a straight line of $x+y$ = constant, for if x represent the age of the father, f, and if y represent that of the mother, m, then $f+m$ = constant. That this alleged law is a fact is conspicuously evident from the two columns headed B+C in Table II. ~~This is the first curious law~~ in successive groups in

a second curious result depends on ~~Again, through~~ a coincidence between the increasing age of either parent and the decrease of fertility, from which it happens that the sum of the three elements of (1) father's age, (2) mother's age, (3) percental birth-rate in a year, has a value that is itself approximately constant, as is seen in the column headed A+B+C. Its lowest limit is $90\frac{1}{2}$ and its highest, up to the isogen of 10 per cent., is 96, but it ~~is~~ increases to 98 at the isogen of 5 per cent. If we accept ~~for it~~ a constant value of 93 or 94 for this sum, we shall never be far wrong in the larger part of the chart.

~~From this~~ From this curious rule follows ~~the second curious law~~ that if we wish to ~~calculate~~ ascertain the percental birth-rate per annum for a married couple, within the limits of the chart where the isogens run straight and parallel, we have ~~only~~ simply to add the ages of the father and mother together and to subtract the total from 93 or 94. This gives the required result ~~in order to obtain it~~ with considerable precision. The approximate limits within which this ~~law~~ rule obtains are: (1) the wife is not to be older than her husband; (2) she is not to be less than twenty-three years of age, nor (3) more than forty.

Example.—In any large number of husbands and wives living under like conditions to the inhabitants of Budapest, whose respective ages at their nearest birthdays, to 21st June, 1892, were: that of the father, thirty-five, that of the mother, twenty-seven; ~~then~~ we may calculate that the number of children born to them during the year 1892 would be at the rate of $93-(35+27)$ per cent. = 31 per cent.; the isogen makes it about 32 per cent.*

I shall not now enter into the other salient peculiarities of the isogens further than to allude to the curious change in their course which occurs when the wife is older than the husband. When she is from thirty to thirty-eight she certainly seems to be appreciably more fertile with a husband of her own age or somewhat older, than she is with one who is younger. I should hesitate to ascribe this to physiological causes without corroborative evidence derived from breeders of stock. It is very possible that a growing indifference on the part of young husbands to ageing wives may have something to do with it.

It is almost needless to say that if it be desired to obtain the observed birth-rates for a mother of any specified age and for fathers of

* A rough mechanical arrangement was exhibited by which isogens may be drawn. It consists of three sliding pieces connected by a string of constant length. A coloured patch is pasted on the back board to show the limits within which the isogens drawn by it are trustworthy.

(In the lower part of the diagram) It will be observed that the isogens ~~come~~ tend to become more & more horizontal. This is due to the fertility of the male enduring considerably beyond the age at which it ceases in the female. ~~The consequence would be that at the extreme limit of female fertility, the isogen would be there~~

various ages, the corresponding line of Table I will give the information, while if the smoothed values are wanted, a similar line in the chart of isogens will give them after being smoothed, not in one dimension only *but in two dimensions.* Similarly, as regards the birth-rates for a father of any specified age and for mothers of various ages, by following the vertical columns instead of the horizontal lines.

call attention to the fact

In conclusion, I would ~~remark~~ that, though the method of isogens ~~applied to Körösi's tables fully discusses the distribution of mean birth-rates, those tables do not enable us to determine~~ the second postulate of paramount importance, namely, the degree of conformity of individual cases to the means of many cases. We ~~know~~ nothing ~~thus far~~ about the Facility of Error at the various positions in the chart, whether or no it conforms to the normal law of frequency; still less, what is ~~its~~ the modulus of Error may be, or whether the modulus is constant throughout the chart or whether it varies in accordance with some definite law.

Italics

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as I conceive

The answer to all these questions admits of being obtained by a moderate amount of work on the original observations, selecting at first a few squares for exploratory purposes, such as are (1) distributed evenly about the chart, and (2) contain each of them not less than some 300 observations, and (3) whose means accord with the smoothed isogens that pass over the squares, thereby affording satisfactory centres of reference.

on which the tables are based

though Körösi's tables give material from which the distribution of mean birth rates may be fully discussed by the method of isogens they do not afford data for the determination of

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To keep
copy returned for press June 29/94
Review of Kidd's book

9 2a

THE PART OF RELIGION IN HUMAN EVOLUTION.

