English MEN of SCIENCE.

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ENGLISH MEN OF SCIENCE.

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A
Physics and Mathematics

Andrews
Brooke
Carrington
Cayley
De la Rue
Evans Captⁿ
Forster
Grove
Haughton
Hirst
Lassell
Main
Maxwell
Miller
Phillipps
Rosse Earl
Sabine
(Salisbury Marquess)
Scott
[Smith Archd]
Smyth Piazzi
Spottiswoode
Stewart Balfour
Strachey

B Chemistry

Buckton
Debus
Frankland
Gilbert
Gladstone
Harcourt
Lawes
Odling
Playfair
Roscoe
Stenhouse
Voelcker
Williamson A.W
Yorke.

C
(Geology)

Ansted
Egerton Sir P.M
Evans John
Forbes
Jones Rupert
Prestwich
Ramsay
Smyth Warⁿ

D
(Biology)

Alderson
Allman
Babington
Balfour J.H.
Ball
Bastian
Bateman Jas
Bentham
Bowman
Carpenter
Carter
Cobbold
Currey.
Darwin
Farre A.
Fergusson
Flower
Fox
Gray Edwᵈ
Günther
[Henslow]
Hooker
Humphry
Huxley
Jeffreys Gwyn
Lubbock
Marshall
Miers
Mivart
Newton
Owen
Paget
Parker Kitchen
Parkes
Sanderson
Sclater
Spencer Herbert
Sloter
Thomson Allen
Watson
Williamson

E
(Geography)

Back
Osborn

F
(Statistics)

Balfour T.G
Greg
Guy
(Hatherly Lord)
Heywood
Hill
Jevons
Newmarch

G
(Mechanics)

Armstrong
Bateman J. Fred
Fairbairn
Galton, Douglas
Jenkyn
Merrifield
Pole
Siemens

Total 105

ENGLISH MEN OF SCIENCE:

THEIR NATURE AND NURTURE.

BY

FRANCIS GALTON, F.R.S.,

AUTHOR OF "HEREDITARY GENIUS," ETC.

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1874.

f. 6v

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PREFACE.

I UNDERTOOK the inquiry of which this volume is the result, after reading the recent work of M. de Candolle,[1] in which he analyses the salient events in the history of 200 scientific men who have lived during the two past centuries, deducing therefrom many curious conclusions which well repay the attention of thoughtful readers. It so happened that I myself had been leisurely engaged on a parallel but more extended investigation—namely, as regards men of ability of all descriptions, with the view of supplementing at some future time my work on Hereditary Genius. The object of that book

[1] "Histoire des Sciences et des Savants depuis deux Siècles." Par Alphonse de Candolle. Corr. Inst. Acad. Sc. de Paris, &c. Genève, 1873.

was to assert the claims of one of what may be called the "pre-efficients"[1] of eminent men, the importance of which had been previously overlooked; and I had yet to work out more fully its relative efficacy, as compared with those of education, tradition, fortune, opportunity, and much else. It was therefore with no ordinary interest that I studied M. de Candolle's work, finding in it many new ideas and much confirmation of my own opinions; also not a little criticism (supported, as I conceive, by very imperfect biographical evidence,)[2] of my published views on heredity. I thought it best to test the value of this dissent at once, by limiting my first publication to the same field as that on which M. de Candolle had worked—namely, to the history of men of science, and to investigate their sociology from wholly new, ample, and trustworthy materials. This I have done in the present volume; and I am confident that

[1] Or, "all that has gone to the making of." The word was suggested to me.

[2] Reference may be made to a short review by me of M. de Candolle's work, in the *Fortnightly Review*, March 1873.

one effect of the evidence here collected will be to strengthen the utmost claims I ever made for the recognition of the importance of hereditary influence.

A few of my results, and some of the evidence on which they were based, were given by me at a Friday evening lecture, February, 1874, before the Royal Institution. I have incorporated parts of that lecture into this volume, with emendations and large additions.

It had been my wish to work up the materials I possess with much minuteness; but some months of careful labour made it clear to me that they were not sufficient to bear a more strict or elaborate treatment than I have now given to them.

The pleasant duty remains of acknowledging a debt to my friend, Mr. Herbert Spencer, for many helpful suggestions, and for his encouragement when I was planning this work; and to reiterate my deep sense of gratitude to numerous correspondents, which I have expressed elsewhere in the following pages.

I may add that four of the scientific men who replied to my questions have passed away since I began to write. Of these, two had sent me complete returns, namely, Professor Phillips, the geologist, and Sir William Fairbairn, the engineer. As regards the other two—Sir Henry Holland, the physician, had published his autobiography, but he gave me much help colloquially, and promised more; and Sir Edmund, better known as Count Strzelecki, the Australian traveller and meteorologist, furnished me with very suggestive information, but too incomplete for statistical use.

FRANCIS GALTON.

42 RUTLAND GATE, *November*, 1874.

P.S.—I have to apologise for some faults of style in the earlier pages, due to my not having had as full an opportunity as I had counted upon of correcting that portion of the press.

After I had sent the above to the printer, a friend happened to point out to me the following passage in the "Sartor Resartus" of Carlyle (Bk. ii., ch. 2). It expresses sentiments so nearly akin to those which induced me to write this book, that I am glad to quote it:—

"It is maintained by Helvetius and his set, that an infant of genius is quite the same as any other infant, only that certain surprisingly favourable influences accompany him through life, especially through childhood, and expand him, while others lie close folded and continue dunces. . . . . With which opinion, cries Teufelsdröckh, 'I should as soon agree as with this other—that an acorn might, by favourable or unfavourable influences of soil and climate, be nursed into a cabbage, or the cabbage-seed into an oak. Nevertheless,' continues he, 'I too acknowledge the all-but omnipotence of early culture and nurture: hereby we have either a doddered dwarf bush, or a high-towering, wide-shadowing tree; either a sick yellow cabbage or an edible luxuriant green one. Of a truth, it is the duty of all men, especially of all philosophers, to note down with accuracy the characteristic circumstances of their Education, what furthered, what hindered, what in any way modified it. . . .'"

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ERRATA.

Page 37, line 12, *for* "30" *read* "50."

Page 78, line 4, in the heading, *for* "forty cases" *read* "forty-two cases."

ENGLISH MEN OF SCIENCE.

CHAPTER I.

ANTECEDENTS.

Object of Book—Definition of Man of Science—Data—Nature and Nurture—Race and Birthplace—Occupation of Parents and Position in Life—Physical Peculiarities of Parents—Primogeniture, &c.—Fertility—Heredity—Pedigrees—Statistical Results.

THE intent of this book is to supply what may be termed a Natural History of the English Men of Science of the present day. It will describe their earliest antecedents, including the hereditary influences, the inborn qualities of their mind and body, the causes that first induced them to pursue science, the education they received and their opinions on its merits. The advantages are great of confining the investigation to men of our own period and nation. Our knowledge of them is more complete, and where deficient, it may be supplemented by further inquiry. They are subject to a moderate range of those influences which have the largest disturbing power, and are therefore well fitted for statistical investigation; lastly, the results we may obtain are of direct practical interest. The inquiry is a complicated one at the best; it is advantageous not to complicate it further by dealing with notabilities whose histories are seldom autobiographical, never complete and not always very accurate; and who lived under the varied and imperfectly appreciated conditions of European life, in several countries, at numerous periods during many different centuries.

Definition of "Man of Science."—I do not attempt to define a "scientific man," because no frontier line or *definition* exists, which separates any group of individuals from the rest of their species. Natural groups have nuclei but no outlines; they blend on every side with other systems whose nuclei have alien characters.

A naturalist must construct his picture of nature on the same principle that an engraver in mezzotint proceeds on his plate, beginning with the principal lights as so many different points of departure, and working outwards from each of them until the intervening spaces are covered. Some definition of an ideal scientific man might possibly be given and accepted, but who is to decide in each case whether particular individuals fall within the definition? It seems to me the best way to take the verdict of the scientific world as expressed in definite language. It may be over lenient in some cases, in others it may never have been uttered, but on the whole it appears more satisfactory than any other verdict which exists or is attainable. To have been elected a Fellow of the Royal Society since the reform in the mode of election, introduced by Mr. Justice Grove nearly thirty years ago, is a real assay of scientific merit. Owing to various reasons, many excellent men of science of mature ages, may not be Fellows, but those who bear that title cannot but be considered in some degree as entitled to the epithet of "scientific." I therefore look upon this fellowship as a "pass examination," so to speak, and from among the Fellows of the Royal Society I select those who have yet further qualifications. One of these is the fact of having earned a medal for scientific work; another, of having presided over a learned Society, or a section of the British Association; another, of having been elected on the council of the Royal Society; another, of being professor at some important college or university. These and a few other similar signs of being appreciated by contemporary men of science, are the qualifications for which I have looked in selecting my list of typical scientific men. I have only deviated from these technical rules in two or three cases, where there appeared good reason for their relaxation and where the returns appeared likely to be of peculiar interest. On these principles I drew up a list of 180 men; most of them were qualified on more than one count, and many on several counts. Also, the list appeared nearly exhaustive in respect to those men of mature age who live in or near London, since other private tests suggested few additions. As two of these tests have been proposed by several correspondents, it may be well to describe them. The one is the election of individuals, on account of their scientific eminence, to a certain well-known literary and scientific club, the name of which it is unnecessary to mention. The committee of this club have the power of electing annually, out of their regular turn, nine persons eminent for science, literature, art, or public services. The two or three men who have in each year received this coveted privilege on the ground of science now amount to a considerable number, and they are all on my list. Again, there are certain dining clubs in connection with the Royal Society, the one meeting on the afternoon of every evening that it meets, and the other more rarely, and there are about fifty members to each of these clubs, the same persons being in many instances members of both. The election to either of the clubs is a testimony of some value to the estimation of the scientific status of a man by his contemporaries; almost all their members are on my list. No doubt, many persons of considerable position living in Edinburgh, Dublin, and elsewhere at a distance from London, are not among those with whose experiences I am about to deal. But that is no objection; I do not profess or care to be exhaustive in my data, only desiring to have a sufficiency of material, and to be satisfied that it is good so far as it goes, and a perfectly fair sample. I do not particularly want a list that shall include every man of science in England, but seek for one that is sufficiently extended for my purposes, and that contains none but truly scientific men, in the usual acceptation of that word.

However, I have made some further estimates, and conclude that an exhaustive list of men of the British Isles, of the same mature ages and general scientific status as those of whom I have been speaking, would amount to 300, but not to more.

Some of my readers may feel surprise that so many as 300 persons are to be found in the United Kingdom who deserve the title of

f. 23br

f. 23 bv

scientific men; probably they have been accustomed to concentrate their attention upon a few notabilities, and to ignore their colleagues. It must, however, be recollected that all biographies, even of the greatest men, reveal numerous associates and competitors whose merit and influence were far greater than had been suspected by the outside world. Great discoveries have often been made simultaneously by workers ignorant of each other's labours. This shows that they had derived their inspiration from a common but hidden source, as no mere chance would account for simultaneous discovery. In illustration of this view it will suffice to mention a few of the great discoveries in this generation. That of photography is most intimately associated with the names of Niépce, Daguerre and Talbot, who were successful in 1839 along different lines of research, but Thomas Wedgewood was a photographer in 1802, though he could not fix his pictures. As to the origin of species, Wallace is well known to have had an independent share in its discovery, side by side with the far more comprehensive investigations of Darwin. In spectrum analysis the remarks of Stokes were anterior to and independent of the works of Kirchhoff and Bunsen. Electric telegraphy has numerous parents, German, English and American. The idea of conservation of energy has unnumbered roots. The simultaneous discovery of the planet Neptune on theoretical grounds by Leverrier and Adams is a very curious instance of what we are considering. In patent inventions the fact of simultaneous discovery is notoriously frequent. It would therefore appear that few discoveries are wholly due to a single man, but rather that vague and imperfect ideas, which float in conversation and literature, must grow, gather, and develop, until some more perspicacious and prompt mind than the rest clearly sees them. Thus, Laplace is understood to have seized on Kant's nebular hypothesis and Bentham on Priestley's phrase, "the greatest happiness of the greatest number," and each of them elaborated the idea he had so seized, into a system.

The first discoverers beat their contemporaries in point of time and by doing so they become leaders of thought. They direct the intellectual energy of the day into the channels they opened; it would have run in other channels but for their labour. It is therefore due to them, not that science progresses, but that her progress is as rapid as it is, and in the direction towards which they themselves have striven. We must neither underrate nor overrate their achievements. I would compare the small band of men who have achieved a conspicuous scientific position, to islands, which are not the detached objects they appear to the vulgar eye, but only the uppermost portions of hills, whose bulk is unseen. To pursue this metaphor; the range of my inquiry dips a few fathoms below the level at which popular reputation begins.

It is of interest to know the ratio which the numbers of the leading scientific men bear to the population of England generally. I obtain it in this way. Although 180 persons only were on my list, I reckon, as already mentioned, that it would have been possible to have included 300 of the same ages, without descending in the scale of scientific position; also it appears that the ages of half of the number on my list lie between 50 and 65, and that about three-quarters of these may be considered, for census comparisons, as English. I combine these numbers, and compare them with that of the male population of England and Wales, between the same limits of age, and find the required ratio to be about one in 10,000. What then are the conditions of nature, and the various circumstances and conditions of life,—which I include under the general name of nurture,—which have selected that one and left the remainder? The object of this book is to answer this question.

DATA.

My data are the autobiographical replies to a very long series of printed questions addressed severally to the 180 men whose names were in the list I have described, and they fill two large portfolios. I cannot sufficiently

Taken in Gilbert
& Fg. omits Henslow

AGES ~~AGES~~ Numbers of "Sc: Men".

under 40	6	27 under 50 years.
40 –	10	
45 –	11	
50 –	16	50 between 50 and 65 yea
55 –	17	
60 –	17	
65 –	7	20 above 65 years
70 –	7	
75 –	2	
80 and above	4	
	97	

thank my correspondents for the courteousness with which they replied to my very troublesome queries, the great pains they have taken to be precise and truthful in their statements, and the confidence reposed in my discretion. Those of the answers which are selected for statistical treatment somewhat exceed 100 in number. In addition to these, I have utilized several others which were too incomplete for statistical purposes, or which arrived late, but these also have been of real service to me; sometimes in corroborating, at others in questioning previous provisional conclusions. I wish emphatically to add that the foremost members of the scientific world have contributed in full proportion to their numbers. It must not for a moment be supposed that mediocrity is unduly represented in my data.

Natural history is an impersonal result; I am therefore able to treat my subject anonymously, with the exception of one chapter in which the pedigrees of certain families are given.

NATURE AND NURTURE.

The phrase "nature and nurture" is a convenient jingle of words, for it separates under two distinct heads the innumerable elements of which personality is composed. Nature is all that a man brings with himself into the world; nurture is every influence from without that affects him after his birth. The distinction is clear: the one produces the infant such as it actually is, including its latent faculties of growth of body and mind; the other affords the environment amid which the growth takes place, by which natural tendencies may be strengthened or thwarted, or wholly new ones implanted. Neither of the terms implies any theory; natural gifts may or may not be hereditary; nurture does not especially consist of food, clothing, education or tradition, but it includes all these and similar influences whether known or unknown.

When nature and nurture compete for supremacy on equal terms in the sense to be explained, the former proves the stronger. It is needless to insist that neither is self-sufficient;

the highest natural endowments may be starved by defective nurture, while no carefulness of nurture can overcome the evil tendencies of an intrinsically bad physique, weak brain, or brutal disposition. Differences of nurture stamp unmistakable marks on the disposition of the soldier, clergyman, or scholar, but are wholly insufficient to efface the deeper marks of individual character. The impress of class distinctions is superficial, and may be compared to those which give a general resemblance to a family of daughters at a provincial ball, all dressed alike, and so similar in voice and address as to puzzle a recently-introduced partner in his endeavours to recollect with which of them he is engaged to dance; but an intimate friend forgets their general resemblance in the presence of the far greater dissimilarity which he has learned to appreciate. There are twins of the same sex so alike in body and mind that not even their own mothers can distinguish them. Their features, voice, and expressions are similar; they see things in the same light, and their ideas follow the same laws of association. This close resemblance necessarily gives way under the gradually accumulated influences of difference of nurture, but it often lasts till manhood. I have been told of a case in which two twin brothers, both married, the one a medical man, the other a clergyman, were staying at the same house. One morning, for a joke, they changed their neckties, and each personated the other, sitting by his wife through the whole of the breakfast without discovery. Shakespeare was a close observer of nature; it is, therefore, worth recollecting that he recognizes in his thirty-six plays three pairs of family likeness so deceptive as to create absurd confusion. Two of these pairs are in the "Comedy of Errors," and the other in "Twelfth Night" (v. 1.) I heard of a case not many years back in which a young Englishman had travelled to St. Petersburg, then much less accessible than now, with no letters of introduction, and who lost his pocket-book, and was penniless. He was walking along the quay in some despair at his prospects, when he was startled by the cheery voice of a stranger who accosted him, saying he required no introduction because his family likeness proclaimed him to be the son of an old friend. The Englishman did not conceal his difficulties, and the stranger actually lent him the sum he needed on the guarantee of his family likeness, confirmed, no doubt, by some conversation. In this and similar instances how small has been the influence of nurture; the child had developed into manhood, along a predestined course laid out in his nature. It would be impossible to find a converse instance in which two persons, unlike at their birth, had been moulded by similarity of nurture into so close a resemblance that their nearest relations failed to distinguish them. Let us quote Shakespeare again as an illustration; in "A Midsummer-Night's Dream" (iii. 2), Helena and Hermia, who had been inseparable in childhood and girlhood, and had identical nurture—

> "So we grew together,
> Like to a double cherry, seeming parted,
> But yet a union in partition,"—

were physically quite unlike: the one was short and dark, the other tall and fair; therefore, the similarity of their nurture did not affect their features. The moral likeness was superficial, because a sore trial of temper, which produced a violent quarrel between them, brought out great dissimilarity of character. In the competition between nature and nurture, when the differences in either case do not exceed those which distinguish individuals of the same race living in the same country under no very exceptional conditions, nature certainly proves the stronger of the two.

RACE AND BIRTHPLACE.

As regards the race of the scientific men on my list, it has already been mentioned that for the purposes of a census enumeration three-fourths may be considered English, but their precise origin is as follows. Omitting a few Germans, out of every 10 scientific men, 5 are pure English; 1 is Anglo-Welsh; 1 is Anglo-Irish; 1 is pure Scotch; 1 includes Anglo-Scotch, Scotch-Irish, pure Irish, Welsh, Manx and Channel Islands; finally, 1 is "unclassed." These unclassed are of extremely mixed origin. One is in about equal degrees English, Irish, French, and German; another is English, Scotch-Creole, and Dutch; another English, Dutch, Creole, and Swedish; and so on. (I trust the reader knows what "creoles" are—namely, the descendants of white families long settled in a tropical colony; and that he does not confound the term with "mulattoes.") I give this information without being able to make much present use of it. It is chiefly intended to serve as a standard with which other natural groups may hereafter be compared, such as groups of artists or of literary men.

One would desire to know whether persons in England generally show so great a diversity of origin; but it is somewhat difficult to answer the question owing to a want of precision in the word "generally." If we were to go to rural districts, or small stagnant towns, we should find much less variety of origin; but I think there would be quite as much in the more energetic classes of the metropolis, who have immigrated from all quarters. Some haphazard selecting which I tried confirmed this view. Then comes the important question, Is this a sign that a mixture of one or more of the various civilized races is conducive to form an able offspring? No doubt the varied "nurture" due to separate streams of tradition has great influence in awakening original thought, but we are not speaking of this now; the question is about "nature." On an analysis of the scientific status of the men on my list, it appeared to me that their ability is higher in proportion to their numbers among those of pure race. The Border men and lowland Scotch come out exceedingly well; the Anglo-Irish and Anglo-Welsh, notwithstanding eminent individual exceptions, would as a whole rank last. Owing to my list not being exhaustive, I hardly like to attempt conclusions as to the precise productiveness of scientific ability of the Scotch, English, and Irish severally, but there cannot be a shadow of doubt that its degrees are in the order I have named.

The birthplaces of scientific men and of their parents are usually in towns, away from the sea coast. Out of every 5 birthplaces I find that 1 lies in London or its suburbs; 1 in an important town, such as Edinburgh, Glasgow, Dublin, Birmingham, Liverpool, or Manchester; 1 is in a small town; and 2 either in a village or actually in the country. These returns are given with more detail in the foot-note.[1] The branch of science pursued is often in curious disaccord with the surrounding influence of the birthplace. Mechanicians are usually hardy lads born in the country, biologists are frequently pure townsfolk. Partly in consequence of the prevalence of their urban distribution I find that an irregular plot may be marked on the map of England which includes much less than one-half of its area, but more than 92 per cent. of the birthplaces of the English scientific men or of their parents. The accompanying diagram shows its position; one thin

[1] London, 16; suburbs, 5; = 21. Edinburgh and Glasgow, 7; Cork, Belfast, and Dublin, 6; Birmingham, Liverpool, and Manchester, 5; total = 18. Smaller towns, 21; elsewhere, 40. General total, 100.

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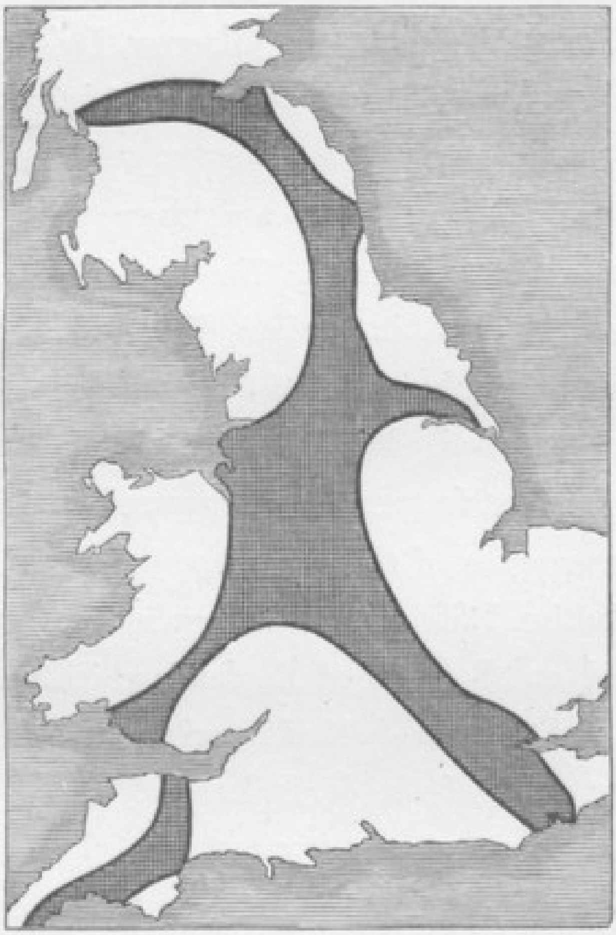
arm abuts on the sea between Hastings and Folkestone, and runs northwards over London and Birmingham, where it is joined by another



thin arm proceeding from Cornwall and Devonshire, crossing the Bristol Channel to Swansea, and thence to Worcester. The two arms are now combined into one of double breadth; it covers Nottingham, Shrewsbury, Liverpool, and Manchester. Above these latitudes it again narrows, and after sending a small branch to Hull, proceeds northwards to Newcastle, Edinburgh, and Glasgow. Thus there are large areas in England and Wales outside this irregular plot which are very deficient in aboriginal science. One comprises the whole of the Eastern Counties, another includes the huge triangle at whose angles Hastings, Worcester, and Exeter, or rather Exmouth, are situated.

OCCUPATION OF PARENTS AND POSITION IN LIFE.

My list contains men who have been born in every social grade, from the highest order in the peerage down to the factory hand and simple peasant, but the returns which I shall discuss do not range quite so widely. These are 96 in number, and may be classified as follows—but the same name appears in two classes on eleven occasions, so that the total entries are raised to 107:—

Noblemen and private gentlemen … …	9
Army and navy, 6; civil service, 9; subordinate officers, 3 … … … …	18
Law, 11; medical, 9; clergy and ministers, 6; teachers, 6; architect, 1; secretary to an insurance office, 1 … … … …	34
Bankers, 7; merchants, 21; manufacturers, 15	43
Farmers … … … … …	2
Others … … … … … …	1
	107

The terms used in the third and fourth groups must be understood in a very general sense; thus, there are some "merchants" on a very small scale indeed, and others on a very large one.

It is by no means the case that those who have raised themselves by their abilities are found to be abler than their contemporaries who began their careers with advantages of fortune and social position. They are not more distinguished as original investigators, neither are they more discerning in those numerous questions, not strictly scientific, which happen to be brought before the councils of scientific societies. There can be no doubt but that the upper classes of a nation like our own, which are largely and continually recruited by selections from below, are by far the most productive of natural ability. The lower classes are, in truth, the "residuum."

Of the 6 clergymen or ministers who were fathers of scientific men, no less than 4 appear in a second category, viz., (1) clergyman and schoolmaster; (2) physician, afterwards clergyman; (3) Unitarian minister and schoolmaster; (4) professor of classics, afterwards an Independent minister. Among the successful graduates of Oxford and Cambridge, and among purely literary men, we find a much larger proportion of sons of clergymen. There is at Cambridge a well-known university scholarship, called the "Bell," which is open only to sons of clergymen of the Church of England. As it has been chiefly given for classical proficiency, we may be almost sure that the senior classic of his year, if he were the son of a clergyman, would also be a Bell scholar. I looked through the lists, and found that out of 45 senior classics (1824–68 inclusive) 10 had gained the scholarship, whence I conclude that at least 1 out of every 4 or 5 Cambridge graduates is the son of a clergyman. At this rate, out of 100 Cambridge graduates, 22 would have had clergymen of the Church of England for their fathers, whereas out of 100 scientific men only 3 or 4 were so circumstanced. It is therefore a fact, that in proportion to the pains bestowed on their education generally, the sons of clergymen rarely take a lead in science. The pursuit of science is uncongenial to the priestly character. It has fallen to my lot to serve for many years on the councils of many scientific societies, and, excepting a very few astronomers and mathematicians, about whom I will speak directly, I can only recall 3 colleagues who were clergymen; curiously enough, 2 of these, the Revs. Baden Powell and Dunbar Heath, have been prosecuted for unorthodoxy; the third was Bishop Wilberforce, who can hardly be said to have loved science; he rarely attended the meetings, but delighted in administration, and sought openings for indirect influence. The reason for the abstinence of clergymen from scientific work cannot be that they are too busy, too much home-tied, or cramped in pecuniary means, because other professional men, more busy, more at the call of others, and having less assured revenues, are abundantly represented on all the council lists.

Not caring to trust my unaided recollections, I have examined the council lists of ten scientific societies at or near the three periods, 1850, 1860, 1870. There have been changes in some of the societies, and there are many trifling peculiarities of detail, tedious and unnecessary here to deal with, but the following statement is substantially correct. The ordinary members of council are on a rough general average 20 in number to each of the following societies: (1) Royal; (2) British Association; (3) Astronomical; (4) Chemical; (5) Geological; (6) Linnæan; (7) Zoological; (8) Geographical; (9 and 10) the two predecessors of the recently-established Anthropological Institute, viz. Ethnological and Anthropological; (11) Statistical.

Therefore as we are dealing with 3 distinct periods, 11 societies, and 20 members of council to each, there have been about ($3 \times 11 \times 20 =$) 660 separate appointments. Clergymen have held only 16 of these, or 1 in 40; and they have in nearly every case been attached to those subdivisions of science which have fewest salient points to scratch or jar against dogma. Thus Prof. Challis, Dr. Lloyd, Dr. Robinson, Dr. Whewell, Rev. J. Fisher, Rev. W. Webb, Rev. Vernon Harcourt, Prof. Pritchard, Prof. Price, Rev. J. Barlow, and Prof. Willis are all chiefly connected with astronomy, physics, and mathematics; the five remaining names are those of the Rev. G. C. Renouard, the geographer; Bishop Wilberforce, and the Rev. Dunbar Heath, of whom I have already spoken; the Rev. Dr. Nicholson, and the Rev. Canon Greenwell: there is not a single biologist among them.

PHYSICAL PECULIARITIES OF PARENTS.

It has been frequently asserted that certain physical peculiarities in the parents clash, and that others combine happily in the offspring. I therefore thought it well to make inquiries as to the figure, complexion, colour of hair, height, and other physical peculiarities of the fathers and mothers of the scientific men. I also asked about the temperaments, if they were marked, but the answers to these were few.

Tables showing the number of cases in which there has been harmony, indifference, or contrast, between various physical peculiarities of the two parents

TEMPERAMENTS OF PARENTS.

(h = harmony, c = contrast).

MOTHERS.	FATHERS.			
	Nervous.	Sanguine.	Bilious.	Lymphatic.
Nervous	h. 6	5	—	c. 0
Sanguine	1	h. 3	—	c. 0
Bilious	4	—	h. 1	—
Lymphatic	c. 0	c. 2	—	h. 0

Summary—Harmony, 10 cases ; contrast, 2 ; indifferent, 10.
Total, 22.

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COLOUR OF HAIR OF PARENTS.

(h = harmony, c = contrast).

MOTHERS.	FATHERS.						
	Black.	Dark.	Dark Brown.	Brown.	Light Brown.	Light.	Fair.
Black . . .	h. 2	h. 2	h. 1	1	c. 0	c. 1	c. 0
Dark	h. 2	h. 5	h. 1	2	2	c. 1	c. 1
Dark Brown .	0	h. 2	h. 4	h. 3	3	0	c. 0
Brown . . .	3	4	h. 2	h. 14	h. 1	0	1
Light Brown .	c. 0	2	2	h. 1	h. 0	h. 0	0
Light	c. 3	c. 0	0	2	h. 0	h. 2	h. 0
Fair	c. 0	c. 0	c. 0	0	h. 1	h. 0	h. 1

Summary—Harmony, 44 cases; contrast, 6; indifferent, 22.
Total, 72.

I have, in addition, 11 cases of coloured hair—yellowish, sandy, red, light auburn, dark auburn, chestnut—but not one case of strict harmony among them.

FIGURE OF PARENTS OF SCIENTIFIC MEN.

(h = harmony, c = contrast).

MOTHERS.	FATHERS.				
	Corpulent, stout, or plump.	Muscular, robust, strong.	Compact, symmetrcl, stately.	Spare, neat, small.	Medium.
Corpulent, stout, or plump . .	h. 3	h. 5	0	c 7	c. 1
Muscular, robust, strong . . .	h. 0	h. 2	1	c. 1	0
Compact, symmetrical, stately.	3	2	h. 2	6	0
Spare, neat, small	c. 9	c. 5	4	h. 12	1
Medium . . .	0	1	1	5	h. 0

Summary—Harmony, 24 cases; contrast, 23; indifferent, 24.
Total, 71.

The foregoing tables show results bearing on the question whether harmony or contrast prevails in the physical characteristics of the parents. I think they must be accepted as decidedly in favour of harmony. The grand totals which they give are 78 cases of harmony, 31 of contrast, and 56 of indifference. In short, there is more purity of breed in scientific men than would have resulted from haphazard marriages. In the temperaments of their parents, harmony strongly prevails over contrast, the proportion being 5 to 1 in favour of the former. In colour of hair, harmony is twice as frequent as contrast. In figure it is equally common, because "corpulent, stout, or plump" persons of one sex seem to have a peculiar and reciprocated liking for "spare, neat, or small" persons of the other. This is literally the only case in these tables where a love of contrast equals that of harmony. I came to much the same conclusions by giving appropriate marks for harmony, contrast, and indifference to each quality in each case, thus obtaining aggregate marks for every pair, which I treated on much the same principle that the separate qualities are treated in the table. As regards height, there is a stricter method of investigation, which statisticians will appreciate. It is well known, by repeated experience, that the heights of men and of women in any large group are distributed according to the "law of frequency of error." In other words, the proportionate number of people of different heights corresponds to what would have been the case supposing stature to be due to the *aggregate action of many small and independent variable causes.* The probability is inconceivably small that all the independent causes should in any given case co-operate to produce an excess of height; if they did so, the result would be a Brobdignagian giant; or that they should all co-operate to produce a deficiency in height, in which case the result would be a Lilliputian dwarf. On the other hand, the probability is great that the number and effects of the causes in excess and those in deficiency of their several average values will be pretty equal. As for these and all other intermediate cases, their relative frequency is determined by the above law, which is based on that by which the relative frequency of different "runs of luck" is calculated.

I now proceed to apply this law. I have 62 cases in which the heights of both parents are given numerically, whence it appears that—(1) the average height of the fathers is between 5 ft. 9 in. and 5 ft. $9\frac{1}{4}$ in., and that their distribution conforms closely to the law of frequency of error, the "probable error" of the series being 1·7 in. (2) The average height of the mothers is 5 ft. $4\frac{1}{2}$ in., and the distribution of their heights conforms fairly to the above-mentioned law, the "probable error" of the series being 1·9 in. It follows from the well-known properties of the law in question, that if there had been no sexual selection in respect of height, the sum of the heights of the two parents would also conform to the law of frequency of error, and that the probable error of the series would be $\sqrt{(1 \cdot 7)^2 + (1 \cdot 9)^2} = 2 \cdot 5$ in. (3) I find that the heights in question do conform pretty closely to the law in question, and that the probable error of the series is 2·3 in., which differs so slightly from the value obtained by calculation, on the supposition of there having been no sexual preference for contrast in height, that we may safely affirm in this case also, that the love of contrast does not prevail over that of harmony.[1]

It is a question of high importance to speculations on the future of our race, whether the instincts of sexual selection are or are not repugnant to an improvement in the human breed. We know perfectly well that they are repugnant to unions where the resemblance is very close; thus near intermarriages shock our feelings, and the maintenance of high-bred artificial varieties in their purity is always effected with difficulty among animals. On the other hand, they are equally repugnant to unions in which there is great contrast; thus, the intermarriage of white and black races rarely takes place, and animals of different species refuse to cross. Where, then, and how wide, is the belt that lies

[1] The series of facts in (1), (2), and (3), and the corresponding figures given by the theory with which they are supposed to conform, are as follows:—

	(1) Father.	(2) Mother.	(3) Both Parents.
Fact . .	3 15 29 30 18 3 2	5 14 32 29 11 6 3	3 18 34 26 13·5 1
Theory .	5 15 27 29 18 5 1	8 18 25 26 15 6 2	6 18 31 29 13 2 1

between close harmony and wide contrast, in which sexual instinct acts most powerfully? It appears from the facts in this chapter, that the marriages of parents of the scientific men on my list actually tended to produce differentiation and purity of race. My data concerning the parents of men of other groups are insufficient to enable me yet to give comparative results showing how far the selective sexual instincts of the population generally would thwart, be indifferent to, or co-operate with the influences of future social restrictions on unsuitable marriages, or encouragement of suitable ones.

PRIMOGENITURE, &c.

The following statement shows, in percentages, the position of the scientific men in respect to age among their brothers and sisters:—

Only sons, 22 cases; eldest sons, 26 cases; youngest sons, 15 cases. Of those who are neither eldest nor youngest, 13 come in the elder half of the family; 12 in the younger half; and 11 are exactly in the middle. Total, 99.

It further appears that, at the time of the birth of the scientific men, the ages of their fathers average 36 years, and those of their mothers 30. The details are shown in the table below :—

No. of Cases.	AGE OF PARENTS AT BIRTH OF SCIENTIFIC MEN.								Total Cases.
	Under 20	20–	25–	30–	35–	40–	45–	50 and above.	
Fathers	0	1	15	34	22	17	7	4	100
Mothers	2	20	26	34	12	5	1	—	100

Putting these facts together, viz.—(1) that elder sons appear nearly twice as often as younger sons ; (2) that, as regards intermediate children, the elder and younger halves of the family contribute equally ; and (3) that only sons are as common as eldest sons, we must conclude that the age of the parents, within the limits with which we chiefly have to deal, has little influence on the nature of the child ; secondly, that the elder sons have, on the whole, decided advantages of nurture over the younger sons. They are more likely to become possessed of independent means, and therefore able to follow the pursuits that have most attraction to their tastes ; they are treated more as companions by their parents, and have earlier responsibility, both of which would develop independence of character; probably, also, the first-born child of families not well-to-do in the world would generally have more attention in his infancy, more breathing space, and better nourishment, than his younger brothers and sisters in their several turns.

The opposing disadvantage of primogeniture, in producing less healthy children and half as many idiots again as the average of the rest of the family, has not been sensibly felt, partly because the latter risk is very small, and partly because the mothers of the scientific men are somewhat less youthful than those from whom the above statistical results were calculated. (See Duncan "On Fertility," &c., second edition, pp. 293, 4, for tabulations of Dr. A. Mitchell's results.) An unusual number of the mothers of the scientific men were between 30—34 at the time of their birth; this is a very suitable age, according to the views of Aristotle, but undoubtedly older than what Dr. Duncan's statistics (pp. 387, 390) recommend. According to these, the most favour-

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able period for the survival of mother and child, and therefore probably the best in every sense, is when she is 20—25 at the time of giving birth. The important question of the effect of the age of the parent on the wellbeing of the offspring seems never yet to have been treated as strictly and as copiously as it deserves. Dr. Duncan, in the chapter of his work above referred to, has discussed the materials at his disposal with great ingenuity and industry; but adequate statistics, sorted according to the various classes of society, are still wanting.

FERTILITY.

The families are usually large to which scientific men belong. I have two sets of returns—the one of brothers and sisters, excluding, for the most part, those who died in infancy; and the other of brothers and sisters who attained 30 years. In these several cases I have included the scientific man himself, and find, on an average of about 100 cases, that the total number of brothers and sisters is 6·30 in the first case, and 4·80 in the second. It is a matter of great interest to compare with these figures the number of the children of the scientific men themselves. It is easy to do so with fairness, because the time of marriage proves to be nearly the same in both cases; if anything, the scientific men marry earlier than their parents. It remains to eliminate all cases of absolutely sterile marriages on the part of the scientific men, and those in which there might yet be other children born. Having attended to these precautions, I find the number of their *living* children (say, of ages between 5 and 50) to be 4·7. This implies a diminution of fertility as compared with that of their own parents, and confirms the common belief in the tendency to an extinction of the families of men who work hard with the brain. On the other hand, I shall show that the health and energy of the scientific men are remarkably high; it therefore seems strange that there should be a falling off in their offspring. I have tried in many ways to find characteristics common to those scientific men whose families were the smallest, but have only lighted upon one general result, which I give provisionally, namely, that a *relative* deficiency of health and energy, in respect to that of their own parents, is very common among them. Their absolute health and energy may be high, far exceeding those of people generally; but I speak of a noticeable falling off from the yet more robust condition of the previous generation: it is this which appears to be dangerous to the continuance of the race. My figures give the remarkable result that there are no children at all in one out of every three of these cases. I think that ordinary observation corroborates this conclusion, and that those of my readers who happen to have mixed much in what is called intellectual society will be able to recall numerous instances of persons of both sexes, but especially of women, possessed of high gifts of every kind, including health and energy, but of less solid vigour than their parents, and who have no children. I do not overlook the fact that the scientific men are an urban population, being mindful of results I have published elsewhere (*Statistical Journal*, 1873), which show a similar diminution in the average fertility of townsmen as compared with country folk; but this would not account for their being less prolific than their parents who were also townsmen, nor for the large number of wholly sterile marriages.

HEREDITY.

The effects of education and circumstances are so interwoven with those of natural character in determining a man's position among his contemporaries, that I find it impossible to treat them wholly apart. Still less is it possible completely to separate the evidences relating to that portion of a man's nature which is due to heredity, from all the rest. Heredity and many other co-operating causes must therefore be considered in connection; but I feel sure that as the reader proceeds, and becomes familiar with the variety of the evidence, he will insensibly effect for himself much of the required separation. Also, from time to time, as opportunity may offer, I shall attempt to draw distinctions.

The study of hereditary form and features in combination with character promises to be of much interest, but it proves disappointing on

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trial, owing to the impossibility of obtaining good historical portraits. The value of these is further diminished by the passion of distinguished individuals to be portrayed in uniforms, wigs, robes, or whatever voluminous drapery seems most appropriate to their high office, forgetting that all this conceals the man. The practice might well become common of photographing the features from different points of view, and at different periods of life, in such a way as would be most advantageous to a careful study of the lineaments of the man and his family. The interest that would attach to collections of these in after-times might be extremely great.

PEDIGREES.

Thirteen families have been selected, out of those to which about 120 of the scientific men on my list belong, as appearing noteworthy for their richness in ability during two, three, or more generations, or for any other peculiarity; in some cases they are also remarkable for purity of type. The facts may for the most part be verified by reference to the publications of which the titles are given; and the whole could have been obtained by any one who cared to search other more or less public sources of information. Five of these families (Bentham, Darwin, Dawson-Turner, Roscoe, and Taylor of Ongar) have already been alluded to in my previous work ("Hereditary Genius"), whence I have extracted what appeared to the point, adding what was necessary. In estimating the number of individuals in each generation, the practice has been usually adopted of not counting those who died young, or have not yet attained their 30th year.

ALDERSON.—Many members of this family have been intellectually gifted. There has been an unusual number of cases of mathematical achievement among them.

First generation.—5 males and 2 females, children of the Rev. J. Alderson and his wife (the latter lived to 94). Of these, 3 males deserve notice:—(1) James Alderson, M.D., of Norwich; (2) Robert Alderson, Recorder of Norwich, Ipswich, and Yarmouth; (3) John Alderson, founder and president of all the literary and scientific institutions of the time in Kingston-upon-Hull. All these were men of considerable local repute.

Second generation.—15 males and 12 females, of whom 5 males and 1 female deserve especial mention:—(1) Sir Edward Hall Alderson, Baron of the Exchequer, who was the first man of his year at Cambridge, both in mathematics and classics, being senior wrangler and senior classical medallist, a distinction barely equalled in the long annals of university achievement; (2) Robert Woodhouse, also a senior wrangler, Lucasian and Plumian Professor of Astronomy at Cambridge; (3) the Rev. Samuel H. Alderson, third wrangler, and tutor of Caius College; (4) Sir James Alderson, M.D., F.R.S. (sixth wrangler), for four years President of the Royal College of Physicians; (5) Colonel Ralph Alderson, R.E., a distinguished officer, and one of the first government commissioners of railways; (*1*) Mrs. Amelia Opie, the novelist.

Third generation.—I have not sufficient information, although I know that it includes many persons of ability, among whom is Major H.

Alderson, R.A., a distinguished officer; also a married lady of high artistic powers.

BENTHAM.—A family consisting of only 3 male representatives, all eminent, and one illustrious.

First generation.—2 brothers:—(1) Jeremy Bentham, jurist of the highest rank (life by Sir J. Bowring, prefixed to the collected works edited by him); (2) General Sir Samuel Bentham, whose early manhood was spent in the Russian service; distinguished for his numerous administrative reforms and singular inventive power. Afterwards inspector-general of naval works in England (life by his widow, 1862).

Second generation.—1 male only:—George Bentham, F.R.S., systematic botanist of the highest rank; in early life, writer on logic; for many years President of the Linnæan Society.

CARPENTER.—Among the characteristics of this family are literary and scientific enterprise, philanthropic effort, nonconformity, and aptitude for oral exposition.

First generation.—Rev. Lant Carpenter, LL.D., Unitarian minister; descended from a non-subscribing Presbyterian family, and married to a wife of similar descent; a leading member of the Liberal party in Exeter and Bristol; extremely active in the promotion of philanthropic objects; both literary and scientific in his studies, and a man of local celebrity (memoirs by his son, 1842).

Second generation.—2 males and 3 females, of whom both the males and 1 female require notice:—(1) William B. Carpenter, F.R.S., Registrar of the London University, physiologist, and frequent writer and speaker on scientific subjects, in many cases connected with social amelioration; (2) Dr. P. P. Carpenter (of Montreal), conchologist; actively engaged in philanthropic work; (*1*) Mary Carpenter, actively engaged in the foundation and organization of philanthropic institutions, especially juvenile reformatories, and promoter of female education in India.

Third generation (too young for special notice) includes an influential dissenting minister and a very successful student.

DARWIN.—There are many instances in this family of a love for natural history and theory, and of an aptitude for collecting facts in business-like but peculiar ways. Speaking from private sources of knowledge, I am sure that these characteristics are hereditary rather than traditional; there is also a strong element of individuality in the race which is adverse to traditional influence.

First generation.—(1) Erasmus Darwin, M.D., F.R.S., physician, physiologist and poet. His "Botanic Garden" had an immense reputation at the time it was written; for, besides its intrinsic merits, it chimed in with the sentiments and mode of expression of his day. The ingenuity of Dr. Darwin's numerous writings and theories is truly remarkable. He was held in very high esteem by his scientific friends, including such celebrities as Priestley and James Watt, and it is by a man's position among his contemporaries and competitors that his worth may most justly be appraised. Unfortunately for his memory, he has had no good biographer. He was a man of great vigour, humour and geniality (Miss Seward's life of him, and latterly a pamphlet by Dr. Richardson;

see also Meteyard's "Life of Wedgewood"); (2) his brother, Robert Waring Darwin, wrote "Principia Botanica," which reached its third edition in 1810. It is said (Meteyard's "Life of Wedgewood") that the Darwins "sprang from a lettered and intellectual race, as his (Dr. Darwin's) father was one among the earliest members of the Spalding Club."

Second generation.—7 males, 3 females, of whom 3 males deserve notice:—(1) Charles Darwin, who died at the age of only 21, poisoned by a dissection wound, but who had already achieved such distinction that his name has been frequently mentioned in biographical dictionaries. His thesis, on obtaining the gold medal of the Edinburgh University, was on the distinction between "pus" and "mucus." It was a real step forward in those early days of exact medical science, and was thought highly of at the time; (2) Robert Waring Darwin, M.D., F.R.S., a physician, and shrewd observer, of great provincial celebrity, on many grounds, who lived at Shrewsbury. He married a daughter of Wedgewood's, and was father of Charles Darwin (see below); (3) Sir

Francis Darwin, originally a physician, but for many years living in a then secluded part of Derbyshire, surrounded by animal oddities; half-wild pigs ran about the woods, tamed snakes frequented the house, and the like.

Third generation.—8 males, 14 females, of whom 3 males may be mentioned; but illustriously among them—(1) Charles Darwin, F.R.S., "the Aristotle of our days," whom all scientific men reverence and love; the simple grandeur of whose conclusions is as remarkable as the magnitude and multifariousness of their foundation. There is much ability in many individuals in this generation who bear the name of Darwin, and it has been strongly directed to natural history in the case of (2) a son of Sir Francis Darwin, a frequent writer, under a well-known *nom de plume*, on sporting matters. Among those who do not bear that name (being children of the daughters of Dr. Erasmus Darwin), I mention (3) myself,[1]

[1] Captain Douglas Galton, F.R.S., distinguished for official activity in many high posts, and now Director of Public Works, is descended maternally, not from the Darwin, but from the Strutt family, which has produced noted mechanicians.

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with all humility, as falling technically within the limits of the group of scientific men under discussion, on the ground of former geographical work, and having had much to do in the administration of various scientific societies.

Fourth generation.—Includes very few individuals who have reached mature manhood; among these are (1) George Darwin, second wrangler at Cambridge, author of an important article on "Restrictions to Liberty of Marriage;" (2) ~~Captain~~ Lieut. Leonard Darwin, R.E., who was second in the competition of his year for Woolwich, and now engaged on the Transit of Venus Expedition; (3) Henry Parker, fellow of University College, Oxford, classical scholar ~~and chemist~~.

Lieut/ E/

DAWSON-TURNER.—This family is characterised by great intellectual activity and much artistic taste.

First generation.—Dawson Turner, F.R.S., botanist, scholar, antiquary; a man of unwearied activity in collecting and compiling, and an encourager of work in others. One of his two uncles was the Rev. Joseph Turner, senior wrangler in 1768, and much distinguished by the personal friendship of Mr. Pitt. Among his 10 male first cousins on the paternal side were the late Lord Justice Turner and his accomplished brothers.

Second generation.—2 males and 6 females. The latter were all remarkable for their energy, accomplishments, and the large share they took in the literary labour of their father and husbands, which was not confined to transcribing. Three were accomplished artists, one a musician, another well versed in Greek.

Third generation.—Of those above the age of 30 there are 5 males and 3 females, of whom 4 males deserve mention :—(1) Dr. Joseph Hooker, president of the Royal Society, very eminent botanist, director of Kew Gardens, and formerly Thibetan traveller, and naturalist to an antarctic expedition ; his father was Sir William Hooker, F.R.S., also one of the first botanists of his day, and director of Kew Gardens ; (2) Francis Palgrave, editor of the "Golden Treasury," scholar and art critic ; (3) Gifford Palgrave, orientalist, Arabian explorer, and author of one of the most remarkable works of travel ever written ; (4) R. H.

Inglis Palgrave, statistician. (The father of the three last was Sir Francis Palgrave, historian.)

HARCOURT.—Scholastic success, with much love for science.

First generation.—The Rev. Vernon Harcourt, archbishop of York; a man of polished intellect and social gifts.

Second generation.—10 males and 3 females, of whom 4 males deserve notice :—(1) The Rev. W. Vernon Harcourt, F.R.S., chemist, the first president and one of the founders of the British Association at a time when science was partly ridiculed and partly denounced. He was the chief framer of its elaborate constitution, which is, I believe, a solitary instance of the invention of a complex administrative machinery which worked perfectly from the first, and has continued working, almost unchanged, for nearly half a century. It has served as a model upon which many other societies have organized themselves. (2) Egerton; and (3) Edward Vernon Harcourt, both double-firsts at Oxford; and (4) Granville Vernon Harcourt, who died when an undergraduate at Oxford, having gained the Latin university prize.

Third generation.—10 males and 13 females, of whom 2 males deserve mention:—(1) Sir William Vernon Harcourt, M.P., lately solicitor-general, professor of international law at Cambridge, well known as a political writer under the name "Historicus"; (2) Augustus G. Vernon Harcourt, F.R.S., a distinguished chemist, Lee's reader in chemistry at Oxford.

HILL.—The characteristics of this family are, active interest in social improvement, power of organization, mechanical aptitude, and general sterling worth. Its type in the second generation seems to have been unusually pure.

First generation.—Thomas Wright Hill, descended from stanch Independents, and married to a wife of equal vigour and fortitude, who came from a family noted for mechanical aptitude, which she transmitted to her descendants. He rose by his own exertions, and (æt. 40) established a school, much spoken of at the time, on an entirely new principle of management at Hazelwood, near Birmingham. The boys were taken into administrative co-operation; they regulated their own discipline, and the things they learnt were of the most varied kind. Some men of high note were educated there, and, among these at least one of the scientific men on my list. He gave much attention to mental calculation, and even on his deathbed (æt. 88) invented and successfully applied a new method for determining for any year the date of Easter. Also known for his analysis of articulate sounds and phonography. (Short biographical notice in Annual Report R. Astronomical Society, Feb. 13, 1852.)

Second generation consisted of 5 males and 2 females.—All 5 males had strong points of resemblance and deserve notice. (1) Sir Rowland Hill, K.C.B. and F.R.S., originator and organizer of the system of penny postage, which is an influence of the first order of magnitude in modern civilization. He was noted in youth for powers of mental calculation, and in some points was superior even to Zerah Colburn and George Bidder; thus he could mentally extract to the nearest integer the cube root of any number not exceeding two thousand millions. First inventor (1835) of rotatory printing, the method which, with slight changes of detail, is still in use for newspapers. Rewarded by three separate grants, viz., in 1846 by a public testimonial of the value of 13,360*l.*, in 1864 by the award from the Treasury of his full salary of 2,000*l.* a year on his retirement, and in the same year by a parliamentary grant of 20,000*l.* (2) Matthew Davenport Hill, Q.C., late recorder of Birmingham; law reformer of note, especially in reference to dealings with the criminal class, substituting promptitude, certainty and strictness for delay, uncertainty and severity (see *Law Magazine*, July 1872); (3) Edwin Hill, superintendent of the stamp department; first inventor of the envelope folding-machine, since improved by Mr. De la Rue. He completely remodelled the stamping machinery at Somerset House; was most highly commended for these improvements in each of the first three reports of the commissioners of Inland Revenue, and again by a minute on his retirement, referring to his "eminent and exceptional service." He, like his brother, was a standard writer on dealings with criminals; also on currency. (4) Arthur, head-master of Bruce Castle school, where he fully developed the principles first laid down by his father; (5) Frederick Hill, formerly inspector of prisons, then assistant-secretary of the Post-office. A great and thorough reformer of the prisons under his observation, aiming to fit prisoners for honest life on their release. Concurrently, he contributed numerous memoirs on social improvements generally.

Third generation.—14 males and 17 females, among many of whom the family characteristics continue well marked. Thus ~~(1) Dr. Berkeley Hill, and (2)~~ Miss Emily Clark of Adelaide, Australia, are both actively engaged in work connected with pauper children.

LATROBE.—A family characterzied by its religious bent and musical and literary tastes, joined to a love of enterprise.

First generation.—Benjamin Latrobe, a con-

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(1) & (2) Miss Florence & Miss Joanna Hill, and

(3)

vert to the Moravians, of which estimable sect he was a patriarch and a mainstay (Aikin's "History of Manchester").

Second generation.—3 males, 0 females; 2 at least of whom deserve notice:—(1) Christian Ignatius Latrobe, author of the well known collection of sacred music; (2) Benjamin Latrobe, architect and engineer in America.

Third generation.—7 males, 2 females, of whom 2 deserve especial notice:—(1) Charles Joseph Latrobe, governor of Victoria at the time of the gold discoveries; author of a once extremely popular book on Switzerland, called the "Alpenstock," which was the precursor of Murray's handbooks and more generally diffused knowledge. Many others of this generation, who bear the Latrobe name, are gifted with the family characteristics. (2) John Frederick Bateman, F.R.S., distinguished engineer.

Fourth generation—(still young)—includes Colonel Osman Latrobe, who was chief of General Lee's staff in America at an early age.

PLAYFAIR.—Among the characteristics of this family is an interest in various branches of science joined to a capacity for official work and public action.

First generation.—Rev. Dr. Playfair, principal of the university of St. Andrews, author of a work on geography.

Second generation.—4 males and 3 females, of whom 3 males deserve notice:—(1) George Playfair, M.D., chief inspector-general of hospitals in Bengal; he was the head of his profession in India, and author of various medical memoirs; (2) Colonel Sir Hugh Lyon Playfair, who on his retirement from service pursued life of incessant activity in public improvement (numerous biographical notices were written of him soon after his death); (3) Colonel William Playfair, whose memory still lives in India as one of the most accomplished amateur actors.

There were two cousins in this generation, the one a very distinguished man, Professor Playfair, the celebrated mathematician, and author of the "Huttonian Theory," the other was Mr. Playfair, an architect of much eminence to whom many of the principal public buildings in Edinburgh are due.

Third generation.—21 males and 20 females, of whom 2 males deserve especial notice:—(1) The Right Hon. Lyon Playfair, M.P., F.R.S., formerly professor of chemistry, long engaged in scientific administration of various kinds, and postmaster-general at the close of the late administration; (2) Colonel R. L. Playfair, R.A., the well-known consul-general of Algiers, and naturalist. A third brother is a professor at King's College.

ROSCOE.—The type of this family is strongly marked; it has been characterized by much cultivation, refinement, and poetical taste.

First generation.—William Roscoe, author of "Lorenzo di Medici," "Leo X." &c. The above mentioned characteristics were strongly marked in him. (Life by his son, Memoirs by Hartley Coleridge in "Northern Worthies," and "Sketches" by Washington Irving.)

Second generation.—7 males and 3 females, of whom 4 males and 2 females deserve notice:—

(1) Thomas Roscoe, editor of Lanzi's "History of Painting," and author of many other works; (2) Henry Roscoe, author of a standard book on the "Law of Evidence," of "British Lawyers," and of the Life of his father; (3) and (4), both decidedly gifted, and authors of poems of merit; (*1*) Jane Elizabeth Roscoe, a woman of superior mind, intensely interested in public affairs, writer of some poems; (*2*) Mary Anne Roscoe, authoress of poems of merit.

Third generation.—17 males, 16 females, of whom 3 males and 1 female deserve notice:—(1) William Caldwell Roscoe, poet and critic (memoirs and collected works by R. H. Hutton); (2) Henry Enfield Roscoe, F.R.S., professor, eminent chemist; (3) William Stanley Jevons, F.R.S., professor, author of the "Coal Question," and of various works on logic and political economy: (*1*) Margaret Roscoe, afterwards Mrs. Sandbach, novelist.

STRACHEY.—An old family, small in numbers, but of a marked and persistent type.

Among its characteristics are an active interest in public matters, and an administrative aptitude.

There have been men of eminence in generations previous to those mentioned below.

First generation.—Sir Henry Strachey, under-secretary of state, and otherwise employed in high official posts in India, America, and England; real negotiator of Peace of Versailles (Stanhope's "History of England"); received medal of Society of Arts for having introduced indigo into Florida.

Second generation.—3 males, 1 female, of whom 2 males deserve notice:—(1) Sir Henry Strachey, Indian judge, called by James Mill, in his "History of India," "the wisest of the Company's servants;" aided much in the organization of the Indian judicial administration; (2) Edward Strachey, author of reports of acknowledged weight on Indian judicial subjects (Vth Report).

Third generation.—6 males and 1 female, of whom 3 males deserve notice:—(1) Sir John Strachey, eminent in all branches of civil administration in India; (2) Henry Strachey, Thibetan explorer, gold medallist of the Royal Geographical Society; (3) Major-General Richard Strachey, R.E., F.R.S., active administrator of Indian engineering work; physical geographer.

TAYLORS OF ONGAR.—Numerous members of this family have shown a curious combination of restless literary talent, artistic taste, evangelical disposition, and mechanical aptitudes. There is an interesting work published upon it, called "The Family Pen," by the Rev. Isaac Taylor, 1867 (see below in the "fourth generation"), which contains a list of 90 publications by 10 different members of the family, up to that time; and there have been more publications, and at least one new writer, since.

First generation.—Isaac Taylor came to London with an artist's ambition, and ended by being a reputable engraver. He acted for many years as secretary to the Incorporated Society of Artists of Great Britain, which was the forerunner of the Royal Academy. All the family characteristics were strongly marked in him.

Second generation consisted of 3 males, all of whom deserve notice:—(1) Charles Taylor, a learned recluse, editor of Calmet's Bible; (2) Rev. Isaac Taylor, author of "Scenes in Europe," &c., educated as an engraver, and far surpassing his father in ability. He married Ann Martyn, a woman of reputed genius, authoress of the "Family Mansion," and the numerous able members of the Taylor family for the two next generations sprung, with one exception, from this fortunate union; (3) Josiah Taylor, eminent publisher of architectural works; he made a large fortune.

Third generation.—Descendants of Isaac Taylor and Ann Martyn, 3 males and 3 females, of whom 2 males and 2 females deserve notice:—(1) Isaac Taylor, author of "Natural History of Enthusiasm;" (2) Jeffreys Taylor, author of "Ralph Richards," "Young Islanders," &c.; (*1*) and (*2*), Ann and Jane Taylor, joint authors of "Original Poems" (Ann married the Rev. Joseph Gilbert). In this same generation

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is ranked the Rev. Howard Hinton, a leading Baptist minister, who was a son of one of the sisters in the previous generation, and is father of a well-known aurist.

Fourth generation.—6 males and 9 females now living, and some few others who are deceased; of these, 5 males and 1 female deserve special notice:—(1) Rev. Isaac Taylor, author of "Words and Places," of "The Family Pen," and of "Etruscan Researches;" (2) Josiah Gilbert, author of "The Dolomite Mountains;" (3) Joseph Gilbert, F.R.S., eminent for his chemical and physiological researches in their relation to agriculture (the paternal race of Gilbert had also a marked type); (4) Thomas Martyn Herbert, Independent minister, scholar, and writer; (5) Edward Gilbert Herbert, of the Chancery bar, who died young of diphtheria; (*1*) Helen Taylor, authoress of "Sabbath Bells."

WEDGEWOOD.—This family is curious for the sporadic character of its ability, as shown by the number of its members in rather distant relationships who have become distinguished.

The Wedgwoods must originally have been of a pure type, because the name was prevalent in the village where the great potter was born, and the bearers of it were largely inter-related, and followed the same craft. He himself married a Wedgwood, who was a third cousin, and both his father and grandfather were potters. (Meteyard's "Life.")

First generation.—Josiah Wedgwood, F.R.S., "Father of British Pottery," whose once abundant works now fetch fabulous prices.

Second generation.—3 sons and 4 daughters; 1 son deserves notice, viz.: Thomas Wedgwood, who died young. His abilities were great; he was an ardent experimentalist, and has some claim to rank as the first person who ever made a photograph. (See p. 7.)

Third generation, including descendants from the sisters of Josiah Wedgwood, contains:—(1) Hensleigh Wedgwood (English Dictionary and "Origin of Language"); (2) Charles Darwin, F.R.S. (see under Darwin); (3) Sir Henry Holland, Bart., M.D., F.R.S., who died subsequently to my having begun this inquiry; (4) S. H.

Parkes, M.D., F.R.S., professor of hygiéne to the Army Medical School.

Fourth generation.—(See under Darwin.)

STATISTICAL RESULTS.

Let us now look at the near relations of the scientific men from a purely statistical point of view, combining those already quoted with the rest, and calculate the proportion of them who have achieved distinction. It appears from my returns, which are rather troublesome to deal with, owing to incompleteness of information, that 120 scientific men have certainly not more than 250 brothers, 460 uncles, and 1,200 male cousins who reach adult life. They have somewhat *less* than 120 fathers and 240 grandfathers, because the list contains brothers and cousins. I will take two groups:—(1) grandfathers and uncles, both paternal and maternal, say about 660 persons; (2) brothers and male cousins on both sides, 1,450 persons. On the supposition, which is somewhat in excess of the fact, that I am dealing with complete information concerning the families of 120 scientific men :—

I find in the first group of 660 persons :—(1) Jeremy Bentham, a great leader of thought and founder of a school of philosophy ; (2) Wedgewood, the founder of a national industry and art ; (3) Compton, the inventor of a machine for cotton manufacture, which gave a timely impetus to that great national industry ; (4) Maskelyne, an astronomer-royal ; (5) Playfair, the scientific head of a Scotch university ; (6) William Smith, founder of British geology ; (7) Harcourt, the lawgiver and first president of the British Association ; (8) Pemberton Milnes, who refused both a secretaryship of state and a peerage ; (9) Latrobe, who was to the very worthy sect of the Moravians much what Barclay was to the Quakers, that is to say, not its founder, but a great support to it ; (10 and 11) two archbishops, Harcourt of York and Brodrick of Cashel ; (12) Erasmus Darwin, poet and philosopher of high repute in his day ; (13) Isaac Taylor, author of "Natural History of Enthusiasm," &c. I will stop here, though it would be easy to extend the list considerably, if I took a slightly lower level of celebrity for my limit.

Every one of these 13 men when he died, was, or would have been, if he had not previously outlived his reputation, the subject of numerous obituary notices, and his death an event of sufficient public interest to warrant his being reckoned as an "eminent man." I formerly calculated, and have since seen no reason to doubt my conclusions, that the annual obituary of the United Kingdom does not include more than 50 men who are eminent in that sense. Therefore this small band of 660 individuals, contains almost one-fourth as much eminence as is *annually* produced by the United Kingdom. A different criterion of eminence may be found in the number of celebrated men reared in the universities, whither a large proportion of the brightest youths of the nation find their way. I examined the list of honours at Cambridge in the ten years 1820–9 inclusive, and also the four years 1842–5, of which I happen to have some personal knowledge, whence it appeared to me that on the average, 660 Cambridge students do not produce more than 3 men whose general eminence is of equal rank to that of the 13 men in the 660 grandfathers and uncles under consideration. A more exact test, and the best of which I can think, is to examine into the fate of the boys at large schools. It is not difficult to learn the productiveness of each school as regards eminence, because there are annual gatherings, to which former school-boys who have won distinction are generally invited and not unfrequently come. As men begin to distinguish themselves at 35, and may be supposed willing to attend on such occasions till 70, the notabilities invited to be present at school gatherings represent the product of, say, 35 years. I feel sure that 660 middle-class boys do not turn out more than a fraction of one eminent man, though they may turn out many who do well in life and earn fortunes and local repute.

The second of the groups consists as already mentioned, of brothers and male cousins, making a total of about 1,450 men. I will examine the achievements of these, solely in respect to high university success, partly because several of the cousins are too young to have had time fully to distinguish themselves otherwise. Let us limit ourselves to the following names (the list would be lengthened if we took a lower level) :—Cambridge : (1) Alderson, both first classic and senior wrangler, that is, first mathematician of his year at Cambridge; (2) Woodhouse, senior wrangler; (3) Main, senior wrangler; (4) Humphrey, senior classic; (5) Scott, joint senior classic. Oxford: here the method of examination affords no means of ascertaining who is absolutely the first of his year, since the men are grouped alphabetically in classes, and not according to their order of merit in those classes. The names I will select are those of men who were in the first class and have subsequently distinguished themselves, viz.: (6) Moberly, head master of Winchester, now Bishop of Salisbury; (7) Francis Palgrave, critic; (8) Hon. George Brodrick, first class both in classics and history, well known as an influential though anonymous writer. It is a remarkable fact or coincidence, that 5 men out of a group of 1,450, or say 1 out of every 300, should be first of his year in the single university of Cambridge, either in mathematics or in classics. This is ~~about~~ less than the proportion that exists among the men who actually go went to Cambridge, and those, as before mentioned, ~~are~~ were no chance selections, but included a large part of the annual pick of the intellectual flower of the whole nation. Moreover, these distinguished brothers and cousins of scientific men are themselves inter-related; the two senior wranglers, Alderson and Woodhouse, being first cousins, and the two classics, Scott and Brodrick, being first cousins also; both families being, in other respects, rich in ability.

We may otherwise appreciate the influence of heredity, as distinguished from that of tradition and education, by observing the similarity of disposition that sometimes prevails among numerous scattered branches of the same family. The two following extracts from the replies I have received, are illustrations of what I mean:—

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(1) "My numerous relatives, though unknown to fame, are mostly characterised by great breadth of thought and rare independence of action." [These characteristics seem clearly traced by the writer to a great grandparent who immigrated from Germany]; (2) "Counting third cousins, I have scores and scores of relatives, and scarcely an *unsteady* person among them."

I have numerous returns, in which the writer analyzes his own nature, and confidently ascribes different parts of it to different ancestors. One correspondent has ingeniously written out his natural characteristics in red, blue, and black inks, according to their origin—a method by which its anatomy is displayed at a glance.

My data afford an approximate estimate of the ratio, according to which effective ability (hereditary gifts *plus* education *plus* opportunity) is distributed throughout the different degrees of kinship. They state—(1) the number of kinsmen in the several near degrees; (2) the number of those among them who were in any sense public men; and (3) the number of those who, not being publicly known, had nevertheless considerable reputation among their friends. It is therefore only requisite (after some previous revision) to add the returns together, and to compare the number of distinguished kinsmen in the various degrees with the total number of kinsmen in those degrees, to obtain results whose *ratio to one another* is the one we are in search of. These conclusions are not materially vitiated by the fact that different correspondents may have different estimates of what constitutes distinction, so long as each writer is consistent to his own scale. I have tried the figures in many ways—without any revision at all, with moderate revision, and with careful sifting, and I find the proportions to come out much the same in every case. In comparing these with previous results, obtained from an analysis of men of much higher general eminence ("Hereditary Genius," p. 317), I find the falling off in ability from the central figure, the hero of the family, to be less rapid as the distance of the kinship increases. There is however one group in that book, consisting of divines, whose general eminence is not so great as the rest, and which also resembles the scientific men in the family distribution of ability. My former figures for 100 divines gave 22 notable fathers, 42 brothers, 28 grandfathers, and 42 uncles; my present results for 100 scientific men are 28, 36, 20, and 40 respectively.

As regards the relative influence of the paternal and maternal lines, I find close equality. My method of comparison is by setting off paternal grandfathers and paternal uncles against maternal grandfathers and maternal uncles, no other near degree of kinship being available for the purpose. My results for 100 scientific men are:—paternal grandfathers, public characters, 10; of high private reputation, 3; paternal uncles, 13 and 8; making a total on the paternal side of 34. On the other hand, the maternal grandfathers are 11 and 4; maternal uncles, 15 and 7; making a total on the maternal side of 37.[1]

[1] In "Hereditary Genius," p. 196, having fewer cases of scientific men to deal with, I extended my inquiries to

I leave to another chapter some remarks about the relative value of maternal and paternal educational influences on scientific men.

nephews and grandsons, and in a second table even to great-grandparents, great-grandsons, and other equally remote degrees, but this latter was confessedly of little value.

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CHAPTER II.

QUALITIES.

Energy—Size of Head—Health—Perseverance — Practical Business Habits—Memory—Independence of Character —Mechanical Aptitude—Religious Bias—Truthfulness.

In this chapter I will speak of the qualities which the returns specify as most conspicuous in scientific men, and I shall endeavour to make them tell their own tale by quoting anonymous extracts from their communications. Some of these qualities are common to all men who succeed in life, others—such as the love for science—are more or less special to scientific men. We will begin with the general qualities, with the view of obtaining as exact an idea as may be of the degree in which they are present in the leaders of science of the present day, neither exaggerating nor under-estimating.

ENERGY.

When energy, or the secretion of nervous force, is small, the powers of the man are overtasked by his daily duties, his health gives way, and he is soon weeded out of existence by the process of natural selection; when moderate, it just suffices for the duties and ordinary amusements of his life: he lives, as it were, up to his income, and has nothing to spare. When it is large, he has a surplus to get rid of, or direct, according to his tastes. It may break out in some illegitimate way, or he may utilise it, perhaps in the pursuit of science. It will be seen that the leading scientific men are generally endowed with great energy; many of the most successful among them have laboured as earnest amateurs in extra-professional hours, working far into the night. They have climbed the long and steep ascent from the lower to the upper ranks of life; they have learnt where the opportunities of learning were few; they have built up fortunes by perseverance and intelligence, and at the same time have distinguished themselves as original investigators in non-remunerative branches of science. There are other scientific men who possess what is sometimes called quiet energy; their vital engine is powerful, but the steam is rarely turned fully on. Again, there are others who have fine intellects, without much energy; but these latter classes are quite in the minority. The typical man of science has been at full work from boyhood to old age, and has exuberant spirits and love of adventure in his short holidays, when the engine of his life runs free—temporarily detached from its laborious tasks.

We must be on our guard against estimating a man's energy too strictly by the work he accomplishes, because it makes great difference whether he loves his work or not. A man with no interest is rapidly fagged. Prisoners are well nourished and cared for, but they cannot perform the task of an ill-fed and ill-housed labourer. Whenever they are forced to do more than their usual small amount they show all the symptoms of being overtasked, and sicken. An army in retreat suffers in every way, while one in the advance, being full of hope, may perform prodigious feats.

In the following extracts I insert everything that seems deserving of mention as regards the energy of either parent. It will be observed how strong is the tendency for this primary quality to be transmitted hereditarily.

Speaking generally of these and all other extracts printed in this book, I should give the following explanation:—

Whenever anything is interpolated by me it is put in square brackets [ ]. All proper names are replaced by dots, because I do not wish to administer to the love of gossip. It is indeed impossible to prevent intimate friends from sometimes guessing the name of the author, but I have taken care that nothing is inserted which can cause annoyance. I have taken some trifling editorial liberties, such as occasionally working the words of the question into the answer, when the latter was too curt to explain itself; and

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in a few cases the third person has been turned into the first, for the sake of uniformity.

Extracts from Returns.

Two

ENERGY MUCH ABOVE THE AVERAGE—FORTY TWO CASES.

Forbes

1. "Travelling almost continually from 1846 up to the present time. Restless. All life accustomed to extremely rough travel; often months without house or tent. *Of mind*—restless.

"*Father*—Very energetic; restless. In old age travelled considerably. Mentally restless. *Mother*—Quiet and delicate."

Hooker

2. "When young, and to æt. thirty or more, worked habitually till two and three A.M., often all night. Travelled much in various climates. Much endurance of fatigue and hard living—[an excellent mountaineer]. *Of mind*—[has risen to the highest position in his branch of science and conducts an enormous correspondence on a variety of technical and scientific subjects].

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"*Father*—Very considerable energy both in body and mind. *Mother*—Below the average in bodily energy, but remarkably active mentally."

3. "When fishing or shooting (my only occupation during the holidays) I am the whole day on my legs. *Of mind*—In thirteen years I examined and named some 40,000 examples, described about 7,000 species, wrote some 6,000 pages of printed matter, carrying on at the same time a great deal of correspondence. Günther

"*Father*—I cannot say. *Mother*—Is active the whole day. At the age of sixty-three she took sole charge of my child, then but a few weeks old, nursing it for three years, night and day. Energy of mind equal to that of her body."

4. "Remarkable energy and activity of body, and power of enduring fatigue and going without food. Extremely fond of and an adept at all field sports. Abstemious. *Of mind*—Vigorous pursuit of scientific experiments and investigations, of investment and management of money, business transactions, &c. Lawes

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"*Father*—Active in field sports; has ridden sixty miles before dinner. Abstemious. Energetic in mind. *Mother*—Much energy, as shown by activity and power of enduring fatigue. Great physical courage and presence of mind in danger."

Alderson 5. "Remarkable for athletic exercises when at Cambridge. In early life encountered great fatigue with the army, as . . . . during the . . . . war.

"*Father*—Great activity and immense energy in the practice of his profession. A man of most powerful intellect."

Egerton 6. "I have been and still am a strong walker, both mountaineering and deer-stalking. I never knew what it was to be tired, but, after the hardest day, was ready to start again with six hours' sleep. Although in my sixty-seventh year, I am still an indefatigable deer-stalker."

Bentham 7. "Strong when young—walked many a time fifty miles a day without fatigue, and kept up five miles an hour for three or four hours.

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"*Father*—Remarkable energy of body up to the age of thirty, as shown . . . . . *Of mind*—Remarkable energy from early youth to his death (brought on by accident at seventy-three), when he was as actively engaged as ever in preparing for experiments [official and of a very multifarious kind]. *Mother*—Remarkable energy of mind in assisting her father in the preparation of his lectures, and afterwards her husband in his official correspondence and writings. After his death she wrote largely in magazines, and æt. eighty-five published "Suggestions for . . . . [certain improvements in administration]."

8. "When under twenty, have walked twenty miles before breakfast; when about thirty-two, walked forty-five miles; dined and danced till two in the morning without fatigue. At the age of twenty-six, during fourteen days, was only three hours per night in bed, and on two of the nights was up all night preparing for . . . [certain scientific work.] Fond of mountaineering." Ramsay

9. "Considerable energy and power of enduring fatigue; rough travelling on small means War: Smyth

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in . . . [partly-civilized countries.] Have rowed myself in a skiff 105 miles in twenty-one hours whilst undergraduate at . . . ; rowed in every race during my stay at the university; rowed two years in the university crew [Oxford and Cambridge races.]

"*Father*—[Many examples of his energy in his . . . life.] *Of mind*—considerable, compiling and writing on a great variety of subjects, whilst at the same time carrying on a system of . . . observations, and for years together. *Mother*—Energy of mind very similar to that of my father; joining nightly in . . . observations, daily in writing or drawing . . ."

Paget

10. "Very active in business, preferring walking to the compulsory driving; occupied fourteen or fifteen hours a day without distress; restlessness kept under conscious restraint; longing for adventurous travel, but hindered. *Of mind*—I doubt whether anyone in my profession has done more work, if I may reckon the total work done in . . . &c., &c.; and I worked nearly as hard while a student.

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"*Father*—As a young man, an active cricketer and volunteer officer. A very earnest, active man in business, heavily engaged in it from the age of eighteen. Besides, he took an active part in town affairs and the management of many associations. *Mother*—A good walker, very active in the management of her house. Although she had a very large family, and took most diligent care of them, she was always at work, collecting all manner of things, arranging, describing, corresponding, painting, copying; she was never idle."

11. "I seem to possess the same unweariedness as my father, and find myself trotting in the streets as my father used to do. Harcourt

"*Father*—Was very untiring; he tells me he has ridden 100 miles in a day. He could walk up one of the North Wales hills when nearly seventy, and used to go long distances in London, passing often from a walk into a run."

12. "In early life, occasionally working the night through. Great adroitness at games; fast runner; got the prize for fencing at . . . On Arch: Smith

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board a man-of-war in 18 . . did feats of agility, such as going up a rope hand over hand, which none of the midshipmen would attempt.

"*Father*—Great amount of quiet energy. In mind, great energy and perseverance, which lasted to the end of his life. Thus he had known little Greek, but studied it when an old man for the sake of his . . . researches; also Aramaic. *Mother*—Active housemother."

Ansted

13. "Habitually travel by night without interfering with work of any kind carried on during the day. Active habits and great power of enduring fatigue."

Hill

14. "I was in youth and early manhood bodily active, a good runner and leaper, excelling almost all my schoolfellows [the school was a large one] in both points, and a persistent walker. *In mind*—During the best fifty years of my life I went through a large amount of brain-work, and vigorously pursued the several interests indicated in the enumeration of my several occupations.

"*Father*—In bodily activity much like myself, with the addition that he was a good swimmer. *In mind*—Capable of great occasional exertion rather than of sustained effort. *Mother* —*In mind*, very energetic within a limited range. Always showed great courage, fortitude, and equanimity. In her nursing duties, whether of young or old, was active, persevering, and remarkably successful."

15. "At the age of sixty made a tour, chiefly pedestrian, of four weeks in the Alps; ascended Cima di Jazi; crossed St. Théodule Pass, walking sometimes thirty miles a day; æt. 67, grouse-shooting and deer-stalking. Walk six miles daily to present date. *Of mind*—See list and dates of works and papers [an enormous amount of work]. Owen

"*Father*—Active disposition; he let his family estate, entered largely into mercantile pursuits, and died [abroad]."

16. "When young, a very quick runner and jumper; good shot with a bow and arrow. In middle age, walked to extent of twenty-five miles a day for many months, forty miles in one day, Phillips

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rarely tired. *Of mind*—In early life, any amount, provided the subject was interesting."

Parkes

17. "At times, great fatigue has been gone through in connection with my profession. *In mind*—A good deal of continued power of brain-work; mental fatigue is a sensation not known.

"*Father*—Very energetic. In mind, remarkably so. Having been ruined in early life, he articled himself to a solicitor when he was thirty-five years of age; procured good practice, and wrote [a small technical book] on law. *Mother*—Loved to go through much fatigue. In mind very energetic; added greatly to the income of her family by her writings."

W. C. Williamson

18. "Active bodily work an absolute necessity of my being; without it my epigastrium would gnaw itself into fiddle-strings. *In mind*—My scientific works must answer this question [they are very considerable].

"*Father*—Decidedly active and energetic; used to go out fossil-hunting when it was too late to follow his occupation [which involved out-of-door work, lasting all day and fatiguing to the muscles]. *Mother*—Very industrious."

19.—"Excelled at school and college in athletic sports, especially in long jumping (18 feet). *In mind*—Almost incapable of fatigue up to the age of thirty-eight. Usually engaged in literary work until long after midnight. Cobbold

"*Father*—Remarkably active habits; a great reader when not engaged in drawing and writing."

20. "Excellent walker; great endurance of fatigue [facts are given.] *In mind*—Active mental effort all my life; have had abundance of active employment; am now doing duty as . . . . [numerous honorary offices of the first rank in importance and labour.] J. H. Balfour

"*Father*—Energetic, with considerable endurance; good swimmer. *In mind*, he had much the same active employment as myself; he took an active share in science, politics, and in religion. *Mother*—Active habits; she had great power of doing work and carrying on business."

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H. Spencer 21. "When a boy of thirteen I walked forty-eight miles in one day, fifty the next, and about twenty the third; when grown up, my powers were ordinary, certainly not above the average. *In mind*—Naturally indolent; disinclined to work unless with a large object. [N.B. I insert this moderate statement because my correspondent adheres to it verbally, and gives facts and reasons which I cannot controvert; nevertheless, if energy is to be measured by work actually accomplished, and if my correspondent's work be compared with that of other men, the estimate of his energy would be prodigiously increased.]

"*Father*—When a young man he and two brothers walked sixty miles in one day. Much mental energy; ready for all purposes. When old he was astonished at the amount of work in . . . . he did when young. *Mother*—Ordinary, both bodily and mental."

J. Fred. Bateman 22. "Has done his chief brain-work between ten p.m. and two a.m., besides all the day labour; rests perfectly during a night railway journey.

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"*Father*—Great energy, and very active; capable of enduring great fatigue."

23. "Active and energetic from infancy to eighty-four years of age. *In mind*—I must leave my works to answer this question; but I believe I have been a hard worker during the whole period of my existence. [N.B. No doubt of it.] Fairbairn

"*Father*—Energetic, both in body and mind; muscular; a great reader. *Mother*—Delicate, but active and intelligent."

24. "A strong walker and oarsman; can write more rapidly than any man I ever met (thirty folios of seventy-two words, equal to 2160 words an hour.) *In mind*—Have always worked long hours and very fast. Merrifield

"*Father*—Remarkable energy and endurance, notwithstanding asthma: very hardworking as a . . . . *Mother*—Physically weak, but has had a large family; has done a great deal of original as well as of steady work."

25. "I am a hard rider with hounds, fond of mountaineering, and not easily tired.

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"*Father*—An active man all his life, riding every day, and always about, although over eighty."

Darwin

26. "Energy shown by much activity, and, whilst I had health, power of resisting fatigue. I and one other man were alone able to fetch water for a large party of officers and men utterly prostrated [other facts given in illustration of undoubted energy.] *In mind*—Shown by vigorous and long-continued work on same subject, as twenty years on . . . . and nine years on . . . .

"*Father*—Great power of endurance, although feeling much fatigue, as after consultations after long journeys; very active; not restless. *In mind*—Habitually very active, as shown in conversation with a succession of people during the whole day."

Ferguson

27. "Considerable enduring power in fulfilling any given task or duty; have dissected continually for three or four weeks eight or nine hours a day, devoting some sixteen hours to the work at critical times. *In mind*—Considerable.

Wrote and superintended first edition of . . . . , giving instructions to artists regarding from 200 to 300 woodcuts, correcting press, &c., without assistance, in about seven months [all this in addition to professional work]; hard work for mind as well as body."

28. "Energetic. *In mind*—[extraordinarily so, both in administrative and in original work]. Lubbock

"*Father*—Energetic. Author of, I think, more than seventy scientific memoirs."

29. [Formerly great power of railway travel without fatigue. *In mind*—Active and energetic in a very high degree, as shown by the amount of his official and private work]. Capt. D. Galton

"*Father*—Always on horseback; travelled very constantly and rapidly. Steady in pursuit of an object. He would break in horses with great skill and patience; would learn languages with great perseverance, even after fifty years of age. *Mother*—Very energetic in . . . inquiries."

30. "Great activity at cricket and football up to age of twenty-five. Captain of . . . . eleven Scott

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for five years; used to row a great deal in heavy boats."

Haughton

31. "I possess considerable bodily energy, and when young excelled in fencing, swimming, and the high jump. *In mind*—Have worked hard with my brain for the last thirty-five years, almost without intermission.

"*Father*—Considerable bodily energy, and a good pedestrian. *Mother*—Sluggish bodily powers, but in mind most energetic when once roused to action by a subject that interested her feelings."

Newton

32. "Sufficiently patient of ordinary fatigue, cold, and hunger, to enable me to enjoy travelling in unfrequented countries when my companions suffered much discomfort. *In mind*—Can commonly work from twelve to fourteen hours a day without any remarkable amount of exhaustion.

Father—Capable of enduring fatigue."

Parker

33. [This is a case of extraordinary mental activity, as shown by evidence which I do not feel justified in quoting. It was rewarded by

success, notwithstanding serious impediments in boyhood].

"*Father*—A most energetic man; all for practical pursuits. *Mother*—An unusually strong mind, and steady love and search for knowledge."

34. "Walked from Cambridge to London in a day. At the age of sixty-eight ascended the Piz Corvatsch, in the Engadine. *In mind.* [Facts evidencing considerable energy are quoted.] Brooke

"*Father*—Fond of exercise; a good walker. *Mother*—decidedly active bodily habits."

35. "I am decidedly lazy; but with due stimulus could always get through a great amount of physical work, and was rather the better for it. *In mind*—As a boy, I worked for three months all day and all night, with not more than four or five hours' sleep. When full of a subject and interested in it, I have written for seven or eight hours without interruption, and without feeling any notable fatigue." Huxley

36. "In early life as a boy I was engaged in business from twelve to fourteen hours a day, yet De La Rue

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always found time to study and make my own instruments. Later on, my studies and scientific work were always accomplished after business hours; and it was generally my habit to commence work after dinner, and to work in science until two, three, or four in the morning, and to begin work in business again at nine. I never thought of rest if I had anything in hand of interest.

"*Father*—Remarkably active and capable of sustaining an amount of bodily exertion which would have destroyed the health of most men; for example, I have known him sustain great fatigue for eighteen hours out of the twenty-four for months at a stretch. A great walker. *In mind*—Of indomitable activity; a great reader; always at work in applying discoveries in . . . . to the arts; an untiring worker in anything he undertook. *Mother*—Busily active; great and rapid reader of current literature—perhaps had read almost every book of interest in fiction which appeared."

Prestwich

37. "Used to work all day at business, and one half or three-quarters of the night at science.

From Saturday afternoons to Monday mornings would walk forty to fifty miles [in pursuit of a branch of natural history]. Could work hard at business all day (and a very anxious business), and at evening and night would work hard at [two branches of science]. Found a wonderful relief in science.

"*Father*—Energetic in travelling; great energy in business."

38. "For several years was engaged in full medical practice, and at the same time was a lecturer on . . . and engaged in investigations on . . . . for which the Royal medal was awarded by the Royal Society. Andrews

"*Father* and *Mother*—Both of active habits."

39. "In professional life I have often been up three successive nights without distress, but did not like a fourth, if it came. Consider *that* my limit. *In mind*—Wrote . . . . [a considerable work] between eleven P.M. and two A.M., after professional hours. All the time that I have devoted to science has been stolen from strictly A. Farre

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professional engagements, but more often from myself."

Main

40. "Considerable power in earlier days of enduring mental fatigue and of taking up without difficulty a considerable range of subjects. Example:—I was for a little while, æt. seventeen to twenty, employed in teaching, and I contrived in my scanty intervals of leisure to read a very large quantity of Greek and Latin, and to become, without any external assistance, a very fair mathematician [my correspondent occupies a high official position, in which considerable mathematical knowledge is essential]. I learnt also Italian at this time."

Pole

41. "I should say considerable, judging by the number of things I have been able to learn and to do since adult age."

Strachey

42. "I think considerable, in mind. Have commonly had it said of me that it was wonderful how I got through so much work.

Father—Was well known as a hard-worker. *Mother*—A great reader; taught herself Greek and Hebrew, and learnt German, in later life, to read Luther and other theological writers in the original. A great student of theology."

CASES OF ENERGY BELOW THE AVERAGE—TWO CASES.

1. "No remarkable energy of body. *In mind*—Never capable of a large *amount* of brain-work; for years have regarded myself as defective in brain-power. [The actual performance of this correspondent is considerable, and of a very high order.] Sanderson

"*Father*—In early life fond of athletic sports, and an enthusiastic sportsman. Energy of mind very remarkable, shown in early university and professional life and all subsequent occupations. He wrote a large number of publications on subjects of . . . and . . . . controversy. *Mother*—Energy of mind remarkable; zeal in pursuit of interests, excessive."

2. "Constitutionally languid, with a strong wish for greater energy and more power of en- Currey

during fatigue. *In mind*—Energetic as far as health permits. Much occupied professionally, but when well, capable of vigorously following up the science of . . . . in leisure hours.

"*Father*—Energetic in body as far as his health allowed; in mind, very energetic. His brain-work from an early age was very large in amount, and he was vigorous and sanguine about anything he undertook. *Mother*—Very languid; incapable of any bodily exertion. Very little energy of mind; too languid to take much interest in anything beyond her own family."

SIZE OF HEAD.

I may mention that energy appears to be correlated with smallness of head, a fact which is well illustrated here, although the average circumference of head among the scientific men is great. Energy is also, as we have seen, strongly marked among them; but it is much more strongly marked among those who have small heads. I have ninety-nine returns, many of which I have verified myself, using the hat-

Under 22 inches	Energetic
Bastian	–
J. Fred. Bateman	–
Cobbold	–
Capt: Douglas Galton	–
F. Galton	...
Haughton	–
Hooker	–
Fleeming Jenkyn	..
Sir J Lubbock	–
Dr. Parkes	–
Richards	–
Earl of Rosse	..
Sabine	–
Warr: Smyth	–
	14 or 13

24 and upwards	Energetic
Gassiot	
Hirst	
Sir Geo: Lefevre	
Osborn	–
Sharpey	
Dr. Stokes	-
Ld. Hatherley	
Grove	
Owen	

maker's whalebone hoop, and measuring inside the hats. It appears that the average circumference of an English gentleman's head is $22\frac{1}{4}$ to $22\frac{1}{2}$ inches. Now, I have only thirteen cases under 22 inches, but eight cases of 24 inches or upwards. The general scientific position of the small-headed (who are mostly slender, but not necessarily short) and large-headed men seems equally good; but the fact is conspicuous that, out of the thirteen of the former, there are only two or three who have not remarkable energy; and out of the eight of the latter there is only one who has. A combination of great energy and great intellectual capacity is the most effective of all conditions; but, like the combination of swiftness and strength in muscular powers, it is very rare.

HEALTH.

The excellence of the health of the men in my list is remarkable, considering that the majority are of middle and many of advanced ages. One quarter of them state that they

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have excellent or very good health, a second quarter have good or fair, a third have had good health since they attained manhood, and only one quarter make complaints or reservations. Here are two examples of excellent health in which some details are given :—1. "Only absent from professional duties two days in thirty years; only two headaches in my life." The next is from a correspondent who is between 70 and 80 years of age. 2. "Never ill for more than two or three days except with neuralgia; no surgical operations except inoculation, drawing of one tooth, and cutting of corns."[1]

A. Farre

Phillips

I may add a characteristic biographical extract from the *Times*, Oct. 31, 1873, relating to the late Sir Henry Holland, who was on my list :— "Certain it is, as all who have fallen in with him by sea or land will attest, that he might be seen in all climates, in the Arctic Regions or the Tropics, on the Prairies or the Pyramids,

[1] I read this at my lecture at the Royal Institution. It was from the pen of the geologist, Professor Phillips; a few days afterwards, he was killed by a fall down stairs at Oxford!

in precisely the same attire—the black dress coat in which he hurried from house to house in Mayfair. Yet he never had a serious illness till his last. There was not a day, probably not an hour, when he could not boast of the *mens sana in corpore sano;* and, without headache or heartache, he attained the extraordinary age of 86."

It is positively startling to observe in these returns the strongly hereditary character of good and indifferent constitutions. I have classified the entries, each entry giving the health of the scientific man, of his father and of his mother respectively, and find as follows:—First, a long row of such terms as these: "Excellent; excellent; excellent;" or "Good; good; good;" then comes another row in which some ailment is specified by the scientific man as affecting himself, and as having also affected one or other of his parents. Examples:—1. "Excellent, but hay fever; father, excellent, but severe hay fever." 2. "Good in early life, subject to headache; father, good, subject to headache." 3. "Delicate in early life, one lung seriously

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affected; mother delicate and phthisical." I can find only two cases, neither very strongly marked, in which both parents are described as unhealthy, although marriages between such persons are not infrequent. The returns seem to show that the issue of these marriages are barely capable of pushing their way to the front ranks of life. All statistical data concur in proving that healthy persons are far more likely than others to have healthy progeny; and this truth cannot be too often illustrated, until it has taken such hold of the popular mind, that considerations of health and energy shall be of recognized importance in questions of marriage, as much so as the probabilities of rank and fortune.

I may mention, as a fact that corroborates my belief in the exceptionally good physique of scientific men, that I find the average height of those who have sent me returns, to be half an inch above that of their fathers.

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PERSEVERANCE.

Steady perseverance is a third quality on which great stress is laid; but this might have been anticipated, and it is unnecessary to quote many instances. Here are a few:—

1. "I have probably beyond the average, steadiness of determination, even when the subject is distasteful." 2. "Steadiness decidedly marked." 3. "Determination never to leave unaccomplished a matter once taken in hand." 4. "Great continuity and steadiness." 5. "Steady and intense perseverance." 6. "Very persevering, not discouraged by defeat." 7. "Determination to succeed when possible; my motto being 'Whatever thy hand findeth to do, do it with all thy might,' for 'the night soon cometh when no man can work.'" 8. "I do all things at a white heat, but never tire of the pursuit." 9. "Continuous pursuit of certain studies from an early age." 10. "Steadiness and perseverance in the pursuit of an object is my most distinctly-marked peculiarity." 11. "The most prominent are perseverance and

1 Main
2 Hirst
3 De La Rue
4 Maxwell
5 Lawes
6 Cobbold
7 Carter
8 Parker
9 Jevons
10 Armstrong
11 Fairbairn

industry. A willing mind and determination to persevere is, in my opinion, the most direct road to success; we must, however, exercise a sound judgment in the selection of subjects on which to exercise our thoughts."

I do not think it necessary to quote the instances where either parent is also spoken of as being remarkably persevering; these may be taken for granted. I find that the father is referred to in strong terms eight times, and the mother only twice.

As a set-off to the above, Impulsiveness is not confessed to by a single physicist, chemist, or mechanician. It is equally absent in their parents, with the exception of the mother of one of them. Among the remaining men of science, I only find 5 cases, but these are mostly combined with some tenacity of purpose, and they are all inherited.

PRACTICAL BUSINESS HABITS.

Some prevalence of practical business habits might also have been anticipated, but they prove to be much more common than I had expected. Among those who have sent me returns, I count no less than seventeen who are active heads of great commercial undertakings. There are also ten medical men in the highest rank of practice, and eighteen others who fill or have filled important official posts. Here are some answers to my special inquiries :—

1.—A most eminent biolcgist wrote as follows, in reply to the inquiry whether he had any special tastes bearing on scientific success, in addition to those for his own line of investigation :—"I have no special talent except for business, as evinced by keeping accounts, being regular in correspondence, and investing money very well." It is clear that method and order are essential to the man who hopes to deal successfully with masses of details. Darwin

2.—"I believe I may say that my organ of order is highly developed. Of my collection of some 7,000 birds' skins every one is always in its place, ticketed with name, &c., all by my Sclater

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own hand. I spend much time, perhaps too much, in putting things straight."

Huxley

3.—"I believe I am reckoned a good chairman at public meetings, and I always find that administrative and other work gravitates towards my hands."

A. Farre

4.—"My professional life is strictly methodical; every working day is still mapped out into hours, half hours, and quarters."

Fully one half of those who state that they possess business habits in a decided degree accredit one or both of their parents with the same faculty.

Only two of my correspondents speak of being deficient in business capacities. Both these are physicists.

The following quotation may with propriety be inserted here, although the first named quality, independence, is the subject of a future chapter.

Pole

"I attribute all the knowledge I have acquired, and any success I may have had, chiefly to three qualities, all of which I believe I inherited. First, independence of judgment which prompted me to learn for myself what I wanted to know. Secondly, earnestness, determination, and perseverance in acquiring such knowledge, often under difficulties, and in the face of routine business occupation; and thirdly, a business-like, practical, logical way of looking at things, which enabled me to direct attention to the important and relevant, neglecting the unimportant and irrelevant points in what I had to study and do."

MEMORY.

Memory is very variable in power and character, perhaps no other quality is more so. It is an important ingredient in that aggregate of faculties which form general scientific ability, as is shown by the fact that about one quarter of the men on my list possess it in a high degree, but it is not an essential one, because it is defective in about one case in fourteen. A good memory is of greater importance to the young student who has much to learn, than to the advanced philosopher who has chiefly to reflect, and who knows where to refer for information. Memory is usually defective in persons of small ability, but not invariably so; even among idiots it may be sharp. There are two cases of this recorded in the autobiography of the late Mrs. Somerville (p. 92.) One cannot but suspect some exaggeration in the statements, and feel regret that the cases were not fully inquired into, both as regards the precise power of memory, and the degree of development of the other faculties. She says of the first idiot, "He never failed to go to kirk, and on returning home could repeat the sermon word for word, saying, "Here the minister coughed, here he stopped to blow his nose." She then speaks of "another idiot who knew the Bible so perfectly, that if you asked him where such a verse was to be found, he could tell without hesitation and repeat the chapter."

I have sorted such of the replies as are of interest, into the following groups. (1) Good verbal memory, as for prose and poetry, 6 cases;

(2) good memory for facts and figures, 9 cases; (3) good memory for form, 6 cases; (4) good memory for names in natural history, 4 cases; (5) good memory, no details, 5 cases; (6) fitful and peculiar memory, 6 cases; (7) bad memory, 7 cases. Total number of noteworthy cases, 43. I have not included in the above, a few instances in which the scientific man has described his own memory, simply as "good," nor others in which he has made no remark, except that one of his parents had very good memory. The hereditary character of this quality is abundantly illustrated.

Good verbal memory, as for prose and poetry.

1. "Very great, both for facts and words; I could in my earlier days often retain poetry after two perusals, and once learned, it was seldom forgotten. I have seldom met a quicker or more retentive memory in any one." Greg

2. "After reading over a lecture or speech of an hour's duration, three times, can recollect nearly the words as written for 8 or 10 days." Playfair

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[I am informed verbally by this correspondent, that he is obliged to abstain from writing out his addresses, &c., beforehand, otherwise he has found the memory of what he wrote to be so strong and exacting as to make it difficult to him to deviate from it and accommodate his language to the current temper of his audience.]