A MEMOIR was contributed three or four years ago by Lady Welby to the Anthropological Institute, entitled "An Apparent Paradox in Mental Evolution." Her main question was plain and definite in substance, and to this effect. In barbaric times all men were superstitious; now, considering the importance of rational action to success in life, how was it that the least superstitious tribes did not prevail over the others, with the result of eliminating a natural bent towards superstition from the minds of men, by the long continued survival of the fittest? This is, as I then thought, and still think to be, a very pertinent question requiring for its answer a fuller discussion from historical data than, so far as I know, it has yet received. My own provisional ideas were that, notwithstanding the obvious disadvantages in practical life of a superstitious tendency, the advantages that accompanied it in barbaric times may have been greater, and in this way. Many influences that bind barbarians are illusions, such as totems, tutelar deities, and much else, but, for all that, they supply sufficient cohesive force to unite ~~men~~ them into organized bodies. Whenever a struggle arose between a compact tribe and an equal number of separate individuals, the former would win, however their compactness may have grown into existence. Or again, between two barbaric nations, the one that was the more superstitious of the two would ~~probably~~ generally be the more united, and therefore the more powerful. Such I imagine to be the explanation of Lady Welby's paradox, but, as already said, it deserves historical investigation before it can be answered with confidence.

A paradox of a somewhat similar kind is the subject of Mr. Kidd's recent work on Social Evolution. He has the high distinction of having compelled many readers to give serious consideration to his arguments by submitting them with a remarkable earnestness, wealth of apposite phrases, and happy turns of expression. Let the ultimate verdict be what it may on the nett value of his conclusions, his readers will have had the feeling which is rare to most of us, of being forced to travel for awhile out of ~~our~~ their habitual lines of thought and to follow new ones. This is no ordinary achievement, and deserves to be praised ungrudgingly.

The problem which presents itself to Mr. Kidd is this. Human evolution has been brought about by the blind action of natural selection, a strenuous struggle for existence taking place unceasingly; the feeble perishing, the strong surviving, and leaving their breed to form the next generation. On the other hand, this struggle is pitiless to the individual, and therefore repugnant to him. His reason protests against submission to such a state of things, saying, Why should we living men care to strive for the good of unborn descendants? Is not the well-being of our precious selves of the first importance to us? Let us be socialists, eliminate the holders of capital and divide the produce, live contentedly, and leave the future to take care of itself. Thus Mr. Kidd's paradox arises, that reason, which is so necessary an end of man's evolution, teaches him to withstand the salutary law of the survival of the fittest, and consequently tends to throw him backwards into degradation. The solution of the paradox Mr. Kidd considers to lie in the interposition of the altruistic sentiments, that is in the preference of the welfare of others to our own. As to the origin and extent of these, he seems to me much too vague, being apparently content to ascribe them wholly to the influence of the "Religious" spirit, using "Religion" in the following sense, which is an abbreviation of his own definition:—

"Religion is a form of belief providing an ultra-rational sanction for conduct by which the interests of the individual are subordinated to those of the evolution of his race."

It is evident that we must be cautious in our phraseology if we pursue the discussion along his lines, because different persons attach different meanings to the words Religion, Reason, and Ultra-rational. Religion, according to Mr. Kidd's definition, is necessarily ultra-rational, but I for one demur to ~~[illegible]~~ views of what is rational and what is not.

A large portion of the altruistic feelings, such as the love of a cat for her young kittens, is altruistic in its main aspect, though, perhaps, selfish in another; anyhow, it cannot be ~~called a~~ religion. It is justly pointed out, however, by Mr. Kidd, that the meanings are very various in which authors of high repute ~~have~~ use that word. He has collected fifteen definitions, of which only three concern us much; they will be distinguished as A, B, and C.

Because it is not rational to pursue any end that is not purely selfish

A is that of J. S. Mill: "The essence of Religion is the direction of the emotions and desires towards an ideal object, recognized as rightly paramount over all selfish objects of desire." B is that of Kant: "Religion consists in our recognizing all our duties as Divine commands." C is that of Gruppe, namely: "A belief in a State or a Being which, properly speaking, lies outside the sphere of human striving and attainment, but which can be brought into this sphere in a particular way, namely, by sacrifices, ceremonies, prayer, penances, and self-denial."

It will be seen that A is more generalized than B, and B than C. Now, Mr. Kidd would certainly refuse to accept A as sufficient, and would insist on the conditions contained in B or C. The paradox that his book is written to explain depends on the supposition that those conditions are necessary. For my part, I think otherwise, and am content with A; so, in my view, there is no paradox at all.

conduce to a ~~happy~~ successful form of human evolution.