"*Mother*—Excellent memory."

J. Evans

3. "Considerable, both verbal and objective; great facility in quotations; familiarity with large collections of coins and specimens.

"*Father* and *Mother*—both good memories."

Phillips

4. "In childhood, all the Psalms, old version; much old English poetry; afterwards, nearly the whole Latin grammar (Eton), Virgil, Ovid, Lucan; still later, considerable parts of the Iliad, Odyssey, &c., could be, and partly can [still] be, repeated *ex memoriâ;* zoological, botanical, mineralogical and paleontological names in abundance."

Debus

5. "My memory was very good. I remember as a boy, to have read Schiller's 'Thirty Years'

War;' I could afterwards without effort, say pages of the work by heart."

6. "At school I used to learn in a single evening 100 lines of Virgil, and repeat them correctly in the morning. J. H. Balfour

"*Father*—very good."

Good memory for facts and figures.

1. "Next to no verbal memory, but good for facts small or great which will fit into any chain of reasoning." Huxley

2. "Of moderate verbal memory, but strongly retentive of facts and figures so far as they are related to any subject on or in which I was engaged. Hill

"*Father*—Memory very retentive, but not systematic. He had a great amount of information, but had not great acquirements; his familiarity with Scripture was, however, remarkable. *Mother*—Very retentive for small facts and figures."

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Carpenter

3. "My memory of things learnt early in life (as dates, rules, examples of grammar, &c.) very retentive, but of all isolated facts of subsequent occurrence, as the birthdays of my children, and the dates of events of my own life, I am singularly destitute of retentive power. On the other hand, of whatever is linked by rational association with any subject in which I take an interest, my memory is very good.

"*Father*—The power of his memory was shown by the great range of his acquirements; he had greater power of remembering isolated facts than I have."

Capt Evans

4. "I should say far above the average. I can now refer to note-books of 30 years past and select a special observation. In other words, it is a capital working memory. I never tried to learn pages of poetry, &c.; in this I should probably have failed."

Newmarch

5. "Memory exceedingly strong and retentive, especially of dates, figures and events.

"*Father* and *Mother*—both had good memories."

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"6. Great memory for figures; can get up pages for examination before committees, and dismiss them from memory afterwards. Strong recollection of scenery." J. Fred Bateman

7. "Very retentive memory, especially of acts, circumstances, and individuals." Alderson

8. "Never kept a diary; clear remembrance of events in childhood with their dates in every year from the age of six onwards. Solve problems better out of doors than in the study. Can forget useless knowledge such as formulæ, rules, gossip, &c., very fast." Maxwell

9. "Bad memory for names and dates, but good as regards facts or circumstances; principles in physical science are clearly retained. Siemens

"*Father*—Excellent memory for historical events, including dates and names in ancient and modern history. *Mother* — Moderately good."

Good memory for form.

1. "Memory most treacherous except in certain respects. Vivid and generally very accurate as Ball

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to places and visual images. As to thousands and perhaps tens of thousands of specimens and plants, can remember the exact spot where each was gathered. As to a multitude of facts that should have interested me, my memory is a blank and the original impression revived with difficulty if at all. . . . Very retentive and accurate as to the *sequence* of impressions from early childhood onwards.

"*Father*—Remarkably retentive memory; quoted long passages from classical authors not seen for a very long time previous. Shortly before his death, at 73, recited a long passage from 'Gibbon,' not read for fifty years before. *Mother*—Memory not reliable generally, but clinging strongly to special scenes and events."

Günther

2. "I recognize most of the animal forms which I have previously examined, but I forget easily the details of their structure, also their systematic names (specific, not generic). Likewise I have a good memory for faces, but not for names of persons; could never remember historical dates."

3. "Great power of remembering forms and points of objective interest; none of numbers or abstract arguments. Languages, poetry, &c., soon lost if not kept up." Hooker

4. "Strong local memory especially of scenery." Back

5. "Very good memory for ideas and general notions, also of persons and places seen; verbal memory not at all good. *Mother*—Good memory."

6. "Great memory for faces and objects once seen." Bastian

7. "A good memory for faces, for locality, for things, for events, for scientific facts; but not particularly good for figures or quantities, except in all necessary routine, as in prescribing and in subjects of lecture. Never failed to recall what I desired, in my lectures. Fergusson

"*Father*—An excellent memory; was a very first-rate whist player. *Mother*—An excellent memory; played a capital game at whist."

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Good memory for names in natural history.

The power of recollecting a multitude of grotesque and barbarous names, which all naturalists must possess to a considerable degree, and which seems so extraordinary to persons who are not naturalists, is hardly alluded to in these returns. It would appear that our most eminent naturalists are not very specially gifted among their fellow-workers in this respect. Here are a few cases of a rather good memory of the kind:—

Cobbold

1. "Memory strong up to the age of 38; still good and capable of recognizing and naming probably between two and three thousand species of animals and plants, including fossil forms.

"*Father*—Remarkable; capable of accurately repeating from memory the substance of speeches delivered at clerical and other meetings."

Bentham

2. "Retentive of botanical names; rather deficient in other respects, especially as to persons."

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3. "Retentive for nomenclature, but not for numbers or history." Egerton

4. ". . . . during practitional life I have gone over the foraminiferæ and remember all their names." Parker

Good memory, no particulars given.

1. "Very remarkable retentiveness of memory. Stokes
"*Father*—Good. *Mother*—Very good, full of anecdote."

2. "Very good memory as far as my 85th birthday." Sabine

3. "Very good. Edw. Gray
"*Father*—Good."

4. "Very retentive, but not exactly accurate." Scott

5. "Retentive memory for what was of interest, and very accurate. Archd Smith
"*Father*—Retentive."

6. "Very good as a boy and young man." ?

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Fitful and peculiar memory.

Grove

1. "Occasionally remarkable, but very fitful. I have occasionally been able to repeat pages after once or twice reading; at other times it is below the average. A power of eliminating and retaining the salient points of what I read, if it interests me, but very bad memory for facts and details."

Haughton

2. "Although I can speak for an hour or two from a few notes, I could not repeat correctly a few sentences from memory.

"*Father*—Remarkable for good verbal memory; could repeat pages of poetry and speeches, without mistake, a striking contrast to my own memory."

Harcourt

3. "My father and myself have memories of the same character; treacherous in matters of business and very retentive of scraps of verse read over and learnt long ago. When my father was to have met me, a little boy returning from school at the end of the half, he would forget

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all about it. My engagements sometimes suffer . . . [from similar forgetfulness]."

4. "Memory very retentive in regard to incidents and events, but could never learn by rote except with great effort. Often surprise my patients by recollection of their symptoms, but am often at a loss to connect their names with their faces. A. Farre

"*Father*—Memory remarkably retentive, especially as to the various events of his life and time."

5. "Memory very bad for dates and for learning by rote, but [extraordinarily] good in retaining a general or vague recollection of many facts. Darwin

"*Father*—Wonderful memory for dates; in old age he told a person, reading aloud to him a book only once read in youth, the passages which were coming; he knew the birthdays and those of the deaths, &c., of all his friends and acquaintances."

6. "A peculiar memory; bad for names of De La Rue

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persons, plants, places, &c.; good for subjects connected with others; not bad for numbers.

"*Father*—A most marvellously retentive memory; he could relate minute details of historical occurrences, names of actors in politics, almost all he had ever read (he was a great reader), and was in consequence a most lively companion. *Mother*—Not very good."

Bad memory.

Huggins

1. [A physicist informs me that his memory is unable to retain even the commonest constants in habitual use, and that the selection of his special line of investigation was governed by his sense of this disability.]

Strachey

2. "Bad memory; from boyhood incapable of learning school tasks by heart, though retaining a knowledge of principles and methods."

Carrington

3. "I have a very poor memory; I was once a whole fortnight in recovering the name of . . . . , but I got it at last. I consider that all attempts at making me learn poetry, and in

particular Latin poetry [at school] were gross mistakes; I was never benefited in the least. Reasoning was my forte, and I could never do anything by rote."

4. "A bad memory, especially for names." Guy

5. "Not possessed of a retentive memory either in small matters or large ones, except in those in which I take a *special* interest." Ld Rosse

6. "I was always slow at learning." Miers

7. "Memory not retentive; very much under the influence of association and suggestion. Thomson

"*Father*—Memory very retentive as to principles, facts, and incidents; not much so as to names of persons and objects. *Mother*—Not retentive."

INDEPENDENCE OF CHARACTER.

We now come to the qualities that are of especial service to scientific men; those already mentioned, of energy, health, steadiness of pursuit, business habits and memory, being of general utility. The first of these is

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independence of character. Fifty of my correspondents show that they possess it in excess, and in only two is it below par. Here are a few examples:—

B. Stewart

1. "Left, æt. 12 [that is, ran away from], a school where I had received injustice from the master."

Sanderson

2. "Opinions in almost all respects opposed to those in which I was educated."

Carpenter

3. "I have always taken my own independent line. My heresy prevented my advancement."

Piazzi Smyth

4. "Preference for whatever is not the fashion, not popular, not rich, not very able to help itself, yet with qualities unworthily overlooked or unjustly oppressed."

The home atmosphere which the scientific men breathed in their youth was generally saturated with the spirit of independence. Examples:—

Spencer

1. "My father was extremely independent, in some respects more so than I am. He never altered the fashion of his dress; he never took off his hat to anyone in his life, and never addressed anyone as Esq."

Hill

2. "My father was a Liberal when Liberalism (then styled Jacobinism) was highly obnoxious, an early denouncer of slavery

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(Note. It would be well to give more numerous extracts) and advocate of religious liberty, a free-trader when the world was protectionist, and an opponent of unrighteous war when war was most popular. He was for mitigating our criminal code when hanging was regarded as the sheet-anchor, and, in a word, was politically and socially a very independent spirit." 3. "My father [an exceedingly humane and courageous man, who was a master in the Royal Navy] would never, unless compelled, attend the flogging of seamen, a punishment mercilessly and unsparingly administered in his days (1800–1815)." 4. "It was marked in my father; he held Jacobite opinions, when it was not very safe to hold them." 5. "Maintenance by my father of religious and political creeds at a time when these creeds were unpopular and often disqualifying."

Capt Evans

Prestwich

Allman

In confirmation of the assertion that the scientific men were usually brought up in families characterized by independence of disposition, I would refer to the strange variety of small and unfashionable religious sects to which they or their parents belonged. We all know that

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Dalton, the discoverer of the atomic theory, and Dr. Young, of the undulatory theory of light, were both Quakers, and that Faraday was a Sandemanian. So I find in these returns numerous cases of Quaker pedigree; and I know of one man, not as yet technically on my list, who was born a Sandemanian. There are also representatives of several other small sects, as Moravians and Bible Christians, and the Unitarians are numerous. It will be understood that the object of saying this is not to throw light on the religious tendencies of the scientific men (concerning which I shall have almost immediately to speak), because so off-hand a statement would mislead, but to prove that they and their parents had the habit of doing what they preferred, without considering the fashion of the day. The man of science is thoroughly independent in character.

James Stuart of Trin: Coll:

MECHANICAL APTITUDE.

There is a prevalent taste for mechanics among scientific men, whose peculiarity it is

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to be interested in things more than in persons. One would have expected to find it developed among physicists; and, as a fact, eight of them possess it in a high degree, and similarly among mechanicians and engineers, all of whom must possess it, and four of whom testify to it, but it seems just as strong among the rest. Here are instances and extracts :—

Chemistry.—1. "Constructed a reflecting telescope, with 12-inch aperture." 2. "Ground, polished, and silvered a 7-inch glass speculum, and mounted it equatorially." *Geology.*—3. "Considerable mechanical skill." *Biology.*—4. "Always fond of constructing; school nickname, 'Archimedes.' If I had followed my profession should probably have been [very successful as] an engineer." 5. "Very fond of mechanical contrivances. Invented and made my own toys as a child. Mechanical tastes are still largely indulged in intervals of leisure." 6. "Special love of mechanics; a good amateur cabinet-maker and blacksmith. Made lithotrites." 7. "Talent for mechanics." 8. [Was extremely ingenious in devising modes of preserving and

1. Buckton
2. Frankland
3. J. Evans
4. Carpenter
5. A. Farre
6. Fergusson
7. Gray
8. Henslow

exhibiting objects of natural history]. 9. "Strong natural inclination towards mechanism." [His present profession was accidental and against the grain]. 10 and 11. "Aptitude for mechanism." 12. "A decided turn for mechanical pursuits, both in arrangement and construction." *Statistics.*—13. "Fond of and quick in understanding machinery." 14. "I always took great interest in mechanical improvement." 15. "I often feel a positive pain in passing an object of which I do not comprehend the meaning and construction."

9. Huxley
10 Miers
11 Sanderson
12 Thomson
13. Greg
14. Hill
15. Jevons

RELIGIOUS BIAS.

It appears that out of every ten scientific men, seven call themselves members of the established Churches of England, Scotland, or of the now disestablished Church of Ireland, and three belong to one or more of the following sects, which I name in the order in which they are most numerously represented :—

1. None whatever; 2. established Church with qualification; 3. Unitarian; 4. Noncon-

formist; 5. Wesleyan; 6. Catholic; 7. Bible Christian. There is much Quaker, and even some Moravian blood, but there are none who have sent me returns who still profess those creeds. The creeds of the parents are somewhat more varied than the above, and the Unitarian element is stronger.

The religious feeling of men of science is necessarily of a peculiar character. Being thoughtful men, they are probably more occupied with religious ideas than the generality of people; but, being exacting of evidence and questioners of authority, they sturdily object to much that others accept easily. But what is "religion?" It is one of the vaguest of words. Let us try to express ourselves more clearly. I think we may assume that the general tendency of scientific men is to take a "philosophic" view of life; that is, to show some disregard of the petty, transient events which chiefly absorb the attention of mean minds, and to feel most at peace when their thoughts are reposing on the larger and more enduring aspects of the moral and material world. Also, it would be easy to show that no class in the community are more active as philanthropists than scientific men. But these tendencies do not cover the meaning of the phrase, "religious bias" in its technical sense. So far as I understand that sense, it comprises three elements :—

1. Great prevalence of the intuitive sentiments; so much so, that conflicting matters of observation are apt to be laid aside, out of sight and mind. The intuitive sense of a supreme God, who communes with our hearts and directs us. 2. A sense of extreme sin and weakness, as expressed by the liturgical phrases, "No power of ourselves to help ourselves," "Through the weakness of our mortal nature we can do no good thing without Thee," &c. 3. Revelation of a future life and of other matters variously interpreted by different sects, which, more or less, satisfy the intuitive sentiments.

I did not enter into these details in framing my questions, but simply asked in general terms whether or no my correspondents had a strong religious bias. The interpretation I put on the answers which are subjoined, is that religion, in the sense of the third paragraph, is not actively accepted by many of those who describe themselves as religiously inclined: they seem singularly careless of dogma, and exempt from mysterious terror. Also, considering the independence of their disposition, their energetic temperament and healthful physique, I should think that religion, in the sense of the second paragraph—that of feeling sinful and weak—would not express the views of many of them. Therefore I look on the intuitive sentiments, as described in the first paragraph, connected with a philosophic frame of mind and a tendency to active philanthropy, as the most likely meaning of the phrase "religious bias," when it is used without any qualification by my correspondents, especially by those who are Unitarians. In this sense, at least, there appear to be about eighteen instances of scientific men who have a decided religious bias; being, I should estimate, at the rate of two or more, in every ten; but I am not able to state with certainty how many of these are religious in the sense of all the three paragraphs.

Religious sentiments weak, accompanied with more or less Scepticism.—1. [Being compelled (Carrington) to attend frequent chapels at college, he, for ten years afterwards, refused to enter either (Strachey) church or chapel]. 2. "The negative tendencies of my family may be absence of piety . . . ." 3. "Religious feeling not great." 4. "Sceptical." 5. "Not much religious bias except in a boundless admiration of nature." 6. (Darwin) "I gave up common religious belief, almost independently from my own reflection." 7. "Bias towards freedom of thought in religious matters."

Intellectual interest in religious topics.—1. (Stewart) "Entertained at an early age independent views regarding the resurrection and salvation of the heathen, which led to frequent disputes." 2. "At school I became a sceptic, and even worked out in my own somewhat (at that time) reserved mind, a kind of idealism. I afterwards had a phase of religious fervour, but worked through (3 Maxwell) it." 3. "Given to theological ideas, and not (4. Main) reticent about them." 4. "Instinctive (or original) religious bias, though this may be in part due to early training. . . . I take considerable pains in the investigation of religious matters, one of my amusements being the collection of a considerable theological library, with the books of which I am familiar."

Dogmatic interest.—"I have no more doubt about the plenary inspiration of Scripture than I have about the simplest axiom in mathematics." [I class this exceptional reply under "dogmatic interest" because the remainder of the writer's brief communication hardly suggests the dependent frame of mind that is characteristic of "piety"—*e.g.*, "Never received or asked a single favour or a single farthing for anything I ever wrote or did."]

Jas. Bateman

Religious bias.—1. "Religious bias." 2. "Of a religious bias of thought." 3. "Religious views liberal, but strongly anti-materialistic." 4. "Early religious impressions strong, but have, on the *dogmatic* side, quite disappeared. The belief in a permanent antithesis between good and

Ball

K 2

evil, irrespective of utilitarian results, has survived, with no keen sense of the need of a dogmatic basis for the belief." 5. "Much religious bias of thought from early education." 6. "I have been the more biased towards religion, in that my father and maternal grandfather lived it and did not prate about it. I am personally only a combination of these two men in this respect (. . . Please take the *sense* of what I have written, and not the *words*)." 7. "Religious bias of thought decided." 8. "Although firmly and thoroughly believing in Christianity, and accepting it as the guide of my life, as far as I can understand it, being also a regular attendant of the Church of England, still I cannot admit the right of that or any other Church to teach dogmatically what truths are necessary for my salvation; and the feelings which ever cause me to resent any interference with the liberty of conscience are quite as strong in me as they were in the breast of my ancestor, when he gave up the land of his birth and property, more than 300 years ago." [My correspondent has shown marked instances of independence of character,

Parker

Lawes

f. 150r

and is descended maternally from both Flemish and French religious refugees, and paternally from an English Nonconformist, who left his country and settled in America.] 9. "It is difficult to estimate one's own peculiarities, but I believe I may credit myself with more than the usual amount of (. . . and) religious bias of thought. I have mixed and worked with Christians of most of the Protestant Churches." 10. "Strong religious feeling. My intention on entering . . . was to devote myself to a missionary life in China; but my unexpected success in . . . persuaded my friends to induce me to abandon my purpose, on the ground that I might serve God better in my new sphere at home. I yielded to their arguments with great reluctance." 11. "Intensely religious; formerly in the Evangelical sense (a tract distributor, promoter of prayer-meetings, bible classes, &c.) Excessive distaste to novels and fictions in any shape." (See "*Indifference to dogma*," p. 137) 12. "I was brought up an ordinary member of the . . . Church, but ultimately came to the conclusion that . . . was essentially illogical. . . . I had

Gladstone

Haughton

Cobbold

Mivart

f. 150v

the happiness of seeing my mother follow me into the . . . Church." [I regret that I am unable, with propriety, to give fuller extracts from the most interesting and instructive replies of this correspondent.]

Religious bias, with intellectual scepticism.—
Capt. Evans 1. "I have not cultivated independence of judgment in religious matters; I have shrunk from so doing in order to retain peace of mind and leisure for my varied studies." 2. "Much religious bias of thought, but no respect for revealed religion as a base for such a bias." Marshall 3. "Religious bias towards natural theology strong, as distinguished from dogma of any kind." 4. "I have, perhaps, a religious bias from early habits and associations, Carpenter rather than from temperament; but I have always had more pleasure in sacred than in secular music, which, perhaps, shows the predominance of the emotional tendency." 5. "A profound religious tendency, capable of fanaticism, but tempered by no less profound theological Huxley scepticism."

Next, as regards the effect of dogmatic teaching, or of "creed," on research. I had expected it to have been much more deterrent and hindering than the answers warrant. The suicide of the geologist, Hugh Miller, whose brain gave way under the conflict between dogmatic creed and scientific doubt, is a terrible tale. One would have thought that the anathemas from the pulpits against most new scientific discoveries, as soon as they became capable of popular application, such as geological history, antiquity of man, and Darwinism, must have deterred many; and, as I have already shown, few of the sons of clergymen are on my list. Nevertheless, in answer to my direct inquiry "Has the religious creed taught in your youth had a deterrent effect on the freedom of your researches?" I am met with an overpowering majority of negatives. Seven or eight say "no," justifying their assertion by various reasons, to one who says "yes," as is shown by the appended replies. These may be sorted into the four following groups:—

(1) "No" deterrent effect—39 cases. (2)

"None," with emphasis—12 cases. Examples: —"None whatever;" "not in the least;" "not in the slightest;" "decidedly not;" "certainly not." (3) "None," with various classes of reasons why it had not—14 cases. (4) Has had a good and not a bad effect—8 cases.

Further specimens of the first two groups "no," with or without emphasis, are needless; but I will give extracts from the remainder, divided under convenient heads.

1, Ball

Have no dread of inquiry.—1. "I do not think so. At the time when I held strongly the . . . dogmatic system I never could apprehend any dread of the results of free inquiry." 2. "None whatever. Absolute and fearless faith in the truth." 3. "I was left free to choose my own religion, and believe that there is no real antagonism between revealed religion and the study of nature."

3. Haughton

Harcourt

Religion and science have different spheres.—1. "No; it [religious creed] has no point of contact with chemistry."

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Indifference to dogma.—1. "Not in the slightest degree; but the method of science has taught me not to put any confidence in creeds or dogmatic statements of any kind." [This correspondent is the tract distributor, &c., of (11) of those having "religious bias" in p. 133.] Cobbold

Liberality of early religious teaching.—1. "None. The teaching was not severe or exclusive in any degree; it was the ordinary teaching of the Church of England." 2. "My religious creed from infancy was that of freedom. I was not taught creed or dogma, and had therefore the great advantage of not having to fight my way out of darkness into light." 3. "I learnt no creed in my youth." 4. "I had no religious instruction at school." 5. "No; freedom of thought was always made a part of the creed practically taught me." 6. "No religious creed was ever taught to me." 7. "None whatever. In fact, no *creed* was taught me." 8. "My religious freedom has enabled me to look every scientific question fairly in the face." 9. "There was no religious coercive education at home,

1. Newmarch
2. Roscoe
3. Bowman
4. Osborn
5. Merrifield
6. Heywood
7. Jevons
8. Carpenter
9. Strachey

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notwithstanding my mother's strong personal religious bent. On the contrary, her influence was quite in the direction of free inquiry, in which she largely indulged herself. My school religious teaching had no effect that I can perceive, either on my intellect or imagination. Its chief result was to make me detest the drudgery of learning catechisms and sitting through dreary sermons."

[2, 3, 6, 7, 8, are children of Unitarian parents.]

1. Huxley

2. Bastian

3. Darwin

Have early abandoned creeds.—1. "At æt. 13, I disbelieved as thoroughly as I do now in the religious creed (that of the Church of England) in which I was brought up; and I had realised Berkeleyan idealism by my own road." [Compare this with the reply, 2, from a different correspondent in p. 130 in the section, "*Intellectual interest in religious topics.*"] 2. "None whatever; I have long since wholly rejected religious creeds." 3. "I gave up common religious belief almost independently from my own reflection." [This quotation is repeated from the last section. The writer's reply to the question of which we are now speaking was a simple "no," and has been classified as such.]

The religious creed has had a good effect on freedom of research.—1. "None [*i.e.* no deterrent effect]; rather the contrary." 2. "On the contrary." 3. "Quite the reverse." 4. "I think none whatever. I have had to overcome some prejudices, but my true religious life has been cognate with my scientific one, and the former has stimulated rather than crippled the latter." 5. "Certainly not! On the contrary, it has had ~~clearly~~ daily the very best effect." 6. "Not a deterrent effect; but it acted as a guide." 7. "Never deterred; now acts as a direct stimulant, since it appears to me that the cultivation of a naturally-implanted intellectual tendency is a religious duty. . . . The most pernicious influence to which I was subjected was that arising from J. Stuart Mill. It took me a long time to work through the sensationalist, empirical philosophy, and to come out at the other side." 8. "No; but the scientific sys-

1. Allman
2. Egerton
3. Thomson
4. W.C. Williamson
5. Parker
6. Günther
7 Mivart
8. Piazzi Smyth

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tem inculcated long prevented me giving my religious feelings and aspirations full sway."

1 Playfair

Has had some deterrent effect.—1. "Certainly the narrow . . . ism of early youth made me for a long time a timid thinker."

2 Capt Evans

2. "To a certain extent, yes—not in philosophical research; but I shrink from the disturbance of mind (not fear of ultimate consequences) which I know would follow diving into certain questions of the day, connected with early religious teachings."

3 Carrington

3. "No; for some time it may have hindered me."

4. Pole

4. "It certainly would have had that tendency, though not that effect, if my researches had taken certain directions."

5. Forbes

5. "Would have been so, had I not fought against it."

~~Owen~~ 6 R. Jones

6. "The 'Biblical faith' prevented my getting good geological views for many years, by having set my thoughts in the old grooves, and thus limited them."

7. Grove

7. "I think not. I emancipated myself from dogmatic trammels early in life, but not without a struggle."

8. Owen

8. "After about ten years' careful consideration of the facts, called

Piazzi Smyth
[illegible].

by theology 'seeming contradictions of science,' I finally discarded the pentateuchal spectacles through which I had previously looked at certain phenomena. I lay to early theological teaching so much hindrance in the quest of the most precious of our possessions—truth."

TRUTHFULNESS.

A curiosity about facts is much spoken of and implied in the answers to my questions; in a few cases it is combined with a curious repugnance to works of avowed fiction. A hunger for truth is a frequent ingredient in the disposition of the abler men of every career; but in all probability it is felt most strongly and continuously by men of science. The most clearly-marked characteristic of scientific society seems to me to lie in the careful accuracy with which facts and anecdotes of all kinds are related. I have the good fortune to be acquainted with a large family circle whose curiosity about facts and practice of scrupulous and, so to speak, *artistic* truthfulness continually excite my admi-

Darwin

ration. It has not unfrequently happened to me to hear a remark or statement, which I had made to one of its members, alluded to by another, in which case I have been usually astonished at the precision with which it was repeated. The repetition of the statement retained the precise shade of sense that I originally intended to convey, yet it was almost always presented in a simpler and more striking form. The essentials had been truthfully adhered to; the non-essentials were pruned off and the language was improved. The rarity of a faculty like this is easily tested by the experience of the well-known game of "Russian Scandal," and has probably been impressed on most of us when we have discovered some misrepresentation of what we did or said. Truthfulness of expression adds greatly to the charm of life; it gives a grateful sense of confidence towards those who are distinguished for it and it makes conversation more real and far more interesting. There is an exact parallel between truthfulness of expression in speech and that of delineation in drawing. In the earliest sketch it is far better to be hard in outline than inaccurate. Subsequent touching up can smooth away the hardness; but there exists no proper material to work upon when there was carelessness in the first design.

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CHAPTER III.

ORIGIN OF TASTE FOR SCIENCE.

Preliminary—Extracts at length—Analysis; Innate tastes—Fortunate accidents—Indirect motives or opportunities—Professional duties—Encouragement at home—Influence and encouragement of friends—Influence and encouragement of tutors—Travel in distant parts—Unclassed residuum—Summary—Partial failures.

WHAT were the motives that first induced the men on my list to occupy themselves with science?

A question such as this may seem hard to answer, except in very general terms. Those who are but little versed in statistics may be daunted by reflecting on the infinite diversity of characters and antecedents; while those who are, will be less easily discouraged. Reiterated experience will have shown them how surely, in every case with which they have dealt, the great majority of causes, or what might be better named "pre-efficients," admitted of being analysed and grouped into natural orders, leaving a minority of unclassed influences, which themselves form a class of their own, and which can be reduced indefinitely, in proportion to the minuteness with which the statistician cares to pursue his analysis. The statistics of railway accidents will serve as an example. When Captain Douglas Galton was secretary of the railway department of the Board of Trade, he succeeded in sorting their causes into the groups in which we have since been accustomed to see them printed year after year. So long as the general system of management of a railway is little changed, the same statistical ratio is maintained among them, a given proportion of accidents being due to this cause, and another to that. We may therefore estimate with some certainty the saving of life and limb, or of material of various descriptions, that will be effected when any one of these causes shall be wholly or in part removed. Similarly my aim is to group the influences which first urged the men on my list to pursue what afterwards became their favourite occupation. We shall learn the relative importance of these influences, and be enabled to estimate with greater precision than before, the value of proposed methods for making the pursuit of science more common than at present.

The returns I am about to quote are replies to the following questions:—"Can you trace the origin of your interest in science in general, and in your particular branch of it? How far do your scientific tastes appear to have been innate?"

The answers were of unequal length and minuteness. From the longer ones I have extracted what was essential, and in these and in the rest I have taken a very few editorial liberties, as already mentioned.

At this stage of the inquiry it becomes advisable to separate the replies according to the branch of science pursued by those who made them. I have not kept geography separate, because there are not many geographers on my list, and those who were, admitted of being sorted under other titles. With this exception the divisions I have adopted are much the same as those of the various Sections and Sub-sections of the British Association.

Some doubt may be felt as to how far the replies may be trusted. For my own part, I believe they are substantially correct, judging principally from internal evidence, and partly from having questioned different members of several families, and finding their opinions corroborative. The greatest difficulty I have had in my inquiries generally is due to reticence on the part of the writers, who say nothing when much was to be said; but even this does not affect *relative* results. Again, many men are conceited; still the forms in which conceit shows itself do not much affect those results. Thus, a too emphatic narration of early achievements does not distort their mutual proportions. If men are too proud to acknowledge their indebtedness to natural gifts, the relative value they ascribe to motives remains unchanged. I am astonished at the unconscious vanity which

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I have elsewhere met with when making inquiries in heredity, shown by men who, owing enormously to natural gifts, wish to accredit their own free will with being the real causes of their success. ~~One phase of this form of vanity is prominently illustrated by the late John Stuart Mill, in his strange and sad autobiography, who declares~~ (p. 30) that he was rather below par in quickness, memory, and energy, and that any boy or girl of average capacity and healthy physical constitution, who was properly taught, could make as rapid progress in learning as he did himself! As regards the scientific men, I find, as I had expected, vanity to be at a minimum, and their returns to bear all the marks of a cool and careful self-analysis. My bias has always been in favour of men of science, believing them to be especially manly, honest, and truthful, and the results of this inquiry has confirmed that bias.

The influences and motives which urged the men on my list to occupy themselves with science fall under the heads given below. I

A curious instance, I ought not to say of unconscious vanity, as I did in the first edition of this book, but rather of ~~extreme~~ ~~[illegible]~~ absurd self-depreciation, is that of the late John Stuart Mill, who declares in his strange and sad autobiography have distinguished each head by a letter, and added to each reply the letters that seemed appropriate to its contents. The replies are subsequently analysed according to these letters.

SIGNIFICATION OF THE LETTERS.

	Number of Instances.	
a.	59	Innate tastes (*mem:* not necessarily *hereditary*).
b.	11	Fortunate accidents. It will be noticed that these generally testify to the existence of an innate taste.
c.	19	Indirect opportunities and indirect motives.
d.	24	Professional influences to exertion.
e.	34	Encouragement at home of scientific inclinations.
f.	20	Influence and encouragement of private friends and acquaintances.
g.	13	Influence and encouragement of teachers.
h.	8	Travel in distant regions.
z.	3	Residual influences, unclassed.

EXTRACTS AT LENGTH.

PHYSICS.

(1) "My tastes are entirely innate; they date from childhood." (*a*) Lassell

(2) "As far back as I can remember, I loved nature and desired to learn her secrets, and have Haughton

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spent my whole life in searching for them. While a schoolboy I taught myself botany, chemistry, &c. . . . . under great difficulties. I had no teacher except a kind apothecary, whose knowledge was limited." (*a*)

Capt. Evans

(3) "From a youth, I always preferred the man of marked ability to the man of action alone. Thrown for so many years of my professional life among men chiefly of the latter class, and my sympathies being more drawn towards those in the decided minority, my tastes were, I conceive, not acquired but innate. In the early days of my professional career I gained the friendship of . . . . , of the highest professional standing, whose acquired general knowledge and love of science and observation were far beyond those of the ordinary . . . . of his time. I was both his young friend and favourite assistant for three years. He imbued me with his respect for science, and formed my character for earnestness and accuracy. . . . . To some extent, my tastes were determined by events after manhood; because in . . . extend-

ing over ten years, I held positions of great responsibility [in different parts of the world], but I consider my scientific tastes were formed in youth, that is, from 16 to 21 years of age." (*a*, *f*, *h*)

(4) "From an early age I was addicted to mechanical pursuits. In the last few years of my schooldays I took to chemistry. Entered . . . . college, expecting, after two or three years there, to [join a relative's] business as calico-printer, and gave especial attention to chemistry on that account. . . . . I had never attended *specially* to physics until appointed professor of natural philosophy. . . . . [This and subsequent similar advancement] determined me to devote myself thenceforward definitely to physics, and not to try for a chemical appointment . . . ." (*a*, *d*) Forster

(5) "Naturally fond of mechanics and of physical science, in which all my study has taken the direction of those departments bearing on . . . . , owing to my feeling that through the possession of special instruments for investigations in it, I could work to greater advantage; Earl of Rosse

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not from any natural preference for . . . . over the other departments of physical science." (*a*, *c*)

Andrews

(6) "My tastes were partly natural, partly encouraged by an eminent friend . . . . , who had been honoured himself by the friendship of most of the leading men of science in the early part of this century." (*a*, *f*)

Stewart

(7) [Yes.] "I remember [incidents which proved an innate taste quoted at length] before I could write, [but] I believe the origin of my pursuit of physical science was when I attended the natural philosophy class at . . . . I was intended for business, but conceiving a distaste for it, I left it and attached myself to science." (*a*, *g*)

Scott

(8) "I cannot say, except that I had an innate wish for miscellaneous information. My interest in science arose from the chance circumstance of my choosing civil engineering as a profession, and having spare time, when studying at . . . , which I devoted to . . . . My scientific tastes were subsequently determined by my not

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having any profession, except civil engineering, which I never followed." (*c*)

(9) "Ocean voyaging in beginning of life. Solitary observing for years in an observatory, placed in a country verging on a desert, but under southern skies, rich in stars unknown to the ancients and not appreciated by the moderns." (*d*, *h*) Piazzi Smyth

(10) "The origin of my interest in science is mainly due to my father's knowledge of geology, navigation, and engineering. My scientific tastes were confirmed by lectures, by . . . . and . . . . and . . . . and especially by the encouragement of the latter." (*e*, *g*) Miller

(11) "Primarily derived [both by inheritance and education] from my father." (*a*, *e*) Brooke

(12) "My first start was reading a child's story, called the 'Ghost,' where a philosophical elder brother cures his younger brother of superstition, by showing him experiments with phosphorus, electricity, &c. This set me on making an electrical machine with an apothe- Grove

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cary's phial, &c. I was then about 12 years old. My grandfather had scientific tastes to some degree. My grandmother's brother . . . . was a good amateur chemist and astronomer. He was a well-known leader of musical, and to some extent, of scientific society, at . . . ." (*a*)

Strachey

(13) "A mathematical tendency, I think, led me first towards . . . . inquiry, to which I have been faithful ever since. Professional duties and civil engineering kept up a disposition to appreciate the material constituents of the world, and led, through surveying, in the direction of physical geography. The distinct origin of my desire to place myself among scientific students was the wonderful impression produced on me by the aspect of nature, as seen in the . . . . combined with what I may call the accident of my having been allowed to explore a part of it in an official capacity. Having thus made rather large botanical and geological collections, I came to England with them, and while employed in arranging and distributing them, picked up a certain rather irregular and un-

systematic scientific education, in the company of . . . . and others. Forced back into professional life, special scientific inquiry has not been possible; but I have had opportunities of aiding the progress of science, which I have endeavoured to make the best of." (*a*, *d*, *f*, *h*)

(14) "Largely determined by my service in north polar and equatorial expeditions." (*d*, *h*) Sabine

(15) "I am not aware of any innate taste for science. I can only remember in boyhood the influence of the Philosophical Society of . . . . and of a juvenile philosophical society in which I took interest. My interest in astronomy, especially, was very small indeed, until I was appointed [to the directorship of an observatory]." (*d*) Main

Mathematical Subsection.

(16) "I always regarded mathematics as the method of obtaining the *best* shapes and dimensions of things; and this meant not only the most useful and economical, but chiefly the most harmonious and the most beautiful. . . . . I was Maxwell

taken to see . . . ., and so, with the help of 'Brewster's Optics' and a glazier's diamond, I worked at polarization of light, cutting crystals, tempering glass, &c. I should naturally have become an advocate by profession, with scientific proclivities, but the existence of exclusively scientific men, and in particular, of . . . . , convinced my father and myself that a profession was not necessary to a useful life." (*a*, *e*, *f*)

Hirst

(17) "My taste for mathematics appears innate. As a boy, I delighted in sums. I trace the origin of my interest in general science to my acquaintance with . . . . , which dates from the time when I was about 15 years of age. I taught myself in mathematics and chemistry during my apprenticeship to a civil engineer and land surveyor, and subsequently studied . . . . [abroad]. My scientific tastes were largely developed through my first going [to the continent] with . . . ." (*a*, *f*)

Cayley

(18) "An early taste for arithmetic, and in particular for long division sums." (*a*)

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(19) [The following is an extract from biographical notes kindly communicated to me of the late Archibald Smith.] "Yachting would give an interest to all nautical matters, and the intimacy of his father with . . . . gave a bias towards magnetism. In a letter to one of his sisters (no date, ? about 1838), he says :—'. . . . told me he was going to write directions for ships, finding and allowing for the error caused by the local attraction of ships. So, for my own amusement and partly to help him, I wrote a set of instructions and gave them to him.' His mind was thus turned to the subject. I think it was natural to him to inquire into the reason of things. Fond of figures when a boy." (*a*, *b*, *c*, *f*) Arch: Smith

(20) "My interest in mathematics began at . . . . [university], and was mainly due to the energy and encouragement of my tutor . . . . ; but Professor . . . . first inspired me with the sense of the magnificence of mathematics." (*g*) Spottiswoode

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CHEMISTRY.

Harcourt (1) "Thoroughly innate. My first taste for chemistry dates from the possession of a chemical box, when I was a little boy. Whenever I had a chance of turning from other studies to natural science, I always turned. I liked play better than all other work, and chemistry better than play." (*a*, *b*)

Buckton (2) "Perhaps wholly innate. My first notions of chemistry were picked up from books, and I got the nickname of 'experimentalizer' at school. My taste for zoology arose through friendship with . . . . My tastes were largely determined by three years' voluntary work at chemistry, under Dr. . . . ." (*a*, *f*)

Roscoe (3) "I was always observing and inquiring, and this disposition was never checked nor ridiculed in my childhood. My taste for chemistry dates from the lectures I attended as a boy, and to the permission to carry on little experiments at home in a room set apart for the purpose. I was encouraged in my tastes at home. Subsequent determining events were my residing abroad, and my mother making a home for me there." (*a*, *b*, *e*)

(4) "They date from a very early period, and there was little to produce them in my early surroundings. As a small boy I was fond of reading books bearing on natural science. I was taught at home with my brothers, and was partially self-taught also. We had always the example of industry, and were encouraged to think for ourselves. I first studied chemistry at . . . . College." (*a*, *e*) Gladstone

(5) "From an early age I had an innate taste for all branches of natural science. As a boy, I made large collections of dried plants, minerals, beetles, butterflies, stuffed birds, &c. At . . . . I studied without regard to future profession for two years, and only took up chemistry as a special study on my third year's residence there." (*a*, *c*) Voelcker

(6) "I cannot trace the origin. I began to study chemistry æt. 18, and pursued it at such Yorke

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times as my duties in . . . . gave me leisure, and without any instructor. The obtaining of correct and accurate results in chemical analysis gave me great satisfaction." (*c*)

Playfair (7) "Scarcely innate. I ascribe the origin of my scientific interests chiefly to being sent as a pupil to an eminent man of science, Professor . . . . Subsequently I was a good deal abstracted from scientific pursuits by an early and lasting friendship with . . . . , who directed my thoughts to public work." (*g*)

Maskelyne (8) "I watched, at school, the building of a steam engine at a factory, and completely got up the whole engine. This gave my mind a start. . . . . My father gave me 'Henry's Chemistry;' that, and afterwards 'Turner's Chemistry,' were more interesting to me than any books of fiction. . . . . I believe at one time I read little else but 'Turner's Chemistry' and books of poetry in whatever holiday I had. . . . I owe to my mother a child's curiosity and afterwards a man's reverence for scientific truth. I cannot tell if my scientific tastes were innate. The university, inviting me to fill the . . . . chair, gave my work its bent. . . . ." (*d*, *e*)

(9) "I can trace my interest in chemistry to reading by accident a book upon it." (*b*) Debus

(10) "I did nothing, even *quasi*-scientific, till after leaving college; nothing serious till æt. 23. My pursuit of chemistry is entirely due to circumstances occurring after manhood, and in direct opposition to family influences." (*z*) Lawes

(11) "To the opportunity afforded for study of science at . . . My tastes received no encouragement whatever from relations, my mother excepted." (*e*, *z*) A.W. Williamson

GEOLOGY.

(1) "Decidedly innate as regards coins and fossils. My father and an aunt collected coins and geological specimens, and I have both coins and specimens which have been in my possession since I was 9 years old. Subsequently my pursuits were influenced to some extent by the discoveries in . . . . , but at that J. Evans

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time I had already a considerable collection." (*a*, *c*, *e*)

Rupert Jones (2) "A natural taste for observing and generalizing, developed by noticing the fossiliferous rocks which happened to occur in the neighbourhood of the school where I was. Afterwards the surgeon to whom I was articled, who had an observant mind, fostered my tastes." (*a*, *b*, *f*)

Ansted (3) "A natural taste. My interest in science began very early, originating in a love of experiment, at first in chemistry. . . . . The ultimate direction of my scientific tastes dates after the completion of my regular education." (*a*, *c*)

Warington Smyth (4) "I believe I may say, innate, to a very considerable extent, not remembering that any definite steps were taken to inculcate science. I was indebted in a high degree to collections made by my father and mother, in . . . . , and to early familiarity with charts of those seas, and conversations on matters pertaining thereto. Afterwards, to going to Germany and finding in the mining officers a body of men receiving a regular scientific education. Lastly, to a great extent by going for a winter to . . . . [in Germany], and by conversations with . . . . and . . . ." (*a, e, f*)

(5) "I was always fond of natural history; collected plants, insects, and birds, at [school] and fossils at [college], where . . . . 's lectures attracted me to geology, and subsequently, by the acquaintance of Professor . . . . , to the particular branch [of it which I have pursued]." (*a, f, g*) Egerton

(6) "As well as I can recollect, they were innate. I remember, as a boy of 6, seeing a spring in Lavender Hill; not being satisfied at the explanation, and determining to work it out for myself. I believe that I should have devoted myself to chemistry and physics, but that I was started, as a youth of 19, to travel 10 months out of the twelve on business, and so continued for 20 years. This led to my visiting all Great Britain, and to great opportunities for geologising and determined me to that study. I Prestwich

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worked hard at business all day (a very anxious business), and at evening and night would work hard at chemistry and geology. I found a wonderful relief in science." (*a*, *c*)

Forbes

(7) "I believe the desire for information and habits of observation to be in a great measure innate. They were first developed by a little elementary teaching in physics and chemistry, at school, æt. 7–13. I worked alone at science at home, from the age of 11 years, where I was encouraged by the example of an elder brother. Subsequently, my pursuits were much influenced by being thrown, at an early age, æt. 19, on my own judgment and resources. I founded a mining colony in the backwoods of . . . . , and had to carry it out with several thousand people, quite alone." (*a*, *e*, *h*)

Ramsay

(8) " . . . . I was always apt to observe stones closely with regard to their qualities" [but the scientific taste for geology was not developed till after manhood]. (*z*)

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BIOLOGY.

Zoological Subsection.

(1) [Yes.] "Inherited from my father's family, who have generally been attached to natural history [especially botany—most remarkable examples are given]. My scientific tastes were largely determined by being appointed . . . . ." (*a*, *d*, *e*) Gray

(2) "Certainly innate. . . . . Strongly confirmed and directed by the voyage in the . . . ." (*a*, *h*) Darwin

(3) "Love of observation and natural history innate; [I had them] as early as I can remember. My grandfather was very fond of natural history, and a [more distant] relative has written an excellent fauna of . . . . The help of Mr. . . . . has aided me immensely, but not, I think, altered my tendency." (*a*, *e*, *f*) Lubbock

(4) "Homology innate, and derived from my mother. I trace the origin of my interest in science decidedly to my mother's observations in Owen

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our childhood rambles, on the plants and animals we saw. She told me that crabs were 'sea-spiders,' and periwinkles (*Littorinæ*) 'sea-snails.' I feel sure she had never read 'De Maillet!' . . . ." (*a, e*)

Cobbold

(5) "I believe I inherited my general taste for scientific pursuits from my grandmother; but my choosing . . . . for special investigation resulted from a positive fascination which the very obscurity of the subject exerted upon my mind. It was perhaps a mere desire to unravel the marvellous. My scientific tastes were largely promoted by the attractive teaching of [. . . . various professors]." (*a, c, e, g*)

Mivart

(6) "Thoroughly innate. I had no regular instruction, and can think of no event which especially helped to develop it. Bones and shells were attractive to me before I could consider them with any apparent profit, and books of natural history were my delight. I had a fair zoological collection by the time I was 15. My father had no scientific knowledge; nevertheless, he encouraged me in all my tastes, giving me

money freely for books and specimens, against the advice of friends; but he was indulgent generally, and not in the scientific direction only." (*a*, *e*)

(7) "Innate, as far as a love of nature and of the observation of natural phenomena. I trace the origin of my interest in science to the love of truth and of mental cultivation in my father, and his encouragement of this love in his children. I do not think it was largely determined by events after manhood." (*a*, *e*) Allman

(8) "I should say innate. I caught at all scraps of lessons for self-improvement. My soon-developed enthusiasm must have been derived from my mother's family. As to whether they were largely developed by events occurring after manhood, I think not. All I can say is, that neither profession nor marriage nor sickness has been able to affect them." (*a*, *e*) Parker

(9) "I cannot recollect the time when I was not fond of animals, and of knowing all I could learn about them. Living in the country, I had Newton

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abundant opportunities for indulging my taste, though, of course, I was not allowed to keep half the number of 'pets' I should have liked. The example of my father and elder brothers, who were all ~~pretty firm~~ greatly given to field sports, was also followed by me, and from field sports to field natural history is but a step. I obtained, by a piece of sheer good luck, the travelling fellowship of . . . ; it was tenable for nine years, and its income was sufficient to keep me during that time without being obliged to enter any profession. Though circumstances subsequently interfered with my using this assistance to the most advantage, in gratifying my taste for natural history, it was enormously furthered thereby." (*a*, *b*, *c*, *e*)

Bastian (10) "My partiality for the natural history sciences was initiated partly by my selection of medicine as a profession, and perhaps even more that, during the period of my apprenticeship, I was much under the influence of a remarkable man . . . , a most accomplished naturalist and of singularly independent judgment . . . For three years I spent every Sunday morning with him. During this time he was constantly stimulating me (a willing follower) to work in his department of natural science, and at the same time, ever inculcating a spirit of scientific scepticism." (*d, f*)

(11) "To love of birds, their study, their dissection. I remember trying to find out in the structure of the oviduct the cause of colour and markings in the different eggs. I discovered hairs sticking in the cuckoo's stomach, arranged in a spiral manner, before I knew that John Hunter had described the same. Then I took to drawing skulls and skeletons, and my fate was sealed. That I inherited a strong love of nature is certain, from my father, who was devoted to horticulture and very fond of birds and of landscape scenery; but I cannot trace any direct tendencies or work on the part of any member of my family, except my brother. I feel that I must have had a taste for science, independently of external circumstances. At the age of 17 or 18, I had dissected every new kind of bird that I Marshall

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met with. Later opportunities were entirely made by myself, or perhaps, rather, taken advantage of by myself." (*a*, *e*)

Flower

(12) "My love of natural history (so common in boys) showed itself in collecting insects, shells, and birds' eggs, and delighting in reading such books as Stanley on Birds, White's Selborne, Waterton, &c., at a very early age (8 years or before), and being rather encouraged than checked, continued to grow till it developed into a fondness for anatomical pursuits generally, which was never abandoned. My taste [for science] was entirely innate; no [other] member of the family nor early friend or acquaintance had any special taste for any of the natural history sciences. Two brothers, of nearly the same age, and with precisely the same surroundings, though joining occasionally in some of the above-mentioned boyish pursuits, never pursued them with real interest, and soon entirely gave them up." (*a*, *e*)

Huxley

(13) "As a boy, I had no taste for natural history, but a passion for mechanical contri-

vances, physics, and chemistry. I earnestly desired to be an engineer, but the fact that I had a . . . . [near relative] a medical man, led to my being apprenticed to him, and I took to physiology and anatomy, as the engineering side of my profession. [The inclinations above mentioned were] altogether innate, and, so far as I know, not hereditary; neither of my parents nor any of the family showing any trace of the like tendencies. My appointment to the surveying ship . . . . made me a comparative anatomist, by affording opportunities for the investigation of the structure of the lower animals. My appointment to . . . . forced me to palæontology." (*a*, *c*, *d*, *h*)

(14) "My school nickname was 'Archimedes;' I was always fond of construction. If I had followed my own bent, I should probably have been [successful as] an engineer. My turn for scientific inquiry led me in early life to systematise and generalise the knowledge of others. Latterly I have felt more interest in original investigations." (*a*, *c*) Carpenter

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Spencer (15) "I was in a general atmosphere of scientific thinking and discipline. My taste for biology began with keeping insects; for chemistry and physics, by being led to try experiments. Largely inherited from my father. I have made my circumstances more than they have made me." (*a*, *c*, *e*)

W. C. Williamson (16) "My father's example influenced me so early that I have no means of judging, but I doubt much their innate character. Their origin was due primarily, beyond all probability of doubt, to my father's influence and example. They were not influenced by subsequent events, but the tastes once planted rather determined the events. My medical profession caused me to suspend my scientific pursuits for some years; but the accidental perusal of . . . . brought me back again to the study of the . . . . , and all the rest followed in due time." (*b*, *e*)

Stokes (17) "They appear to have been inherited. My interest in science arose from the example of my father, and the fact of my being for a year the assistant and close companion of Professor . . . . of . . . . . at whose side I visited the poor in the lanes of . . . . , day and night. First began to work and concentrate energies to one branch æt. 21, when appointed . . . ." (*a*, *d*, *e*, *g*)

(18) "They have been, I believe, nearly in an equal degree the mixed result of a natural bias and education, and were determined by professional study, when a love of scientific knowledge for its own sake first took possession of my mind." (*a*, *d*) Thomson

(19) "How far innate, and how far acquired and developed from my early youth, I cannot say. My love for animals of all kinds was very strong, and to gratify it I overcame every obstacle put in my way at home, when I was a boy. I trace the origin of my interest in science to the earliest impressions of my childhood, all of which, so far as I recollect them, are connected with my father, and the various animals he brought me as pets. They were not largely determined by events after manhood. I should have been an observer of animal life Günther

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under any conditions under which I might have lived." (*a*, *e*)

Sclater

(20) "I cannot trace the origin of my interest in geology. I believe it to have been innate. I began collecting birds and studying them before I went to school, and without any inducement. I was always told by my relations that my scientific pursuits would stand in my way, but adhered to them notwithstanding. They were not at all determined by events occurring after I reached manhood; they simply increased as I grew older." (*a*)

Parkes

(21) "I perceive no evidence of their being innate [? hereditary], unless I derived any tendency from my mother, who was at one time much with her great-uncle [. . . . the founder of one of our great industries] and greatly interested in his pursuits. She worked a good deal at chemistry, and was well acquainted with many of the processes in pottery. I belonged to an industrious family and saw everyone working. The attraction I have for chemistry (which is a strong one, only my profession has never

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allowed me to follow it very closely) arose from being sent to work, æt. 15, in a chemical laboratory." (*e*)

(22) "I do not consider them innate, but induced by the following circumstances:—When I was at school, æt. 13–15, a lady, an old friend of my mother, gave me a few British shells, with their names, and a copy of 'Turton's Conchological Dictionary.' I thenceforth diligently collected British shells, and afterwards extended my researches." (*b*) Jeffreys

(23) "To my father's example (in science); to the profession of medicine (in physiology, anatomy, and . . . .). It was my interest in my profession to work at scientific subjects, while young and while waiting for practice. The example of many men whom I knew when young proved a great stimulus and incentive." (*e, d, f*) Bowman

(24) "Not at all innate. I can trace it distinctly to my intercourse with certain professors . . . . ; subsequently to my desire to investigate Sanderson

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certain scientific questions bearing on medicine, and later to my intercourse with . . . . and . . . ." (*c*, *d*, *f*, *g*)

BIOLOGY.