Even if the case were, as Mr. Kidd's optimistic views would have it, that under the influence of a religious sentiment men are becoming increasingly sensitive to the well-being of their fellows, sacrificing privileges that they have the power to maintain, to a prevalent sentiment of doing equal kindness to all, I cannot see how it could ~~conduce to a happy end~~. By his own theory, the further opening of the careers would make the struggle for place more strenuous than it is even now, ~~while that if taken by itself~~ which would, as he ~~thinks~~ asserts, further the evolution of man. On other aspects of the case he is silent. My view is very different, ~~knowing~~ believing that over-severe competition degrades. The fir trees near the upper limit of vegetation, that are only just able to hold their own against the inclemency of the weather, are stunted specimens of their race. The over-worked man or woman is feeble and neurotic. Then again, as to the terrible question of over-population, the social condition that Mr. Kidd thinks so favourable to humanity would not check it in the least, but would seriously intensify it.

[Par] According to my own views of the question, any guiding idea that takes passionate possession of the mind of a person or of a people is an adequate adversary to purely selfish considerations, without being a "religion" in the B or C senses at all.

Run on

Many of the ordinary emotions which influence conduct admit of being excited to so high a pitch that the merely self-regarding feelings do not attempt to withstand them, but yield themselves unresistingly to be sacrificed to the furtherance of a cause. That the emotions can be so excited, whether in a party or in a nation, easily and often irrationally, is one of the common teachings of history. No ultra-rational sentiment in Mr. Kidd's sense is necessary to this end. Take for example the passionate patriotism among that large part of the French nation, who, however easily carried away by an idea, cannot be called religious in the sense B or C. It supported them through the German War, and it supported the Commune. Glory and victory and the ideas that inspired the Marseillaise were those that animated the earlier armies of the first French Republic, and were as a religion to them. Loyalty to a chief as that of the Italian revolutionists to Garibaldi, or of the French to the first Napoleon, or of Jacobites in Scotland to the Stuarts, have all of them been sufficiently passionate to subordinate the merely selfish feelings. Of course a religious enthusiasm in the B and C senses ~~will~~ may give a vast help to all this, but I cannot think it indispensable. The ambitions, loves, jealousies, ~~repugnances~~, and hates of nations, families, and persons, seem fully strong enough to force men who are under their influence to disregard what is commonly understood by the phrase of selfish desires.

It seems then, from abundant experience, that we are perfectly justified in interchanging parts of the definitions of Mill and Kidd, and then, by inverting their order, to say as follows:—

> "The direction of the emotions and desires towards the furtherance of human evolution, recognised as rightly paramount over all objects of selfish desire, justly merits the name of a religion."

It is under a conviction of the truth of this affirmation that the following remarks are made.

Mr. Kidd complains, and from his point of view the fact is deplorable, that many scientific writers should greatly occupy themselves in destroying the fabric of religions in the B and C senses, while they ~~have~~ do not replace them by other fabrics of similar efficacy. It may fairly be answered that the destructive task is a necessary though painful preliminary, because until obstructions are thoroughly cleared away, and the view is quite open, the character and exigencies of the vacant space cannot be rightly understood, nor can a judgment be formed as to how far and in what way rebuilding is needful. It is also pardonable enough that the work of destruction should be over zealously indulged in by some who have long chafed under what they consider to be the irrationality of one or other of the many conflicting creeds. They are like terriers who, not contented with killing a rat, shake and toss it afterwards.

Run on

All earnest inquirers recognize the awful mysteries that surround human life, but they are angered by theosophies that attempt to solve part of its ~~pretentious~~ problems by means of ~~improbable~~ hypotheses that are improbable in themselves while they introduce gratuitous complications. For instance, if we strip from Milton's fable and from the *dramatis personæ* of "Paradise Lost" all the glamour thrown over them by his superb diction, a grotesquely absurd frame-work remains behind. His high undertaking to justify the ways of God to man becomes ludicrously inadequate. The same spirit under another guise that moved our ancestors in the days of the Reformation to shatter the authority of Rome, is abroad again, but ~~it~~ is now directed against the dogmas of the time. The spirit is that of a determination to face and view the grand and terrible problem of life in the clear light of day, and not through artificial mediums that partly hide ~~and~~ and partly colour ~~to~~ refract it. It is not an easy matter to pass from theory to practice, the difficulty being great in taking wisely the earlier steps. It is comparatively simple to dig out tares, but very difficult not to destroy the wheat among which they grow. The social system of every nation, including its religion, has adjusted itself into a position of stability which is dangerous to disturb. Deep sentiments and prejudices, habits and customs, all more or less entwined with the established religion of ~~that~~ each nation, are elements of primary importance to its social fabric. It is true that vast changes become obvious in the social system of every progressive people, whenever its habits and customs at one period are compared with those of another long after, but, as a rule, the changes are piecemeal. Each change is primarily confined to a single part, the remainder adapting itself to ~~it~~ the new condition with a comparatively small shift of the position of the centre. Common sense teaches how much can be thus done with safety at any given time. Great and sudden changes in religion are hardly to be attempted except when the stability of the existing system is tottering and on the point of falling.

In order to realise the large part that a religion (merely of the A kind) ~~without any supernatural sanctions~~ might play in human evolution,

B or C

Let us suppose a nation whose ~~established~~ religion, whatever it may be, has become discredited by the majority of its people, including most of those who were trusted as leaders of opinion. Further, let us suppose the nation to be suffering in a still more acute form than our own, from poverty, toil, and an unduly large contingent of the weakly, the inefficient, and the born-criminal classes, and that the existing social arrangements are acknowledged to be failures. Further, again, I will make the reasonable supposition that socialistic experiments on various scales and in various ways had been largely tried and confessedly found ineffective, owing to the moral and intellectual incompetence of the average citizen. There would then be ominous signs of ~~an anarchic uprising of the lower classes, and of~~ ruin impending over the nation, ~~and~~ a bitter cry would arise for light and leading. A state of things like this is by no means impossible in the near future, even here in England, and, therefore, it deserves some consideration as being ~~little~~ more than a merely academic question. In the imagined event, preachers of all sorts of nostrums would abound, mostly fanatics who could see only one side of a question, and on that account they would be all the more earnest in their opinions and persuasive to the multitude. I will endeavour to present in a clear light what one of these, a professed agnostic, might say. It will be put in a very brief form, in order to bring out, as vividly as I can, one possible line of argument with which I have much sympathy, but to which I would by no means commit myself without first insisting on serious reservations. These have nothing to do with the present supposed case, and, if introduced now, would merely distract the argument. So I do not speak further about them. My object is only to show how a religion, in the A sense, might grow up that could effectively deal with difficulties which existing religions, in the B or C sense, seem incapable of meeting. Of course, however, some totally new interpretations of the B and C sanctions might be put forward for some new religion of ~~that~~ kind might arise.

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the B or C

The supposed agnostic and somewhat fanatic preacher ~~would~~ ~~say~~. The mystery is unfathomed as to whence the life of each man came, whether it pre-existed in any form or not. The mystery is equally great as to what will become of his life after the death of the body, whether it will be perpetuated in a detached form as some creeds say, whether it will be absorbed into an unlimited sea of existence, as other creeds assert, or whether it will cease entirely. As regards this life, there are also mysteries. Every act may or may not have been determined by previous conditions, but man has the sense of being free and responsible: he is accustomed to do and to be done by as if he were so, therefore we may provisionally believe that he is free and should act on that supposition. There is a further mystery as regards the cosmic conditions under which we live, for no assurance can as yet be obtained of any supernatural guidance, the facts alleged in evidence of its existence being more than counterbalanced by those that point the other way. We cannot, in consequence, tell with certainty whether human life is subject to an autocracy, or whether, at least for practical purposes, it exists as an isolated republic; but the latter appears at present to be the more probable, and should, therefore, guide our conduct. Each man's destiny during his life may then be viewed with propriety as depending entirely on his physiological peculiarities and on his surroundings. He has, consequently, to conduct himself as a member of a free executive committee during his brief life, guiding his actions by whatever he can learn of the tendencies of the cosmos, in order to co-operate intelligently with them.

might

express himself as follows.

[illegible]

own

The sense of responsibility that is imposed by this view would sober, brace, and strengthen the character, just as that of dependence on an autocratic power effeminates and enfeebles it. As was said by J. S. Mill, and quoted by Kidd, but in both cases for a different purpose to that of its present application, "a people who look habitually to their Government to command and prompt them in all matters of self-concern, who expect everything to be done for them, except what can be made an affair of mere habit and routine, have their faculties only half developed; their education is defective in one of its most important branches."