Botanical Subsection.

Hooker (1) "My scientific tastes were inborn" [and strongly hereditary]. (*a*)

Ball (2) "As far as the word applies in any case, I should say decidedly innate. Excepting such influence as a little encouragement at home, I am unable to trace any external stimulus. At æt. 6, I was given Joyce's 'Scientific Dialogues,' which I soon mastered, then other books; before æt. 8, I commenced making star maps; æt. 12–13, I made some geological sections with tolerable correctness; and so on. It [then] seemed as if any accident and the love of new vistas were enough to lead me from one branch of science to another." (*a*)

Jas. Bateman (3) "Always fond of plants." (*a*)

Carter (4) "Was always fond of objective and experimental knowledge. I date my first efforts of any

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consequence from an early intimacy with Professor . . . , whose pupil and assistant I was. I had a fondness for science before, but the necessity for accurate and rigid observation then first dawned upon me. Subsequent events were going to . . . . [abroad], and appointments in . . . . [a foreign country, where I was much detained indoors that] compelled me to take to the microscope and study of the lower orders of plants and animals, many of which I could grow in my own room." (*a*, *c*, *g*)

(5) "As a youth, I followed, of my own free will, mineralogy, chemistry, anatomy, and mechanics, but chiefly chemistry. My tastes were certainly not hereditary. They were directed to botany purely through accidental circumstances [which led to a prolonged residence in an imperfectly civilized country]. I examined its plants, then wholly unknown to Europeans, but was at that time wholly ignorant of the very elements of botany. Was subsequently encouraged by . . . [eminent botanists of the day]; went to and from England and made extensive collections. Miers

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My wife actively assisted me in my botanical and other scientific pursuits, and to her advice and assistance I owe much of my success in life." (*a*, *f*, *h*)

Balfour

(6) "The love for botany was instilled into me in very early youth by my father. We lived in the house of . . . . [a very eminent geologist], in the vicinity of . . . . , and I often took walks to those hills and collected plants. I also cultivated plants in our garden. A taste for natural science, especially botany, seems to have been innate. The companionship of . . . . incited me to prosecute botany with vigour. I was one of his best pupils, and travelled over a great part of . . . . with him." (*e*, *g*)

Henslow

(7) [A posthumous account.] "He appears to have been attached to natural history all his life through, but never took up botany to any extent till the professorship was vacant. [There is some conflict of testimony here.] I think his scientific tastes were innate. I have excellent drawings of insects made by him as a schoolboy; also, he made a model of a caterpillar; tried a

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little chemistry; made lace with bobbins of his own contriving. . . . . It was said, 'Nothing escapes that boy's eyes.'" (*a*, *d*)

(8) "To my father's encouragement of a natural inclination." (*a*, *e*) Babington

(9) "I cannot trace the origin of my interest in any particular branch of science further than that as far as regards . . . . botany, I was thrown into the society of a gentleman who took much interest in it. My scientific tastes originated, as a matter of fact, after leaving . . . . [the university]." (*f*) Currey

(10) "Not innate. I trace the origin of my botanical tastes to leisure; to the accidental receipt of De Candolle's 'Flore française,' whilst resident in that country; and to encouragement from my mother. They were determined afterwards by independence (considering my absence of ambition to rise in the world) and by friendship and encouragement from . . . . , the four greatest British botanists of the day." (*b*, *e*, *f*) Bentham

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BIOLOGY.

Medical Subsection.

Sir J. Paget (1) "Innate in a great degree. I trace the origin of my interest in science (1) to my mother's mental activity and love of collecting and arranging, and my father's constant encouragement of my pursuits; (2) to the friendship of [three eminent botanists], by whom I was chiefly induced to study botany; (3) to my profession, the choice of which was in some measure determined by my taste for collecting and studying." (*a, d, e, f*)

Farre (2) "I selected the medical profession because it was that of my father. This choice led me to scientific pursuits, for which I had no previous predilection, as I had no opportunities that way. I conclude the tastes were innate, as they certainly showed themselves the moment the opportunity for developing them occurred, namely, at the commencement of my professional studies, æt. 17." (*a, d*)

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(3) "Not at all especially innate. I could have taken to any other subject quite as well, so far as I know. I trace the origin of my interest in science to the knowledge that I must do my best in it to earn a livelihood and to please my parents. I did not follow my own branch from any special liking—indeed, I disliked it; but it was necessary to follow some branch. The connection with an hospital and medical school in . . . . have been inducements to continue work, and all my life I have worked pretty steadily." (*d*) Humphry

(4) "I cannot perceive that they were innate. Possibly my tastes were due to retentiveness of memory as to objects and facts, and a strong impression that good surgery is a great fact. Subsequently, by the approval of teachers, when between æt. 18 and 20, having been selected chief assistant to the most popular teacher of anatomy of his day, and also to a professor of surgery." (*c*, *g*) Ferguson

(5) "Had an interest excited in philosophical Alderson

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inquiries by my father's acute observations in all such topics." (*e*)

Watson (6) "I cannot say that I had naturally a turn for any pursuit in particular. My addiction to medicine was purely the result of accident. I never gave a thought to physic as a subject of study until I was 27 years old." (*d*)

Fox (7) "Accidentally [directed] to medicine by associating with a medical friend in a superficial study of botany." (*c*, *d*)

STATISTICS.

Newmarch (1) "Certainly my scientific tastes appear to me to have been, so to say, innate." (*a*)

T.G. Balfour (2) "My interest in science was due to my having been officially employed in the early part of [my career, in a very important statistical inquiry]." (*d*)

Guy (3) "Innate, I think. I inherit many mental peculiarities and talents from my paternal grand-father, amongst which is a love of figures and

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tabulation; none from my father. I cannot [otherwise] trace the origin of my interest in science, nor were my tastes largely determined by events after manhood." (*a*)

Jevons

(4) "I should be much inclined to think there was an innate tendency, but that the tastes were developed by a good and for the most part suitable education. When at my first school, æt. 10½–12, the head-master gave very clear occasional lessons in moral and economical subjects. I can remember vividly to the present day the impression which those lessons made upon me. As I am not aware that the other boys in the class were equally impressed, I think I must have had an innate interest in those subjects; but the lessons probably increased the interest very much." (*a*, *b*, *g*)

Hill

(5) "I cannot distinguish between what I may have derived from nature and what I may have acquired from intercourse with my father and certain of his friends. When I was 11 years old, my father gave a series of lectures on electricity, mechanics, astronomy, and pneuma-

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tics, to all of which, but especially to the last, I paid delighted attention. I presently began to construct apparatus for myself. Subsequently practice in teaching led me to seek for knowledge. Intercourse with men of higher attainments became a great spur; my turn for . . . . was favoured by my opportunities as an early member of the . . . . Society." (*a*, *e*, *f*)

Heywood

(6) "Professor . . . .'s lectures on geology were the origin of my interest in that science; the work of the . . . . statistical society in educational inquiries influenced my taste for statistical science; frequent attendance at meetings of the British Association encouraged my scientific tastes." (*d*, *g*)

MECHANICAL SCIENCE.

Armstrong

(1) "If any tastes be innate, mine were; they date from beyond my recollection. They were not determined by events after manhood, but, I think the reverse; they were discouraged in every way." (*a*)

Jenkyn

(2) "Decidedly innate. The science of . . . . was well taught at the university of . . . . , where I studied, æt. 16–18, and accidentally this became serviceable to me when employed as an engineer by . . . . The friendship of . . . . materially affected my career. My tastes were not largely developed by events occurring after manhood." (*a*, *b*, *d*, *f*)

J. F. Bateman

(3) "Family tradition derived through my mother's side. My profession fell in with my natural tastes, such as sketching." (*c*, *d*, *e*)

Pole

(4) "Innate, I think, as regards certain qualities of mind, which led me, under the pressure of circumstances, to direct my attention to certain things in a certain way, namely, (1) independence of judgment; (2) earnestness of purpose; (3) a practical, clear-headed, common sense, logical way of viewing things." (*c*, *d*)

Merrifield

(5) "I cannot say whether they were innate. I was always brought up in a half-scientific, half-literary atmosphere, and was a fair mathematician as a boy, as well as a fair classic and

linguist. My tastes were not determined by after events, but my avocations were rather determined by my scientific habits." (*e*)

ANALYSIS OF REPLIES.

Having given the replies in gross, it now becomes our business to sort their contents under different heads. It would be useless and even embarrassing to make lengthy extracts from them; short abstracts will therefore be given, which the reader may verify whenever he pleases by the help of the reference number, printed in parentheses ( ), which is the same both here and in the original.

§ A. INNATE TASTES.

Instances of a strong taste for science being decidedly innate. I have not included among these the whole of the cases to which an *a* has been affixed:—

Physics and Mathematics.—12 cases out of 20 replies. (1) My tastes are entirely innate;

they date from childhood. (2) As far back as I can remember, I loved Nature and desired to learn her secrets. (3) Always attracted by men of ability. (4) From an early age I was addicted to mechanical pursuits; then to chemistry. (5) Naturally fond of mechanics and physical science. (6) My tastes were partly natural, partly encouraged. (7) I remember [incidents which proved an innate taste] before I could write. (8) I had an innate wish for miscellaneous information. (11) Primarily derived [both by inheritance and education] from my father. (16) I always regarded mathematics as the method of obtaining both the most useful and the most harmonious, &c. (17) My taste for mathematics appears innate; as a boy I delighted in sums. (18) An early taste for arithmetic, and in particular for long division sums.

Chemistry.—5 cases out of 11. (1) Thoroughly innate. (2) Perhaps wholly innate. (3) I was always observing and inquiring. (4) They date from a very early period, and there was little to produce them in my early surroundings. (5) From an early age I had an innate taste for all branches of science.

Geology.—At least 7 out of 8 cases. (1) Decidedly innate. (2) A natural taste for observing and generalizing, developed. (3) A natural taste; my interest in science began very early. (4) I believe I may say innate to a very considerable extent. (5) I was always fond of natural history. (6) As well as I can recollect, they were innate. (7) I believe the desire for information and habits of observation to be in great measure innate.

Zoology.—18 cases out of 24. (1) [Yes.] Inherited from my father's family. (2) Certainly innate. (3) Love of observation and natural history innate. (4) Homology innate. (5) I believe I inherited my general taste for scientific pursuits. (6) Thoroughly innate. bones and shells were attractive to me before I could consider them with any apparent profit. (7) Innate love of nature and observation of natural phenomena. (8) I should say innate;

I caught at all scraps of lessons for self-improvement. (9) I cannot recollect the time when I was not fond of animals and of knowing all I could learn about them. (11) Love of birds and their study . . . I feel that I must have had a taste for science independently of external circumstances. (12) My taste [for science] was entirely innate. (13) As a boy I had a passion for mechanical contrivances; [my scientific tastes are] altogether innate. (14) I was always fond of construction; my turn for scientific inquiry led me in early life to systematise the knowledge of others. (15) Largely inherited from my father. (17) They appear to have been inherited. (18) Nearly in an equal degree the mixed result of a natural bias and education. (19) I should have been an observer of animal life under whatever conditions I might have lived. (20) I believe my interest in zoology to have been innate.

Botany.—8 cases out of 10. (1) My scientific tastes were inborn. (2) As far as the word applies in any case, I should say decidedly in-

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nate. (3) Always fond of plants. (4) Was always fond of objective and experimental knowledge. (5) As a youth I followed of my own free will chemistry and other sciences. (6) A taste for natural science, especially botany, seems to have been innate. (7) [Scientific tastes apparently innate.] (8) A natural inclination.

Medical Science.—Only 2 cases out of 7. (1) Innate in a great degree. (2) I conclude the tastes were innate, as they showed themselves the moment the opportunity for developing them occurred.

Statistics.—3 cases out of 6. (1) Certainly my scientific tastes appear to me to have been, so to say, innate. (3) Innate, I think. (4) Much inclined to think there was an innate tendency.

Mechanical Science.—At least 2 cases out of 5. (1) If any tastes be innate, mine were; they date from beyond my recollection. (2) Decidedly innate.

INSTANCES OF TASTES BEING DECIDEDLY NOT INNATE.

Physics and Mathematics.—1 case out of 20. (15) I am not aware of any innate taste for science.

Chemistry.—1 case out of 11. (10) I did nothing serious till æt. 23. My pursuit of chemistry is entirely due to circumstances occurring after manhood.

Zoology.—3 cases out of 24. (16) I doubt much their innate character. (22) I do not consider them innate, but induced. (24) Not at all innate.

Botany.—1 case out of 10. (10) Not innate.

Medical.—4 cases out of 7. (3) Not at all especially innate. (4) I cannot perceive that they were innate. (6) I cannot say that I had naturally a turn for any pursuit in particular. (7) Accidentally [directed] to medicine.

Statistics.—1 at most out of 6. (2) My interest in science was due to my having been officially employed in a statistical inquiry. [It is with much hesitation that I consent to enter this as a case of "not innate."]

SUMMARY OF RESULTS AS TO INNATE TASTES.

	Total cases.	Decidedly innate.	Decidedly not innate.	Doubtful.
Physics and Mathematics	20	12	1	7
Chemistry and Mineralogy	11	5	1	5
Geology	8	7	0	1
Biology—Zoology	24	17	3	4
Botany	10	8	1	1
Medical Science	7	2	4	1
Geography (not discussed separately)	0	0	0	0
Statistical Science	6	3	1	2
Mechanical Science	5	2	0	3
	91	56	11	24

A mere glance at the table and at the foregoing extracts will probably be enough to convince the reader that a strong and innate taste for science is a prevailing characteristic among scientific men; also that the taste is enduring. This latter peculiarity is by no means a necessary consequence of the former;

on the contrary, the ruling motives in the disposition of a man usually change as he grows older, the love of inquiry in childhood being superseded by the fierce passions of youth, and these by the ambitions of more mature life. But a special taste for science seems frequently to be so ingrained in the constitution of scientific men, that it asserts itself throughout their whole existence. Obviously it must have had great influence in directing their early studies and in ensuring their successful prosecution of them in after years.

It would be a curious inquiry to seek the limits of a special taste, that is, the diversity of the objects, any one of which would satisfy it. I think the indications are clear that the tastes of some of my correspondents are far more special than those of others, and that the latter have checked a tendency to desultoriness by their strength of will, or have had it checked by the necessities of their position as professors or professional men; or, most of all, by the possession of that strange quality

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which the phrenologists call adhesiveness, but which seems to defy analysis. It exists in very different strength in different persons, and I know not where to find a better illustration of its power than in the ordinary case of a man falling in love for the first time. Few lookers-on will doubt that almost any young man is capable of falling in love with any one of at least one-third of the presentable young women of his race and social position, if they happen to see much of one another under favourable circumstances and without other distraction; yet, although the innate taste is of so general a character, it becomes specialised at once by the mere act of falling in love. Then the image of one woman takes complete possession of his thoughts; she is for a considerable period the only female who has attractions for him, although he might previously have been equally attracted by any one of tens of thousands of her sex.

A strong taste bearing remotely on science may prove very helpful. The love of collecting, which is a trifling tendency in itself, common to children, idiots, and magpies, often leads to the study of the things collected, and is of immense use to a man who wishes to study objects that must be collected in large numbers. I have been told of an astronomer whose primary taste was a love of polished brass instruments and smooth mechanical movements, that nothing satisfied this taste so fully as work with telescopes, and from loving the instruments he soon learnt to love the work for which they were used. A taste for careful drawing works well into engineering and into systematic botany or zoology. A love of adventure and field sports may be an extremely useful element in the character of a man who follows geology or zoology.

As a rough numerical estimate, it seems that 6 out of every 10 men of science were gifted by nature with a strong taste for it; certainly not 1 person in 10, taken at haphazard, possesses such an instinct; therefore I contend that its presence adds five-fold at least, to the chance of scientific success. The converse way of looking at the question gives a similarly large estimate. Certainly one-half of the population have no care for science, and an extremely small proportion of that half succeed in it. Nay, further, it appears (though I cannot publish facts in evidence, without violating my rule of avoiding personal allusions) that of the men who have no natural taste for science and yet succeed in it, many belong to gifted families, and may therefore be accredited with sufficient general abilities to leave their mark on whatever subject it becomes their business to undertake. We may therefore rest assured that the possession of a strong special taste is a precious capital, and that it is a wicked waste of national power to thwart it ruthlessly by a false system of education. But I can give no test which shall distinguish in boyhood between a taste that is destined to endure and a passing fancy, further than by remarking that whenever the aptitudes seem hereditary, they deserve peculiar consideration.

Instinctive tastes for science are, generally speaking, not so strongly hereditary as the more elementary qualities of the body and mind. I have tabulated the replies, and find the proportion to be 1 case of inheritance to 4 that are not inherited from either parent. There is no case in which the correspondent speaks of having inherited a love of science from his mother, though, of course, she may, and probably has, often transmitted it from a grand-parent. I have a curious case among the returns sent to me of a passion for heraldry characterising a great-nephew and a great-uncle, the latter of whom had died before the former was born. I have another of an eminent statistician, in whom a love of figures and tabulation was highly characteristic of his grand-parent and is very strongly marked in himself, but was wholly absent in his parent and all other known members of his small family. There have been numerous and most curious cases of a love of figures and tabulation in my own family, which richly deserve a full description. It was carried to so strange an extravagance by one of its members, a lady now deceased, that I can do no sufficient justice to her peculiarities by speaking in general terms; I ought to give pages of anecdote.

G. Darwin

Guy

Mrs Booth

§ B. FORTUNATE ACCIDENTS.

We next come to a group of cases which imply a latent taste for science, namely, where a lifelong pursuit of it was first determined by some small accident. The previous indifference or equilibrium of the mind was unstable, a push was accidentally given, its position was wholly changed, and it rested in one of stable equilibrium. These cases are not numerous—only 10 altogether—but I put them in the second place on account of their affinity to those in the first.

Physics and Mathematics.—(19). [Refer to this.]

Chemistry.—(1) Possession of a chemical box when I was a little boy. (3) From lectures I attended when a boy. (9) To reading by accident a book on chemistry.

Geology.—(2) Fossiliferous rocks near the school where I was.

Zoology.—(9) A travelling fellowship. (16) Accidentally reading a book brought me back to scientific studies, previously suspended owing to my profession. (22) Gift, when a boy, of a box of British shells with a book to explain them.

Botany.—(10) Accidental receipt of De Candolle's "Flore française," when residing in France.

Medical Science.—None.

Statistics.—(4) Very clear occasional lectures when a boy.

Mechanics.—(2) A particular study at a university, which accidentally became of professional importance.

§ C. INDIRECT MOTIVES OR OPPORTUNITIES.

This group has also considerable affinity to group (A) and has been alluded to in the remarks appended to the extracts referring to it. It includes those cases in which the mind was partly, but not largely, deflected from its natural bent; that portion of the innate tendency which admitted of being "resolved in the direction" of the scientific pursuit, being satisfied, the remainder being wasted. These cases are not numerous—only 16 altogether—but I give them the third place for the same reason that I gave group (B) the second.

Physics and Mathematics.—(5) Possession of special instruments. (8) Choosing engineering as a profession, but not following it. (19) Love of yachting (leading to researches on magnetism of ships).

Chemistry.—(6) The obtaining of correct and accurate results in chemical analysis gave me great satisfaction.

Geology.—(1) Interest in discoveries made in . . . . . (3) A very early love of experiment and chemistry. (6) Should have followed chemistry and physics, but circumstances . . . . gave opportunities for geology.

Zoology.—(5) My choosing . . . . for special investigation was due to a positive fascination

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from the obscurity of the subject. (9) My father's and brother's pursuit of field sports, and thence indirectly to natural history. (13) An early passion for mechanism, which led me to take to physiology and anatomy, as the engineering side of my profession. (15) My taste for biology began with keeping insects. (24) . . . . subsequently to the desire to investigate certain questions bearing on medicine.

Botany.—None.

Medical Science.—(3) Connection of hospital and medical school with the place of his residence. (4) Love of facts and the impression that good surgery is a great fact.

Statistics.—None.

Mechanics.—(3) Profession fell in with natural tastes, such as sketching. (4) Innate faculties, serviceable to profession under the pressure of circumstances.

§ D. PROFESSIONAL DUTIES.

The fourth group comprises instances in which professional duty was a principal cause of the interest first felt in scientific pursuits, or else of the energies being concentrated upon some branch of science towards which no special inclination had previously been exhibited. Two or three, of the 21 cases which I shall quote, may perhaps be thought doubtful examples and more appropriate to the preceding group; but after all possible deductions have been made, there will remain ample evidence of the magnitude of the influence we are considering. A wise administrator, desirous, even at some cost, of promoting original investigation, would establish many professional offices of a scientific character, having responsible duties of a prominent kind attached to them. They would create much new interest in science, and would compel those who held them, to work steadily and to a purpose in scientific harness.

Physics and Mathematics.—(4) Had never attended *specially* to physics till appointed professor of natural philosophy. This induced me to give up chemistry, and to devote myself definitively to physics. (9) Solitary observing for years [as director of an observatory]. (13) Professional duties and civil engineering . . . . ; official exploration of . . . . (14) Largely determined by service in north polar and equatorial expeditions. (15) My interest in astronomy was very small indeed, until I was appointed [to the directorship of an observatory].

Chemistry.—(8) The university inviting me to fill the chair of . . . , gave my work its bent.

Geology.—None.

Zoology.—(1) Largely determined by being appointed . . . . (10) Partly by my selection of medicine as a profession. (13) My appointment to a surveying ship made me a comparative anatomist . . . . , that to . . . . forced me to palæontology. (17) First began to concentrate energies to one branch, when appointed . . . . (18) [My scientific tastes] were determined by professional study. (23) To the profession of medicine [in physiology, anatomy and . . . .] (24) Subsequently to my desire to investigate certain subjects bearing on [my profession of] medicine.

Botany.—(7) Never took up botany to any extent till the professorship was vacant. [There is some conflict of testimony here.]

Medical Science.—(1) Partly to my profession. (2) I selected the medical profession because it was that of my father; this choice led me to scientific pursuits. (3) I did not follow my own branch from any special liking—indeed, I rather disliked it, but it was necessary to earn a livelihood and to follow some branch. (6) My addiction to medicine was purely the result of accident: I never gave a thought to physic as a subject of study, until I was 27 years old. (7) Accidental to medicine.

Statistics.—(2) Due to official employment when young, in a very important statistical inquiry.

Mechanics.—(2) The science of . . . . , which I had learnt accidentally, became serviceable to me when employed as an engineer. (3) My profession fell in with my natural tastes. (4) Pressure of circumstances.

§ E. ENCOURAGEMENT AT HOME.

Nearly one-third of the scientific men have expressed themselves indebted to encouragement at home. They received it in various ways; sometimes the influence of the parent was strong and direct, as "their origin was due beyond all doubt to my father's influence;" sometimes it was strong but general, as "I was in a general atmosphere of scientific thinking and discussion;" sometimes it went no further than indulgence, as "permission to carry on little experiments at home in a room set apart for the purpose." Under each and all of these shapes it was truly welcome, and its effectiveness may be in some measure estimated by the vastly smaller number of cases in which success

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was obtained in direct opposition to family influences.

Scientific studies in boyhood are apt to meet with scant favour at home; they deal too much in abstractions on the one hand, and sensible messes and mischief to furniture and clothes on the other. They lead to no clearly lucrative purpose, and occupy time which might be apparently better bestowed. These hindrances were far more seriously felt when the men on my list were young, when apparatus was hardly to be procured, and when scientific work was exceptional. I ascribe many of the cases of encouragement to the existence of an hereditary link; that is to say, the son had inherited scientific tastes, and was encouraged by the parent from whom he had inherited them, and who naturally sympathized with him.

Attention should be given to the relatively small encouragement received from the mother. I have sorted the extracts so as to permit the comparison to be easily made. The female mind has special excellencies of a high order, and the value of its influence in various ways is one that I can never consent to underrate; but that influence is towards enthusiasm and love (as distinguished from philanthropy), not towards calm judgment, nor, inclusively, towards science. In many respects the character of scientific men is strongly anti-feminine; their mind is directed to facts and abstract theories, and not to persons or human interests. The man of science is deficient in the purely emotional element, and in the desire to influence the beliefs of others. Thus I find that 2 out of every 10 do not care for politics at all; they are devoid of partisanship. They school a naturally equable and independent mind to a still more complete subordination to their judgment. In many respects they have little sympathy with female ways of thought. It is a curious proof of this, that in the very numerous answers which have reference to parental influence, that of the father is quoted three times as often as that of the mother. It would not have been the case, judging from inquiries I elsewhere made, if I had been discussing the antecedents of literary

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men, commanders, or statesmen, or, still more, of divines.

Physics and Mathematics.—(10) The origin of my interest in . . . . is mainly due to my father's knowledge of geology, navigation, and engineering. (11) Primarily derived [both by education and inheritance] from my father.

Chemistry.—(3) Permission to carry on little experiments at home, in a room set apart for the purpose. . . . . Subsequently residing abroad and my mother making a home for me there. (4) I was taught at home with my brothers; we had always the example of industry, and were encouraged to think for ourselves. (8) My father gave me [some books on chemistry, and] I owe to my mother a child's curiosity and afterward a man's reverence for scientific truth. (11) My tastes received no encouragement whatever from relations, my mother excepted.

Geology.—(1) My father and an aunt collected specimens. (4) I was indebted in a high degree to collections made by my father and mother. (7) I was encouraged by the example of an elder brother.

Zoology.—(9) (The example of my father and elder brothers, who were all ~~pretty firm~~ to field sports, was also followed by me, and from field sports to field natural history is but a step). (15) Largely inherited from my father. I was in a general atmosphere of scientific thinking and discussion. (21) I may have derived [? inherited] the tendency from my mother; I belonged to an industrious family, and saw every one working. (1) [Traditionally derived, and] inherited from my father's family [*i.e.* from father, grandfather, &c.] (6) My father had no scientific knowledge, nevertheless he encouraged me. (7) I trace it to the love of truth and of mental cultivation in my father, and to his encouragement of this love in his children. (11) That I inherited a strong love of nature from my father is certain, who was devoted to horticulture and very fond of birds. (16) Their origin was due, beyond all doubt, to my

greatly given

father's influence. (17) My interest in science arose from the example of my father, and . . . . &c. (19) I trace it to the earliest impressions of my childhood, all of which are connected with my father and the animals he brought me as pets. (23) To my father's example (in science). (4) Decidedly to my mother's observations in our childhood rambles. (8) My soon-developed enthusiasm must have been derived from my mother's family.

Botany.—(2) A little encouragement at home. (6) The love of botany was instilled into me in very early youth by my father. (8) To my father's encouragement of a natural inclination. (10) And to encouragement from my mother.

Medical Science.—(1) [Partly] to my mother's mental activity and love of collecting and arranging, and to my father's constant encouragement of my pursuit.

Statistics.—(5) [Partly] acquired from intercourse with my father and . . . .

Mechanics.—(5) I was always brought up in a half scientific, half literary atmosphere. (3) Family tradition derived through my mother's side.

Two cases are mentioned in which the origin of the scientific tastes was partly due to the active assistance of the wife. One of these is Botany (5), and the other I have ventured to suppress, as it did not appear to me sufficiently decided.

§ F. THE INFLUENCE AND ENCOURAGEMENT OF FRIENDS.

This group has much in common with that of the indirect influences already classed under group C; it includes cases where a fortuitous acquaintance has been the means of deciding a career, probably by revealing a latent taste, or showing how some obstacle in the way of indulging it could easily be removed. There is a wide interval, often very difficult to get over, between the study of a subject out of books and the practical investigation of it for oneself. At this point of a man's mental progress the help of a friend may be of immense assistance; he may give elementary hints which will remove formidable difficulties to a beginner, who is utterly unused to experiment. It is told, I think, of a scholar, that he laboured for successive days to make with his own hands in his own chambers a plum-pudding according to a time-honoured family recipe, but he produced nothing except thick pastes or stirabouts of different degrees of lumpiness, revolting to the sight. At length he confided his difficulties to a lady, who explained that in making plum-puddings it was a matter of course, and therefore not spoken of in the recipe, to put the ingredients into a bag before beginning to boil them. The example of a friend encourages a young man to overcome his diffidence and to firmly occupy any position that he knows by his own judgment to be true. Perhaps the greatest help of all is the consciousness of strength which is given by co-operation on not very unequal terms with a veteran in performance and reputation.

Out of the 91 cases, 18 speak gratefully of the influence and encouragement of friends.

Physics and Mathematics.—(3) . . . . I was both his young friend and assistant for 3 years. He imbued me with his respect for science, ... , earnestness, and accuracy. (6) Partly encouraged by an eminent friend. (13) Picked up an unsystematic education [in science] in the company of . . . . (16) I was taken to see . . . . [which was the origin of my experimentalising]. (17) I trace it to my acquaintance with . . . . and to going abroad with him. (19) The intimacy of his father with . . . . gave a bias towards magnetism.

Chemistry.—(2) My taste for zoology arose through friendship with . . . .

Geology.—(2) The surgeon to whom I was articled fostered my tastes. (4) To mining officers in Germany; to conversations with . . . . and . . . . , and acquaintance of . . . . (5) Through the acquaintance of . . . . , to the particular branch [of geology, that I have pursued].

Zoology.—(3) The help of . . . . has aided me immensely. (10) I was much under the influence of a remarkable man, a most accomplished naturalist. (23) The example of many men whom I knew when I was young, proved a great stimulus and incentive. (24) I can trace it distinctly to my intercourse with certain professors.

Botany.—(5) . . . . was subsequently encouraged by [eminent botanists]. (9) I was thrown into the society of a gentleman who took much interest in botany. (10) They were determined afterwards by . . . . and the friendship and encouragement of the four greatest British botanists of the day.

Medical Science.—(1) [Partly] to the friendship of three eminent botanists. (7) Accidentally [directed] to medicine by associating with a medical friend in a superficial study of botany.

Statistics.—(5) [Partly] from intercourse with my father and certain of his friends.

Mechanical Science.—(2) The friendship of . . . . materially influenced my career.

§ G. INFLUENCE AND ENCOURAGEMENT OF TUTORS.

This group of 13 cases refers to the influence and encouragement of masters, tutors and professors. It is a small one; not because persons in those positions are incapable of exerting much salutary influence, but because the scientific men on my list seldom had the advantage of receiving congenial instruction. This is clearly proved by a comparison of the replies referring to Scotch and to English tuition. In Scotland the university programme and the general method of teaching is much more suited to men of a scientific bent of mind than those in England; consequently the influence of tutors has been testified to far more abundantly by those men on my list who have been educated in Scotland than by the rest. The proportions are striking and instructive. I find that about one-sixth of those from whom I have received returns have studied in Scotland; hence, if professorial influences had been equally efficacious on both sides of the Tweed, there would have been 5 times as many expressions of gratitude to English teachers as to Scotch. But the facts show that no less than 8 out of the 13 cases refer to teachers in Scotland, 1 to a Scotch teacher settled in England, and only 4 to English professors. It would have been ($8 \times 5 =$) 40 and not 4, if the English education had been as profitable to science as the Scotch. I willingly admit that the smallness of the numbers, namely, only 13 cases, renders precise figures open to question; however, the superiority of the Scotch system is supported by other evidence which I shall speak of in the chapter on education.

Physics and Mathematics.—(7) I believe the origin was when I attended the natural philosophy classes at . . . . (10) Tastes confirmed by lectures, and especially by the encouragement of [certain professors]. (20) Interest in mathematics due to the encouragement of . . . . , and influence of [professors at a university].

Chemistry.—(7) Chiefly to being sent as a pupil to an eminent man of science.

Geology.—(5) Lectures by . . . .

Zoology.—(5) My scientific tastes were largely promoted by the attractive teaching of . . . . [various professors]. (17) And to being the assistant and close companion of . . . . (24) I can trace it [in part] distinctly to my intercourse with certain professors.

Botany.—(4) I date my first efforts of any consequence from an early intimacy with . . . ., whose pupil and assistant I was; the necessity of accurate work then dawned upon me. (6) The companionship of . . . . incited me to prosecute botany with vigour; I was one of his best pupils, and travelled with him.

Medical Science.—(4) Subsequently by the approval of teachers, having been selected chief-assistant.

Statistics.—(4) Very clear occasional lectures, when a boy, on moral and economical subjects;

the tastes were afterwards developed by a good education. (6) Professor . . . .'s lectures were the origin of my interest in geology [It was the earliest scientific pursuit of this correspondent].

Mechanical Science.—None.

§ H. TRAVEL IN DISTANT PARTS.

There are only 8 cases in this group, namely, those in which the aspects of nature under new conditions have developed a love for science. Few men of scientific training have had opportunities of distant travel, but on those few their action has been very strong, especially as regards biologists and physicists. I say nothing here in respect to mere geographers, and quote none of their replies, because its importance to them requires neither proof nor comment. Men are too apt to accept as an axiomatic law, not capable of further explanation, whatever they see recurring day after day without fail. So the dog in the back yard looks on the daily arrival of the postman, butcher, and baker as so many elementary phenomena, not to be barked at or wondered about. Travel in distant countries, by unsettling these quasi-axiomatic ideas, restores to the educated man the freshness of childhood in observing new things and in seeking reasons for all he sees.

I believe that a handsome endowment of travelling fellowships, thoroughly well paid, with extra allowance for any special work allotted to their holders, given only to young men of high qualifications, and lasting for at least 5 years, would be money well bestowed in the furtherance of science.

Physics and Mathematics.—(3) To some extent my tastes were determined by events after manhood, because for 10 years I held positions of great responsibility [in distant parts of the world], but I consider they were formed in my youth. (9) Ocean voyaging in the beginning of life; solitary observing for years in a country verging on a desert, under southern skies. (13) The distinct origin . . . . was the wonderful effect produced on me by the aspects of nature, as seen in the . . . . . , combined with what I may call the accident of having been allowed to explore part of it in an official capacity. (14) Largely determined by my service in north polar and equatorial expeditions.

Chemistry.—None.

Geology.—(7) Subsequently much influenced by being thrown, at æt. 19, on my own judgment and resources in founding a mining colony in the backwoods of . . . . and carrying it out quite alone.

Zoology.—(2) Strongly confirmed and directed by the voyage in the . . . . (13) My appointment to the surveying ship . . . . made me a comparative anatomist, by affording opportunities for the investigation of the structure of the lower animals.

Botany.—(5) They were directed to botany purely through accidental circumstances [which led to a prolonged residence in an imperfectly civilized country].

§ Z. UNCLASSED RESIDUUM.

We now come to the final group, namely, those influences which cannot be sorted into any of the 8 groups with definite titles, which we have already examined. At the outset I spoke of these unclassed conditions as forming a class by themselves, of no great importance, and which might be indefinitely reduced in proportion as we chose to pursue our analysis. I estimate that the 91 replies which I have received and analysed assign a total of 191 causes. It now appears that no less than 188 of these fall into one or other of 8 definite groups, and that there remain only 3 on our hands for the unclassed residuum. Even these are apparently due to aggregates of conditions, the more important of which would probably find their place among the 8 groups, leaving a still minuter residue. We may lightly dismiss them as of inappreciably small importance in our present inquiry.

Chemistry.—(10) Entirely due to circumstances after manhood, and in direct opposition to family influences. (11) To opportunity at [a foreign university].

Geology.—(8) The tastes developed gradually after manhood.

SUMMARY.

If we take a general survey of our national stock of capabilities and their produce, we see that the larger part is directed to gain daily bread and necessary luxuries, and to keep the great social machine in steady work. The surplus is considerable, and may be disposed of in various ways. Let us now put ourselves in the position of advocates of science solely, and consider from that point of view how the surplus capabilities of the nation might be diverted to its furtherance. How can the tastes of men be most powerfully acted upon, to affect them towards science?

The large category (A) of innate tastes is practically beyond our immediate influence; but though we cannot increase the national store, we need not waste it, as we do now. Every instance in which a man having an aptitude to succeed in science, is tempted by circumstances which might be controlled, to occupy himself with subjects of less national value, is a public calamity. Aptitudes and tastes for occupations which enrich the thoughts and productive powers of man are as much articles of national wealth as coal and iron, and their waste is as reprehensible. Educational monopolies which offer numerous and great prizes for work of other descriptions, have caused enormous waste of scientific ability, by inducing those who might have succeeded in science, to spend their energies with small effect on uncongenial occupations. When a pursuit is instinctive and the will is untaxed, an immense amount of work may be accomplished with ease. Witness, to take an extreme case, the sustained action of the wholly involuntary muscles. The heart does its work unceasingly, from birth to death; and it is no light work, but such as the arm, working a pump-handle, would soon weary of maintaining;

or again, think of the migratory flight of birds, in obedience to an instinct; or of the muscular force, astonishing both in magnitude and endurance, exhibited by lunatics, who have some real though morbid passion which goads them to exercise it. We must therefore learn to respect innate tastes, which directly, as in A, or indirectly, as in C, serve the cause of science. As regards B, the fortunate accidents, we can multiply opportunities. There is great hope in respect to D, the professional influences. It is clear to all who have knowledge of the scope of modern science, that there exists an immense deal of national work which has to be performed, and which none but men of scientific culture are qualified to undertake. Scientific superintendence is required for all kinds of technical education, for statistical investigations of innumerable kinds, and deductions from them; for sanitary administration in the broadest sense; for agriculture, mining, industrial occupations, war, engineering. There is everywhere a demand for scientific assessors, who shall discover how to economise effort and find out new processes and fruitful principles. Professional duties generally, ought to be more closely bound up with strictly scientific work than they are at present; and this requirement would tend to foster scientific tastes in minds which had little inborn tendency that way. In respect to G, the influence and encouragement of tutors, seeing how far Scotland has surpassed England in the attractiveness of her mode of teaching, which is by professorial lectures rather than by class-work, it is clear that the English system admits of being greatly improved, and the influence of her teachers proportionately increased, in turning the minds of youths to science. Lastly, as regards H, travel in distant lands, its indirect value deserves far more than the moderate sums assigned to its prosecution, in the way of starved travelling fellowships and rare voyages of surveying ships.

To sum up in a few words: it seems to me that the interpretation to be put on the replies we have now been considering, is that a love of science might be largely extended by fostering, and not thwarting, innate tendencies, by the extension of scientific professional appointments and professorships, by assimilating in some cases the English system of teaching to that of the Scotch, and by creating travelling and other fellowships which shall enable their holders to view nature in various aspects, and to work with foreigners whose habits of thought are fruitful in themselves, but of a different kind to our own.

I will take this opportunity of drawing attention to what appears to me one of the greatest of desiderata of this kind in the present day, namely, the establishment of medical fellowships amply sufficient to enable the best youths, who intend to follow medicine as a profession, to spend their early manhood in prosecuting independent medical researches. I appeal to capitalists, who know not what use, free from abuse, to make of their surplus wealth, to consider this want. They might greatly improve the practical skill of the English medical profession by affording opportunities of prolonged study. They might perhaps themselves, reap some part of the benefit of it. A young medical man has now to waste the most vigorous years of his life in miserable routine work simply to obtain bread, until he has been able to establish his reputation. He has no breathing-time allowed him; the cares of mature life press too closely upon his student days to give him the opportunities of prolonged study that are necessary to accomplish him for his future profession.

The influences we have been considering, are those which urge men to pursue science rather than literature, politics, or other careers; but we must not forget that there are deep and obscure movements of national life, which may quicken or depress the effective ability of the nation as a whole. I have not considered the reasons why one period is more productive of great men than another, my inquiry being limited, for the reasons stated in the first pages of this book, to one period and nation. But it may be remarked, that the national condition most favourable to general efficiency is one of self-confidence and eager belief in the existence of great works capable of accomplishment. The

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opposite attitude is indifferentism, founded on sheer uncertainty of what is best to do, or on despair of being strong enough to achieve useful results; a feeling such as that which has generally existed in recent years among wealthy men in respect to pauperism and charitable gifts. A common effect of indifferentism is to dissipate the energy of the nation upon trifles; and this tendency seems to be a crying evil of the present day in our own country. In illustration of this view, I will quote the following extract from a letter of one of my correspondents, who, I should add, is singularly well qualified to form a just opinion on the matter to which he so forcibly calls attention:—"The principal hindrance to inquiry and all other intellectual progress in the people of whom I see much, is the elaborate machinery for wasting time which has been invented and recommended under the name of 'social duties.' Considering the mental and material capital of which the richer classes have the disposal, I believe that much more than half the progressive force of the nation runs to waste from this cause."

Marquess of Salisbury

A great deal of energy is wasted in attempting to seize more than can be grasped. There is a feverish tendency, fostered by the daily press, to interest oneself in all that goes on, which leads to perpetual distraction, and curtails the time available for serious and sustained effort. It may be worth while to mention a curious little morbid experience of my own, as suggestive of much more mischief; it is this:—A few years ago, I had foolishly overworked myself, as many others have done, misled by a perverted instinct which goaded to increased exertion, instead of dictating rest. The consequence was, that I fairly broke down, and could not, for some days, even look at a book or any sort of writing. I went abroad; and though I grew much better and could amuse myself with books, the first town where I experienced real repose was Rome. There was no doubt of the influence of the place—it was strongly marked; and for a long time I sought in vain for the reason of it. At last, what I accept as a full and adequate explanation, occurred to me; simply that there were no advertisements on the walls. There was a pic-

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turesqueness and grandeur in its streets which sufficed to fill the mind, and there were no petty distractions to fret a weakened eye and brain. When we are in health we take little count of the racket of English life, which may keep apathetic minds from stagnation, but which causes needless wear and tear to active ones, suggesting nothing useful, and teasing, distracting and wearying. I have heard German professors speak with wonder at our waste of energy in mere fidget, and in so-called amusements, which are mostly very dull, and ascribe the successful laboriousness of their own countrymen to the greater simplicity of the lives they lead; and they are a happier people than we are.

Partial Failures.—We have seen that energy, health, steady pursuit of purpose, business habits, independence of views, and a strong innate taste for science, are generally combined in the character of a successful scientific man. Probably one-half of the men on my list possess every one of these qualities in a considerable, and some in a high degree. If one or more of these qualities be deficient, success becomes impossible, unless its absence be appropriately supplemented by other qualities or conditions. Cases may be specified, in which too few of the above-mentioned qualities were present, and which consequently ended in an abortive career. One, is the possession of energy, health, and independence of character in excess, and little else to control them. These are dangerous gifts. Those who have them are apt to renounce guidances by which the great body of mankind move safely, and to follow out a career in which they are almost certain to blunder and fail egregiously. Probably every large emigrant ship takes out many such men, full of unjustifiable self-confidence, who, to use a current phrase, "knock about in the world," waste their health, youth, and opportunities, and end broken down. Another case, is that in which a strong innate taste for science is accompanied by independence of character and steadiness of pursuit, but with no other quality helpful to success, and which therefore leads to no useful result. There is hardly a village where some ingenious man may not be found who has ideas and much shrewdness, but is crotchety and impracticable. He wants energy and business habits, so he never rises. Many of these men brood over subjects like perpetual motion: their peculiarities are well illustrated in De Morgan's Book of Paradoxes. Again we frequently meet persons of a stamp that justifies the old-fashioned caricature of scientific men, who are absorbed in some petty investigation, utterly deficient in business habits, and noted for absence of mind. Even idiots have often strongly quasi-scientific tastes, as love for simple mechanism, or objects of natural history; and they have, as already remarked, a pleasure in collecting. Madmen have often persistency, as is shown by their brooding on a single topic. We all of us must have met with curious cases of failures, where a mind and disposition that promise much for success, never achieve it. It may be that some mental screw is loose, or there is some irreparable weakness of judgment, or some untimely irresolution or rashness; any fault of this kind is sufficient to mar a man's chances when competition is keen. To obtain the highest order of success, two things are wanted; first, the qualities of the man must either be good all round, or else he must be so circumstanced as to be able, when the need arises, to supplement his deficiencies by extraneous help; secondly, he must have some very useful qualities highly developed. It is said that "genius" is required for high success, and there is much talk about what genius is, and on the failures of men of genius, while some persons go so far as to doubt the existence of genius as a separate quality. It appears to me, that what is generally meant by genius, when the word is used in a special sense, is the automatic activity of the mind, as distinguished from the effort of the will. In a man of genius, the ideas come as by inspiration; in other words, his character is enthusiastic, his mental associations are rapid, numerous and firm, his imagination is vivid, and he is driven rather than drives himself. All men have some genius; they are all apt, under excitement, to show flashes of unusual enthusiasm, and to experience swift and strange associations of ideas; in dreams, all men commonly exhibit more vivid powers of imagination than are possessed by the greatest artists when awake. Sober, plodding will is quite another quality, and its over-exercise exhausts the more sprightly functions of the mind, as is expressed in the proverb, "too much work makes a dull boy." But no man is likely to achieve very high success in whom the automatic power of the mind, or genius in its special sense, and a sober will, are not well developed and fairly balanced.

CHAPTER IV.

EDUCATION.

Preliminary—Education praised throughout or nearly so—Merits in Education—Merits and demerits balanced—Demerits—Summary—Conclusion.

I NOW pass on to the education which the scientific men had in their youth, in the hope that my results may give assistance to those who are endeavouring to frame systems of education suitable to the wants of the day. What I have to say is very partial; it refers solely to the opinions the scientific men entertain of the merits and faults of their own several educations in bygone days. Their views are remarkably unanimous, considering the very different branches of inquiry they are interested in, and the great dissimilarities in their education.

One-third of those who sent replies have been educated at Oxford or Cambridge, one-third at Scotch, Irish, or London universities, and the remaining third at no university at all. I am totally unable to decide which of the three groups occupies the highest scientific position: they seem to me very much alike in this respect.

The questions to which the following replies were given, were as follows:—"Was your education especially conducive to, or restrictive of, habits of observation?" "Was your education eminently conducive to health or the reverse?" "What do you consider to have been peculiar merits in your education?" "What were the chief omissions in it, and what faults of commission can you indicate?" I also asked for information concerning the places of education, both schools and colleges, and as regards home and self-instruction. The answers were, in some cases, very interesting from their minute elaboration, but I am, of course, restricted on this occasion to a simple treatment of them. I cannot now paint with delicate tints, but must content myself with broad lights and shades. The following answers are extracts, and, in some few cases, abstracts; they convey the general tone of the several replies as nearly as possible.

The groups under which I have sorted them are these :—

Merits :—		
,, Education praised throughout, or nearly so . .	10	replies
,, Variety of subjects .	10	,,
,, A little science at school	3	,,
,, Simple things well taught	3	,,
,, Liberty and leisure .	3	,,
,, Home teaching and encouragement . .	8	,,
Merits and demerits balanced .	4	,,
Demerits :—		
,, Narrow education . .	32	,,
,, Want of system and bad teaching	10	,,
,, Unclassed . . .	4	,,
Total	87	

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There are a few cases in which an answer, already given in combination, has been extracted and repeated.

MERITS: EDUCATION PRAISED THROUGHOUT, OR NEARLY SO—TEN CASES.

Main (1) "Was admirably taught, æt. 13–16½, to reason, use my own mind, and depend on myself. Was taught to acquire large masses of information by reading. There was a little tendency to a vagrant style of reading, but this was probably neutralised by other influences."

Haughton (2) "Well taught in classics and mathematics. If possible my education should have afforded facilities for the study of the science of observation, but I doubt the practicability of this at school. While a schoolboy I taught myself botany, chemistry, &c., under great disadvantages."

Humphry (3) "Careful and good early education at home by my mother and father; then rather strict training by my father and by my first schoolmaster. Being carefully looked after by my father and expected to do my best."

(4) "My education was well balanced; it was general and of a very complete kind, including chemistry, botany, logic and political economy; but 3 years (æt. 12–15) spent in learning the Latin and Greek grammars were a blank waste of time." Jevons

(5) "Education included French, German, logic, natural philosophy, chemistry, besides mathematics. I lived in a house where I saw many people whose interests were of various kinds, and I went to a day-school where I mixed with the boys only when they were fresh and active. Thus I had two outer worlds to balance against each other. On the whole, I had, I think, the greatest degree of freedom possible to a boy." Maxwell

(6) "Was at school till æt. 16, and with a tutor in Germany for 6 months; after then, technical training and teaching. The education was conducive both to observation and health. J. Evans

Variety of subjects and attention to details. A combination of home and school education, my father having been head master of the school."

Hill

(7) "My father being a schoolmaster, I was at some sort of school work nearly all my life, but from the age of 12 I was occupied more in teaching than in learning. My education included the various subjects usually taught in English schools, with something of astronomy, pneumatics, electricity, and mechanics. I learnt much in conversation with my father, which chiefly took an instructive form. Was led to think and speak freely, also to engage frequently in domestic discussions on questions of general policy. I had also early access to tools and materials."

Roscoe

(8) "I was fortunate in obtaining at school (æt. 8–16) an insight into the phenomena of nature, a subject entirely ignored at that time in almost all schools. My peculiar bent for experiment was encouraged at home by my mother, and there were peculiar merits in my

training under Professors . . . . at . . . . , and especially in Germany, under . . . ."

(9) "The steadiness with which I was taught by one eccentric schoolmaster reading and accurate spelling, clear, neat, and intelligible writing, and quick and accurate computation by all the primary rules of arithmetic. Faults in these several branches were never overlooked, and all competition was for excellence in each; Latin and French were evidently thrown in to please parents. Going to sea, at the age of 13, I really think I started with the best education I could have had. Compared with my youthful messmates, some of whom had passed through public schools, I was far their superior in writing (I soon acquired chart-drawing and sketching from nature), and in calculation of the day's work, and in astronomical observations." Captn Evans

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MERITS IN EDUCATION: VARIETY OF SUBJECTS —NINE REPLIES.

Roscoe (1) "Not tied down to old courses of classics and mathematics."

Newton (2) "My master (æt. 15–17) was a man of scientific and generally liberal turn of mind."

Prestwich (3) "Sufficient groundwork in many subjects to avoid error."

Jenkyn (4) "Early introduced to many subjects of interest."

Jevons (5) "A well-balanced education [including chemistry, botany, logic, and political economy]."

Playfair (6) "A variety of subjects and attention to details. Coming in contact with persons of every rank [in Scotland], and sitting on the same form with the sons of tradesmen and ploughmen, as well as of gentlemen."

Hooker, Cobbold } (7 & 8) Two cases; both [being Englishmen] praise Scotch system of education.

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(9) "Living in a house where there were many interests, and going thence to a day-school, where there were other and different ones." Maxwell

MERITS IN EDUCATION: A LITTLE SCIENCE AT SCHOOL—THREE REPLIES.

(1) "Only one good thing; that was object lessons, though given badly and only for a short time." Carrington

(2) "All the merits [of my schooling] I attribute to a little elementary physics and chemistry, taught me between the ages of 7 and 13." Forbes

(3) "Science taught me at school between the ages of 11 and 16." W. Fox

MERITS IN EDUCATION: SIMPLE THINGS WELL TAUGHT—THREE REPLIES.

(1) "Clear, neat, and intelligible writing, accurate spelling, and simple computation." Capt Evans

(2) "Was very well grounded in arithmetic at school." Stenhouse

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Carter (3) "Forced accuracy of delineation at home, æt. 14–16."

MERITS IN EDUCATION: LIBERTY AND LEISURE.—THREE REPLIES.

Williamson, A.W. (1) "Unusual degree of freedom."

Flower (2) "Freedom to follow my own inclinations and choose my own subjects of study, or the reverse."

Owen (3) "The great proportion of time left free to do as I liked, unwatched and uncontrolled."

MERITS IN EDUCATION: HOME TEACHING AND HOME ENCOURAGEMENT—EIGHT REPLIES.

Roscoe (1) "Encouragement by my mother."

Lubbock (2) "Encouragement by my father."

Humphry (3) "Carefully looked after by my father and expected to do my best."

Hill (4) (See (7), in "Education praised throughout or nearly so.")

(5) "During 1 year (æt. 17) I resided and studied with my uncle [by marriage] and learnt there more of the dead languages than in all my school time." Owen

(6) "My private education at home was much the more valuable." Thomson

(7) "Home and self-education developed my observing faculties." Allman

(8) "Pretty much self-taught, but encouraged to use my eyes, wits, and independent thought." Merrifield

MERITS AND DEMERITS IN EDUCATION BALANCED —FOUR REPLIES.

(1) "Left to myself, and I pursued a discursive line. As compared with ordinary schools, I think self-teaching has many advantages for boys of active minds; but intelligent teaching and insisting on accuracy and completeness would have produced a much more efficient man." Ball

(2) "The merits of my education consisted in the great number of studies connected with Stokes

nature; but there was a want of system and of consecutive study."

Back (3) "The demerit of my education was the want of being thoroughly grounded; this gave me great trouble, but made me think for myself; often an advantage to me."

Bateman (4) "No sound instruction; the education was too general and desultory, but it gave wide interest."

DEMERITS: NARROW EDUCATION—THIRTY-TWO CASES.

Darwin (1) "No mathematics nor modern languages, nor any habits of observation or reasoning."

Forbes (2) "Enormous time devoted to Latin and Greek, with which languages I am not conversant."

Fergusson (3) "Omission of almost everything useful and good, except being taught to read. Latin! Latin! Latin!"

Guy (4) "Latin through Latin—nonsense verses."

(5) "Limitation of subjects practically to classics." Salisbury

(6) "Absence of any scientific training; too much confined to classics." Lubbock

(7) "Omission of mathematics, German, and drawing." Egerton

(8) "Latin and Greek were more insisted on than modern languages." Heywood

(9) "In an otherwise well-balanced education, 3 years, æt. 12–15, at a private school were spent on Latin and Greek grammar—a blank waste of time." Jevons

(10) "School work directed to the cultivation of literary tastes only, and therefore not adapted to a variety of intellects." Thomson

(11) "Elements of natural science omitted; nothing taught of the nature of the world around us." Owen

(12) "Not taught mathematics, nor any natural science, to which I could have taken *con amore*." Harcourt

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Cobbold (13) "Absence of instruction in the modern languages."

Stenhouse (14) "Want of the modern languages and of chemistry."

Jeffreys, Gwyn. (15) "Want of logical and mathematical training."

Hooker (16) "Want of training in the habits of observation."

W. C. Williamson. (17) "Neglect of mathematics; too much reliance on mere work of memory. Mental training overlooked in the mere acquisition of routine."

War: Smyth (18) "I could now wish that I had gone through at the university a good course of chemistry and physics, as a preparation for the other branches; but the main obstacle was lack of time."

R. Jones (19) "Want of education of faculties of observation; want of mathematics, and of modern languages."

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(20) "Not allowing my mind to follow its natural bias." Armstrong

(21) "Neglect of many subjects for the attainment of one or two; not pushing mathematics to a useful end." Prestwich

(22) "Not enough liberty; put back by too much grounding at Cambridge." Carrington

(23) "At school the classical education, viz., construing, parsing and learning grammatical rules, was not to my taste. At Oxford I wasted much time, having little sympathy with the university pursuits and habits." Grove

(24) "Having so exclusively devoted myself to mathematics at Cambridge." Arch: Smith

(25) "The classical teaching was said to be good, but I did not assimilate it. Perhaps my mental peculiarities and my special inaptitude to commit words to memory would have rendered most education, such as it was when I was a boy, ineffectual for much good. The main defect for me certainly was that *precise* verbal Strachey

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memory was the test of all knowledge. No doubt, in some things, such as languages, precise knowledge of words is essential, and therefore I refer to my own special defect in saying this."

Foster (26) "My school work was too predominantly classical, and nearly everything was taught on *authority*.'

Maskelyne (27) "Persistence in giving me no holiday, and overstraining my memory when I was very young."

Bastian (28) "My principal regret is that I was unable to pursue the study of mathematics."

Marshall (29) "Mathematics were not pushed far enough; natural science was left to the boys themselves."

Huxley (30) "My boyhood was utterly wasted, and the efforts of my manhood have not sufficed, and never will suffice, to repair the loss."

Sanderson (31) "Omission of all subjects excepting the classics, but particularly [faulty] in the want of intellectual training."

(32) [A military man.] "The *authority* of a military education is prejudicial to the development of thought and education in matters of opinion." Capt. D. Galton

DEMERITS IN EDUCATION: WANT OF SYSTEM AND BAD TEACHING—TEN CASES.

(1) "Want of system." Miller

(2) "Want of system." Prestwich

(3) "Want of system." Jenkyn

(4) "Want of system; absence of necessary control." Ansted

(5) "Bad early masters; neglect at public school." Babington

(6) "Essentially defective; no competition nor supervision." Osborn

(7) "The very mistaken way in which languages, as it now seems to me, especially Latin and Greek, were taught." Newton

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War: Smyth (8) "Too much for memory; nothing for thought."

Wilson Fox (9) "Want of thoroughness in early teaching."

Parker (10) "Careless and superficial ~~reading~~." teaching

DEMERITS IN EDUCATION: UNCLASSED—FOUR CASES.

Salisbury (1) "Brought up in an idle class, and never realised the necessity of labour in acquirement."

Scott (2) "Too much cramming for examinations. Too much isolated, being the youngest son and educated at home."

Playfair (3) "Too great changes in system, having been educated at 5 universities (3 of which were Scotch, 1 London, and 1 in Germany)."

Gladstone (4) "Being brought up at home; was perhaps too much shut out from the company of other boys."

SUMMARY.

The scientific men on my list have very generally ascribed high merits to a varied education. They say, as we have just seen:—"Not tied down to old courses of classics and mathematics."—"Sufficient groundwork in many subjects to avoid error."—"A well-balanced education, including chemistry, botany, logic, and political economy."—"Coming in contact with persons of every rank, and sitting in the same form [in a Scotch school] with the sons of tradesmen and ploughmen, as well as gentlemen." In contrast to this, others who speak of the faults of their education, say:—"No mathematics, nor modern languages, nor any habits of observation or reasoning."—"Enormous time devoted to Latin and Greek, with which languages I am not conversant."—"In an otherwise well-balanced education, three years were spent on Latin and Greek grammar—a blank waste of time."—"Neglect of many subjects for the attainment of one or two; not pushing mathematics to a useful end." Evidence such as this, fully establishes the advantage of a variety of study. One group of men speak gratefully because they had it, and another speak regretfully because they had it not. I find none who had a reasonable variety who disapproved of it, none who had a purely old-fashioned education who were satisfied with it. The scientific men who came from the large public schools usually did nothing when there; they could not assimilate the subjects taught, and have abused the old system heartily. There are several serious complaints about superficial and bad teaching which I need not quote afresh. Overteaching is thoroughly objected to; thus, in speaking of merits of education, I find:—"Freedom to follow my own inclinations, and to choose my own subjects of study, or the reverse."—"The great proportion of time left free to do as I liked, unwatched and uncontrolled."—"Unusual degree of freedom." There is much scattered evidence throughout the replies to my questions generally, in addition to what I have extracted, which implies that this feeling is a very common one. There are many touching evidences of the strong effect of home encouragement and teaching; of this I have already spoken, and need not dwell upon afresh.

In corroboration of the conclusions stated in p. 216, on the favourable influence of the Scotch system in developing a taste for science, I remark that in these replies, a large proportion of the scientific men who have mentioned any merits in their education, were educated in Scotland.

As regards the subjects specially asked for, even by biologists, mathematics take a prominent place. Two of my correspondents speak strongly of the advantages derived from logic, and the weighty judgment of the late John S. Mill powerfully corroborates their opinions. Accuracy of delineation is also spoken of, and, owing to the extraordinary prevalence of mechanical aptitudes, I believe that the teaching of mechanical drawing and manipulation would be greatly prized.

The interpretation that I put on the answers as a whole is as follows: To teach a few congenial and useful things very thoroughly, to encourage curiosity concerning as wide a range of subjects as possible, and not to overteach. As regards the precise subjects for rigorous instruction, the following seem to me in strict accordance with what would have best pleased those of the scientific men who have sent me returns:—1. Mathematics pushed as far as the capacity of the learner admits, and its processes utilized as far as possible for interesting ends and practical application. 2. Logic (on the grounds already stated, but on those only). 3. Observation, theory, and experiment, in at least one branch of science; some boys taking one branch and some another, to ensure variety of interests in the school. 4. Accurate drawing of objects connected with the branch of science pursued. 5. Mechanical manipulation, for the reasons already given, and also because mechanical skill is occasionally of great use to nearly all scientific men in their investigations. These five subjects should be *rigorously* taught. They are anything but an excessive programme, and there would remain plenty of time for that variety of work which is so highly prized, as—ready access to books; much reading of interesting literature, history and poetry; languages learnt, probably best during the vacations, in the easiest and swiftest manner, with the sole object of enabling the learners to read ordinary books in them. This seems sufficient, because my returns show that men of science are not made by much teaching, but rather by awakening their interests, encouraging their pursuits when at home, and leaving them to teach themselves continuously throughout life. Much teaching fills a youth with knowledge, but tends prematurely to satiate his appetite for more. I am surprised at the mediocre degrees which the leading scientific men who were at the universities have usually taken, always excepting the mathematicians. Being original, they are naturally less receptive; they prefer to fix of their own accord on certain subjects, and seem averse to learn what is put before them as a task. Their independence of spirit and coldness of disposition are not conducive to success in competition: they doggedly go their own way, and refuse to run races.

CONCLUSION.

Science has hitherto been at a disadvantage, compared with other competing pursuits, in enlisting the attention of the best intellects of the nation, for reasons that are partly inherent and partly artificial. To these I will briefly refer in conclusion, with especial reference to the very important question as to how far the progress of events tends to counterbalance or remove them.

If we class energy, intellect, and the like, under the general name of ability, it follows that, other circumstances being the same, those able men who have vigour to spare for extra professional pursuits, will be mainly governed in the choice of them by the instinctive tastes of their manhood. The majority will address themselves to topics nearly connected with human interests; a few only will turn to science. This tendency to abandon the colder attractions of science for those of political and social life, must always be powerfully reinforced by the very general inclination of women to exert their influence in the latter direction. Again, those who select some branch of science as a profession, must do so in spite of the fact that it is more unremunerative than any other pursuit. A great and salutary change has undoubtedly come over the feeling of the nation since the time when the present leading men of science were boys, for education was at that time conducted in the interests of the clergy, and was strongly opposed to science. It crushed the inquiring spirit, the love of observation, the pursuit of inductive studies, the habit of independent thought, and it protected classics and mathematics by giving them the monopoly of all prizes for intellectual work, such as scholarships, fellowships, church livings, canonries, bishoprics, and the rest. This gigantic monopoly is yielding, but obstinately and slowly, and it is unlikely that the friends of science will be able, for many years to come, to relax their efforts in educational reform. As regards the future provision for successful followers of science, it is to be hoped that, in addition to the many new openings in industrial pursuits, the gradual but sure development of sanitary administration and statistical inquiry may in time afford the needed profession. These and adequately paid professorships may, as I sincerely hope they will, even in our days, give rise to the establishment of a sort of scientific priesthood throughout the kingdom, whose high duties would have reference to the health and well-being of the nation in its broadest sense, and whose emoluments and social position would be made commensurate with the importance and variety of their functions.

APPENDIX.

My schedule of printed questions, together with the ample spaces left for replies, filled, I am half ashamed to acknowledge, seven huge quarto pages. It would be a cumbrous addition to a publication like the present to reproduce these in the same form in which they were framed; and as the following extracts (with trifling variations rendered necessary by the change of form) cover precisely the same ground, and are sufficient for explanation, I abstain from doing so.[1]

A circular letter, in which I explained briefly the object of the inquiry, accompanied the schedule, and I

[1] I also omit the description of a notation I proposed to replace indefinite words such as "large," "considerable," because I have made no use of it in this volume. It is a modification of the class notation used by me in my "Hereditary Genius," and was alluded to and illustrated in my lecture before the Royal Institution, 1874. I have by no means abandoned its advocacy, but have learnt the necessity of explaining and exemplifying it in considerable detail before its merits and convenience are likely to become as generally recognised as I believe they deserve to be.

appended to it a reprint of a short article which I had written in the *Fortnightly Review* early in 1873, partly to show the interest with which I had pursued cognate inquiries, and partly as a guarantee of the tone and spirit in which the inserted communications would be treated. Also I presumed, and, as it has proved, not without reason, that being more or less personally acquainted with a large majority of the scientific men on my list, they would be inclined to put greater faith in my discretion than if I had been a stranger. Subject to these preparatory explanations, the following are the questions that I circulated :—

INQUIRY INTO THE ANTECEDENTS OF SCIENTIFIC MEN.

Please return this schedule at your earliest convenience, with answers to as many of the questions as you consider to be unobjectionable, and send on a separate paper any further information that you may think germane to the inquiry. Entries marked "Private" will be dealt with in *strict confidence;* they will be used only as data for general statistical conclusions.

NOTE.—Whenever you consider the grade of the quality about which a question is asked, to fall near mediocrity, *do not make any entry at all.*

Christian names of yourself, your father, and your mother, also her maiden name? Designation and principal titles of yourself, your father, and the father of your mother? Your father and mother, are they respectively English, Welsh, Scotch, Irish, Jewish, or foreign? If foreign, of what country? Wholly or in what degree? Was either your father or your mother descended from persons persecuted for political or religious opinions, or from political or religious refugees? If so, state the precise relationship. Mention whether their political or religious opinions became traditional in the family. Occupation of yourself, your father, and the father of your mother? Specify any interests that have been very actively pursued by them, in addition to their regular occupation or profession.

All the questions in the following paragraph are asked concerning yourself, your father, and your mother respectively:—

Date of the birth of? Place of the birth of (if you do not remember that of either your father or mother, state where he or she resided in early life)? Mention if it was in a large or small town, a suburb, a village, or a house in the country. To what religious bodies have you (self, father and mother) respectively belonged? To what political parties? Health at the various periods of life? In early adult life, what was your height (to be estimated, where not accurately remembered)? Was there anything distinctive in the figure, &c. (spare, symmetrical, muscular, &c.)? Colour of hair? Complexion (if remarkably fair, dark, ruddy, pale, sallow, &c.)? Temperament, if distinctly nervous, sanguine, bilious, or lymphatic? Measurement round inside of rim of your hat? Energy of body, if remarkable; as shown by power of activity, power of enduring fatigue, restlessness, requiring but little sleep (state how much), early rising, adventures, travel, mountaineering, &c. (give a few facts)? Energy of mind, if remarkable; as shown by power of accomplishing a large *amount* of brain work, by the vigorous pursuit of interests, whatever they may be, &c. (give a few facts)? Retentiveness of memory (give facts)? Studiousness of disposition and mental receptivity, as shown by large acquirements? Independence of judgment in social political, or religious matters (give illustrations)? Originality or eccentricity of character (give illustrations)? Special talents, as for mechanism, practical business habits, music, mathematics, &c.? Strongly marked mental peculiarities, bearing on scientific success, and not specified above: the following list may serve to suggest—impulsiveness, steadiness, strong feelings and partisanship, social affections, religious bias of thought, love of the new and marvellous, curiosity about facts, love of pursuit, constructiveness of imagination, foresight, public spirit, disinterestedness.

Are any peculiarities either very uniformly developed, or also very irregularly developed among yourself, your brothers and sisters, or in the family of your father, or in that of your mother?

State the number of males and that of the females in each of the following degrees of relationship who have attained 30 years of age, or thereabouts:—Grandparents, both sides; parents, uncles and aunts, both sides; brothers and sisters; first-cousins of all four descriptions; nephews and nieces. In each of these several degrees of relationship, state the names of those who have occupied prominent positions or written well-known works, or who from any other cause may be considered as public characters. State their principal achievements, mention the best biographies, and the most useful among the scattered biographical notices that may exist of them; terms of award of medals, &c. Also, in each of the above degrees of relationship, give the number (with initials or names) of those whose ability *in any respect* was considerable, but who did not become public characters (fuller information to be sent on a separate paper). Similar information is acceptable concerning other more remote degrees of relationship. Brief notes concerning hereditary peculiarities of any kind in your family, bodily or mental, would be acceptable. How many brothers and

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sisters had you older than yourself, and how many younger?

How long were you at small schools, large schools, universities, and at what ages? Name or place of school or university, and chief subjects taught there. Mention any honours of importance gained by you at schools or universities. To what extent were you educated elsewhere, taught at home, or self-taught? Was your education especially conducive to, or restrictive of habits of observation? Was it eminently conducive to health or the reverse? What do you consider to have been peculiar merits in it? What were the chief omissions in it, and what faults of commission can you indicate? Has the religion taught in your youth had any deterrent effect on the freedom of your researches? Can you trace the origin of your interest in science in general and in your particular branch of it? How far do your scientific tastes appear to have been innate? Were they largely determined by events occurring after you reached manhood, and by what events?

Have you been married? Year in which you were married? Maiden name of your wife? Number of living sons and daughters (of all ages)? State any facts of peculiar interest in your wife's family.

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THE END.

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From the PHILOSOPHICAL MAGAZINE for January 1875.

STATISTICS BY INTERCOMPARISON,

WITH

REMARKS ON THE LAW

OF

FREQUENCY OF ERROR.

BY

FRANCIS GALTON, F.R.S.

MY object is to describe a method for obtaining simple statistical results which has the merit of being applicable to a multitude of objects lying outside the present limits of statistical inquiry, and which, I believe, may prove of service in various branches of anthropological research. It has already been proposed (Lecture, Royal Institution, Friday evening, February 27, 1874), and in some degree acted upon ('Hereditary Genius,' p. 26), by myself. What I have now to offer is a more complete explanation and a considerable development of previous views.

The process of obtaining mean values &c. now consists in measuring each individual with a standard that bears a scale of equal divisions, and afterwards in performing certain arithmetical operations upon the mass of figures derived from these numerous measurements. I wish to point out that, in order to procure a specimen having, in one sense, the mean value of the quality we are investigating, we do not require any one of the appliances just mentioned: that is, we do not require (1) independent measurements, nor (2) arithmetical operations; we are (3) able to dispense with standards of reference, in the common acceptation of the phrase, being able to create and afterwards indirectly to define them; and (4) it will be explained how a rough division of our standard into a scale of degrees may not unfrequently be effected. Therefore it is theoretically possible, in a great degree, to replace the ordinary process of obtaining statistics by another, much simpler in conception, more convenient in certain cases, and of incomparably wider applicability.

Nothing more is required for the due performance of this process than to be able to say which of two objects, placed side by side, or known by description, has the larger share of the quality we are dealing with. Whenever we possess this power of discrimination, it is clear that we can marshal a group of objects in the order in which they severally possess that quality. For example, if we are inquiring into the statistics of height, we can marshal a number of men in the order of their several heights. This I suppose to be effected wholly by *intercomparison,* without the aid of any external standard. The object then found to occupy the middle position of the series must possess the quality in such a degree that the number of objects in the series that have more of it is equal to that of those that have less of it. In other words, it represents the *mean* value of the series in at least one of the many senses in which that term may be used. Recurring to the previous illustration, in order to learn the mean height of the men, we have only to select the middlemost one and measure him; or if no standard of feet and inches is obtainable, we must describe his height with reference to numerous familiar objects, so as to preserve for ourselves and to convey to strangers as just an idea of it as we can. Similarly the mean speed of a number of horses would be that of the horse which was middlemost in the running.

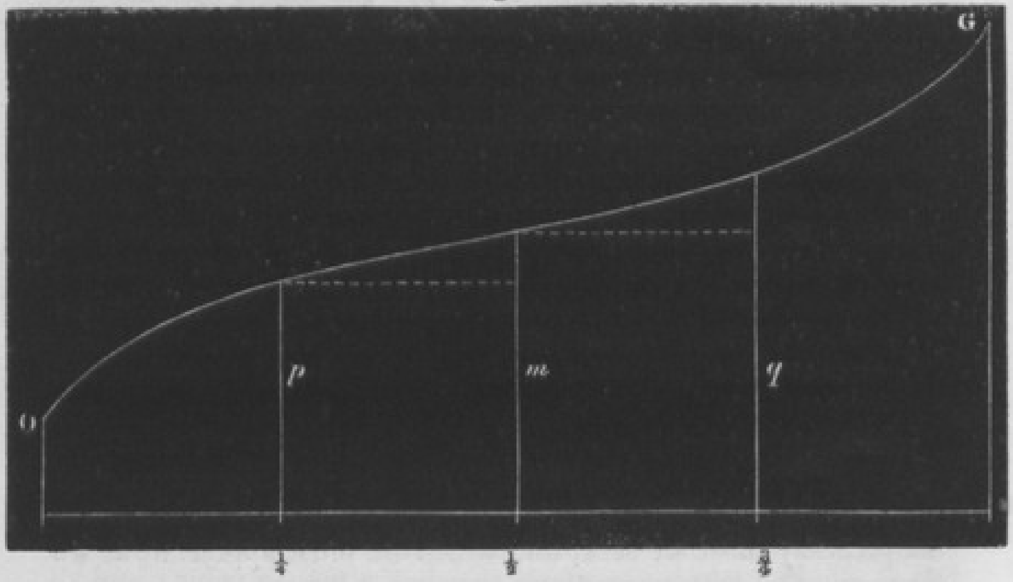
If we proceed a step further and desire to compare the mean height of two populations, we have simply to compare the representative man contributed by each of them. Similarly, if we wish to compare the performances of boys in corresponding classes of different schools, we need only compare together the middle boys in each of those classes.

The next great point to be determined is the divergency of the series—that is, the tendency of individual objects in it to diverge from the mean value of all of them. The most convenient measure of divergency is to take the object that has the mean value, on the one hand, and those objects, on the other, whose divergence in either direction is such that one half of the objects in the series on the same side of the mean diverge more than it does, and the other half less. The difference between the mean and either of these objects is the measure in question, technically and rather absurdly called the "probable error." Statisticians find this by an arithmetical treatment of their numerous measurements; I propose simply to take the objects that occupy respectively the first and third quarter points of the series. I prefer, on principle, to reckon the divergencies in excess separately from those in deficiency. They cannot be the same unless the series is symmetrical, which experience shows me to be very rarely the case. It will be observed that my process fails in giving the difference (probable error) in numerical terms; what it does is to select specimens whose differences are precisely those we seek, and which we must appreciate as we best can.

We have seen how the mean heights &c. of two populations may be compared; in exactly the same way may we compare the divergencies in two populations whose mean height is the same, by collating representative men taken respectively from the first and third quarter points of the series in each case.

We may be confident that if any group be selected with the ordinary precautions well known to statisticians, it will be so far what may be called "generic" that the individual differences of members of that group will be due to various combinations of pretty much the same set of variable influences. Consequently, by the well-known laws of combinations, medium values will occur very much more frequently than extreme ones, the rarity of the latter rapidly increasing as the deviation slowly increases. Therefore, when the objects are marshalled in the order of their magnitude along a level base at equal distances apart, a line drawn freely through the tops of the ordinates which represent their several magnitudes will form a curve of ~~double curvature~~ contrary flexure. It will be nearly horizontal over a long space in the middle, if the objects are very numerous; it will bend down at one end until it is nearly vertical, and it will rise up at the other end until there also it is nearly vertical. Such a curve is called, in the phraseology of architects, an "ogive," and is represented by O G in the diagram (fig. 1), in which the process of statistics by intercomparison is clearly shown. If $n =$ the length of the base of the ogive, whose ordinate y represents the magniude of the object that stands at a distance x from that end of the base where the ordinates are smallest, then the number of

Fig. 1.



objects less than y: the number of objects greater than $y :: x : n-x$. The ordinate m at $\frac{1}{2}$ represents the mean value of the series, and p, q at $\frac{1}{4}$ and $\frac{3}{4}$, taken in connexion with m, give data for estimating the divergence; thus $q-m$ is the divergence (probable error) of at least that portion of the series that is in excess of the mean, and $m-p$ is that of at least the other portion. When the series is symmetrical, $q-m=p-q$, and either, or the mean of both, may be taken as the divergence of the series generally. No doubt we are liable to deal with cases in which there may be some interruption in the steady sweep of the ogive; but the experience of qualities which we *can* measure, assures us that we need fear no large irregularity of that kind when dealing with those which, as yet, we have no certain means of measuring.

When we marshal a series, we may arrange them roughly, except in the neighbourhood of the critical points; and thus much labour will be saved. But the most practical way of setting to work would probably depend not on the mere discrimination of greater and less, but also on a rough sense of what is much greater or much less. We have called the objects at the $\frac{1}{4}$, $\frac{1}{2}$, and $\frac{3}{4}$ distances p, m, and q respectively; let us sort the objects into two equal portions P and Q, of small and great, taking no more pains about the sorting than will ensure that P contains p and all smaller than p, and that Q contains q and all larger than q. Next, beginning, say, with group P, sort away alternately to right and left the larger and the smaller objects, roughly at first, but proceeding with more care as the residuum diminishes and the differences become less obvious. The last remaining object will be p. Similarly we find q. Then m will be found in the same way from the group compounded of those that were sorted to the right from P and to the left from Q.

There are not a few cases where both the ordinary method and that by intercomparison are equally applicable, but in which the latter would prove the more rapid and convenient. I would mention one of some importance to those anthropologists who may hereafter collect data in uncivilized countries. A barbarian chief might often be induced to marshal his men in the order of their heights, or in that of the popular estimate of their skill in any capacity; but it would require some apparatus and a great deal of time to measure each man separately, even supposing it possible to overcome the usually strong repugnance of uncivilized people to any such proceeding.

The practice of sorting objects into classes may be said to be coextensive with commerce, the industries, and the arts. It is adopted in the numerous examinations, whether pass or competitive, some or other of which all youths have now to undergo. It is adopted with every thing that has a money-value; and all acts of morality and of intellectual effort have to submit to a verdict of "good," "indifferent," or "bad."

The specimen values obtained by the process I have described are capable of being reproduced so long as the statistical conditions remain unchanged. They are also capable of being described in various ways, and therefore of forming permanent standards of reference. Their importance then becomes of the same kind as that which the melting-points of well-defined alloys or those of iron and of other metals had to chemists when no reliable thermometer existed for high temperatures. These were excellent for reference, though their relations *inter se* were subject to doubt. But we need never remain wholly in the dark as to the relative value of our specimens, methods appropriate to each case being sure to exist by which we may gain enlightenment. The measurement of work done by any faculty when trained and exerted to its uttermost, would be frequently available as a test of its absolute efficacy.

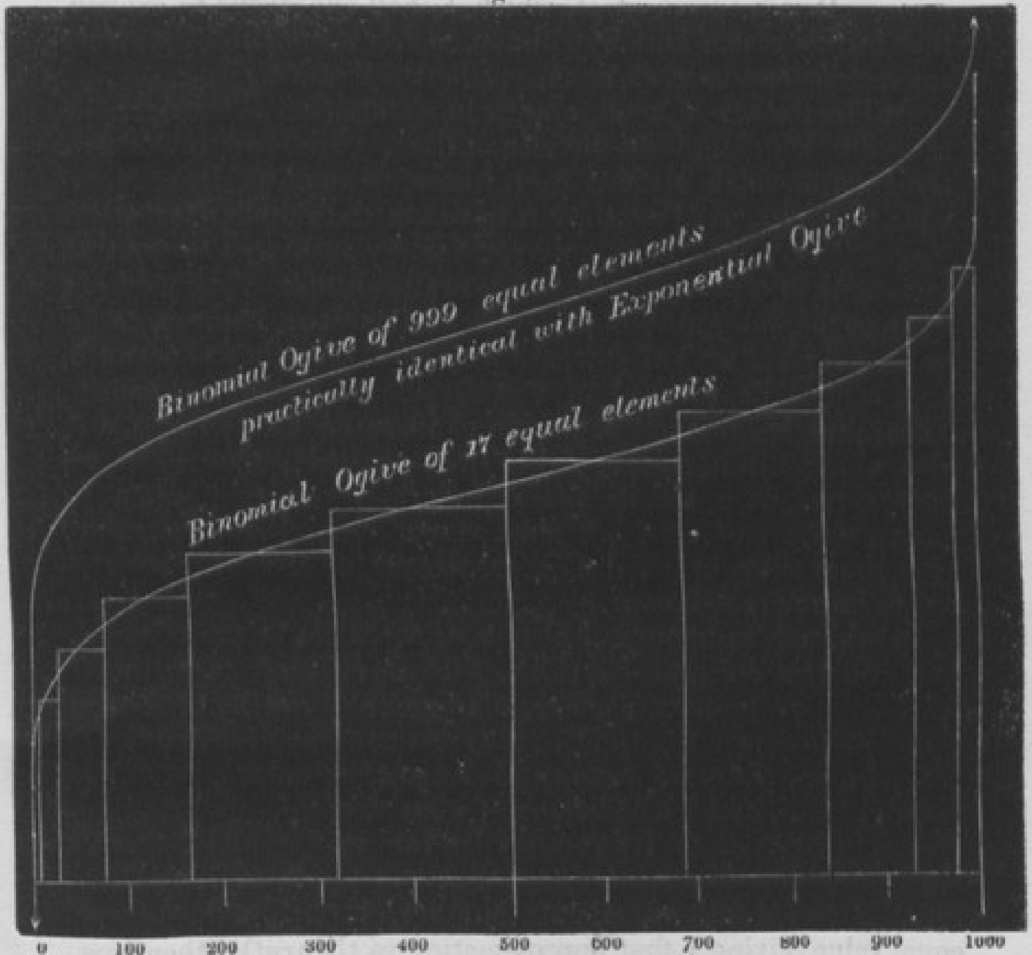
There is another method, which I have already advocated and adopted, for gaining an insight into the absolute efficacies of qualities, on which there remains more to say. Whenever we have grounds for believing the law of frequency of error to apply, we may *work backwards*, and, from the relative frequency of occurrence of various magnitudes, derive a knowledge of the true relative values of those magnitudes, expressed in units of probable error. The law of frequency of error says that "magnitudes differing from the mean value by such and such multiples of the probable error, will occur with such and such degrees of frequency." My proposal is to reverse the process, and to say, "since such and such magnitudes occur with such and such degrees of frequency, therefore the differences between them and the mean value are so and so, as expressed in units of probable error." According to this process, the positions of the first divisions of the scale of divergence, which are those of the mean value *plus* or *minus* one unit of probable error, are of course p and q, lying at the $\frac{1}{4}$ and $\frac{3}{4}$ points of the ogive, or, if the base consist of 1000 units, at the 250th point from the appropriate end. The second divisions being those of mean value *plus* or *minus* two units of probable error, will, according to the usual Tables, be found at the 82nd point from the appropriate end, the third divisions will be at the 17th, and the fourth at the 3rd. If we wished to pursue the scale further, we should require a base long enough to include very many more than 1000 units.

Remarks on the Law of Frequency of Error.

Considering the importance of the results which admit of being derived whenever the law of frequency of error can be shown to apply, I will give some reasons why its applicability is more general than might have been expected from the highly artificial hypotheses upon which the law is based. It will be remembered that these are to the effect that individual errors of observation, or individual differences in objects belonging to the same generic group, are entirely due to the aggregate action of variable influences in different combinations, and that these influences must be (1) all independent in their effects, (2) all equal, (3) all admitting of being treated as simple alternatives "above average" or "below average;" and (4) the usual Tables are calculated on the further supposition that the variable influences are infinitely numerous.

As I shall lay much stress on matters connected with the last condition, it will save reiteration if I be permitted the use of a phrase to distinguish between calculations based on the supposition of a moderate number (r) of elements (in which case the frequency of error or the divergence is expressed by the coefficients of the expansion of the binomial $(a+b)^r$) and one based on the supposition of the number being infinite (which is expressed by the exponential $e^{-\frac{x^2}{c^2}}$), by calling the one the binomial and the other the exponential process, the latter being the process to be understood whenever the "law of frequency of error" is spoken of without further qualification. When the results of these two processes have to be protracted, as in figure 2, the unit of vertical measurement in the case of a series of bino-

Fig. 2.



mial grades will be a single grade, or, what comes to the same thing, the difference of the effect produced by the plus and minus phase of any one of the alternative elements, upon the value of the whole. The unit of the exponential curve will be $q-m$ of fig. 1, or the probable error. This latter unit is equally applicable to what we may call the binomial ogive, which is the curve drawn with a free hand through the grades. The justification for such a conception as a binomial ogive will be fully established further on. Suffice it for the present to remark that, by the adoption of a unit of this kind, the middle portion of a binomial ogive of 999 elements is compared in the figure with one of 17.

The first three of the above-mentioned conditions may occur in games of chance, but they assuredly do not occur in vital and social phenomena; nevertheless it has been found in numerous instances, where measurement was possible, that the latter conform very fairly, within the limits of ordinary statistical inquiry, to calculations based on the (exponential) law of frequency of error. It is a curious fact, which I shall endeavour to explain, that in this case a false hypothesis, which is undoubtedly a very convenient one to work upon, yields true results.

In illustration of what occurs in nature, let us consider the causes which determine the size of fruit. Some are important, the chief of which is the Aspect, whose range of influence is too wide to permit us to consider it in one of the simple alternatives "good" or "bad." It is no satisfactory argument to say that variations in aspect are *partly* due to a multitude of petty causes, such as the interposition of leaves and boughs, because, so far as they depend on well-known functions of altitude and azimuth, they cannot be reduced to a multitude of elementary causes. There has been much confusion of ideas on this subject, and also a forgetfulness of another fact—namely, that when we once arrive at a simple alternative, there our subdivision of causes must stop, and we must accept that alternative, however great may be its influence, as one of the primary elements in our calculation.

In addition to important elements, there are others of small, but still of a recognizable value, such as exposure to prevalent winds, the pedigree of the tree, the particular quality of the soil on which it stands, the accident of drains running near to its root, &c. Again, there are a multitude of smaller influences, to the second, third, and fourth orders of minuteness.

I shall proceed to define what I mean by "small;" then I shall show how this medley of causes may admit of being theoretically sorted into a moderate number of small influences of equal value, giving a first approximation to the truth; then how, by a second approximation, the grades of the binomial expansion thence derived become smoothed into a flowing curve. Lastly, I shall show by quite a different line of argument that the exponential view contains inherent contradictions when nature is appealed to, that the binomial of a moderate power is the truer one, and that we have means of ascertaining a limit which the number of its elements cannot exceed. My conclusion, so far as this source of difficulty is concerned, is that the exponential law applies because it nearly resembles the curve based on a binomial of moderate power, within the limits between which comparisons are usually made.

We observe in fig. 2 how closely the outline of an exponential ogive resembles that of a binomial of a very moderate number of elements, within the narrow limits chiefly used by statisticians. The figure expresses a series of 1000 objects marshalled according to their magnitudes. In the one case the magnitudes are supposed to be wholly due to the various combinations of 17 alternatives, and the elements of the drawing are obtained from the several terms of the expansion of $(1+1)^{17}$, all multiplied into $\frac{1000}{2^{17}}$. These form the following series, reckoning to the nearest integer; and their sum, of course, $=1000$:—0, 0, 1, 5, 18, 47, 95, 148, 186, 186, 148, 95, 47, 18, 5, 1, 0, 0. In the figure these proportions are protracted so far as possible; but the numbers even in the fourth grade are barely capable of being represented on its small scale; after the fourth, the several grades are manifest until we reach the corresponding point at the opposite end of the series. Then, with a free hand, a curve is drawn through them, which gives as their mean value 8·5, as it ought to do. Now, referring to our p and q at the 250th division from either end, I measure the value of $q-m$ (or $m-p$), which is the unit to which I must reduce any other ogive that I may desire to compare with the present one. Also I can find the values for $m+2(q-m)$ and $m+3(q-m)$, which is going as far as a figure on this small scale admits. I now protract the central portion of an exponential ogive to the same scale, horizontally and vertically. Not knowing its base, I start from its middle point, placing it arbitrarily at a convenient position in the prolongation of the m of the binomial; and I lay off, in the prolongation of p and q, points that are respectively 1 unit of probable error less and greater than m. The Tables of the law of error tell me where to lay off the other points; and so the curve is determined. It must be clearly understood that whereas in the figure both the ogive and the base are given for the binomial series of 17 elements, it is only the ogive that is given for the exponential, there being no data to determine the position of its base. The comparison is simply between the middle portions of the ogives. To speak correctly, I have not actually used the exponential Tables to draw the exponential curve, but have used Quetelet's expansion of a binomial of 999 elements, the results of which are identical, as he has shown, with those of the exponential to within extremely minute fractions, utterly insensible in a scale more than a hundred times as great as the present one.

I find the position of the various points in the two ogives, measured from the appropriate end of the base, to be as is expressed in the following Table:—

	In binomial ogive of 17 elements.	In exponential ogive, or in binomial ogive of 999 elements.
The mean	500	500
The mean ±1 unit probable error ...	250	250
" ±2 units	71	82
" ±3 units	16	17

The closeness of the resemblance is striking. It rapidly increases and extends in its range as the number of elements in the binomial increases; there need therefore be no hesitation in recognizing the fact that a binomial of, say, 30 elements or upwards is just as conformable to ordinary statistical observation as is the exponential. If one agrees, the other does, because they agree with one another.

The fewest number of elements that suffice to form a binomial having the above-mentioned conformity is a criterion of the meaning of the word "small," which was lately employed, because each of those elements would be just entitled to rank as small.

I obtain the value of any one of them in an ogive by protracting the series and noticing how many grades are included in the interval $q-m$. It will be found that in a binomial of 17 elements $q-m$ is equal to eight fifths of one grade. Thence I conclude that in any generic series an influence the range of whose mean effects in the two alternatives of above and below average is not greater than, say, one half of the probable error of the series, is entitled to be considered "small."

I now proceed to show how a medley of small and minute causes may, as a first approximation to the truth, be looked upon as an aggregate of a moderate number of "small" and *equal* influences. In doing this, we may accept without hesitation, the usual assumption that all small, and *à fortiori* all minute influences, may be dealt with as simple alternatives of excess or deficiency—the values of this excess and deficiency being the mean of all the values in each of these two phases. The way in which I propose to build up the fictitious groups may be exactly illustrated by a game of odd and even, in which it might be agreed that the *predominance* of "heads" in a throw of three fourpenny pieces, shall count the same as the simple "head" of a shilling. The three fourpenny pieces may fall all heads, 2 heads and 1 tail, 1 head and 2 tails, or all tails—the relative frequency of these events being, as is well known, 1, 3, 3, 1. But by our hypothesis we need not concern ourselves about these minute peculiarities; the question for us is simply the alternative one, are the "heads" in a majority or not? We may therefore treat a ternary system of the third order of smallness exactly as a simple alternative of the first order of smallness. Or, again, suppose a crown were our "small" unit, and we had a medley of 10 crowns, 33 shillings, and 100 fourpenny pieces, with which to make successive throws, throwing the whole number of them at once: we might theoretically sort them into fictitious groups each equivalent to a crown. There would be 29 such groups, viz.:—10 groups, each consisting of 1 crown; 6 groups, each of 5 shillings; 1 group of three shillings and 6 fourpenny pieces; 6 groups each of 15 fourpenny pieces; and a residue of 4 fourpenny pieces, which may be disregarded. Hence, on the already expressed understanding that we do not care to trouble ourselves about smaller sums than a crown, the results of the successive throws of the medley of coins would be approximately the same as those of throwing at a time 29 crowns, and would be expressed by the coefficients of a binomial of the 29th power. Hence I conclude that all miscellaneous influences of a few small and many minute kinds, may be treated for a first approximation exactly as if they consisted of a moderate number of small and equal alternatives.

The second approximation has already been alluded to; it consists in taking some account of the minute influences which we had previously agreed to ignore entirely, the effect of which is to turn the binomial grades into a binomial ogive. I effect it by drawing a curve with a free hand through the grades, which affords a better approximation to the truth than any other that can *à priori* be suggested.

I will now show from quite another point of view (1) that the exponential ogive is, on the face of it, fallacious in a vast number of cases, and (2) that we may learn what is the greatest possible number of elements in the binomial whose ogive most nearly represents the generic series we may be considering. The value of $\frac{m}{q-m}$ is directly dependent on the number of elements; hence, by knowing its value, we ought to be able to determine the number of its elements. I have calculated it for binomials of various powers, protracting and interpolating, and obtain the following very rough but sufficient results for their ogives (not *grades*):—

Number of (equal) elements.	Value of $\frac{m}{q-m}$.
17	5
32	10
65	15
107	20
145	25
186	30
999	48

Now, if we apply these results to observed facts, we shall rarely find that the series has been due to any large number of equal elements. Thus, in the stature of man, ~~the probable error~~, $\frac{m}{q-m}$, is about 30, which makes it impossible that it can be looked upon as due to the effect of more than 200 equally small elements. On consideration, however, it will appear that in certain cases the number may be *less,* even considerably less, than the tabular value, though it can never exceed it. As an illustration of the principle upon which this conclusion depends, we may consider what the value of $\frac{m}{q-m}$ would be in the case of a wall built of 17 courses of stone, each stone being 3 inches thick, and subject to a mean error in excess or deficiency of one fifth of an inch. Obviously the mean height m of the wall would be 3×17 inches; and its probable error $q-m$ would be very small, being derived from a binomial ogive of 17 elements, each of the value of only one fifth of an inch. Now we saw from our previous calculation that this would be eight fifths, or 1·6 inch, which would give the value to $\frac{m}{q-m}$ of $\frac{51}{1 \cdot 6}$, or about 321; consequently we should be greatly misled if, after finding by observation the value of that fraction, and turning to the Table and seeing there that it corresponded to more than 200 equal elements, we should conclude that that was the number of courses of stones. The Table can only be trusted to say that the number of courses certainly does not exceed that number; but it may be less than that.