On the foregoing basis our agnostic ~~would~~ say, Let ~~us~~ consider what is profitable and proper for man to attempt. One of the most prominent conditions to which life has been subject is the newly discovered law of the survival of the fittest, whose blind action results in the progressive production of more and more vigorous animals. Any action that causes the breed or nature of man to become more vigorous than it was in former generations is therefore accordant with the process of the cosmos, or, if we cling to teleological ideas, we should say with ~~cosmic~~ purpose.

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It has now become a serious necessity to better the breed of the human race. The average citizen is too base for the every day work of modern civilization. Civilized man has become possessed of vaster powers than in old times for good or ill, but has made no corresponding advance in wits and goodness to enable him to direct his conduct rightly. It would not require much to raise the natural qualities of the nation high enough to render some few Utopian schemes feasible that are necessary failures now. Conceive, for sake of argument, the nation to be divided in the imagination into three equal groups L, M, N, in order of their natural civic capacities. At present the production of the ~~next~~ generation is chiefly effected by L and M, the lowest and the middle; if it were hereafter effected by M and N, the middle and the highest, a distinct gain would be achieved in the lifetime of many of those who initiated the reform, for it is probable that the inefficient multitude of weaklings in brain, character and physique would be sensibly diminished in thirty years.

forthcoming

Our agnostic preacher ~~would~~ go on to say that this terrible question of over population and of the birth of children who will necessarily (in a statistical sense) grow into feeble and worse than useless citizens must be summarily stopped, cost what it may. The nation is starved and crowded out of the conditions needed for healthy life by the pressure of a ~~weakish~~ contingent of born weaklings and criminals. We of the living generation are dispensers of the natural gifts of our successors, and we should rise to the level of our high opportunities.

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huge

The course of nature is exceedingly wasteful ^in every way. It is careless^ of germs, tens of thousands of pollen grains perishing of which none ~~have~~ had the chance of effecting fertilisation by being transported to the proper spot at the proper moment, whether by the blind agency of an insect ferreting among flowers in search of food ~~or by the intelligent agency of an horticulturist who displaces the pollen with a delicate brush~~. The same is true of the microbes whose part in the animal world is identical with that of the pollen in flowers; they are too minute to be distinguished by the naked eye, they are produced in myriads and are fairly tenacious of life, though only one of them is needed for fertilisation. It is no exaggeration to say that the number of these microbes produced in each year by each average male ~~would suffice~~ of any of the larger animals, would suffice, if every one of them were utilised, to fertilise a million of females. The course of nature is ^also^ indifferent to life and ruthless to survivors, but reason can teach

~~The course of nature is wasteful in respect to germs to each, to life, and is ruthless to survivors. Reason teaches~~ us to effect with pity, intelligence, and speed ~~those~~ many objects that nature would otherwise effect remorselessly, unintelligently, and tediously. By ~~the~~ its action ~~of reason~~, suffering ~~may be~~ is minimized and waste diminished. Wherever intelligence chooses to intervene, the struggle for existence ceases, that struggle being by no means ~~such~~ an absolute necessity in evolution as Mr. Kidd assumes it to be. ~~[struck]~~ It hardly affects our monogamous pigeons, whose breeding is somewhat under control, and it does not at all affect our polygamous ~~cattle~~ ~~[struck]~~ cattle, for who ever heard of foals being slaughtered because they were feeble, ~~[struck]~~, or ~~[struck]~~ of an overwhelming productiveness of horses which it was impossible to check, and which threatened to increase in excess of the means of subsistence? Horses are bred in the number and of the stamp required, within the limits of excellence that experience has taught to be possible. A general high level of the qualities that make a good horse has been attained without any aid from natural selection, artificial selection having superseded it.

as even a fanatic must allow

Before, however, any form of artificial selection could be applied ^a little way back^ to the human race, other than such moderate, yet not ineffective, reforms as ~~should~~ might produce the results mentioned ~~above~~, much ^preceded by^ knowledge has to be obtained on numerous ~~points~~ details connected with productiveness, of which we are now ignorant ^Cautiously and carefully to study^, ~~and~~ customs ^which the national would^ have to be profoundly modified. ~~In~~ considering what ~~may be~~ feasible, and what not, ~~we may~~ ^should be^ freely accept the three following canons:— ^The fanatic might however urge that in^

"1. The customs of every nation are liable to change to an extent that is hardly credible to those who do not bear history in mind; therefore the existing customs of any nation may be lightly regarded while discussing future possibilities.

"2. No custom can be considered seriously repugnant to human feelings that has ever prevailed extensively in a contented nation, whether barbarous or civilised.

"3. Any custom established by a powerful authority soon becomes looked upon as a duty, and, before long, as an axiom of conduct which is rarely questioned."

Fortified by these three canons, an anthropologist who is necessarily familiar with the customs of many nations will find abundant elbow-room for his wildest speculations. There is hardly any proposition, however monstrous it may seem to us now, that is thereby precluded from consideration. Thus much for the arguments of the supposed agnostic and fanatic.

Par [Space of half a line here

So much of my own opinion as I shall offer is purely general, and as follows. It is quite credible that a nation whose old religious notions and social practices ^without any violence^ have avowedly failed ~~which has~~ ^, who have^ been aroused to the knowledge that man possesses vast and hitherto unused powers over the very nature of unborn generations, ~~which~~ ^; who^ ~~has~~ ^are^ learnt to realise the dilatoriness, ruthlessness, and pain that accompany the evolution of man, when it is left as now to cosmic influences, ~~which~~ ^and who^ have ~~[struck]~~ satisfied ^themselves^ that the present low state of ~~humanity~~ ^their race^ might be ~~rapidly and~~ materially improved by concerted national action, should seize with irresistible ardour upon the idea of ~~applying it~~ ^utilising their powers^.

~~That is to say, to devote itself to the life of raising the natural level of the race, of keeping its numbers within appropriate bounds, of nourishing and educating, and of giving fair opportunities in life to all its members.~~

A passionate aspiration to improve the ^heritable^ powers of ~~humanity~~ ^a nation^ to their utmost ~~[struck]~~ seems to have all the requirements ^in efficiency^ needed for the basis of ~~a~~ national religion, in the sense of that word as defined by J. S. Mill, for it would "direct the emotions and desires of a nation towards an ideal object, recognized as rightly paramount over all selfish objects of desire." ^without any extra-natural sanctions^

FRANCIS GALTON.

That is to say, the nation might devote its best energies to the self-imposed duty of carrying out in its manifold details the following general programme,—(1) of raising the natural level of successive generations, morally, physically, and intellectually, by every reasonable means ~~way~~ that could be suggested; one of these ~~[struck]~~ ^being^ by attracting suitable immigrants, much as Colleges attract gifted students by offering exhibitions and fellowships; (2) of keeping its numbers within appropriate limits, partly by the discouragement in every way of the production of feeble children, as perhaps by laying penalties on their parents and expatriating whole families; (3) of developing the health and vigour of the people. In short to endeavour to make every individual as efficient as possible both by nature and by nurture.

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From the PROCEEDINGS OF THE ROYAL SOCIETY, VOL. 60.

NOTE TO THE MEMOIR BY PROFESSOR KARL PEARSON, F.R.S., ON SPURIOUS CORRELATION.

BY

FRANCIS GALTON, F.R.S.

[*From the* PROCEEDINGS OF THE ROYAL SOCIETY, Vol. 60.]

"Note to the Memoir by Professor Karl Pearson, F.R.S., on Spurious Correlation." By FRANCIS GALTON, F.R.S. Received January 4,—Read February 18, 1897.

I send this note to serve as a kind of appendix to the memoir of Professor K. Pearson, believing that it may be useful in enabling others to realise the genesis of spurious correlation. It is important though rather difficult to do so, because the results arrived at in the memoir, which are of serious interest to practical statisticians, have at first sight a somewhat paradoxical appearaanc.

b

The diagrams show how a table of frequency of the various combinations of two independent and normal variables may be changed into one of A/C, B/C, where C is also an independent and normal variable in respect to its intrinsic qualities, but subjected to the condition that the same value of C is to be used as the divisor of *both* members of the same couplet of A and B. In short, that the couplets shall always be of the form A/C_n, B/C_n, and never that of A/C_n, B/C_m.

For the sake of clearness, the simplest possible suppositions, that are at the same time serviceable, will be made in regard to the particular case illustrated by the diagrams, namely, that A, B, and C, severally, are sharply divided into three, and only into three, equal grades of magnitude, distinguished as AI, AII, AIII; BI, BII, BIII; and CI, CII, CIII; also that the frequency with which these three grades occur is expressed by the three terms of the binomial $(1+1)^2$. Consequently there is one occurrence of I to two occurrences of II and to one occurrence of III. Roman and italic figures are here used to keep the distinction clear between magnitudes and frequencies. It will be easily gathered as we proceed, without the need of special explanation, that the smallness of the value of the binomial index has no influence either on the general character of the operation or on its general result.