The difficulty we have next to consider is that which I first mentioned, but have intentionally postponed. It is due to the presence of influences of extraordinary magnitude, as Aspect in the size of fruit. These influences must be divided into more than two phases, each differing by the same constant amount from the next one, and that difference must not be greater than exists between the opposite phases of the "small" alternatives. If we had to divide an influence into three phases, we should call them "large," "moderate," and "small;" if into four, they would be "very large," "moderately large," "moderately small," and "very small," and so on. Any objects (say, fruit) which are liable to an influence so large as to make it necessary to divide it into three phases, really consist of three series generically different which are entangled together, and ought theoretically to be separated. If there had been two influences of three phases, there would be nine such series, and so on. In short, the fruit, of which we may be considering some hundred or a few thousand specimens, ought to be looked upon as a multitude of different sorts mixed together. The proportions *inter se* of the different sorts may be accepted as constant; there is no difficulty arising from that cause. The question is, why a mixture of series radically different, should in numerous cases give results apparently identical with those of a simple series.

For simplicity's sake, let us begin with considering only one large influence, such as aspect on the size of fruit. Its extreme effect on their growth is shown by the difference in what is grown on the north and south sides of a garden-wall, which in such kinds of fruit as are produced by orchard-trees, is hardly deserving of being divided into more than three phases, "large," "moderate," and "small." Now if it so happens that the "moderate" phase occurs approximately *twice as often* as either of the extreme phases (which is an exceedingly reasonable supposition, taking into account the combined effects of azimuth, altitude, and the minor influences relating to shade from leaves &c.), then the effect of aspect will work in with the rest, just like a binomial of two elements. Generally the coefficients of $(a+b)^n$ are the same as those of $(a+b)^{n-r} \times (a+b)^r$. Now the latter factor may be replaced by any variable function the frequency and number of whose successive phases, into which it is necessary to divide it, happen to correspond with the value of the coefficients of that factor.

It will be understood from what went before, that we are in a position to bring these phases to a common measure with the rest, by the process of fictitious grouping with appropriate doses of minute influences, as already described.

On considering the influences on which such vital phenomena depend as are liable to be treated together statistically, we shall find that their mean values very commonly occur with greater frequency than their extreme ones; and it is to this cause that I ascribe the fact of large influences frequently working in together with a number of small ones without betraying their presence by any sensible disturbance of the series.

The last difficulty I shall consider, arises from the fact that the individuals which compose a statistical group are rarely affected by exactly the same number of variable influences. For this cause they ought to have been sorted into separate series. But when, as is usually the case, the various intruding series are weak in numbers, and when the number of variable influences on which they depend does not differ much from that of the main series, their effect is almost insensible. I have tried how the figures would run in many supposititious cases; here is one taken at haphazard, in which I compare an ordinary series due to 10 alternatives, giving $2^{10}=1024$ events, with a compound series.

The latter also comprises 1024 events; but it is made up of three parts: viz. nine tenths of it are due to a 10-element series; and of the remaining tenth, half are due to a 9 and half to an 11 series. I have reduced all these to the proper ratios, ignoring fractions. It will be observed how close is the correspondence between the compound and the simple series.

Total cases.	Number of elements.	Successive grades in the series.											
		1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.
52	9	0	1	4	8	13	13	8	4	1	0	0	0
924	10	1	9	41	108	189	227	189	108	41	9	1	0
48	11	0	0	1	4	8	11	11	8	4	1	0	0
1024	Compound series.	1	10	46	120	210	251	208	120	46	10	1	0
1024	10	1	10	45	120	210	252	210	120	45	10	1	0
	Difference	0	0	+1	0	0	−1	−2	0	+1	0	0	0

It appears to me, from the consideration of many series, that the want of symmetry commonly observed in the statistics of vital phenomena is mainly due to the inclusion of small series of the above character, formed by alien elements; also that the disproportionate number of extreme cases, as of giants, is due to this cause.

The general conclusion we are justified in drawing appears to be, that, while each statistical series must be judged according to its peculiarities, a law of frequency of error founded on a binomial ogive is much more likely to be approximately true of it than any other that can be specified *à priori*; also that the exponential law is so closely alike in its results to those derived from the binomial ogive, under the circumstances and within the limits between which statisticians are concerned, that it may safely be used as hitherto, its many well-known properties being very convenient in all cases where it is approximately true. Therefore, if we adopt any uniform system (such as already suggested) of denoting the magnitudes of qualities for the measurement of which no scale of equal parts exists, such system may reasonably be based on an *inverse* application of the law of frequency of error, in the way I have described, to statistical series obtained by the process of *intercomparison.*

f. 295v

f. 296av

The Galton Laboratory,
University College London,
Wolfson House,
4, Stephenson Way, London, N.W.1.

f. 296br

ANTECEDENTS OF SCIENTIFIC MEN.

To

42, Rutland Gate, London.

In the pursuit of an inquiry parallel to that by M. de Candolle, I have been engaged for some time past in collecting information on the Antecedents of Eminent Men. My present object is to set forth the influences through which the dispositions of Original Workers in Science have most commonly been formed, and have afterwards been trained and confirmed. As a ready means of directing attention to the importance and interest of this inquiry, I append, overleaf, a reprint of a short review of the work of M. de Candolle, which I contributed to the 'Fortnightly Review' of March, 1873.

The result of my past efforts has clearly impressed upon me the fact that a sufficiency of data cannot be obtained from biographies without extreme labour, if at all; therefore, instead of imperfectly analysing the past, it seems far preferable to deal with contemporary instances, and none are more likely to appreciate the inquiry or to give correct information than Men of Science.

The number of persons in the United Kingdom who have filled positions of acknowledged rank in the scientific world, is quite large enough for statistical treatment. Thus, the Medallists of the chief scientific societies; the Presidents of the same, now and in former years; those who have been elected to serve at various times on the Council of the Royal Society, and similarly, the Presidents of the several sections of the British Association, form a body of little less than two hundred men, now living, a considerable portion of whom stand in more than one of the above categories. Other methods of selection give fifty or a hundred additional names.

Falling as you do within the range of this inquiry, may I ask of you the favour of furnishing me with information? If you should desire any portions of what you may send to be considered as private, they will be used in no other way than to afford material for general conclusions.

I send herewith a schedule, which contains the questions to which I am seeking replies.

Francis Galton.

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From the Fortnightly Review, *March* 1, 1873.

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ON THE CAUSES WHICH OPERATE TO CREATE SCIENTIFIC MEN.

By Francis Galton, F.R.S.

On more than one occasion I have maintained that intellectual ability is transmitted by inheritance; and in a memoir published last year in the "Proceedings of the Royal Society," I endeavoured to explain what ought to be understood by that word "inheritance." Two points were especially urged—the first, that each personality originates in a small selection out of a large batch of wonderfully varied elements, which were all latent and competing; and secondly, that these batches, and not the persons derived from them, form the principal successive stages in the line of direct descent. Hence follows the paradoxical conclusion, that, however much the child may resemble his parents, he must not be looked upon as directly descended from them. His true relation to them is both circuitous and complicated, but admits of being easily expressed by an illustration. Suppose an independent nation, A, to have been formed by colonists from two other similarly constituted nations, B and C; then the relation borne by the *representative government* of A to that of B and of C is approximately similar to what I suppose to be the relation of a child to each of his parents. But the existence of a slender strain of direct descent is shown by the fact of acquired habits being occasionally transmitted. We must therefore amend our simile by supposing the members of the governments of B and C to have the privilege of making emigration easy and profitable to their constituents, and also, perhaps, the governments themselves to have the power of nominating a few individuals to seats in the Legislative Council of A.

It appears to me of the highest importance, in discussing heredity, to bear the character of this devious and imperfect connection distinctly in mind. It shows what results we may and may not expect. For instance, if B and C contain a large variety of social elements, it would be impossible, without a very accurate knowledge of them and of the conditions of selection, to predict the characters of their future governments. Still less would it be possible to predict that of A. But if the social elements of B and C were alike, and in each case simple, such as might be found in pastoral tribes, then the character of their governments and that of A could be predicted with some certainty. The former supposition illustrates what must occur when the breed of the parents is mongrel; the latter, when it is pure. Now, no wild or domestic animal is so mongrel as man, especially as regards his mental faculties; therefore, we cannot expect to find an invariable resemblance between the faculties of children and those of their parents. All that could be expected on the hypothesis of strict inheritance we do find: that is, occasional startling resemblances, and much more frequently partial ones. From this we have a right to argue that if the breed of men were more pure, the intellectual resemblance of child to parent would be as strict as in the forms of the equally pure breeds of our domestic animals.

I propose to refer in this article to a volume written by M. de Candolle,[1] son of the late famous botanist, and himself a botanist and scientific man of high reputation, in which my name is frequently referred to and used as a foil to set off his own conclusions. The author maintains that minute intellectual peculiarities do not go by descent, and that I have overstated the influence of heredity, since social causes, which he analyses in a most instructive manner, are much more important. This may or may not be the case; but I am anxious to point out that the author contradicts himself, and that expressions continually escape from his pen at variance with his general conclusions. Thus he allows (p. 195) that in the production of men of the highest scientific rank, the influence of race is superior to all others ("prime les autres en importance"); that (p. 268) there is a yet greater difference between families of the same race than between the races themselves; and that (p. 326) since most, and probably all, mental qualities are connected with structure, and as the latter is certainly inherited, the former must be so as well. Consequently I propose to consider M. de Candolle as having been my ally against his will, notwithstanding all he may have said to the contrary.

The most valuable part of his investigation is this: What are the social conditions most likely to produce scientific investigators, irrespective of natural ability, and, *à fortiori*, irrespective of theories of heredity? This is, necessarily, a one-sided inquiry, just as an inquiry would be that treated of natural gifts alone. But for all that, it admits of being complete in itself, because it is based on statistics which afford well-known means of disentangling the effect of one out of many groups of contemporaneous influences. The author, however, continually trespasses on hereditary questions, without, as it appears to me, any adequate basis of fact, since he has collected next to nothing about the relatives of the people upon whom all his statistics are founded. The book is also so unfortunately deficient in method, that the author's views on any point have to be sought for in passages variously scattered; but it is full of original and suggestive ideas, which deserve to have been somewhat more precisely thought out and much more compendiously stated.

Its scheme is to analyse the conditions of social and political life under which the principal men of science were severally living at the four epochs 1750, 1789, 1829, and 1869. The list of names upon which he depends is that of the foreign members of the three great scientific societies of Europe—namely, the French Academy, the Royal Society, and the Academy of Berlin—in each case about fifty in number. There is a yet stricter selection on the part of the foreign associates of the French Academy, who number only eight at a time, and of whom there have been only ninety-

[1] Histoire des Sciences et des Savants depuis deux Siècles. Par Alphonse de Candolle (Membre Corr. de l'Acad. Sciences, Paris; Foreign Member, Royal Soc. etc.). Geneva, 1873.

two in the last two hundred years. It is remarkable that we find in this very select list four cases of father and son—namely, a Bernoulli and two of his sons, the two Eulers, and the two Herschels.

From an examination of these lists the author draws a large variety of interesting deductions. He traces the nationalities and the geographical distribution of the distinguished men of science, and compares the social conditions under which they lived. He finds them to be confined to a triangular slice of Europe, of which Middle Italy forms the blunt apex, and a line connecting Sweden and Scotland forms the base; and then he shows that out of a list of eighteen different influences favourable to science, such as liberty of publication, tolerant church and temperate climate, a large majority were found in the triangular space in question, and there alone. The different nations vary at the different epochs in their scientific productiveness;[1] and he elaborately shows how closely the variation depends on a certain proportion of the eighteen influences becoming favourable or unfavourable. The author, himself descended from the Huguenots, lays just stress on the influence of religious refugees, whose traditions were to work in a disinterested way for the public good, and at the same time to avoid politics. The refugees rarely had their property in land, of which the oversight occupies time, but in moveable securities; thus they had leisure for work. Then, again, as they were debarred from local politics, the ambition, especially of those who had taken refuge in small countries, was to earn the approval of the enlightened men all over Europe, and this could most easily be effected by doing good work in science. Out of the ninety-two foreign associates of the French Academy, no less than ten were descended from religious refugees, usually in the third or fourth generation. Switzerland had eight out of the ten, and we may thence easily gather how enormously she is indebted to the infusion of immigrant blood. Similarly, the only two American associates—Franklin and Rumford—were descended from Puritans.

The blighting effect of dogmatism upon scientific investigation is shown both in Catholic and Protestant countries. The Catholics are the more dogmatic of the two, and they supply, in proportion to their population, less than one quarter as many of the foremost scientific men as the Protestants. There is not a single English or Irish Catholic among the ninety-two French foreign associates. Austria contributes no name, and the rest of Catholic Germany is almost barren. In Switzerland, the scientific productiveness of the Catholics is only 1–26th that of the Protestants. Again, the Catholic missionaries have done nothing for science, notwithstanding their splendid opportunities. In past days, when they were absolute masters of vast countries, as Paraguay and the Philippines, the smallest encouragement and instruction given at the college of the Propaganda to young and apt missionaries would have enriched Rome with collections of natural history. If any city more than others deserved to have the finest botanical garden and richest herbarium, it is Rome; but she has almost nothing to show.

[1] The author's tables of the scientific productiveness per million, of different nations at different times, are affected by a serious statistical error. He should have reckoned per million of men above fifty, instead of the population generally. In a rapidly increasing country like England, the proportion of the youthful population to those of an age sufficient to enable them to become distinguished, is double what it is in France, where population is stationary; and injustice may be done by these tables to England in something like that proportion. They require entire reconstruction.

The most notable instance of the repressive force of Protestant dogmatism is to be found in the history of the republic of Geneva. During nearly 200 years (1535 to 1725) its laity as well as clergy were absolutely subject to the principles of the early Reformers. Instruction was imposed on them; nearly every citizen was made to pass through the college, and many attended special courses at the Academy, yet during the whole of that period not a single Genevese distinguished himself in science. Then occurred the wane of the Calvinist authority, between 1720 and 1735. Social life and education became penetrated with liberal ideas;[1] and since 1739, the date of the first election of a Genevese to an important foreign scientific society—our own Royal Society—Geneva has never ceased to produce mathematicians, physicists, and naturalists, in a number wholly out of proportion to her small population.

The author argues from these and similar cases that it is not so much the character of the dogma taught that is blighting to science as the dogmatic habit in education. It is the evil custom of continually telling young people that it is improper to occupy their minds about such and such things, and to be curious, that makes them timid and indifferent. Curiosity about realities, not about fictions of the imagination, is the motive power of scientific discovery, and it must be backed up by a frank and fearless spirit. M. de Candolle, in spite of his anti-heredity declarations, enunciates an advanced pro-heredity opinion well worthy of note. He says, it is known that birds originally tame, when found on a desolate island, soon acquire a fear of man, and transmit that fear as an instinctive habit to their descendants. Hence we might expect a population reared for many generations under a dogmatic creed to become congenitally indisposed to look truth in the face, and to be timid in intellectual inquiry.

Can, then, religion and science march in harmony? It is true that their methods are very different; the religious man is attached by his heart to his religion, and cannot endure to hear its truth discussed, and he fears scientific discoveries which might in some slight way discredit what he holds more important than all the rest. The scientific man seeks truth regardless of consequences; he balances probabilities, and inclines temporarily to that opinion which has most probabilities in its favour, ready to abandon it the moment the balance shifts, and the evidence in favour of a new hypothesis may prevail. These, indeed, are radical differences, but the two characters have one powerful element in common. Neither the religious nor the scientific man will consent to sacrifice his opinions to material gain, to political ends, nor to pleasure. Both agree in the love of intellectual pursuits, and in the practice of a simple, regular, and laborious life, and both work in a disinterested way for the public good. A strong evidence of this fundamental agreement is found in the number of sons of clergymen who have distinguished themselves as scientific investigators it

[1] In 1735, public opinion had become so tolerant that it was enacted that candidates for the ministry should no longer be required to make a declaration of faith, but simply to promise to teach and preach conformably to the Bible and to the light of their own consciences (p. 204).

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is so large that we must deplore the void in the ranks of science caused by the celibacy of the Catholic clergy. If Protestant ministers, like them, had never married, Berzelius, Euler, Linnæus, and Wollaston would never have been born. But to revert to what we were speaking about. There are some six different objects in the pursuit of which most men spend their energies; three of them refer to self—namely, property, pleasure, and political advancement; the other three imply devotion to ideas—namely, religion, science, and art. Without a doubt, as M. de Candolle says, the former three occupy one half of the moral sphere of the human character, and the latter three the other.

It appears that the men distinguished in science have usually been born in small towns, and educated by imperfect teachers, who made the boys think for themselves. Nothing is brought out more clearly in the work than that the first desideratum in scientific education is to stimulate curiosity and the observation of real things, and that too much encouragement of the receptive faculty is a serious error. The author justly laments that the art of observation is not only untaught, but is actually discouraged by modern education. Children are apt and eager to observe, but, instead of encouraging and regulating their instinct, the schoolmasters keep them occupied solely on internal ideas, such as grammar, the vocabularies of different languages, arithmetic, history, and poetry. They learn about the living world which surrounds them out of books, and not through their own eyes. One of the reformations he proposes, is to make much more use of drawing as a means of careful observation, compelling the pupils to draw quickly the object they have to describe, from memory, after a short period allowed for its examination. He is a strong advocate for the encouragement of a class of scientific sinecurists like the non-working fellows of our colleges, who should have leisure to investigate, and not be pestered by the petty mechanical work of continual teaching and examining. Science has lost much by the suppression of the ecclesiastical sinecures at the time of the French Revolution, for there used to be many abbés on the lists of foreign scientific members, but they have now almost wholly disappeared. The modern ideas of democracy are adverse to places to which definite work is not attached, and from which definite results do not regularly flow. This principle is a wise one for the mass of mankind; but how utterly misplaced when applied to those who have the zeal for investigation, and who work best when left quite alone!

There is a curious chapter on the probability of English becoming the dominant language of the world in fifty or a hundred years, and being the one into which the more important scientific publications of all nations will, as a matter of course, be translated. It is not only that the English-speaking population will outnumber the German and the French, as these now outnumber the Dutch and the Swedish, but that the language has peculiar merits, through its relationship with both the Latin and the Teutonic tongues. It also seems that in families where German and French are originally spoken, French always drives out the German on account of its superior brevity. When people are in a hurry, and want to say something quickly, it is more easily said in French than in German. Precisely in the same way English beats French. Our sentences don't even require to be finished in order to be understood, because the leading ideas come out first; but as for old-fashioned tongues, their roundabout construction would be perfectly intolerable. Fancy languages like Latin and Greek, in which people did not say "yes" or "no." M. de Candolle is very disrespectful to classical Latin. He says that one must have gone through the schools not to be impressed by its ridiculous construction. Translate an ode of Horace literally to an unlettered artisan, keeping each word in its place, and it will produce the effect upon him of a building in which the hall-door was up in the third storey. It is no longer a possible language, even in poetry.

I have only space for one more of the many subjects touched upon in his book—that of acquired habits being transmitted hereditarily—and which has also formed the subject of a recent essay by Dr. Carpenter. That some acquired habits in dogs are transmitted appears certain, but the number is very small, and we have no idea of the cause of their limitation. With man they are fewer still; indeed it is difficult to point out any one to the acceptance of which some objection may not be offered. Both M. de Candolle and Dr. Carpenter have spoken of the idiocy and other forms of nervous disorder which beyond all doubt afflict the children of drunkards. Here, then, appears an instance based on thousands of observations at lunatic asylums and elsewhere, in which an acquired habit of drunkenness, which ruins the will and nerves of the parent, appears to be transmitted hereditarily to the child. For my own part, I hesitate in drawing this conclusion, because there is a simpler reason. The fluids in an habitual drunkard's body, and all the secretions, are tainted with alcohol; consequently the unborn child of such a woman must be an habitual drunkard also. The unfortunate infant takes its dram by diffusion, and is compulsorily intoxicated from its earliest existence. What wonder that its constitution is ruined, and that it is born with unstrung nerves, or idiotic, or insane? And just the same influence might be expected to poison the reproductive elements of either sex. I am also informed, but have not yet such data as I could wish, that the children of recent teetotallers who were formerly drunkards are born healthy. If this be really the case, it seems to settle the question, and to show that we must not rely upon the above-mentioned facts as evidence of a once acquired habit being hereditarily transmitted.

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INQUIRY INTO THE ANTECEDENTS OF SCIENTIFIC MEN.

From FRANCIS GALTON,
42 *Rutland Gate*,
London, S.W.

To ____________________

PLEASE return this Schedule at your earliest convenience, with answers to as many of the questions as you consider to be unobjectionable, and send on a separate paper any further information that you may think germane to the inquiry.

Entries marked "Private" will be dealt with in *strict confidence*; they will be used only as data for general statistical conclusions.

General Remarks on filling up the Schedule.

Qualifications to which no strictly defined meaning is attached, such as "large," "considerable," necessarily convey different ideas to different persons, and are unfit to be used statistically. I therefore beg to recommend in their place a notation of Classes which I proposed, justified, and employed in my 'Hereditary Genius,' and which is explained in the following Table.

The successive graduations divide the Classes and start from Mediocrity as a zero point, and they proceed both upwards and downwards. They are calculated to show equal increments or decrements of the quality in question. The Classes fill the intervals of, and are bounded by, the successive graduations.

The amount of the quality in question, indicated by each graduation, is that which exists at the limit which divides a select body of individuals from the residue of the population out of whom they have been selected, on the ground of their possessing that quality in an exceptional degree, either of excess or deficiency. The population is understood to be limited to individuals of the same sex, age, and circumstances, and the severity of selection to be as is stated in the first column of the Table.

STANDARD SCALE OF CLASSES APPLICABLE TO ANY QUALITY OR GROUP OF QUALITIES, MENTAL OR PHYSICAL; FOUNDED ON THE WELL-KNOWN LAW OF DEVIATION FROM STATISTICAL AVERAGES.

Severity of the Selection, in round Numbers. (1 to 1 of the population means no selection at all.)	Graduations which divide the Classes.	Proposed Notation for the several Classes.		Number per Million in each Class.
		Above the Average.	Below the Average.	
1 to 1 of the population	0			
		A	a	256,791
" 4 " "	I.			
		B	b	161,279
" 12 " "	II.			
		C	c	63,563
" 50 " "	III.			
		D	d	15,696
" 350 " "	IV.			
		E	e	2,423
" 4,000 " "	V.			
		F	f	233
" 70,000 " "	VI.			
		G	g	14
" 1,000,000 " "	VII.			
		X stands for all classes above G	x stands for all classes below g	1
				500,000 on each side of Mediocrity. 1,000,000; sum of numbers on both sides.

NOTE.—Whenever you consider the grade of the quality, about which a question is asked, to fall so near Mediocrity as to lie between C and c, *do not make any entry at all.*

(Note.—Attend to the paragraph at bottom of the first page.)	YOURSELF?
Christian name and surname of Yourself, of your Father, and of your Mother?	
Designation and principal scientific titles of	
Your Father and your Mother, are they respectively English, Welsh, Scotch, Irish, Jewish, or Foreign? If Foreign, of what country?	(Make no entry in this space.)
" " " Wholly, or in what degree?	(Make no entry in this space.)
Was either your Father or your Mother descended from persons persecuted for political or religious opinions, or from political or religious refugees? If so, state the precise relationship. Mention whether their political or religious opinions became traditional in the family.	(Make reply in this space to the last paragraph only of the question.)
Date of the birth of	
Place of the birth of:—(if you do not remember that of either your father or mother, state where he or she resided in early life) Mention if it was in a large or small town, a suburb, a village, or a house in the country.	
Occupation or profession of	
Specify any interests that have been very actively pursued (in addition to the above mentioned regular occupation or profession) by	
To what religious body or bodies have You, your Father, and your Mother respectively belonged?	
To what political party or parties have You, your Father, and your Mother, or her family, respectively belonged?	
Health at the various periods of life of	
In early adult life: Height? (to be estimated, if not accurately remembered.) Anything remarkable in the figure, &c.? (spare, symmetrical, muscular, &c.)	Height of Yourself? — Figure, &c., of Yourself? — Measurement round inside of your hat?
In early adult life: Colour of hair? Complexion? (if remarkably fair, dark, ruddy, pale, sallow, &c.)	Colour of Hair? — Complexion?
Temperament? if distinctly nervous, sanguine, bilious, or lymphatic	

YOUR FATHER?	YOUR MOTHER?
	Enter here your mother's maiden name.
	The entry here is to refer to the father of your mother.
	The entry here is to refer to the father of your mother.

Height of your Father?	Figure, &c., of your Father?	Height of your Mother?	Figure, &c., of your Mother?
Colour of Hair?	**Complexion?**	**Colour of Hair?**	**Complexion?**

(Note.—Attend to the paragraph at bottom of the first page.)	YOURSELF?
Energy of body, if remarkable; as shown by activity, power of enduring fatigue; restlessness; requiring but little sleep (how much?); early rising; adventurous travel; mountaineering, &c.? Give a few facts referring to	
Energy of mind, if remarkable; as shown by power of accomplishing a large amount of brain-work; by the vigorous pursuit of interests, whatever they may be, &c.? Give a few facts referring to	
Retentiveness of memory? Give facts referring to	
Studiousness of disposition and mental receptivity, as shown by large acquirements? . .	
Independence of judgment in social, political, or religious matters? Give illustrations . .	
Originality or eccentricity of character? Give illustrations	
Special talents; as for mechanism, practical business habits, music, mathematics, &c.? . .	
Strongly marked mental peculiarities, bearing on scientific success, and not specified above? The following list may serve to suggest:—Impulsiveness or steadiness; strong feelings and partisanship; social affections; religious bias of thought; love of the new and marvellous; curiosity about facts; love of pursuit; constructiveness of imagination; foresight; public spirit; disinterestedness	
	Yourself, your Brothers and Sisters?
Are any peculiarities either very uniformly developed, or else very irregularly developed, among	

YOUR FATHER?	YOUR MOTHER?
THE FAMILY OF YOUR FATHER?	THE FAMILY OF YOUR MOTHER?

Degrees of Relationship.		State the Number of your Relatives in each of these Degrees who have attained 30 years of age, or thereabouts. Males.	Females.	Names of Relatives in each Degree who have occupied prominent Positions or written well-known Works, or who, from any other cause, may be considered as Public Characters.	State their principal Achievements.
Grandparents:	The Father and the Mother of your Father.	1	1		
	The Father and the Mother of your Mother.	1	1		
Parents:	Your Father and your Mother.	1	1		
Uncles and Aunts:	The Brothers and the Sisters of your Father.				
	The Brothers and the Sisters of your Mother.				
	Your own Brothers and Sisters.				
First Cousins:	The Sons and the Daughters of the Brothers of your Father.				
	The Sons and the Daughters of the Sisters of your Father.				
	The Sons and the Daughters of the Brothers of your Mother.				
	The Sons and the Daughters of the Sisters of your Mother.				
Nephews and Nieces:	The Sons and the Daughters of your Brothers.				
	The Sons and the Daughters of your Sisters.				
In other more remote degrees of relationship:	Specify the Degree of Relationship.				

Mention the best Biographies, and the most useful among the scattered Biographical Notices, which may exist of them; terms of Award of Medals, &c.	Number (and Initials or Names) of Relatives in each Degree, whose Ability in *any respect* was considerable (not less than D in my Schedule), but who did not become Public Characters. (Fuller information to be sent in a separate paper.)			
	Males.		Females.	
	Number.	Initials of each.	Number.	Initials of each.

Brief notes concerning hereditary peculiarities of any kind in your family, bodily or mental, would be acceptable.

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	Name or Place of School or University?	From Age of	To Age of	What were the chief Subjects taught to you there?
How long were you at small schools?				
At large schools?				
At universities?				

Mention any honours of importance gained by you at schools or universities?

	older than yourself?	younger than yourself?
How many brothers had you		
How many sisters had you		

To what extent were you educated elsewhere, taught at home, or self-taught?

Was your education especially conducive to or restrictive of habits of observation?

Was your education eminently conducive to health, or the reverse?

What do you consider to have been peculiar merits in your education?

What where the chief omissions in it, and what faults of commission can you indicate?

Has the religious creed taught in your youth had any deterrent effect on the freedom of your researches?

Can you trace the origin of your interest in science in general, and in your particular branch of it?

How far do your scientific tastes appear to have been innate?

Were they largely determined by events occurring after you reached manhood? and by what events?

DESCENDANTS.

				Number at *all ages* of living Sons.	Daughters.
Have you been married?		Maiden name of your wife?			
Year in which you were married?		State any facts of peculiar hereditary interest in her family.			

relation to his subjects as the ancient Pharaohs, and may, like them, be able to coerce a patient, if hardly a docile, people into modes of action highly repugnant to their natural inclinations. Mr. Taylor's chapters originally appeared as correspondence in the *New York Tribune*, which excuses the slightness and sketchiness of treatment. Publication under such circumstances is usually much in need of excuse, amply afforded here by the writer's eye for colour and heart for human interests. The latter is conspicuous in the letters from Iceland, whither the author proceeded in company with Mr. Gladstone, jun., Mr. E. Magnussen, and other distinguished fellow-travellers, on the great twofold occasion of the millennial anniversary of the discovery of the island, and of the proclamation of a free Constitution. Mr. Taylor has the happiest knack and the sincerest pleasure in discovering native talent in out-of-the-way nooks and corners, and his sketches of such interest us more than his descriptions of public festivities, or even of natural scenery, where he is somewhat at a loss for his colour-box.

It is gratifying to learn that, in the opinion of so observant a cosmopolitan, the English have made almost as much progress as the Egyptians :—

> Although it is only six years since I last saw London, the mighty capital has changed quite as much as New York is accustomed to do in the same space of time. Certainly, under a clear summer sun, with so little coal-smoke that the dome of St. Paul's can be seen six miles away, with new thoroughfares cut through the narrow and tangled old alleys, and gay suburbs planted wherever you remember a field or common, the city seems to have become a soberer Paris. The embankment along the Thames, with its spacious drive, its trees and gardens, is an astonishing embellishment; but in all other quarters a similar work is going on—a more cheerful style of architecture, greater use of colour and ornament, ampler space and air, more abundant signs of a cosmopolitan diversity of taste and habit.
>
> A kindred change is slowly creeping upon the people. The Englishman (if not more than sixty years old) is decidedly a mellower and more sympathetic creature than he was twenty years ago. My experience during the past two years on the Continent indicates that it is rather easier to become acquainted with English than American travellers. Outside of a certain range of conventionalities (constantly growing smaller), the former are generally very free, cordial, and companionable.

Miss Kate Field is another agreeable American traveller, shrewd, saucy, and wayward, who brings the characteristics of her country and her sex to bear smartly upon the mouldering magnificence and tattered pomp of Spain. She is kindly but irreverent, and gives the impression, on the whole, of a female Mark Tapley, managing to extract joviality from the most dismal of countries and the most inefficient of guides, to whom the narrative is dedicated "in sweet revenge." It must be borne in mind that her visit was principally to the decayed cities of Castile, and that she saw nothing of the flourishing seats of commerce in the south and east. This may also have been the reason why she failed to discover any Republicans, unless any one is minded to believe that they had all gone off to fight the Carlists. It is some consolation to be informed that neither could she discover any trace of any other political party: an interminable series of sterile political agitations has apparently extinguished all interest in public life, except among the classes for whose benefit *pronunciamientos* are made. Miss Field's little book is beautifully printed and cleverly illustrated.

Reversing the method of his celebrated letters to the *Telegraph*, Mr. D. Ker has this time "endeavoured to group real scenes around an imaginary hero." His former labours having by general consent been assigned to the department of fiction, it seems but just to place the present work among travels. Though in the form of a story, it evinces ample knowledge of scenery and people, is very entertaining, and might be unreservedly recommended to young readers but for the scenes of blood and horror with which, too faithful this time to *couleur locale*, the author has seen fit to crowd his pages.

It can hardly be necessary to state that Captain Butler's 'Wild North Land' is one of the most picturesque of books of travel, as the public have evinced their recognition of the fact by calling for a fifth edition. Few works of its class, indeed, are better adapted for universal popularity than this frank, soldierly, unpretentious, yet powerfully-written narrative of the lonely yet cheery journey of a bold man and a faithful dog.

ALL IN ALL.

All in All, and other Poems. By Philip Bourke Marston.

Mr. Philip Bourke Marston's first volume, 'Songtide and other Poems,' contained so much that was good in thought and expression, so much that was well and pathetically told in "gentle music" and "tender harmonies," that the appearance of a second work by the same author could not fail to arouse a feeling of keen interest and pleasure. It is, however, with a sense of disappointment that we lay down his new collection of poems, for, as compared with his former work, it shows a decided falling off in purpose and power. We say a falling off in purpose, though the contents of the present volume are connected by a main idea —the depicting of one of love's various aspects—because the leading thought contained in most of the poems is less apparent than formerly; and a falling off in power, because the expression, though still sweet and fluent, is rarely, if ever, as forcible as in some of Mr. Marston's earlier work. This is more especially noticeable in the longer poems, where we look in vain for anything as truly passionate and mournful as the prelude to "Songtide," or as clearly and musically told as "Shake Hands and Go." If we except "The Season's Associations"—a most tuneful and lovely little poem in four verses—we must turn to the sonnets for aught as complete as Mr. Marston's first book gave us a right to expect. And here again we would question whether the unvarying theme on which he dwells has not become merely a literary cultus with the poet, rather than a source of constant and genuine inspiration, as we sometimes see a painter treat one class of subjects in one way, until the first living impulse degenerates into weakness and mannerism. We do not quarrel with Mr. Marston for those repeated minor tones which, under his skilled fingers, so often ring out full of plaintive harmony, for he himself has warned us that this volume would be a sombre one, nor is he, indeed, the first poet who has told us that we must look for few sunny pictures in his book, since in life such pictures are few also; but we deprecate the possibility of his growing monotonous over his cherished theme, and following out his love-thoughts, even when they become attenuated and thin. There is surely a want of direct purpose in such sonnets as "Love's Answer," "Dreamless Life," the "Fifth Prelude," "Rebuked of Love." We feel certain that Mr. Marston, with his facile verse and his choice of one subject, might write such indefinitely to the detriment of his poetical reputation, and to the disappointment of those who appreciate his undoubted gift of song. This tendency is the more to be deplored as he is truest to himself when he expresses a tangible thought clearly and tenderly; his individual style cannot bear the lack of what we have already called a direct purpose, for if he does not overload his verse with colour and ornament like some of his young contemporaries, but writes with almost unvarying refinement and sobriety of taste, he has not, on the other hand, that richness of imagery and power of language which may take the place of a more distinct aim, as some gorgeous brocade wrought with flower and arabesque will stand alone without the help of a wearer.

The sonnet, about which much has lately been written, is decidedly Mr. Marston's favourite form of expression, and in it he has achieved much success. The series contained in 'All in All' is less equal than the two series first published; and here we notice that the want of a direct purpose is often made up for by a closing couplet of an "epigrammatical" nature, which quite destroys the beauty of the sonnet form. The legitimacy of the closing couplet is still a vexed question, and Mr. Marston has used it in such fashion that he has at once justified all that has been said in dispraise of it, and vindicated, like other sonnet-writers before him, the legitimacy of a form which can produce such exquisite results. The thought contained in a sonnet should have one smooth and even crescendo of expression, like the gradual swell of an uninterrupted sound; when this rule has been observed, the closing couplet only adds to the choiceness and music of the whole. But when the thought and expression are undecided and spasmodic, the final couplet makes a distinct point at once, and is undoubtedly a blemish and a weakness. The two following sonnets will show what good and bad use Mr. Marston can make of this form, and will justify a sense of irritation at any falling-off in a poet who can produce such work as the first, and a feeling of disappointment when we come across the second and others like it:—

TOO NEAR.

So close we are, and yet so far apart,
So close, I feel your breath upon my cheek;
So far, that all this love of mine is weak
To touch in any way your distant heart;
So close, that, when I hear your voice, I start
To see my whole life standing bare and bleak;
So far, that though for years and years I seek,
I shall not find thee other than thou art;
So, while I live, I walk upon the verge
Of an impassable and changeless sea,
Which more than death divides me, love, from thee:
The mournful beating of its leaden surge
Is all the music now that I shall hear;
O, love! thou art too far, and yet too near.

VAIN DREAMS.

I and my love are parted; many days,
Sad days must be before we meet again;
But surely we shall meet, and all the pain
Of separation die as we embrace;
When on her bosom lies again my face,
And lips dissevered reunite and strain
Together in a kiss that shall enchain
Our souls too much for any speech of praise.
And when at length we speak, I think I know
Of what our speech shall be. Oh, vain my soul!
Put by these dreams, take up thy load and go;
Each lot, however bitter, hath its goal.
Thy goal is death, not life, and when life ends
The night that hides thy love, on thee descends.

Why should Mr. Marston write such things as this latter sonnet, when he can give us so much that is worthier of his graceful and decided gifts, such as "Love's Quest," "Dead Joys," "Desolate Love," "The Stranger," "Divine Possibility," and other sonnets we could name, which are really beautiful and satisfactory?

Mr. Marston has promised us yet another series of love-poems, to be called "The Pilgrimage;" and though we would not presume to ask, like his dream lady—

"can you sing no song to prove
The bliss as well as sorrow of great love?"

yet we cannot help hoping that in this further development of his theme the poet's tone will gain instead of losing in intensity, and that the ease and tenderness of his expression will no longer be marred by even occasional carelessness of versification. Mr. Marston is so genuine and graceful a poet that the perfecting of his special gifts is a matter of real moment to all lovers of poetry.

HEREDITARY SCIENTIFIC CAPACITY.

English Men of Science: Their Nature and Nurture. By Francis Galton, F.R.S. London: Macmillan and Co.

The position of the scientific class in England is so peculiar at the present time, and, we may add also, so precarious, that an inquiry into what Mr. Galton calls their natural history, or the conditions favourable to them, is bound to excite a wide-spread interest. The study is also interesting in itself. The English nation is distinguished as having produced, in former generations and in the present, a very large proportion of the master-spirits of the scientific world, and, as a nation of the first rank, it is specially distinguished at present for the meagre contingent that it supplies to the rank-and-file of scientific workers. In a study of the scientific aptitude one would follow with interest how the national characteristics of energy and love of freedom act as determining causes, and how the no less characteristic wealth of the nation acts as a controlling cause. We might, in fact, have had a rigorous handling of Wordsworth's rhetorical statement that high thinking has disappeared along with plain living, and of the many more colloquial statements, as, for example, that we have no love for things of the mind, that we are continually driving at practice, or of the more startling statement that it is better in England for a man's worldly prospects to be a drunkard than to be smitten with the divine dipsomania of the original investigator. Such a systematic inquiry would have shown us how far, in a country of great material resources, with the principle of individual liberty or self-government in its institutions, the study of science for its own sake was likely to flourish. Adopting the "struggle for existence" way of speaking, we might have learned whether in England, at the present day, the scientific aptitude is or is not one of those modes of energy in the individual that tend to be suppressed, and what are the modifying circumstances under which the faculty for research is not overborne. There are very definite facts to indicate that some such way of treating the subject of English men of science would not have been amiss. On the one hand we have the salient fact that English science is largely bound up with the trade of the country, as in metallurgy, applied chemistry, engineering, geographical exploration, and the like; and, on the other hand, we find that, where scientific investigations are not stimulated by their direct bearing on practice, they are mostly carried on by a class of wealthy amateurs, whose scientific work is of the first importance, and who, in a great measure, sustain the reputation of English science abroad. This class is sufficiently characteristic of the nation to have attracted the attention of foreigners. In the eulogy lately pronounced on the English-bred Montalembert by his successor at the French Academy, it formed a principal topic of discourse. The Germans, again, are constantly making the mistake of supposing Mr. Darwin to be a professor; and when they learn that our greatest man of science is, as regards position, a country gentleman, they come to see that England has *savants* that have no counterpart in Germany, while, at the same time, their own learned class is poorly represented in England.

Mr. Galton has narrowed the range of his inquiry very greatly within the limits that one conceives to be adequate to the case. Apparently a firm believer in the statistical method, he has not gone farther than statistics can carry him; and we are bound to add that he has not always used his favourite method to the best advantage. From the 500 or more Fellows of the Royal Society, he has selected 180, by means of various tests, some of which are, for his purpose, plainly faulty. To the selected 180 he addressed a circular letter of questions. Many of the questions had reference to parentage, and were meant to test the doctrine of heredity; others referred to personal qualities, physical, intellectual, and moral; a third set were concerned with the tendencies shown in early life; and the remainder referred to educational opportunities. Mr. Galton got replies from only 100 or thereby of his selected 180, and it is on those replies, which he quotes at considerable length, that his conclusions are based. We have Mr. Galton's assurance that his correspondents may be taken as fairly representing the scientific aptitude and the ordinary conditions of its existence. We learn, however, that they are nearly all included within the necessarily limited circle of Mr. Galton's own acquaintance; and although we find no systematic account of their position in life, there is everywhere indirect evidence to show that they belong to the well-to-do classes. Mr. Galton expresses, with an appearance of fervour, his opinion that "the upper classes of a nation like our own, which are largely and continually recruited by selections from below, are by far the most productive of natural ability;" the lower classes, says he, are in truth the "residuum." This sounds very like a gospel for the rich. Must we then conclude, after all, that an aristocracy of talent is no mere figure of speech, and that all we have believed and hoped of the pent-up energies and glowing virtues of the poor is a poetic fiction? At all events, if chill penury ever does repress their noble rage, we do not look for an exposure of that fact in Mr. Galton's pages.

We do injustice, however, to the objectivity of Mr. Galton's studies. He follows the natural history method, gathering information about a large number of particular cases, and generalising upon it in the usual way. This method stands out in bold contrast with the way in which some former writers have treated the same subject. The German philosopher Fichte published in 1806 a small volume of University lectures, under the title 'Ueber das Wesen des Gelehrten'—On the Nature of the Scholar. "The true-minded scholar," he says, "looks upon his vocation—to become a partaker of the Divine Thought of the universe—as the purpose of God in him; and, therefore, both his person and his occupation become to him, before all other things, honourable and holy; and this holiness shows itself in all his outward manifestations." Now, when a writer on the same subject considers it to be within the scope of his inquiry to go about among his scientific acquaintances and measure inside the rim of their hats with a piece of whalebone, we feel that we have come a long way from Fichte, and that German transcendentalism has had its day. There is, however, a curious resemblance between the most salient conclusions of Mr. Galton's study and the pervading idea of the transcendentalist. According to Fichte, the true *Gelehrter*, or man of science, was he who could penetrate through the sensuous veil of the universe and discover the hidden idea beyond. This is the well-known faculty that Mr. Carlyle has illustrated in all his writings with a wealth of metaphor that is of itself sufficient to render him immortal. It is the faculty of taking an unconventional view of things; of describing, for example, the net import and upshot of war "in quite unofficial language," or of imagining a royal levée at which nobody wore clothes. Now, on a sustained perusal of Mr. Galton's work, the quality that stands out most prominently, as being special to men of science, is "independence of character." Mr. Galton's questions were not very well adapted to extract good analytic information about the strictly intellectual side of the scientific aptitude, and still more his correspondents have gone off in an ethical direction. But, by the light of previous and well-known psychological studies, one is enabled to see what Mr. Galton and his correspondents are driving at, just as we may understand also the drift of Fichte's ideas. Thus, when one of Mr. Galton's correspondents writes that he has "a preference for whatever is not the fashion, not popular, not rich, not very able to help itself, yet with qualities unworthily overlooked or unjustly oppressed," we at once recognise the ethical counterpart of what is, according to psychological analysis, the characteristic scientific intellect. The latter is described, in a great variety of terms, as the faculty of seeing likeness in the midst of difference, of establishing an identity between phenomena that appear to the common eye to have no relation to each other, of being able to set aside the adventitious circumstances and to fix the attention on what is relevant, and so on. Mr. Galton found that fifty of his correspondents had independence of character in excess, while it was below par in two only. Besides truthfulness, which, as a scientific quality, is somewhat colourless, memory is the only other strictly relevant faculty about which statistics are given. Mr. Galton has dealt separately with the verbal memory, the memory "for facts and figures," and the memory for form, but he has strangely neglected to sort the replies of his correspondents according to the departments of science, more or less abstract, or more or less concrete, in which they were respectively engaged; and it is hardly to be wondered that his conclusions on this head are indefinite. For the rest we learn that the successful *savants* have the qualities that accompany success in general. They are remarkably healthy, they are possessed of bodily energy in a high degree, and of mental energy to an extent that has led many of them to take up scientific investigation in addition to their ordinary calling. As to the size of the head, we are re-assured that "the general scientific position of the small-headed and the large-headed men seems equally good," but the small-headed men are by far the most energetic. The geographical distribution of scientific ability is a very curious matter. Mr. Galton gives a map in which the scientific areas are marked a deep black colour, while the rest is of virgin whiteness. It is easy to see at a glance that the districts traversed by the South-Western and the Great Eastern railways, together with the entire sea-board, are greatly wanting in scientific capacity; while the diffusion of the scientific aptitude northwards into Scotland comes to a dead stop at the Forth and Clyde canal. Facts are, no doubt, inexorable, and the sociological method is not to be lightly set aside. But those who fondly believe in the scientific capacity of certain corners of the land, that are not included in Mr. Galton's areas of ability, may be permitted to hope that some, at least, of those other 400 Fellows of the Royal Society, or of "the many excellent men of science, who, for various reasons, are not Fellows," may be doing credit, in a humble way, to the benighted parts of the country. Sociology, indeed, would seem to be a game that two or more can play at.

By far the most valuable part of Mr. Galton's work is that having reference to the inheritance of qualities. Although, as we have attempted to show, his data were quite insufficient for making an induction as to the special scientific aptitude, and the conditions favourable to it, they were very well suited to throw light upon the abstract question of heredity. The various qualities of body and mind that Mr. Galton has taken up one by one were sufficiently well adapted for testing the resemblance between parent and offspring, although they may have been partly relevant to the scientific faculty, and partly irrelevant, or entirely unconnected with it. The statistical returns show pretty clearly that the 100 successful men who furnished them were not only born in a good social position, but that they inherited also physical and mental qualities of great value.

NEW BOOKS AND NEW EDITIONS.

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Women, Rights of. (8vo, pp. 104.) Trübner.

DRAMA.

ROYAL AMPHITHEATRE, HOLBORN.

There was a time when, as Dryden tells us in his "Essay on Dramatic Poesy," Beaumont and Fletcher's plays were "the most pleasant and frequent entertainments of the stage;" in the easy epoch that followed the Restoration the gaiety of their comedies, the flagrant vices of their heroes and heroines, and the romantic pathos of their tragical scenes, suited exactly the humours of the town. That day is completely passed; their writings are wholly unknown to the play-going world, and the only instance of their revival is the occasional representation of the "Maid's Tragedy" in a form so garbled as to be scarcely recognisable. This play, as originally altered for Macready by Sheridan Knowles, and brought out at the Haymarket Theatre, Mr. Hollingshead has put on the boards of his popular house in Holborn, and it attracts an appreciative but not numerous audience. The "Maid's Tragedy" is probably the best of Beaumont and Fletcher's works; "Philaster" contains more romantic scenes and more beautifully-written passages; "King and No King" has more stirring interest; some of the comedies are brighter and easier, but the "Maid's Tragedy" seems to carry off the palm for seriousness and depth of thought. It is the most Shakespearian of the series, more full than any other of high and intense feeling, and of sad and pure pathos. The female characters of Beaumont and Fletcher, even when they are meant to be most rigidly chaste, usually destroy the sense of their own purity by an offensive levity of expression. Aspatia stands almost alone among them—a serene and saintly figure, fair as a bride, meek as a maid, with unchapleted and unfilleted hair, an unsullied sacrifice to a great tragic purpose. The plot of the piece, too, is unusually clear and coherent; it is not marred by any difficult under-plot, and there is no disagreeable comic work to be disgusted with, or to be disdainful over.

It is distinctly matter for regret that Mr. Hollingshead, in determining to revive this play, should have adopted the form Sheridan Knowles manufactured for Macready. We are far from advocating the restoration in full of a play so coarse in detail as this; but it does seem as if a version less completely at variance with the original might judiciously have been chosen. The first act, as it at present stands, mars the whole development of the piece. In the "Maid's Tragedy," Aspatia appears but seldom, and whenever she opens her mouth it is with a solemn music as of an incantation; there hangs about all her utterances a witchery of grave and sonorous melody. We see her never unless reserved, dignified, and steeped in sorrow. The scenes Sheridan Knowles has added, besides dragging in a new and useless element of horror in making the King insult her with a dishonourable proposal, bring her mysterious nature completely down to the level of commonplace, and rob the drama of its finest feature. Again, on what possible pretext is the foolish old father, Calianax, the comic personage of the "Maid's Tragedy," robbed of all his humorous characteristics and made a mere dull and pompous courtier? Knowles' additions are almost all stupid and ill-considered, and it is certainly time to prune away most of these foolish excrescences. The beautiful scene in the end of the second act, where Aspàtia sits among her girls and mourns, while the picture of Ariadne is held before her, now opens the first act, and in order to make it intelligible a good deal of spurious conversation is put into the girls' mouths. The famous song, "Lay a garland on my hearse," instead of being sung at its right place on Evadne's wedding-night, is wailed out by Aspasia behind the scenes at the very beginning of the piece. And that beautiful and unblemished scene in Evadne's chamber, where Aspatia damps the general mirth with her sadness, and goes out with a farewell speech to Amintor, full of exquisite melody, is for no apparent reason left out. But perhaps the most foolish omission is of the first appearance of Aspatia. When Melantius has just returned, and is being told that it is Amintor's wedding-night, Aspatia crosses the stage; he thinking her to be the bride, congratulates her, and she bids him not add scorn to her sorrow, going out as she says it. This brief appearance, giving a key-note to the mystery of her despair, is one of the most subtle touches in the play, and this the sapient Sheridan Knowles has omitted, because it clashed with his own scenes at the beginning. The character of Evadne is little changed; its repulsive details are softened down with judgment, and as she is here presented to us, we are quite ready to exclaim, with Amintor:—

> Hadst thou been thus, thus excellently good
> Before that devil-king tempted thy frailty,
> Sure thou hadst made a star.