The large figures in the outlined square, occupying the lower right hand portion of fig. 1, show the distribution of frequency of the various combinations of A and B. The scales running along the top and down the left side of the figure, which are there assigned to the values of A/C, B/C, apply to these entries also. The latter run in the same way as those in Table I below, or when quadrupled, as they will be for purposes immediately to be explained, as in Table II.

Table I.

1	2	1
2	4	2
1	2	1

Table II.

4	8	4
8	16	8
4	8	4

Let us now follow the fortunes of one of the large figures in fig. 1, say that which refers to A = I, B = III, of which the frequency is only 1. When the latter is expanded into the three possible values of the form A/C, B/C, caused by the three varieties of C, it yields $\frac{1}{4}$ case of frequency to (I/I, III/I), $\frac{2}{4}$ case to (I/II, III/II), and $\frac{1}{4}$ case to (I/III, III/III), for entry at the intersections of the lines (I, III), (I/II, III/II), and (I/III, I) respectively.

But, in order to avoid the inconvenience of quarter values, it is better to suppose the original figures in the fig. and in Table I above to have been replaced by those in Table II; then the original entry

Fig. 1.

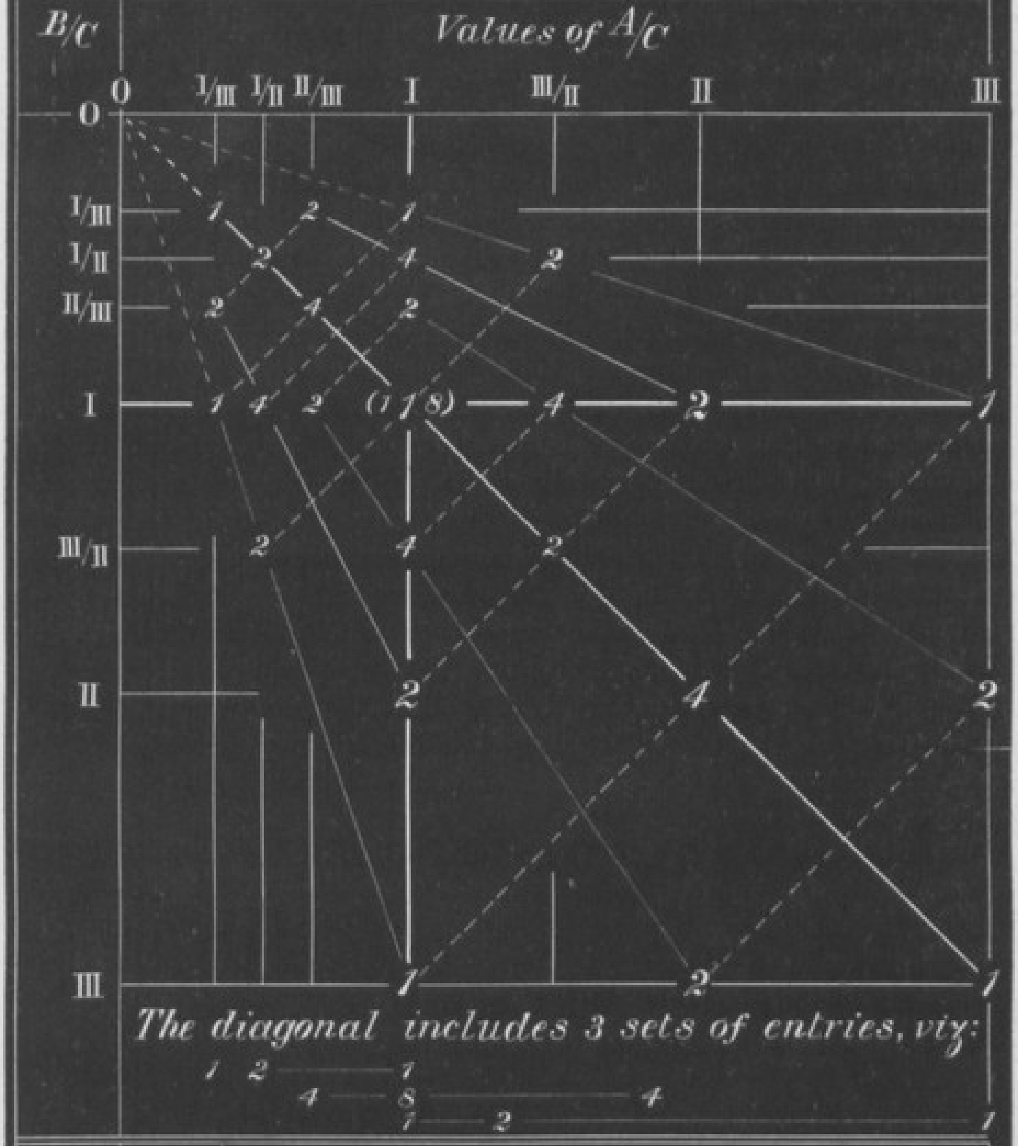
B/C
Values of A/C
0 I/III I/II II/III I III/II II III
The diagonal includes 3 sets of entries, viz:

Fig. 2.


0 I II III
0 I/III I/II II/III I

from which we start will have become four, to be expanded into three derivative entries, having respectively the frequencies 1, 2, and 1; these latter figures are entered in fig. 1 at the intersections of the lines just named. Under this arrangement the large figure from which we started, which had been changed from 1 to 4, again assumes its original value of 1. It will easily be understood, that the positions of the three derivative entries necessarily lie in the same straight line, and that this line necessarily runs towards the (O, O) corner of the figure. The same is true for every other set of derivative entries, with the result that whereas the original set of large figures, referring to the combinations of A and B, are symmetrically disposed on either side of the horizontal, of the vertical, and of the diagonal lines passing through their common centre at (II, II), the derivative values of A/C, B/C are disposed symmetrically only in respect to the diagonal line that runs from the (O, O) corner. Their symmetry, in this sense, is well shown by the dotted connections between the corresponding figures on either side of the diagonal. Also, it will be seen that the diagonal passes through the regions of greatest frequency. It follows that the diagonal in question represents the *locus* of average frequency. Now, along that diagonal, each value of A/C is associated with identically the same value of B/C; in other words, a correlation is found to have become established between them, which is solely due to the fact that *each* member in every couplet of A/C, B/C values is divided by the same value of the variable C.

We will now submit the above process to the test of extreme cases.

First, let the variability of A be so small that it may be treated as a constant, and take it = 1.

Then the values of A/C and B/C, that are severally associated with the three values of C, are as follows:—

Table III.

C.	A/C.	B/C.			Corresponding frequencies.
I	I	I	II	III	1 2 1
II	I/II	I/II	I	III/II	1 2 1
III	I/III	I/III	II/III	I	1 2 1

These frequencies are laid down at their proper places in fig. 2, where the three entries, corresponding to each successive value of A/C, run in vertical lines, but, on connecting the entries of maximum frequency it is seen that they coincide with the diagonal from the O/O corner; also that the entries of minimum frequency are disposed symmetrically on either side of that diagonal and converge towards the same corner. Consequently, the existence of spurious correlation is manifest here. If B be the constant, and A and C the variables, the general results will of course be the same.

Secondly, let both A and B be constant and equal to I, and C the only variable; then there are only three possible combinations of A/C and B/C. In one of them both values are equal to I, in another to I/II, and in the third to I/III, all of which lie along the diagonal from (O, O), and thus testify to intimate correlation.

Lastly, let C be the only constant and equal to 1. Then A/C, B/C, become A and B, and the table of frequency of their various combinations is that shown in Table I and by the large figures in fig. 1, whose symmetrical disposition in all directions proves that there is no correlation.

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UNIVERSITY OF LONDON.

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A Research Fellowship has been founded by Mr Francis Galton, F.R.S., to promote the Study of National Eugenics, *i.e.*, the study of the agencies under social control that may improve or impair the racial qualities of future generations either physically or morally.

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(2) The endowment not absorbed by the stipend of the Francis Galton Research Fellow, amounting to about £250 per annum, will be placed at the disposal of the Committee for the purpose of aiding his work, subject to the general approval of the Senate.

(3) The duties of the Fellow will be to devote the whole of his time to the study of Eugenics, subject to the approval and under the general direction of the Committee. In particular he will be required :—

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(*d*) To prepare for publication at such times and in such manner as may be approved by the Committee (and at least at the end of his tenure of the Fellowship), a Memoir or Memoirs on the investigations which he has carried out.

Applications for the above Fellowship must be sent in duplicate, accompanied by copies in duplicate of not more than three testimonials, and must reach the University not later than November 10th, 1904. Additional references may be given.

ARTHUR W. RÜCKER,
Principal.

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