The fine scene in which she kills the king in the beginning of the fifth act is cut out, and instead of it we have a rather long soliloquy outside the door of the king's chamber, at the end of which she plucks a dagger from her bosom, and rushes in; a mode of procedure which is perhaps quite as effective and certainly more modest than the horrible Judith-and-Holophernes scene in the original play. The plot becomes completely changed in the end of the fifth act; the whole action and denouement is altered, and, instead of the death of the two heroines, they fall, one into the arms of Amintor, the other on the breast of Melantius. To conclude, the point of personal and awe-inspiring dignity in the king, the fear that circles his person at all times and makes him so hard to slay, Sheridan Knowles has completely lost; his king is weak and wanton, snubbed by all his courtiers, and thwarted at every turn.

Mr. Creswick plays Melantius conscientiously. His dress is very unbecoming, but when the eyes have become accustomed to the annoyance of the red-striped blanket he flourishes over his shoulders, he is seen to be acting carefully and effectively. He has caught the true manner of a Fletcher hero, an unbending, unsoftened man of war, unable, like Shakespeare's soldiers, to melt into more genial and humorous moods. He acts the scenes in which there is much movement,—the one in which he extracts the wretched secret from Amintor, and then quarrels with him, after the fashion of Brutus and Cassius,—the one in which he calls on his brother to aid him in revenge, and, the best of all, the magnificent scene in which he crushes the will and wakens the conscience of his wicked sister Evadne, very excellently. Miss Leighton, who plays Evadne, does her part well also. At first the piece drags wearisomely; it takes a good deal of patience to listen to the stupid interpolations and trumped-up sensations of Sheridan Knowles, but when the undiluted Beaumont and Fletcher begins, Miss Leighton warms to her work. Her first good piece of acting is the scene in her chamber on her marriage-night, with Amintor, a passage that gives her great opportunities for brilliant tragic impersonation. Miss Edgar was creditable but rather cold as Aspatia; in the real "Maid's Tragedy" she would probably do better than in this version; the ingeniously ill-advised scene when the King is made, by Mr. Knowles, to attempt her honour in the first act, makes it almost impossible for her to act her part satisfactorily in the future. Mr. Pennington is a showy but not wholly agreeable Amintor; the weakness, excitability, and vacillating affectionateness are well rendered, but he raves too much, sometimes most unsuitably, as in the wedding-night scene with Evadne. These four actors save the piece from complete mediocrity, but the rest of the company are either very bad indeed or else completely uninteresting. The whole performance is a curious proof of how thoroughly Beaumont and Fletcher, in spite of their great natural gifts, richness of fancy, brilliancy of dialogue, and knowledge of stage requirements, have passed into the honourable retirement of the library. It may well be true, as has been said, that in them the English language arrived at its highest perfection, but certainly in regard to dramatic vitality they marked the beginning of a decadence, and threw out the sterile shoots that broke into such barren blossoms as Congreve and Southern.

EDMUND W. GOSSE.

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English
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of
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Natural Inheritance

1888

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NATURAL INHERITANCE

BY

FRANCIS GALTON, F.R.S.

AUTHOR OF

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NATURAL INHERITANCE.

NATURAL INHERITANCE.

CHAPTER I.

INTRODUCTORY.

I HAVE long been engaged upon certain problems that lie at the base of the science of heredity, and during several years have published technical memoirs concerning them, a list of which is given in Appendix A. This volume contains the more important of the results, set forth in an orderly way, with more completeness than has hitherto been possible, together with a large amount of new matter.

The inquiry relates to the inheritance of moderately exceptional qualities by brotherhoods and multitudes rather than by individuals, and it is carried on by more refined and searching methods than those usually employed in hereditary inquiries.

One of the problems to be dealt with, refers to the curious regularity commonly observed in the statistical peculiarities of great populations during a long series of generations. The large do not always beget the large, nor the small the small, and yet the observed proportions between the large and the small in each degree of size and in every quality, hardly varies from one generation to another.

A second problem regards the average share contributed to the personal features of the offspring by each ancestor severally. Though one half of every child may be said to be derived from either parent, yet he may receive a heritage from a distant progenitor that neither of his parents possessed as *personal* characteristics. Therefore the child does not on the average receive so much as one half of his *personal* qualities from each parent, but something less than a half. The question I have to solve, in a reasonable and not merely in a statistical way, is, how much less?

The last of the problems that I need mention now, concerns the nearness of kinship in different degrees. We are all agreed that a brother is nearer akin than a nephew, and a nephew than a cousin, and so on, but how much nearer are they in the precise language of numerical statement?

These and many other problems are all fundamentally connected, and I have worked them out to a first degree of approximation, with some completeness. The conclusions cannot however be intelligibly presented in an introductory chapter. They depend on ideas that must first be well comprehended, and which are now novel to the large majority of readers and unfamiliar to all. But those who care to brace themselves to a sustained effort, need not feel much regret that the road to be travelled over is indirect, and does not admit of being mapped beforehand in a way they can clearly understand. It is full of interest of its own. It ~~also~~ familiarizes us with the measurement of variability, and with curious laws of chance that apply to a vast diversity of social subjects. This part of the inquiry may be said to run along a road ~~a ta~~ high level, that affords wide views in unexpected directions, and from which easy descents may be made to totally different goals to those we have now to reach. I have a great subject to write upon, but feel keenly my literary incapacity to make it easily intelligible without sacrificing accuracy and thoroughness.

CHAPTER II.

PROCESSES IN HEREDITY.

Natural and Acquired Peculiarities.—Transmutation of Female into Male Measures.—Particulate Inheritance.—Family Likeness and Individual Variation.—Latent Characteristics.—Heritages that Blend and those that are Mutually Exclusive.—Inheritance of Acquired Faculties.—Variety of Petty Influences.

A CONCISE account of the chief processes in heredity will be given in this chapter, partly to serve as a reminder to those to whom the works of Darwin especially, and of other writers on the subject, are not familiar, but principally for the sake of presenting them under an aspect that best justifies the methods of investigation about to be employed.

Natural and Acquired Peculiarities.—The peculiarities of men may be roughly sorted into those that are natural and those that are acquired. It is of the former that I am about to speak in this book. They are noticeable in every direction, but are nowhere so remarkable as in those twins[1] who have been dissimilar

[1] See *Human Faculty*, p. 237.

in features and disposition from their earliest years, though brought into the world under the same conditions and subsequently nurtured in an almost identical manner. It may be that some natural peculiarity does not appear till late in life, and yet may justly deserve to be considered natural, for if it is decidedly exceptional in its character its origin could hardly be ascribed to the effects of nurture. If it was also possessed by some ancestor, it must be considered to be hereditary as well. But "Natural" is an unfortunate word for our purpose; it implies that the moment of birth is the earliest date from which the effects of surrounding conditions are to be reckoned, although nurture begins much earlier than that. I therefore must ask that the word "Natural" should not be construed too literally, any more than the analogous phrases of inborn, congenital, and innate. This convenient laxity of expression for the sake of avoiding a pedantic periphrase need not be accompanied by any laxity of idea.

Transmutation of Female into Male Measures.—We shall have to deal with the hereditary influence of parents over their offspring, although the characteristics of the two sexes are so different that it may seem impossible to speak of both in the same terms. The phrase of "Average Stature" may be applied to two men without fear of mistake in its interpretation; neither can there be any mistake when it is applied to two women, but what meaning can we attach to the word "Average" when it is applied to the stature of two such different bei/ ngs as the Father and the Mother? How can we appraise the hereditary contributions of different ancestors whether in this or in any other quality, unless we take into account the sex of each ancestor, in addition to his or her characteristics? Again, the same group of progenitors transmits qualities in different measure to the sons and to the daughters; the sons being on the whole, by virtue of their sex, stronger, taller, hardier, less emotional, and so forth, than the daughters. A serious complexity due to sexual differences seems to await us at every step when investigating the problems of heredity. Fortunately we are able to evade it altogether by using an artifice at the outset, else, looking back as I now can, from the stage which the reader will reach when he finishes this book, I hardly know how we should have succeeded in making a fair start. The artifice is never to deal with female measures as they are observed, but always to employ their male equivalents in the place of them. I transmute all the observations of females before taking them in hand, and thenceforward am able to deal with them on equal terms with the observed male values. For example: the statures of women bear to those of men the proportion of about twelve to thirteen. Consequently by adding to each observed female stature at the rate of one inch for every foot, we are enabled to compare their statures so increased and transmuted, with the observed statures of males, on equal terms. If the observed stature of a woman is 5 feet, it will count by this rule as 5 feet + 5 inches; if it be

6 feet, as 6 feet + 6 inches; if $5\frac{1}{2}$ feet, as $5\frac{1}{2}$ feet + $5\frac{1}{2}$ inches; that is to say, as 5 feet + $11\frac{1}{2}$ inches.[1]

Similarly as regards sons and daughters; whatever may be observed or concluded concerning daughters will, if transmuted, be held true as regarding sons, and whatever is said concerning sons, will if re-transmuted, be held true for daughters. We shall see further on that it is easy to apply this principle to all measurable qualities.

Particulate Inheritance.—All living beings are individuals in one aspect and composite in another. They are stable fabrics of an inconceivably large number of cells, each of which has in some sense a separate life of its own, and which have been combined under influences that are the subjects of much speculation, but are as yet little understood. We seem to inherit bit by bit, this element from one progenitor that from another, under conditions that will be more clearly expressed as we proceed, while the several bits are themselves liable to some small change during the process of transmission. Inheritance may therefore be described as largely if not wholly "particulate," and as such it will be treated in these pages. Though this word is good English and accurately expresses its own meaning, the application

[1] The proportion I use is as 100 to 108; that is, I multiply every female measure by 108, which is a very easy operation to those who possess that most useful book to statisticians, *Crelle's Tables* (G. Reimer, Berlin). It gives the products of all numbers under 1000, each into each; so by referring to the column headed 108, the transmuted values of the female statures can be read off at once.

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now made of it will be better understood through an illustration. Thus, many of the modern buildings in Italy are historically known to have been built out of the pillaged structures of older days. Here we may observe a column or a lintel serving the same purpose for a second time, and perhaps bearing an inscription that testifies to its origin, while as to the other stones, though the mason may have chipped them here and there, and altered their shapes a little, few, if any, came direct from the quarry. This simile gives a rude through true idea of the exact meaning of Particulate Inheritance, namely, that each piece of the new structure is derived from a corresponding piece of some older one, as a lintel was derived from a lintel, a column from a column, a piece of wall from a piece of wall.

I will pursue this rough simile just one step further, which is as much as it will bear. Suppose we were building a house with second-hand materials carted from a dealer's yard, we should often find considerable portions of the same old houses to be still grouped together. Materials derived from various structures might have been moved and much shuffled together in the yard, yet pieces from the same source would frequently remain in juxtaposition and it may be entangled. They would lie side by side ready to be carted away at the same time and to be re-erected together anew. So in the process of transmission by inheritance, elements derived from the same ancestor are apt to appear in large groups, just as if they had clung together in the pre-embryonic stage, as perhaps they did. They form what is well expressed by the word "traits," traits of feature and character—that is to say, continuous features and not isolated points.

We appear, then, to be severally built up out of a host of minute particles of whose nature we know nothing, any one of which may be derived from any one progenitor, but which are usually transmitted in aggregates, considerable groups being derived from the same progenitor. It would seem that while the embryo is developing itself, the particles more or less qualified for each new post wait, as it were in competition to obtain it. Also that the particle that succeeds, must owe its success partly to accident of position and partly to being better qualified than any equally well placed competitor to gain a lodgment. Thus the step by step development of the embryo cannot fail to be influenced by an incalculable number of small and mostly unknown circumstances.

Family Likeness and Individual Variation.—Natural peculiarities are apparently due to two broadly different causes, the one is Family Likeness and the other is Individual Variation. They seem to be fundamentally opposed, and to require independent discussion, but this is not the case altogether, nor indeed in the greater part. It will soon be understood how the conditions that produce a general resemblance between the offspring and their parents, must at the same time give rise to a considerable amount of individual differences. Therefore I need not discuss Family Likeness and Individual Variation under separate heads, but as different effects of the same underlying causes.

The origin of these and other prominent processes in heredity is best explained by illustrations. That which will be used was suggested by those miniature gardens, self-made and self-sown, that may be seen in crevices or other receptacles for drifted earth, on the otherwise bare faces of quarries and cliffs. I have frequently studied them through an opera glass, and have occasionally clambered up to compare more closely their respective vegetations. Let us then suppose the aspect of the vegetation, not of one of these detached little gardens, but of a particular island of substantial size, to represent the features, bodily and mental, of some particular parent. Imagine two such islands floated far away to a desolate sea, and anchored near together, to represent the two parents. Next imagine a number of islets, each constructed of earth that was wholly destitute of seeds, to be reared near to them. Seeds from both of the islands will gradually make their way to the islets through the agency of winds, currents, and birds. Vegetation will spring up, and when the islets are covered with it, their several aspects will represent the features of the several children. It is almost impossible that the seeds could ever be distributed equally among the islets, and there must be slight differences between them in exposure and other conditions, corresponding to differences in pre-natal conditions. All of these would have some influence upon the vegetation; hence there would be a corresponding variety in the results. In some islets one plant would prevail, in others another; nevertheless there would be many traits of family likeness in the vegetation of all of them, and no plant would be found that had not existed in one or other of the islands.

Though family likeness and individual variations are largely due to a common cause, some variations are so large and otherwise remarkable, that they seem to belong to a different class. They are known among breeders as "sports"; I will speak of these later on.

Latent Characteristics.—Another fact in heredity may also be illustrated by the islands and islets; namely, that the child often resembles an ancestor in some feature or character that neither of his parents personally possessed. We are told that buried seeds may lie dormant for many years, so that when a plot of ground that was formerly cultivated is again deeply dug into and upturned, plants that had not been known to grow on the spot within the memory of man, will frequently make their appearance. It is easy to imagine that some of these dormant seeds should find their way to an islet, through currents that undermined the island cliffs and drifted away their *débris*, after the cliffs had tumbled into the sea. Again, many plants on the islands may maintain an obscure existence, being hidden and half smothered by successful rivals; but whenever their seeds happened to find their way to any one of the islets, while those of their rivals did not, they would sprout freely and assert themselves. This illustration partly covers the analogous fact of diseases and other inheritances skipping a generation, which by the way I find to be by no means so usual an occurrence as seems popularly to be imagined.

Heritages that Blend and those that are Mutually Exclusive.—As regards heritages that blend in the offspring, let us take the case of human skin colour. The children of the white and the negro are of a blended tint; they are neither wholly white nor wholly black, neither are they piebald, but of a fairly uniform mulatto brown. The quadroon child of the mulatto and the white has a quarter tint; some of the children may be altogether darker or lighter than the rest, but they are not piebald. Skin-colour is therefore a good example of what I call blended inheritance. It need be none the less "particulate" in its origin, but the result may be regarded as a fine mosaic too minute for its elements to be distinguished in a general view.

Next as regards heritages that come altogether from one progenitor to the exclusion of the rest. Eye-colour is a fairly good illustration of this, the children of a light-eyed and of a dark-eyed parent being much more apt to take their eye-colours after the one or the other than to have intermediate and blended tints.

There are probably no heritages that perfectly blend or that absolutely exclude one another, but all heritages have a tendency in one or the other direction, and the tendency is often a very strong one. This is paralleled by what we may see in plots of wild vegetation, where two varieties of a plant mix freely, and the general aspect of the vegetation becomes a blend of the two, or where individuals of one variety congregate and take exclusive possession of one place, and those of another variety congregate in another.

A peculiar interest attaches itself to mutually exclusive heritages, owing to the aid they afford to the establishment of incipient races. A solitary peculiarity that blended freely with the characteristics of the parent stock, would disappear in hereditary transmission, as quickly as the white tint imported by a solitary European would disappear in a black population. If the European mated at all, his spouse must be black, and therefore in the very first generation the offspring would be mulattoes, and half of his whiteness would be lost to them. If these mulattoes did not interbreed, the whiteness would be reduced in the second generation to one quarter; in a very few more generations all recognizable trace of it would have gone. But if the whiteness refused to blend with the blackness, some of the offspring of the white man would be wholly white and the rest wholly black. The same event would occur in the grandchildren, mostly but not exclusively in the children of the white offspring, and so on in subsequent generations. Therefore, unless the white stock became wholly extinct, some undiluted specimens of it would make their appearance during an indefinite time, giving it repeated chances of holding its own in the struggle for existence, and of establishing itself if its qualities were superior to those of the black stock under any one of many different conditions.

Inheritance of Acquired Faculties.—Before closing ~~the chapter~~ a few words ~~must be said~~ on the obscure, unsettled, and much discussed subject of the possibility of transmitting acquired faculties. The main evidence in its favour is the gradual change of the instincts of races at large, in conformity with changed habits, and through their ~~self~~-adaptation to their surroundings, otherwise apparently than through the influence of Natural Selection. There is very little direct evidence of its influence in the course of a single generation, if the phrase of Acquired Faculties is used in perfect strictness and all inheritance is excluded that could be referred to some form of Natural Selection, or of Infection before birth, or of peculiarities of Nurture and Rearing. Moreover, a large deduction from the collection of rare cases must be made on the ground of their being accidental coincidences. When this is done, the remaining instances of acquired disease or faculty, or of any mutilation being transmitted from parent to child, are very few. Some apparent evidence of a positive kind, that was formerly relied upon, has been since found capable of being interpreted in another way, and is no longer adduced. On the other hand there exists such a vast mass of distinctly negative evidence, that every instance offered to prove the transmission of acquired faculties requires to be closely criticized. For example, a woman who was sober becomes a drunkard. Her children born during the period of her sobriety are said to be quite healthy; her subsequent children are said to be neurotic. The objections to accepting this as a valid instance in point are many. The woman's tissues must have been drenched with alcohol, and the unborn infant alcoholised during all its existence in that state. The quality of the mother's milk would be bad. The surroundings of a home under the charge of a drunken woman would be prejudicial to the health of a growing child. No wonder that it became neurotic. Again, a large number of diseases are conveyed by germs capable of passing from the tissues of the mother into those of the unborn child otherwise than through the blood. Moreover it must be recollected that the connection betwen the ~~mother and her~~ unborn child is hardly more intimate than that between a parasite and the animal on which it lives. Not a single nerve has been traced between them, not a drop of blood [1] has been found to pass from the mother to the child. The unborn child together with the growth to which it is attached, and which is afterwards thrown off, have their own vascular system to themselves, entirely independent of that of the mother. If in an anatomical preparation the veins of the mother are injected with a coloured fluid, none of it enters the veins of the child; conversely, if the veins of the child

[1] See *Lectures* by William O. Priestley, M.D. (Churchill, London, 1860), pp. 50, 52, 55, 59, and 64.

are injected, none of the fluid enters those of the mother. Again, not only is the unborn child a separate animal from its mother, that obtains its air and nourishment from her purely through soakage, but its constituent elements are of very much less recent growth than is popularly supposed. The ovary of the mother is as old as the mother herself; it was well developed in her own embryonic state. The ova it contains in her adult life were actually or potentially present before she was born, and they grew as she grew. There is more reason to look on them as collateral with the mother, than as parts of the mother. The same may be said with little reservation concerning the male elements. It is therefore extremely difficult to see how acquired faculties can be inherited by the children. It would be less difficult to conceive of their inheritance by the grandchildren. Well devised experiment into the limits of the power of inheriting acquired faculties and mutilations, whether in plants or animals, is one of the present desiderata in hereditary science. Fortunately for us, our ignorance of the subject will not introduce any special difficulty in the inquiry on which we are now engaged.

Variety of Petty Influences.—The incalculable number of petty accidents that concur to produce variability among brothers, make it impossible to predict the exact qualities of any individual from ~~those of his parents.~~ hereditary data. But we may predict average results with great certainty, as will be seen further on, and we can also

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obtain precise information concerning the penumbra of uncertainty that attaches itself to single predictions. It would be premature to speak further of this at present; what has been said is enough to give a clue to the chief motive of this chapter. Its intention has been to show the large part that is always played by chance in the course of hereditary transmission, and to establish the importance of an intelligent use of the laws of chance and of the statistical methods that are based upon them, in ~~establishing~~ expressing the conditions under which heredity acts.

I may here point out that, as the processes of statistics are themselves processes of intimate blendings, their results are the same, whether ~~or no~~ the materials had been partially ~~gone through the process of~~ blended, or not, before they were statistically taken in hand.

CHAPTER III.

ORGANIC STABILITY.

Structure.—Filial relation.—Stable Forms.—Subordinate positions of Stability.—Model.—Stability of Sports.—Evolution not by ~~small~~ minute steps only. (Infertility of mixed types.—)

Structure.—The total heritage of each man must include a greater variety of material than was utilised in forming his personal structure. The existence in some latent form of an unused portion is proved by his power, already alluded to, of transmitting ancestral characters that he did not personally exhibit. Therefore the organised structure of each individual should be viewed as the fulfilment of only one out of an indefinite number of mutually exclusive possibilities. His structure is the coherent and more or less stable development of what is no more than an imperfect sample of a large variety of elements.

The precise conditions under which each several element or particle (whatever may be its nature) finds its way into the sample are, it is needless to repeat, unknown, but we may provisionally classify them under one or other of the following three categories, as they apparently exhaust all reasonable possibilities: first, that in which each element selects its most suitable immediate neighbourhood, in accordance with the guiding idea in Darwin's theory of Pangenesis; secondly, that of more or less general co-ordination of the influences exerted on each element, not only by its immediate neighbours, but by many or most of the others as well; finally, that of accident or chance, under which name a group of agencies are to be comprehended, diverse in character and alike only in the fact that their influence on the settlement of each particle was not immediately directed towards that end. In philosophical language we say that such agencies are not purposive, or that they are not teleological; in popular language they are called accidents or chances.

Filial Relation.—A conviction that inheritance is mainly particulate and much influenced by chance, greatly affects our idea of kinship and makes us consider the parental and filial relation to be curiously circuitous. It appears that there is no direct hereditary relation between the personal parents and the personal child, except perhaps through little-known channels of secondary importance, but that the main line of hereditary connection unites the sets of elements out of which the personal parents had been evolved with the set out of which the personal child was evolved. The main line may be rudely likened to the chain of a necklace, and the personalities to pendants attached to its links. We are unable to see the particles and watch their grouping, and we know nothing directly about them, but we may gain some idea of the various possible results by noting the differences between the brothers in any large fraternity (as will be done further on with much minuteness), whose total heritages must have been much alike, but whose personal structures are often very dissimilar. This is why it is so important in hereditary inquiry to deal with fraternities rather than with individuals, and with large fraternities rather than small ones. We ought, for example, to compare the group containing both parents and all the uncles and aunts, with that containing all the children. The relative weight to be assigned to the uncles and aunts is a question of detail to be discussed in its proper place further on (see Chap. XI).

Stable Forms.—The changes in the substance of the newly-fertilised ova of all animals, of which more is annually becoming known,[1] indicate segregations as well as aggregations, and it is reasonable to suppose that repulsions concur with affinities in producing them. We know nothing as yet of the nature of these affinities and repulsions, but we may expect them to act in great numbers and on all sides in a space of three dimensions, just as the personal likings and dis-

[1] A valuable memoir on the state of our knowledge of these matters up to the end of 1887 is published in Vol. XIX. of the *Proceedings of the Philosophical Society of Glasgow*, and reprinted under the title of *The Modern Cell Theory, and theories as to the Physiological Basis of Heredity*, by Prof. John Gray McKendrick, M.D., F.R.S., &c. (R. Anderson, Glasgow, 1888.)

likings of each individual insect in a flying swarm may be supposed to determine the position that he occupies in it. Every particle must have many immediate neighbours. Even a sphere surrounded by other spheres of equal sizes, like a cannon-ball in the middle of a heap, when they are piled in the most compact form, is in actual contact with no less than twelve others. We may therefore feel assured that the particles which are still unfixed must be affected by very numerous influences acting from all sides and varying with slight changes of place, and that they may occupy many positions of temporary and unsteady equilibrium, and be subject to repeated unsettlement, before they finally assume the positions into which they severally remain at rest.

The whimsical effects of chance in producing stable results are common enough. Tangled strings variously twitched, soon get themselves into tight knots. Rubbish thrown down a sink is pretty sure in time to choke the pipe; no one bit may be so large as its bore, but several bits in their numerous chance encounters will at length so come into collision as to wedge themselves into a sort of arch across the tube, and effectually plug it. Many years ago there was a fall of large stones from the ruinous walls of Kenilworth Castle. Three of them, if I recollect rightly, or possibly four, fell into a very peculiar arrangement, and bridged the interval between the jambs of an old window. There they stuck fast, showing clearly against the sky. The oddity of the structure attracted continual attention, and its stability was much commented on. These hanging stones, as they were called, remained quite firm for many years; at length a storm shook them down.

In every congregation of mutually reacting elements, some characteristic groupings are usually recognised that have become familiar through their frequent recurrence and partial persistence. Being less evanescent than other combinations, they may be regarded as temporarily Stable Forms. No demonstration is needed to show that their number must be greatly smaller than that of all the possible combinations of the same elements. I will briefly give as great a diversity of instances as I can think of, taken from Governments, Crowds, Landscapes, and even from Cookery, and shall afterwards draw some illustrations from mechanical inventions, to illustrate what is meant by characteristic and stable groupings. From some of them it will also be gathered that secondary and other orders of stability exist besides the primary ones.

In Governments, the primary varieties of stable forms are very few in number, being such as autocracies, constitutional monarchies, oligarchies, or republics. The secondary forms are far more numerous; still it is hard to meet with an instance of one that cannot be pretty closely paralleled by another. A curious evidence of the small variety of possible ~~constitutions~~ governments is to be found in the constitutions of the governing bodies of the Scientific Societies of London and the Provinces, which are numerous and independent. Their development seems to follow a single course that has many stages, and invariably tends ~~towards~~ to establish the following staff of officers: President, vice-Presidents, a Council, Honorary Secretaries, a paid Secretary, Trustees, and a Treasurer. As Britons are not unfrequently servile to rank, some seek a purely ornamental Patron as well.

Every variety of Crowd has its own characteristic features. At a national pageant, an evening party, a race-course, a marriage, or a funeral, the groupings in each case recur so habitually that it sometimes appears to me as if time had no existence, and that the ceremony in which I am taking part is identical with others at which I had been present one year, ten years, twenty years, or any other time ago.

The frequent combination of the same features in Landscape Scenery, justifies the use of such expressions as "true to nature," when applied to a pictorial composition or to the descriptions of a novel writer. The experiences of travel in one part of the world may curiously resemble those in another. Thus the military expedition by boats up the Nile was planned from experiences gained on the Red River of North America, and was carried out with the aid of Canadian *voyageurs.* The snow mountains all over the world present the same peculiar difficulties to the climber, so that Swiss experiences and in many cases Swiss guides have been used for the exploration of the Himalaya, the Caucasus, the lofty mountains of New Zealand, the Andes, and Greenland. Whenever the general conditions of the country and climate are alike, we recognise characteristic and familiar features at every turn, whether we

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are walking by the brookside, along the seashore, in the woods, or on the hills.

Even in Cookery it seems difficult to invent a new and good dish, though the current recipes are few, and the proportions of the flour, sugar, butter, eggs, &c., used in making them might be indefinitely varied and be still eatable. I consulted cookery books to learn the facts authoritatively, and found the following passage: "I have constantly kept in view the leading principles of this work, namely, to give in these domestic recipes *the most exact quantities*. . . . I maintain that one cannot be too careful; it is the only way to put an end to those approximations and doubts which will beset the steps of the inexperienced, and which account for so many people eating indifferent meals at home."[1]

It is the triteness of these experiences that makes the most varied life monotonous after a time, and many old men as well as Solomon have frequent occasion to lament that there is nothing new under the sun.

The object of these diverse illustrations is to impress the meaning I wish to convey, by the phrase of stable forms or groupings, which, however ~~vague~~ uncertain it may be in outline, is perfectly distinct in substance.

Every one of the meanings that have been attached by writers to the vague but convenient word "type" has for its central idea the existence of a limited number

[1] *The Royal Cookery Book.* By Jules Gouffé, Chef de Cuisine of the Paris Jockey Club; translated by Alphonse Gouffé, Head Pastry Cook to H.M. the Queen. Sampson Low. 1869. Introduction, p. 9.

of frequently recurrent forms. The word etymologically compares these forms to the identical medals that may be struck by one or other of a set of dies. The central idea on which the phrase "stable forms" is based is of the same kind, while the phrase further accounts for their origin, vaguely it may be, but still significantly, by showing that though we know little or nothing of details, the result of organic groupings is analogous to much that we notice elsewhere on every side.

Subordinate positions of Stability.—Of course there are different degrees of stability. If the same structural form recurs in successively descending generations, its stability must be great, otherwise it could not have withstood the effects of the admixture of equal doses of alien elements in successive generations. Such a form well deserves to be called typical. A breeder would always be able to establish it. It tends of itself to become a new and stable variety; therefore all the breeder has to attend to is to give fair play to its tendency, by weeding out from among its offspring such reversions to other forms as may crop up from time to time, and by preserving the breed from rival admixtures until it has become confirmed, and adapted in every minute particular to its surroundings.

Personal Forms may be compared to Human Inventions, as they also may be divided into types, sub-types, and deviations from them. Every important invention is a new type, and of such a definite kind as to admit of clear verbal description, and so of becoming the subject of patent rights; at the same time it need not be so minutely defined as to exclude the possibility of small improvements or of deviations from the main design, any of which may be freely adopted by the inventor without losing the protection of his patent. But the range of protection is by no means sharply distinct, as most inventors know to their cost. Some other man, who may or may not be a plagiarist, applies for a separate patent for himself, on the ground that he has introduced modifications of a fundamental character; in other words, that he has created a fresh type. His application is opposed, and the question whether his plea be valid or not, becomes a subject for legal decision.

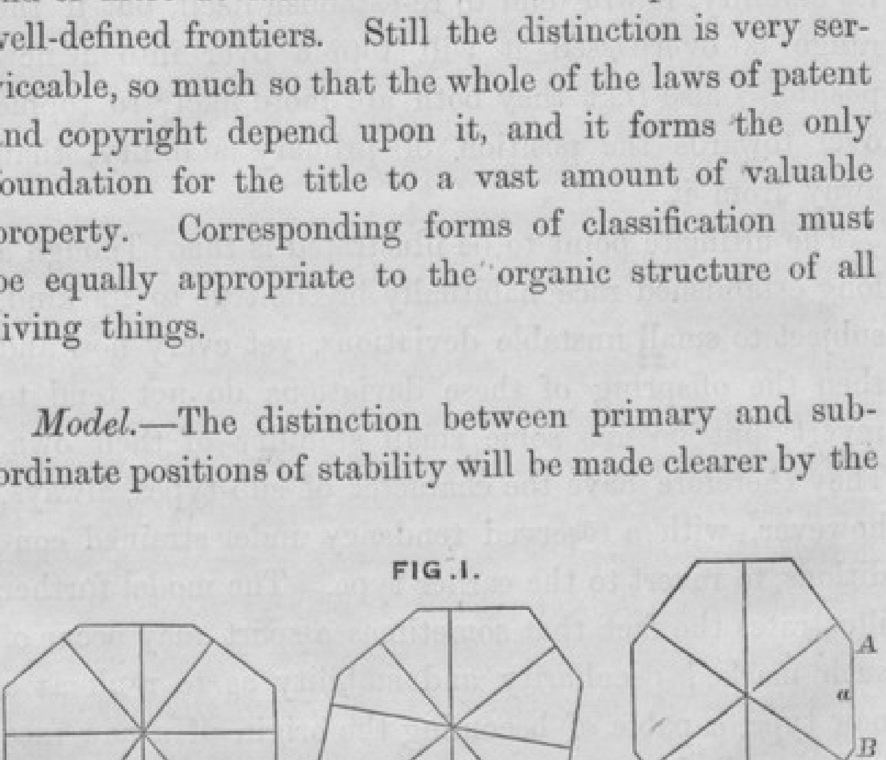
Whenever a patent is granted subsidiary to another, and lawful to be used only by those who have acquired rights to work the primary invention, then we should rank the new patent as a secondary and not as a primary type. Thus we see that mechanical inventions offer good examples of types, sub-types, and mere deviations.

The three kinds of public carriages that characterise the streets of London; namely, omnibuses, hansoms, and four-wheelers, are specific and excellent illustrations of what I wish to express by mechanical types, as distinguished from sub-types. Attempted improvements in each of them are yearly seen, but none have as yet superseded the old familiar patterns, which cannot, as it thus far appears, be changed with advantage, taking the circumstances of London as they are. Yet there have been numerous subsidiary and patented contrivances, each a distinct step in the improvement of one or other of the three primary types, and there are or may be in each of the three an indefinite number of varieties in details, too unimportant to be subjects of patent rights.

The broad classes, of primary or subordinate types, and of mere deviations from them, are separated by no well-defined frontiers. Still the distinction is very serviceable, so much so that the whole of the laws of patent and copyright depend upon it, and it forms the only foundation for the title to a vast amount of valuable property. Corresponding forms of classification must be equally appropriate to the organic structure of all living things.

Model.—The distinction between primary and subordinate positions of stability will be made clearer by the

FIG. 1.



help of Fig 1, which is drawn from a model I made. The model has more sides, but Fig. 1 suffices for illustration. It is a polygonal slab that can be made to stand on any one of its edges when set upon a level table, and is intended to illustrate the meaning of primary and subordinate stability in organic structures, although the conditions of these must be far more complex than anything we have wits to imagine. The model and the organic structure have the cardinal fact in common, that if either is disturbed without transgressing the range of its stability, it will tend to re-establish itself, but if the range is overpassed it will topple over into a new position ; also that ~~they~~ both of them are more likely to topple over towards the position of primary stability, than away from it.

The ultimate point to be illustrated is this. Though a long established race habitually breeds true to its kind, subject to small unstable deviations, yet every now and then the offspring of these deviations do not tend to revert, but possess some small stability of their own. They therefore have the character of sub-types, always, however, with a reserved tendency under strained conditions, to revert to the earlier type. The model further illustrates the fact that sometimes a sport may occur of such marked peculiarity and stability as to rank as a new type, capable of becoming the origin of a new race with very little assistance on the part of natural selection. Also, that a new type may be reached without any large single stride, but through a fortunate and rapid succession of many small ones.

The model is a polygonal slab, the polygon being one that might have been described within an oval, and it is so shaped as to stand on any one of its edges. When the slab rests as in Fig. 1, on the edge A B, corresponding to the shorter diameter of the oval, it stands in its most stable position, and in one from which it is equally difficult to dislodge it by a tilt either forwards or backwards. So long as it is merely tilted it will fall back on being left alone, and its position when merely tilted corresponds to a simple deviation. But when it is pushed with sufficient force, it will tumble on to the next edge, B C, into a new position of stability. It will rest there, but less securely than in its first position; moreover its range of stability will no longer be disposed symmetrically. A comparatively slight push from the front will suffice to make it tumble back, a comparatively heavy push from behind is needed to make it tumble forward. If it be tumbled over into a third position (not shown in the Fig.), the process just described may recur with exaggerated effect, and similarly for many subsequent ones. If, however, the slab is at length brought to rest on the edge C D, most nearly corresponding to its longest diameter, the next onward push, which may be very slight, will suffice to topple it over into an entirely new system of stability; in other words, a "sport" comes suddenly into existence. Or the figure might have been drawn with its longest diameter passing into a projecting spur, so that a push of extreme strength would be required to topple it entirely over.

If the first position, A B, is taken to represent a type, the other portions will represent sub-types. All the stable positions on the same side of the longer diameter are subordinate to the first position. On whichever of

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of them the polygon may stand, its principal tendency on being seriously disturbed will be to fall back towards the first position; yet each position is stable within certain limits.

Consequently the model illustrates how the following conditions may co-exist: (1) Variability within narrow limits without prejudice to the purity of the breed. (2) Partly stable sub-types. (3) Tendency, when much disturbed, to revert from a sub-type to an earlier form. (4) Occasional sports which may give rise to new types.

Stability of Sports.—Experience does not show that those wide varieties which are called "sports" are unstable. On the contrary, they are often transmitted to successive generations with curious persistence. Neither is there any reason for expecting otherwise. While we can well understand that a strained modification of a type would not be so stable as one that approximates more nearly to the typical centre, the variety may be so wide that it falls into different conditions of stability, and ceases to be a strained modification of the original type.

The hansom cab was originally a marvellous novelty. In the language of breeders it was a sudden and remarkable "sport," yet the suddenness of its appearance has been no bar to its unchanging hold on popular favour. It is not a monstrous anomaly of incongruous parts, and therefore unstable, but quite the contrary. Many other instances of very novel and yet stable inventions could be quoted. One of the earliest electrical batteries was that which is still known as a Grove battery, being the invention of Sir William Grove. Its principle was quite new at the time, and it continues in use without alteration.

The persistence in inheritance of trifling characteristics, such as a mole, a white tuft of hair, or multiple fingers, has often been remarked. The reason of it is, I presume, that such characteristics have inconsiderable influence upon the general organic stability; they are mere excrescences, that may be associated with very different types, and are therefore inheritable without let or hindrance.

It seems to me that stability of type, about which we as yet know very little, must be an important factor in the general theory of heredity, when the theory is applied to cases of high breeding. It will be shown later on, at what point a separate allowance requires to be made for it. But in the earlier and principal part of the inquiry, which deals with the inheritance of qualities that are only exceptional in a small degree, a separate allowance does not appear to be required.

Infertility of Mixed Types.—It is not difficult to see in a general way why very different types should refuse to coalesce, and it is scarcely possible to explain the reason why, more clearly than by an illustration. Thus a useful blend between a four-wheeler and a hansom would be impossible; it would have to run on three wheels and the half-way position for the driver would be upon its roof. A blend would be equally impossible between an omnibus and a hansom, and it would be difficult between an omnibus and a four-wheeler.

Evolution not by Minute Steps Only.—The theory of Natural Selection might dispense with a restriction, for which it is difficult to see either the need or the justification, namely, that the course of evolution always proceeds by steps that are severally minute, and that become effective only through accumulation. That the steps *may* be small and that they *must* be small are very different views; it is only to the latter that I object, and only when the indefinite word "small" is used in the sense of "barely discernible," or as small compared with such large sports as are known to have been the origins of new races. An apparent ground for the common belief is founded on the fact that whenever search is made for intermediate forms between widely divergent varieties, whether they be of plants or of animals, of weapons or utensils, of customs, religion or language, or of any other product of evolution, a long and orderly series can usually be made out, each member of which differs in an almost imperceptible degree from the adjacent specimens. But it does not at all follow because these intermediate forms have been found to exist, that they are the very stages that were passed through in the course of evolution. Counter evidence exists in abundance, not only of the appearance of considerable sports, but of their remarkable stability in hereditary transmission. Many of the specimens of intermediate forms may have been unstable varieties, whose descendants had reverted; they might be looked upon as tentative and faltering steps taken along parallel courses of evolution, and afterwards retracted. Affiliation from each generation to the next requires to be proved before any apparent line of descent can be accepted as the true one. The history of inventions fully illustrates this view. It is a most common experience that what an inventor knew to be original, and believed to be new, had been invented independently by others many times before, and had never become established. Even when it has new features, the inventor usually finds, on consulting lists of patents, that other inventions closely border on his own. Yet we know that inventors often proceed by strides, their ideas originating in some sudden happy thought suggested by a chance occurrence, though their crude ideas may have to be laboriously worked out afterwards. If, however, all the varieties of any machine that had ever been invented, were collected and arranged in a Museum in the apparent order of their Evolution, each would differ so little from its neighbour as to suggest the fallacious inference that the successive inventors of that machine had progressed by means of a very large number of hardly discernible steps.

The object of this and of the preceding chapter has been first to dwell on the fact of inheritance being "particulate," secondly to show how this fact is compatible with the existence of various types, some of which are subordinate to others, and thirdly to argue that Evolution need not proceed by small steps only. I have largely used metaphor and illustration to explain the facts, wishing to avoid entanglements with theory as far as possible, inasmuch as no complete theory of inheritance has yet been propounded that meets with general acceptation.

CHAPTER IV.

SCHEMES OF DISTRIBUTION AND OF FREQUENCY.

Fraternities and Populations to be treated as Units.—Schemes of Distribution and their Grades.—The Shape of Schemes is independent of the number of observations.—Data for Eighteen Schemes.—Application of the method of Schemes to inexact Measures.—Schemes of Frequency.

Fraternities and Populations to be Treated as Units.—The science of heredity is concerned with Fraternities and large Populations rather than with individuals, and must treat them as units. A compendious method is therefore requisite by which we may express the distribution of each faculty among the members of any large group, whether it be a Fraternity or an entire Population.

The knowledge of an average value is a meagre piece of information. How little is conveyed by the bald statement that the average income of English families is 100*l.* a year, compared with what we should learn if we were told how English incomes were distributed; what proportion of our countrymen had just and only just enough means to ward off starvation, and what were the proportions of those who had incomes in each and every other degree, up to the huge annual receipts of a few great speculators, manufacturers, and landed proprietors. So in respect to the distribution of any human quality or faculty, a knowledge of mere averages tells but little; we want to learn how the quality is distributed among the various members of the Fraternity or of the Population, and to express what we know in so compact a ~~manner~~ form that it can be easily grasped and dealt with. A parade of great accuracy is foolish, because precision is unattainable in biological and social statistics; their results being never strictly constant. Over-minuteness is mischievous, because it overwhelms the mind with more details than can be compressed into a single view. We require no more than a fairly just and comprehensive method of expressing the way in which each measurable quality is distributed among the members of any group, whether the group consists of brothers or of members of any particular social, local, or other body of persons, or whether it is co-extensive with an entire nation or race.

A knowledge of the distribution of any quality enables us to ascertain the Rank that each man holds among his fellows, in respect to that quality. This is a valuable piece of knowledge in this struggling and competitive world, where success is to the foremost, and failure to the hindmost, irrespective of absolute efficiency. A blurred vision would be above all price to an individual man in a nation of blind men, though it would hardly enable him to earn his bread elsewhere. When the distribution of any faculty has been ascertained, we can tell from the measurement, say of our child, how he ranks among other children in respect to that faculty, whether it be a physical gift, or one of health, or of intellect, or of morals. As the years go by, we may learn by the same means whether he is making his way towards the front, whether he just holds his place, or whether he is falling back towards the rear. Similarly as regards the position of our class, or of our nation, among other classes and other nations.

Schemes of Distribution and their Grades.—I shall best explain my graphical method of expressing Distribution, which I like the more, the more I use it, and which I have latterly developed, by showing how to determine the Grade of an individual among his fellows in respect to any particular faculty. Suppose that we have already put on record the measures of many men in respect to Strength, exerted as by an archer in pulling his bow, and tested by one of Salter's well-known dial instruments with a movable index. Some men will have been found strong and others weak, how can we picture in a compendious diagram, or how can we define by figures, the distribution of this faculty of Strength throughout the group? How shall we determine and specify the Grade that any particular person would occupy in the group? The first step is to marshal our measures in the orderly way familiar to statisticians, which is shown in Table I. I usually work to about twice its degree of minuteness, but enough has been entered in the Table for the purpose of illustration, while its small size makes it all the more intelligible.

The fourth column of the Table headed "Percentages" of "Sums from the beginning," is pictorially translated into Fig. 2, and the third column headed "Percentages" of "No. of cases observed," into Fig. 3. The scale of

FIG. 2.

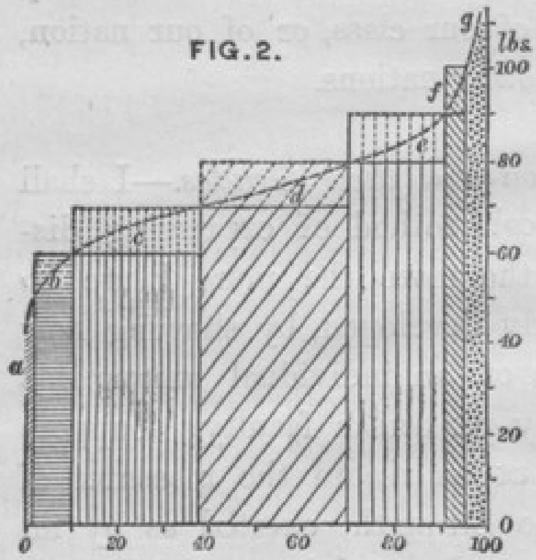
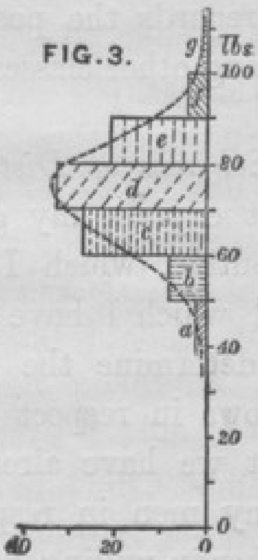


FIG. 3.

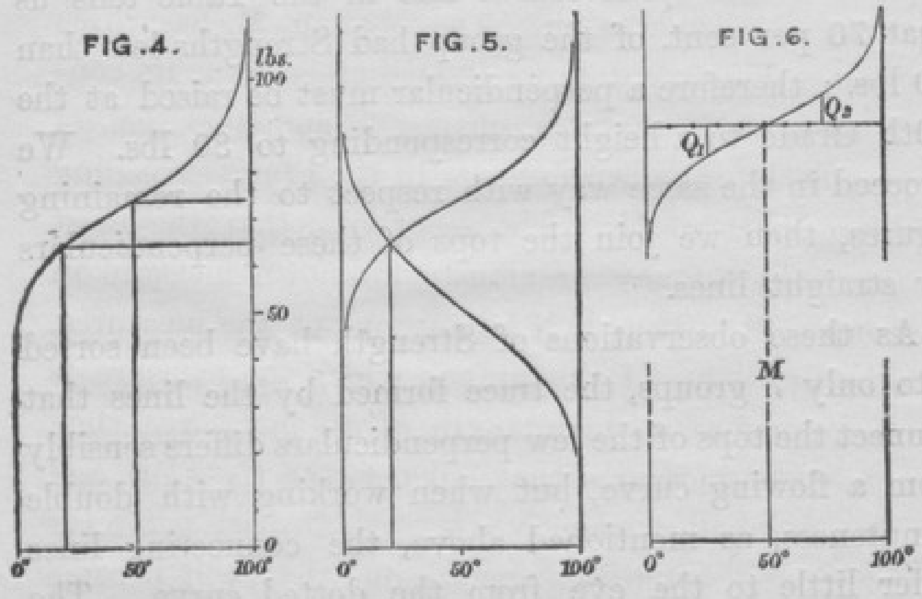


lbs. is given at the side of both Figs.: and the compartments *a* to *g*, that are shaded with *broken* lines, have the same meaning in both, but they are differently disposed in the two Figs. We will now consider Fig. 2 only, which is the one that principally concerns us. The percentages in the last column of Table I. have been marked off on the bottom line of Fig. 2, where they are called (centesimal) Grades. The number of lbs. found in the first column of the Table determines the height of the vertical lines to be erected at the corresponding Grades when we are engaged in constructing the Fig.

Let us begin with the third line in the Table for illustration: it tells us that 37 per cent. of the group had Strengths less than 70 lbs. Therefore, when drawing the figure, a perpendicular must be raised at the 37th grade to a height corresponding to that of 70 lbs. on the side scale. The fourth line in the Table tells us that 70 per cent. of the group had Strengths less than 80 lbs.; therefore a perpendicular must be raised at the 70th Grade to a height corresponding to 80 lbs. We proceed in the same way with respect to the remaining figures, then we join the tops of these perpendiculars by straight lines.

As these observations of Strength have been sorted into only 7 groups, the trace formed by the lines that connect the tops of the few perpendiculars differs sensibly from a flowing curve, but when working with double minuteness, as mentioned above, the connecting lines differ little to the eye from the dotted curve. The dotted curve may then be accepted as that which would result if a separate perpendicular had been drawn for every observation, and if permission had been given to slightly smooth their irregularities. I call the figure that is bounded by such a curve as this, a Scheme of Distribution; the perpendiculars being first rubbed out that formed the scaffolding by which it was constructed. (See Fig 4.)

A Scheme enables us in a moment to find the Grade of Rank (on a scale reckoned from 0° to 100°) of any person in the group to which he belongs. The measured strength of the person is to be looked for in the side scale of the Scheme; a horizontal line is thence drawn until it meets the curve; from the point of meeting a perpendicular is dropped upon the scale of Grades at the base; then the Grade on which it falls is



the one required. For example: let us suppose the Strength of Pull of a man to have been 74 lbs., and that we wish to determine his Rank in Strength among the large group of men who were measured at the Health Exhibition in 1884. We find by Fig. 4 that his centesimal Grade is 50°; in other words, that 50 per cent. of the group will be weaker than he is, and 50 per cent. will be stronger. His position will be exactly Middlemost, after the Strengths of all the men in the group have been marshalled in the order of their magnitudes. In other words, he is of mediocre strength. The accepted term to express the value that occupies the Middlemost position is "Median," which may be used either as an adjective or as a substantive, but it will be usually replaced in this book by the abbreviated form M. I also use the word "Mid" in a few combinations, such as "Mid-Fraternity," to express the same thing. The Median, M, has three properties. The first follows immediately from its construction, namely, that the chance is an equal one, of any previously unknown measure in the group exceeding or falling short of M. The second is, that the most probable value of any previously unknown measure in the group is M. Thus if N be any one of the measures, and u be the value of the unit in which the measure is recorded, such as an inch, tenth of an inch, &c., then the number of measures that fall between $(N - \frac{1}{2}u)$ and $(N + \frac{1}{2}u)$, is greatest when $N = M$. Mediocrity is always the commonest condition, for reasons that will become apparent later on. The third property is that whenever the curve of the Scheme is symmetrically disposed on either side of M, except that one half of it is turned upwards, and the other half downwards, then M is identical with the ordinary Arithmetic Mean or Average. This is closely the condition of all the curves I have to discuss. The reader may look on the Median and on the Mean as being practically the same things, throughout this book.

It must be understood that M, like the Mean or the Average, is almost always an interpolated value, corresponding to no real measure. If the observations were infinitely numerous its position would not differ more than infinitesimally from that of some one of them; even in a~~n~~ ~~ordinary~~ series of ~~a few~~ one or two hundred in number, the difference is insignificant.

Now let us make our Scheme answer another question. Suppose we want to know the percentage of men in the group of which we have been speaking, whose Strength lies between any two specified limits, as between 74 lbs. and 64 lbs. We draw horizontal lines (Fig. 4) from points on the side scale corresponding to either limit, and drop perpendiculars upon the base, from the points where those lines meet the curve. Then the number of Grades in the intercept, is the answer. The Fig. shows that the number in the present case is 30; therefore 30 per cent. of the group have Strengths of Pull ranging between 74 and 64 lbs.

We learn how to transmute female measures of any characteristic into male ones, by comparing their respective schemes, and devising a formula that will change the one into the other. In the case of Stature, the the simple multiple of 1·08 was found to do this with sufficient precision.

If we wish to compare the average Strengths of two different groups of persons, say one consisting of men and the other of women, we have simply to compare the values at the 50th Grades in the two schemes. For even if the Medians differ considerably from the Means,

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both the ratios and the differences between either pair of values would be sensibly the same.

A different way of comparing two Schemes is sometimes useful. It is to draw them in opposed directions, as in Fig. 5, p. 40. Their curves will then cut each other at some point, whose Grade when referred to either of the two Schemes (whichever of them may be preferred), determines the point at which the same values are to be found. In Fig. 5, the Grade in the one Scheme is 20°; therefore in the other Scheme it is 100° − 20°, or 80°. In respect to the Strength of Pull of men and women, it appears that the woman who occupies the Grade of 96° in her Scheme, has the same strength as the man who occupies the Grade of 4° in his Scheme.

I should add that this great inequality in Strength between the sexes, is confirmed by other measurements made at the same time in respect to the Strength of their Squeeze, as tested by another of Salter's instruments. Then the woman in the 93rd and the man in the 7th Grade of their resective Schemes, proved to be of equal strength. In my paper[1] on the results obtained at the laboratory, I remarked: "Very powerful women exist, but happily perhaps for the repose of the other sex such gifted women are rare. Out of 1,657 adult women of all ages measured at the laboratory, the strongest could only exert a squeeze of 86 lbs., or about that of a medium man."

[1] *Journ. Anthropol. Inst.* 1885. *Mem.:* There is a blunder in the paragraph, p. 23, headed "Height Sitting and Standing." The paragraph should be struck out.

The Shape of Schemes is Independent of the Number of Observations.—When Schemes are drawn from different samples of the same large group of measurements, though the number in the several samples may differ greatly, we can always so adjust the horizontal scales that the breadth of the several Schemes shall be uniform. Then the shapes of the Schemes drawn from different samples will be little affected by the number of observations used in each, supposing of course that the numbers are never too small for ordinary statistical purposes. The only recognisable differences between the Schemes will be, that, if the number of observations in the sample is very large, the upper margin of the Scheme will fall into a more regular curve, especially towards either of its limits. Some irregularity will be found in the above curve of the Strength of Pull; but if the observations had been ten times more numerous, it is probable, judging from much experience of such curves, that the irregularity would have been less conspicuous, and perhaps would have disappeared altogether.

However numerous the observations may be, the curve will always be uncertain and incomplete at its extreme ends, because the next value may happen to be greater or less than any one of those that preceded it. Again, the position of the first and the last observation, supposing each observation to have been laid down separately, can never coincide with the adjacent limit. The more numerous the observations, and therefore the closer the perpendiculars by which they are represented, the nearer will the two extreme perpendiculars approach the limits, but they will never actually touch them. A chess board has eight squares in a row, and eight pieces may be arranged in order on any one row, each piece occupying the centre of a square. Let the divisions in the row be graduated, calling the boundary to the extreme left, 0°. Then the successive divisions between the squares will be 1°, 2°, 3°, up to 7°, and the boundary to the extreme right will be 8°. It is clear that the position of the first piece lies half-way between the grades (in a scale of eight grades) of 0° and 1°; therefore the grade occupied by the first piece would be counted on that scale as 0·5°; also the grade of the last piece as 7·5°. Or again, if we had 800 pieces, and the same number of class-places, the grade of the first piece, in a scale of 800 grades, would exceed the grade 0°, by an amount equal to the width of one half-place on that scale, while the last of them would fall short of the 800th grade by an equal amount. This half-place has to be attended to and allowed for when schemes are constructed from comparatively few observations, and always when values that are very near to either of the centesimal grades 0° or 100° are under observation; but between the centesimal grades of 5° and 95° the influence of a half class-place upon the value of the corresponding observation is insignificant, and may be disregarded. It will not henceforth be necessary to repeat the word centesimal. It will be always implied when nothing is said to the contrary, and nothing henceforth will be said to the contrary except once in the next paragraph.

Data for Eighteen Schemes.—Sufficient data for reconstructing any Scheme, with much correctness, may be printed in a single line of a Table, and according to a uniform plan that is suitable for any kind of values. The measures to be recorded are those at a few definite Grades, beginning say at 5°, ending at 95°, and including every intermediate tenth Grade from 10° to 90°. It is convenient to add those at the Grades 25° and 75°, if space permits. The former values are given for eighteen different Schemes, in Table 2. In the memoir from which this table is reprinted, the values at what I now call (centesimal) Grades, were termed Percentiles. Thus the values at the Grades 5° and 10° would be respectively the 5th and the 10th percentile. It still seems to me that the word percentile is a useful and expressive abbreviation, but it will not be necessary to employ it in the present book. It is of course unadvisable to use more technical words than is absolutely necessary, and it will be possible to get on without it, by the help of the new and more important word "Grade."

A series of Schemes that express the distribution of various faculties, is valuable in an anthropometric laboratory, for they enable every person who is measured to find his Rank or Grade in each of them.

Diagrams may also be constructed by drawing parallel lines, each divided into 100 Grades, and entering each round number of inches, lbs., &c., at their proper places. A diagram of this kind is very convenient for reference, but it does not admit of being printed; it must be drawn or lithographed. I have constructed one of these

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from the 18 Schemes, and find it is easily understood and much used at my laboratory.

Application of Schemes to Inexact Measures.—Schemes of Distribution may be constructed from observations that are barely exact enough to deserve to be called measures.

I will illustrate the method of doing so by marshalling the data contained in a singularly interesting little memoir written by Sir James Paget, into the form of such a Scheme. ~~It~~ The memoir is published in vol. v. of St. Bartholomew's Hospital Reports, and is entitled "What Becomes of Medical Students." He traced with great painstaking the career of no less than 1,000 pupils who had attended his classes at that Hospital during various periods and up to a date 15 years previous to that at which his memoir was written. He thus did for St. Bartholomew's Hospital what has never yet been done, so far as I am aware, for any University or Public School, whose historians count the successes and are silent as to the failures, giving to inquirers no adequate data for ascertaining the real value of those institutions in English Education. Sir J. Paget divides the successes of his pupils in their profession into five grades, all of which he carefully defines; they are *distinguished; considerable; moderate; very limited success;* and *failures.* Several of the students had left the profession either before or after taking their degrees, usually owing to their unfitness to succeed, so after analysing the accounts of them given in the memoir, I drafted several into the list of failures and distributed the rest, with the result that the number of cases in the successive classes, amounting now to the full total of 1,000, became 28, 80, 616, 151, and 125. This differs, I should say, a little from the inferences of one author, but the matter is here of small importance, so I need not go further into details.

If a Scheme is drawn from these figures, in the way described in page 39, it will be found to have the characteristic shape of our familiar curve of Distribution. If we wished to convey the utmost information that this Scheme is capable of giving, we might record in much detail the career of two or three of the men who are clustered about each of a few selected Grades, selecting those that are used in Table II., or fewer of them. I adopted this method when estimating the variability of the Visualising Power (*Inquiries into Human Faculty*). My data were very lax, but this method of treatment got all the good out of them that they possessed. In the present case, it appears that towards the summit of the successes achieved within fifteen years of taking their degrees, stand the three Professors of Anatomy at Oxford, Cambridge, and Edinburgh; that towards the bottom of the failures, lie two men who committed suicide under circumstances of great disgrace, and lowest of all Palmer, the Rugeley murderer, who was hung.

We are able to compare any two such Schemes as the above, with numerical precision. The want of exactness in the data from which they are drawn, will of course cling to the result, but no new error will be introduced by the process of comparison. Suppose the second Scheme to refer to the successes of students from another hospital, we should draw the two Schemes in opposed directions, just as was done in the Strength of Pull of Males and Females, Fig. 5, and determine the Grade in either of the Schemes at which success was equal.

Schemes of Frequency.—The method of arranging observations in an orderly manner that is generally employed by statisticians, is shown in Fig. 3, page 38, which expresses the same facts as Fig. 2 under a different aspect, and so gives rise to the well-known Curve of "Frequency of Error," though in Fig. 3 the curve is turned at right angles to the position in which it is usually drawn. It is so placed in order to show more clearly its relation to the Curve of Distribution. The Curve of Frequency is far less convenient than that of Distribution, for the purposes just described and for most of those to be hereafter spoken of. But the Curve of Frequency has other uses, of which advantage will be taken later on, but to which it is unnecessary now to refer.

A Scheme as explained thus far, is nothing more than a compendium of a mass of observations which, on being marshalled in an orderly manner, fall into a diagram whose contour is so regular, simple, and bold, as to admit of being described by a few numerals (Table 2.), from which it can at any time be drawn afresh. The regular distribution of the several faculties among a large population is little disturbed by the fact that its

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members are varieties of different types and sub-types. So the distribution of a heavy mass of foliage gives little indication of its growth from separate twigs, of separate branches, of separate trees.

The application of theory to Schemes, their approximate description by only two values, and the properties of their bounding Curves, will be described in the next chapter.

CHAPTER V.

NORMAL VARIABILITY.

Schemes of Deviation.—Normal Curve of Distribution.—Comparison of the observed with the Normal Curve.—The value of a single Deviation at a known Grade determines a Normal Scheme of Deviations.—Two Measures at two known Grades determine a Normal Scheme of Measures.—The Charms of Statistics.—Mechanical illustration of the Cause of the Curve of Frequency.—Order in apparent Chaos.—Problems in the Law of Error.

Schemes of Deviations.—We have now seen how easy it is to represent the distribution of any quality among a multitude of men, either by a simple diagram or by a line containing a few figures. In this chapter it will be shown that a considerably briefer description is approximately sufficient.

Every measure in a Scheme is equal to its Middlemost, or Median value, or M, a certain *plus* or *minus* Deviation from M. The Deviation, or "Error" as it is technically called, is *plus* for all grades above 50°, zero for 50°, and *minus* for all grades below 50°. Thus if (±D) be the deviation from M in any particular case, every measure in a Scheme may be expressed in the form of $M + (\pm D)$. If $M = O$, or if it is subtracted from every measure, the residues which are the different values of $(\pm D)$ will form a Scheme by themselves. Schemes may therefore be made of Deviations as well as of Measures, and one of the former is seen in the upper part of Fig. 6, page 40. It is merely the upper portion of the corresponding Scheme of Measures, in which the axis of the curve plays the part of the base.

A strong family likeness runs between the 18 different Schemes of Deviations that may be respectively derived from the data in the 18 lines of Table 2. If the slope of the curve in one Scheme is steeper than that of another, we need only to fore-shorten the steeper Scheme, by inclining it away from the line of sight, in order to reduce its apparent steepness and to make it look almost identical with the other. Or, better still, we may select appropriate vertical scales that will enable all the Schemes to be drawn afresh with a uniform slope, and be made strictly comparable.

Suppose that we have only two Schemes, A. and B., that we wish to compare. Let $L._1$, $L._2$ be the lengths of the perpendiculars at two specified grades in Scheme A., and $K._1$ $K._2$ the lengths of those at the same grades in Scheme B.; then if every one of the data from which Scheme B. was drawn be multiplied by $\frac{L._1 - L._2}{K._1 - K._2}$, a series of transmuted data will be obtained for drawing a new Scheme B., on such a vertical scale that its general slope between the selected grades shall be the same as in Scheme A. For practical convenience the selected Grades will be always those of 25° and 75°. They stand at the first and third quarterly divisions of the base, and are therefore easily found by a pair of compasses. They are also well placed to afford a fair criterion of the general slope of the Curve. If we call the perpendicular at 25°, Q.$_1$; and that at 75°, Q.$_2$, then, the unit by which every Scheme will be defined is ~~the~~ its value ~~in it~~ of $\frac{1}{2}$(Q.$_2$ – Q.$_1$), and will be called its Q. As the M measures the Average Height of the curved boundary of a Scheme, so the Q measures its general slope. When we wish to transformn many different Schemes, numbered I., II., III., &c., whose respective values of Q are q_1, q_2, q_3, &c., to others whose values of Q are in each case equal to q_0, then all the data from which Scheme I. was drawn, must ~~all~~ be multiplied by $\frac{q_0}{q_1}$; those from which Scheme II. was drawn, by $\frac{q_0}{q_2}$, and so on, and new Schemes have to be constructed from these transmuted values.

Our Q has the further merit of being practically the same as the value which mathematicians call the "Probable Error," of which we shall speak further on.

Want of space in Table 2 prevented the insertion of the measures at the Grades 25° and 75°, but those at 20° and 30° are given on the one hand, and those at 70° and 80° on the other, whose respective averages differ but little from the values at 25° and 75°. I therefore will use those four measures to obtain a value for our unit, which we will call Q′, to distinguish it from Q.

These are not identical in value, because the outline of the Scheme is a curved and not a straight line, but the difference between them is small, and is approximately the same in all Schemes. It will shortly be seen that $Q' = 1{\cdot}015 \times Q$ approximately; therefore a series of Deviations measured in terms of the large unit Q' are numerically smaller than if they had been measured in terms of the small unit (for the same reason that the numerals in 2, 3, &c., *feet* are smaller than those in the corresponding values of 24, 36, &c., *inches*), and they must be multiplied by 1.015 when it is desired to change them into a series having the smaller value of Q for their unit.

All the 18 Schemes of Deviation that can be derived from Table 2 have been treated on these principles, and the results are given in Table 3. Their general accordance with one another, and still more with the mean of all of them, is obvious.

Normal Curve of Distribution.—The values in the bottom line of Table 3, which is headed "Normal Values when $Q = 1$," and which correspond with minute precision to those in the line immediately above them, are not derived from observations at all, but from the well-known Tables of the "Probability Integral" in a way that mathematicians will easily understand by comparing the Tables 4 to 8 inclusive. I need hardly remind the reader that the Law of Error upon which these Normal Values are based, was excogitated for the use of astronomers and others who are concerned with extreme accuracy of measurement, and without the slightest idea until the time of Quetelet that they might be applicable to human measures. But Errors, Differences, Deviations, Divergencies, Dispersions, and individual Variations, all spring from the same kind of causes. Objects that bear the same name, or can be described by the same phrase, are thereby acknowledged to have common points of resemblance, and to rank as members of the same species, class, or whatever else we may please to call the group. On the other hand, every object has Differences peculiar to itself, by which it is distinguished from others.

This general statement is applicable to thousands of instances. The Law of Error finds a footing wherever the individual peculiarities are wholly due to the combined influence of a multitude of "accidents," in the sense in which that word has already been defined. All persons conversant with statistics are aware that this supposition brings Variability within the grasp of the laws of Chance, with the result that the relative frequency of Deviations of different amounts admits of being calculated, when those amounts are measured in terms of any self-contained unit of variability, such as our Q. The Tables 7, 8, and 9 give the results of these purely mathematical calculations, and the Curves based upon them may with propriety be distinguished as "Normal." Tables 7 and 8 are based upon the familiar Table of the Probability Integral, given in Table 5, *viâ* that in Table 6, in which the unit of variability is taken to be the "Probable Error" or our Q, and not the "Modulus." Then I turn Table 6

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inside out, as it were, deriving the "arguments" for Tables 7 and 8 from the entries in the body of Table 6, and making other easily intelligible alterations.

Comparison of the Observed with the Normal Curve.—I confess to having been amazed at the extraordinary coincidence between the two bottom lines of Table 3, considering the great variety of faculties contained in the 18 Schemes; namely, three kinds of linear measurement, besides one of weight, one of capacity, two of strength, one of vision, and one of swiftness. It is obvious that weight cannot really vary at the same rate as height, even allowing for the fact that tall men are often lanky, but the theoretical impossibility is of the less practical importance, as the variations in weight are small compared to the weight itself. Thus we see from the value of Q in the first column of Table 3, that half of the persons deviated from their M by no more than 10 or 11 lbs., which is about one-twelfth part of ~~its~~ the value of M. Although the several series in Table 3 run fairly well together, I should not have dared to hope that their irregularities would have balanced one another so beautifully as they have done. It has been objected to some of my former work, especially in *Hereditary Genius*, that I pushed the applications of the Law of Frequency of Error somewhat too far. I may have done so, rather by incautious phrases than in reality; but I am sure that, with the evidence now before us, the applicability of that law is more than justified within the reasonable limits asked for in the present book. I am satisfied to claim that the Normal Curve is a fair average representation of the Observed Curves during nine-tenths of their course; that is, for so much of them as lies between the grades of 5° and 95°. In particular, the agreement of the Curve of Stature with the Normal Curve is very fair, and forms a mainstay of my inquiry into the laws of Natural Inheritance.

It has already been said that mathematicians laboured at the law of Error for one set of purposes, and we are entering into the fruits of their labours for another. Hence there is no ground for surprise that their Nomenclature is often cumbrous and out of place, when applied to problems in heredity. This is especially the case with regard to their term of "Probable Error," by which they mean the value that one half of the Errors exceed and the other half fall short of. This is practically the same as our Q.[1] It is strictly the same whenever the two halves of the Scheme of Deviations to which it applies are symmetrically disposed about their common axis.

The term Probable Error, in its plain English interpretation of the *most* Probable Error, is quite misleading, for it is *not* that. The most Probable Error (as Dr. Venn has pointed out, in his *Logic of Chance*)

[1] The following little Table may be of service :—

Values of the different Constants when the Prob. Error is taken as unity, and their corresponding Grades.

Prob. Error	1·000 ;	corresponding Grades	25°·0, 75°·0
Modulus	2·097 ;	„ „	7°·9, 92°·1
Mean Error................	1·183 ;	„ „	21°·2, 78°·8
Error of Mean Squares	1·483 ;	„ „	16°·0, 84°·0

is zero. This results from what was said a few pages back about the most probable measure in a Scheme being its M. In a Scheme of Errors the M is equal to 0, therefore the most probable Error in such a Scheme is 0 also. It is astonishing that mathematicians, who are the most precise and perspicacious of men, have not long since revolted against this cumbrous, slip-shod, and misleading phrase. They really mean what I should call the Mid-Error, but their phrase is too firmly established for me to uproot it. I shall however always write the word Probable when used in this sense, in the form of "Prob."; thus "Prob. Error," as a continual protest against its illegitimate use, and as some slight safeguard against its misinterpretation. Moreover the term Probable Error is absurd when applied to the subjects now in hand, such as Stature, Eye-colour, Artistic Faculty, or Disease. I shall therefore usually speak of Prob. Deviation.

Though the value of our Q is the same as that of the Prob. Deviation, Q is not a convertible term with Prob. Deviation. We shall often have to speak of the one without immediate reference to the other, just as we speak of the diameter of the circle without reference to any of its properties, such as, if lines are drawn from its ends to any point in the circumference, they will meet at a right angle. The Q of a Scheme is as definite a phrase as the Diameter of a Circle, but we cannot replace Q even in that phrase by the words Prob. Deviation, and speak of the Prob. Deviation of a Scheme, without doing some violence to language. We

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should have to express ourselves from another point of view, and at much greater length, and say "the Prob. Deviation of any, as yet unknown measure in the Scheme, from the Mean of all the measures from which the Scheme was constructed."

The primary idea of Q has no reference to the existence of a Mean value from which Deviations take place. It is half the difference between the measures found at the 25th and 75th Centesimal Grades. In this definition there is not the slightest allusion, direct or indirect, to the measure at the 50th Grade, which is the value of M. When a Scheme is normal, the measure at Grade 25° is M—Q, and that at Grade 75° is M + Q, but all this is superimposed upon the primary conception. Q stands essentially on its own basis, and has nothing to do with M. It will often happen that we shall have to deal with Prob: Deviations, but that is no reason why we should not use Q whenever it suits our purposes better, especially as statistical statements tend to be so cumbrous that every abbreviation is welcome.

The stage to which we have now arrived is this. It has been shown that the distribution of very different human qualities and faculties is approximately Normal, and it is inferred that with reasonable precautions we may treat them as if they were wholly so, in order to obtain approximate results. We shall thus deal with an entire Scheme of Deviations in terms of its Q, and with an entire Scheme of Measures in terms of its M and Q, just as we deal with an entire Circle in terms of its radius, or with an entire Ellipse in terms of its major and minor axes. We can also apply the various beautiful properties of the Law of Frequency of Error to the observed values of Q. In doing so, we act like woodsmen who roughly calculate the cubic contents of the trunk of a tree, by measuring its length, and its girth at either end, and submitting their measures to formulæ that have been deduced from the properties of ideally perfect straight lines and circles. Their results prove serviceable, although the trunk is only rudely straight and circular. I trust that my results will be yet closer approximations to the truth than those usually arrived at by the woodsmen.

The value of a single Deviation at a known Grade determines a Normal Scheme of Deviations.—When Normal Curves of Distribution are drawn within the same limits, they differ from each other only in their general slope; and the slope is determined if the value of the Deviation is given at any one specified Grade. It must be borne in mind that the width of the limits between which the Scheme is drawn, has no influence on the values of the Deviations at the various Grades, because the latter are proportionate parts of the base. As the limits vary in width, so do the intervals between Grades. When measuring the Deviation at a specified Grade for the purpose of determining the whole Curve, it is of course convenient to adhere to the same Grade in all cases. It will be recollected that when dealing with the observed curves a few pages back, I

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used not one Grade but two Grades for the purpose, namely 25° and 75°; but in the Normal Curve, the *plus* and *minus* Deviations are equal in amount at all pairs of symmetrical distances on either side of grade 50°; therefore the Deviation at either of the Grades 25° or 75° is equal to Q, and suffices to define the entire Curve, ~~as completely as a radius defines an entire circle.~~ [The reason why a certain value Q′ was stated a few pages back to be equal to 1·015 Q, is that the Normal Deviations at 20° and at 30°, (whose average we called Q′) are found in Table 8, to be 1·25 and 0·78; and similarly those at 70° and 60°. The average of 1·25 and 0·78 is 1·015, whereas the Deviation at 25° or at 75° is 1·000.

Two Measures at known Grades determine a Normal Scheme of Measures.—If we know the value of M as well as that of Q we know the entire Scheme. M expresses the mean value of all the objects contained in the group, and Q defines their variability. But if we know the Measures at any two specified Grades, we can deduce M and Q from them, and so determine the entire Scheme. The method of doing this is explained in the foot-note.[1]

[1] The following is a fuller description of the~~se two leading~~ propositions in this and in the preceding paragraph:—

(1) In any Normal Scheme, and therefore approximately in an observed one, if the value of the Deviation is given at any *one* specified Grade the whole Curve is determined. Let D be the given Deviation, and d the tabular Deviation at the same Grade, as found in Table 8; then multiply every entry in Table 8 by $\frac{D}{d}$. As the tabular value of Q is 1, it will be changed by the multiplication into $\frac{D}{d}$.

The Charms of Statistics.—It is difficult to understand why statisticians commonly limit their inquiries to Averages, and do not revel in more comprehensive views. Their souls seem as dull to the charm of variety as that of the native of one of our flat English counties, whose retrospect of Switzerland was that, if its mountains could be thrown into its lakes, two nuisances would be got rid of at once. An Average is but a solitary fact, whereas if a single other fact be added to it, an entire Normal Scheme, which nearly corresponds to the observed one, starts potentially into existence.

Some people hate the very name of statistics, but I find them full of beauty and interest. Whenever they are not brutalised, but delicately handled by the higher methods, and are warily interpreted, their power of dealing with complicated phenomena is extraordinary. They are the only tools by which an opening can be cut

(2) If the Measures at any *two* specified Grades are given, the whole Scheme of Measures is thereby determined. Let A, B be the two given Measures of which A is the larger, and let a, b be the values of the tabular Deviations for the same Grades, as found in Table 8, not omitting their signs of *plus* or *minus* as the case may be.

Then the Q of the Scheme $= \pm\frac{A-B}{a-b}$. (The sign of Q is not to be regarded; it is merely a magnitude.)

$$M = A - aQ \text{; or } M = B + bQ.$$

Example: A, situated at Grade 55°, = 14·38
B, situated at Grade 5°, = 9·12

The corresponding tabular Deviations are:—$a = +0·19$; $b = -2·44$.

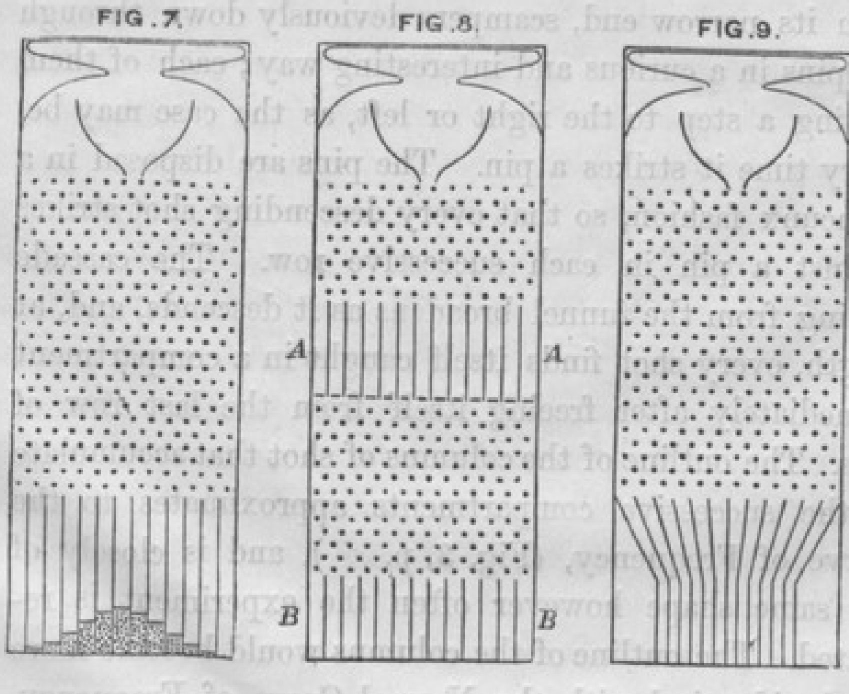
$$\text{Therefore } Q = \frac{14·38 - 9·12}{0·19 + 2·44} = \frac{5·26}{2·63} = 2·0$$

$$M = 14.38 - 0·19 \times 2 = 14·0$$

$$\text{or } = 9·12 + 2·44 \times 2 = 14·0$$

through the formidable thicket of difficulties that bars the path of those who pursue the Science of man.

Mechanical Illustration of the Cause of the Curve of Frequency.—The Curve of Frequency, and that of Distribution, are convertible: therefore if the genesis of either of them can be made clear, that of the other becomes also intelligible. I shall now illustrate the origin of the Curve of Frequency, by means of an apparatus shown in Fig. 7. that mimics in a very pretty way the conditions



on which Deviation depends. It is a frame glazed in front, leaving a depth of about a quarter of an inch behind the glass. Strips are placed in the upper part to act as a funnel. Below the outlet of the funnel stand a succession of rows of pins stuck squarely into the back-board, and below these again are a series of vertical compartments. A charge of small shot is enclosed. When the frame is held topsy-turvy, all the shot runs to the upper end; then, when it is turned back into its working position, the desired action commences. Lateral strips, shown in the diagram, have the effect of directing all the shot that had collected at the upper end of the frame to run into the wide mouth of the funnel. The shot passes through the funnel and issuing from its narrow end, scampers deviously down through the pins in a curious and interesting way; each of them darting a step to the right or left, as the case may be, every time it strikes a pin. The pins are disposed in a quincunx fashion, so that every descending shot strikes against a pin in each successive row. The cascade issuing from the funnel broadens as it descends, and, at length, every shot finds itself caught in a compartment immediately after freeing itself from the last row of pins. The outline of the columns of shot that accumulate in the successive compartments approximates to the Curve of Frequency, (Fig. 3, p. 38), and is closely of the same shape however often the experiment is repeated. The outline of the columns would become more nearly identical with the Normal Curve of Frequency, if the rows of pins were much more numerous, the shot smaller, and the compartments narrower; also if a larger quantity of shot was used.

The principle on which the action of the apparatus depends is, that a number of small and independent accidents befall each shot in its career. In rare cases, a long run of luck continues to favour the course of a particular shot towards either outside place, but in the large majority of instances the number of accidents that cause Deviation to the right, balance in a greater or less degree those that cause Deviation to the left. Therefore most of the shot finds its way into the compartments that are situated near to a perpendicular line drawn from the outlet of the funnel, and the Frequency with which shots stray to different distances to the right or left of that line diminishes in a much faster ratio than those distances increase. This illustrates and explains the reason why mediocrity is so common.

If a larger quantity of shot is put inside the apparatus, the resulting curve will be more humped, but one half of the shot will still fall within the same distance as before, reckoning to the right and left of the perpendicular line that passes through the mouth of the funnel. This distance, which does not vary with the quantity of the shot, is the "Prob: Error," or "Prob: Deviation," of any single shot, and has the same value as our Q. But a Scheme of Frequency is unsuitable for finding the values of either M or Q. To do so, we must divide its strangely shaped *area* into four equal parts by vertical lines, which is hardly to be effected except by a tedious process of "Trial and Error." On the other hand M and Q can be derived from Schemes of Distribution with no more trouble than is needed to divide a *line* into four equal parts.

Order in Apparent Chaos.—I know of scarcely anything so apt to impress the imagination as the wonderful form of cosmic order expressed by the "Law of Frequency of Error." The law would have been personified by the Greeks and deified, if they had known of it. It reigns with serenity and in complete self-effacement amidst the wildest confusion. The huger the mob, and the greater the apparent anarchy, the more perfect is its sway. It is the supreme law of Unreason. Whenever a large sample of chaotic elements are taken in hand and marshalled in the order of their magnitude, an unsuspected and most beautiful form of regularity proves to have been latent all along. The tops of the marshalled row form a flowing curve of invariable proportions; and each element, as it is sorted into place, finds, as it were, a pre-ordained niche, accurately adapted to fit it. If the measurement at any two specified Grades in the row are known, those that will be found at every other Grade, except towards the extreme ends, can be predicted in the way already explained, and with much precision.

Problems in the Law of Error.—All the properties of the Law of Frequency of Error can be expressed in terms of Q, or of the Prob: Error, just as those of a circle can be expressed in terms of its radius. The visible Schemes are not, however, to be removed too soon from our imagination. It is always well to retain a clear geometric view of the facts when we are dealing with statistical problems, which abound with dangerous pitfalls, easily overlooked by the unwary, while they are cantering gaily along upon their arithmetic. The Laws of Error are beautiful in themselves and exceedingly fascinating to inquirers, owing to the thoroughness and simplicity with which they deal with masses of materials that appear at first sight to be entanglements on the largest scale, and of a hopelessly confused description. I will mention five of the laws.

(1) The following is a mechanical illustration of the first of them. In the apparatus already described, let q stand for the Prob: Error of any one of the shots that are dispersed among the compartments BB at its base. Now cut the apparatus in two parts, horizontally through the rows of pins. Separate the parts and interpose a row of vertical compartments AA, as in Fig. 8, page 63, where the bottom compartments, BB, corresponding to those shown in Fig. 7, are reduced to half their depth, in order to bring the whole figure within the same sized outline as before. The compartments BB are still deep enough for their purpose. It is clear that the interpolation of the AA compartments can have no ultimate effect on the final dispersion of the shot into those at BB. Now close the bottoms of all the AA compartments; then the shot that falls from the funnel will be retained in them, and will be comparatively little dispersed. Let the Prob: Error of a shot in the AA compartments be called a. Next, open the bottom of any one of the AA compartments; then the shot it contains will cascade downwards and disperse themselves among the BB compartments on either side of the perpendicu-

lar line drawn from its starting point, and each shot will have a Prob: Error that we will call b. Do this for all the AA compartments in turn; b will be the same for all of them, and the final result must be to reproduce the identically same system in the BB compartments that was shown in Fig. 7, and in which each shot had a Prob: Error of q.

The dispersion of the shot at BB may therefore be looked upon as compounded of two superimposed and independent systems of dispersion. In the one, when acting singly, each shot has a Prob: Error of a; in the other, when acting singly, each shot has a Prob: Error of b, and the result of the two acting together is that each shot has a Prob: Error of q. What is the relation between a, b, and q? Calculation shows that $q^2 = a^2 + b^2$. In other words, q corresponds to the hypothenuse of a right-angled triangle of which the other two sides are a and b respectively.

(2) It is a corollary of the foregoing that a system Z, in which each element is the Sum of a couple of independent Errors, of which one has been taken at random from a Normal system A and the other from a Normal system B, will itself be Normal.[1] Calling the Q of the Z system q, and the Q of the A and B systems respectively, a and b, then $q^2 = a^2 + b^2$.

[1] We may see the rationale of this corollary if we invert part of the statement of the problem. Instead of saying that an A element deviates from its M, and that a B element also deviates independently from its M, we may phrase it thus: An A element deviates from its M, and its M deviates from the B element. Therefore the deviation of the B element from the A element is compounded of two independent deviations, as in Problem 1.

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(3). Suppose that a row of compartments, whose upper openings are situated like those in Fig. 7, page 63, are made first to converge towards some given point below, but that before reaching it their sloping course is checked and they are thenceforward allowed to drop vertically as in Fig. 9. The effect of this will be to compress the heap of shot laterally; its outline will still be a Curve of Frequency, but its Prob: Error will be diminished.

The foregoing three properties of the Law of Error are well known to mathematicians and require no demonstration here, but two other properties that are not familiar will be of use also; proofs of them by Mr. J. Hamilton Dickson are given in Appendix B. They are as follows. I purposely select a different illustration to that used in the Appendix, for the sake of presenting the same general problem under more than one of its applications.

(4) Bullets are fired by a man who aims at the centre of a target, which we will call its M, and we will suppose the marks that the bullets make to be painted red, for the sake of distinction. The system of lateral deviations of these red marks from the centre M will be approximately Normal, whose Q we will call c. Then another man takes aim, not at the centre of the target, but at one or other of the red marks, selecting these at random. We will suppose his shots to be painted green. The lateral distance of any green shot from the red mark at which it was aimed will have a Prob: Error that we

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will call b. Now, if the lateral distance of a particular green mark from M is given, what is the *most probable* distance from M of the red mark at which it was aimed? It is $\sqrt{\frac{c^2}{c^2+b^2}}$.

(5) What is the Prob: Error of this determination? In other words, if estimates have been made for a great many distances founded upon the formula in (4), they would be correct on the average, though erroneous in particular cases. The errors thus made would form a normal system whose Q it is desired to determine. Its value is $\frac{bc}{\sqrt{(b^2+c^2)}}$.

By the help of these five problems the statistics of heredity become perfectly manageable. It will be found that they enable us to deal with Fraternities, Populations, or other Groups, just as if they were units. The largeness of the number of individuals in any of our groups is so far from scaring us, that they are actually welcomed as making the calculations more sure and none the less simple.

CHAPTER VI.

DATA.

Records of Family Faculties, or R. F. F. data.—Special Data.—Measures at Anthropometric Laboratory.—Experiments on Sweet Peas.

I had to collect all my data for myself, as nothing existed, so far as I know, that was suitable even to my primary purpose. This was to obtain records of at least two successive generations of some population of considerable size. They must have lived under conditions that were of a usual kind, and in which no great varieties of nature were to be found. Natural selection must have had little influence on the characteristics that were to be examined. These must be variable, measurable, and fairly constant in the same individual. The result of numerous inquiries, made of the most competent persons, was that I began my experiments many years ago on the seeds of sweet peas, and that at the present time I am breeding moths, as will be explained in a later chapter, but this book refers to a human population, which, take it all in all, is the easiest to work with when the data are once obtained, to say nothing of its being more interesting by far than one of sweet peas or of moths.

Record of Family Faculties, or R.F.F. data.—The source from which the larger part of my data is derived consists of a ~~large~~ valuable collection of "Records of Family Faculties," obtained through the offer of prizes. They have been much tested and cross-tested, and have borne the ordeal very fairly, so far as it has been applied. It is well to reprint the terms of the published offer, in order to give a just idea of the conditions under which they were compiled. It was as follows:

"Mr. Francis Galton offers 500*l.* in prizes to those British Subjects resident in the United Kingdom who shall furnish him before May 15, 1884, with the best Extracts from their own Family Records.

"These Extracts will be treated as confidential documents, to be used for statistical purposes only, the insertion of names of persons and places being required solely as a guarantee of authenticity and to enable Mr. Galton to communicate with the writers in cases where further question may be necessary.

"The value of the Extracts will be estimated by the degree in which they seem likely to facilitate the scientific investigations described in the preface to the "Record of Family Faculties."

"More especially:

"(*a*) By including every direct ancestor who stands within the limits of kinship there specified.

"(*b*) By including brief notices of the brothers and sisters (if any) of each of those ancestors. (Importance will be attached both to the completeness with which each family of brothers and sisters is described, and also to the number of persons so described.)

"(*c*) By the character of the evidence upon which the information is based.

"(*d*) By the clearness and conciseness with which the statements and remarks are made.

"The Extracts must be legibly entered either in the tabular forms contained in the copy of the "Record of Family Faculties" (into which, if more space is wanted, additional pages may be stitched), or they may be written in any other book with pages of the same size as those of the Record, provided that the information be arranged in the same tabular form and order. (It will be obvious that uniformity in the arrangement of documents is of primary importance to those who examine and collate a large number of them).

"Each competitor must furnish the name and address of a referee of good social standing (magistrate, clergyman, lawyer, medical practitioner, &c.), who is personally acquainted with his family, and of whom inquiry may be made, if desired, as to the general trustworthiness of the competitor.

"The Extracts must be sent prepaid and by post, addressed to Francis Galton, 42 Rutland Gate, London, S.W. It will be convenient if the letters 'R.F.F.' (Record of Family Faculties) be written in the left-hand corner of the parcel, below the address.

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"The examination will be conducted by the donor of the prizes, aided by competent examiners.

"The value of the individual prizes cannot be fixed beforehand. No prize will, however, exceed 50*l.*, nor be less than 5*l.*, and 500*l.* will on the whole be awarded.

"A list of the gainers of the prizes will be posted to each of them. It will be published in one or more of the daily newspapers, also in at least one clerical, and one medical Journal."

The number of Family Records sent in reply to this offer, that deserved to be seriously considered before adjudging the prizes, barely reached 150; 70 of these being contributed by males, 80 by females. The remainder were imperfect, or they were marked "not for competition," but at least 10 of these have been to some degree utilised. The 150 Records were contributed by persons of very various ranks. After classing the female writers according to the profession of their husbands, if they were married, or according to that of their fathers, if they were unmarried, I found that each of the following 7 classes had 20 or somewhat fewer representatives: (1) Titled persons and landed gentry; (2) Army and Navy; (3) Church (various denominations); (4) Law; (5) Medicine; (6) Commerce, higher class; (7) Commerce, lower class. This accounts for nearly 130 of the writers of the Records; the remainder are land agents, farmers, artisans, literary men, schoolmasters, clerks, students, and one domestic servant in a family of position.

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Three cases occurred in which the Records sent by different contributors overlapped. The details are complicated, and need not be described here, but the result is that five persons have been adjudged smaller prizes than they individually deserved.

Every one of the replies refers to a very large number of persons, as will easily be understood if the fact is borne in mind that each individual has 2 parents, 4 grandparents, and 8 great parents; also that he and each of those 14 progenitors had usually brothers and sisters, who were included in the inquiry. The replies were unequal in merit, as might have been expected, but many were of so high an order that I could not justly select a few as recipients of large prizes to the exclusion of the rest. Therefore I divided the sum into two considerable groups of small prizes, all of which were well deserved, regretting much that I had none left to award to a few others of nearly equal merit to some of those who had been successful. The list of winners is reproduced below, the four years that have elapsed have of course made not a few changes in the addresses, which are not noticed here.

LIST OF AWARDS.

A Prize of £7 was awarded to each of the 40 following Contributors.

Amphlett, John, Clent, Stourbridge; Batchelor, Mrs. Jacobstow Rectory, Stratton, N. Devon; Bathurst, Miss K., Vicarage, Biggleswade, Bedfordshire; Beane, Mrs. C. F., 3 Portland Place, Venner Road, Sydenham; Berisford, Samuel, Park Villas, Park Lane, Macclesfield; Carruthers, Mrs., Brightside, North Finchley; Carter, Miss Jessie E., Hazelwood, The Park, Cheltenham; Cay, Mrs., Eden House, Holyhead; Clark, J. Edmund,

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Feversham Terrace, York; Cust, Lady Elizabeth, 13 Eccleston Square, S.W.; Fry, Edward, Portsmouth, 5 The Grove, Highgate, N.; Gibson, G. A., M.D., 1 Randolph Cliff, Edinburgh; Gidley, B. Courtenay, 17 Ribblesdale Road, Hornsey, N.; Gillespie, Franklin, M.D., 1 The Grove, Aldershot; Griffith-Boscawen, Mrs., Trevalyn Hall, Wrexham; Hardcastle, Henry, 38 Eaton Square, S.W.; Harrison, Miss Edith, 68 Gloucester Place, Portman Square, W.; Hobhouse, Mrs. 4 Kensington Square, W.; Holland, Miss, Ivymeath, Snodland, Kent; Hollis, George, Dartmouth House, Dartmouth Park Hill, N.; Ingram, Mrs. Ades, Chailey, Lewis, Sussex; Johnstone, Miss C. L., 3 Clarendon Place, Leamington; Lane-Poole, Stanley, 6 Park Villas East, Richmond, Middlesex; Leathley, D. W. B., 59 Lincoln's Inn Fields (in trust for a competitor who desires her name not to be published); Lewin, Lieutenant-Colonel T. H., Colway Lodge, Lyme Regis; Lipscomb, R. H., East Budleigh, Budleigh Salterton, Devon; Malden, Henry C., Windlesham House, Brighton; Malden, Henry Elliot, Kitland, Holmwood, Surrey; McCall, Hardy Bertram, 5 St. Augustine's Road, Edgbaston, Birmingham; Moore, Miss Georgina M., 45 Chepstow Place, Bayswater, W.; Newlands, Mrs., Raeden, near Aberdeen; Pearson, David R., M.D., 23 Upper Phillimore Place, Kensington, W.; Pearson, Mrs., The Garth, Woodside Park, North Finchley: Pechell, Hervey Charles, 6 West Chapel Street, Curzon Street, W.; Roberts, Samuel, 21 Roland Gardens, S.W.; Smith, Mrs. Archibald, Riverbank, Putney, S.W.; Strachey, Mrs. Fowey Lodge, Clapham Common, S.W.; Sturge, Miss Mary C., Chilliswood, Tyndall's Park, Bristol; Sturge, Mrs. R. F., 101 Pembroke Road, Clifton; Wilson, Edward T., M.D., Westall, Cheltenham.

A PRIZE OF £5 WAS AWARDED TO EACH OF THE 44 FOLLOWING CONTRIBUTORS.

Allan, Francis J., M.D., 1 Dock Street, E.; Atkinson, Mrs., Clare College Lodge, Cambridge; Bevan, Mrs., Plumpton House, Bury St. Edmunds; Browne, Miss, Maidenwell House, Louth, Lincolnshire; Cash, Frederick Goodall, Gloucester; Chisholm, Mrs., Church Lane House, Haslemere, Surrey; Collier, Mrs. R., 7 Thames Embankment, Chelsea; Croft, Sir Herbert G. D., Lugwardine Court, Hereford; Davis, Mrs. (care of Israel Davis, 6 King's Bench Walk, Temple, E.C.); Drake, Henry H., The Firs, Lee, Kent; Ercke, J. J. G., 13, Brownhill Road, Catford, S.E.; Flint, Fenner Ludd, 83 Brecknock Road, N.; Ford, William, 4 South Square, Gray's Inn, W.C.; Foster, Rev. A. J., The Vicarage, Wootton, Bedford; Glanville-Richards, W. V. S., 23 Endsleigh Place, Plymouth; Hale, C. D. Bowditch, 8 Sussex Gardens, Hyde Park, W.; Horder, Mrs. Mark, Rothenwood, Ellen Grove, Salisbury; Jackson, Edwin, 79 Withington Road, Whalley Range, Manchester; Jackson, George, 1 St. George's Terrace, Plymouth; Kesteven, W. H., 401 Holloway Road, N.; Lawrence, Mrs.

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Alfred, 16 Suffolk Square, Cheltenham ; Lawrie, Mrs., 1 Chesham Place, S.W. ; Leveson-Gower, G. W. G., Titsey Place, Limpsfield, Surrey ; Lobb, H. W., 66 Russell Square, W. ; McConnell, Miss M. A. Brooklands, Prestwich, Manchester ; Marshall, Mrs., Fenton Hall, Stoke-upon-Trent ; Meyer, Mrs., 1 Rodney Place, Clifton, Bristol; Milman, Mrs., The Governor's House, H.M. Prison, Camden Road ; Olding, Mrs. W. 4 Brunswick Road, Brighton, Sussex ; Passingham, Mrs., Milton, Cambridge ; Pringle, Mrs. Fairnalie, Fox Grove Road, Beckenham, Kent ; Reeve, Miss, Foxholes, Christchurch, Hants ; Scarlett, Mrs., Boscomb Manor, Bournemouth ; Shand, William, 57 Caledonian Road, N. ; Shaw, Cecil E., Wellington Park, Belfast ; Sizer, Miss Kate T., Moorlands, Great Huntley, Colchester ; Smith, Miss A. M. Carter, Thistleworth, Stevenage ; Smith, Rev. Edward S., Viney Hall Vicarage, Blakeney, Gloucestershire ; Smith, Mrs. F. P., Cliffe House, Sheffield ; Staveley, Edw. S. R., Mill Hill School, N.W. ; Sturge, Miss Mary W., 17 Frederick Road, Edgbaston, Birmingham ; Terry, Mrs., Tostock, Bury St. Edmunds, Suffolk ; Utley, W. H. Alliance Hotel, Cathedral Gates, Manchester ; Weston, Mrs. Ensleigh, Lansdown, Bath ; Wodehouse, Mrs. E. R. 56 Chester Square, S.W.

The material in these Records is sufficiently varied to be of service in many inquiries. The chief subjects to which allusion will be made in this book concern Stature, Eye colour, Temper, the Artistic Faculty, and some forms of Disease, but others are utilized that refer ~~also~~ to Marriage Selection and Fertility, &c.

The following remarks in this chapter refer almost wholly to the data of Stature.

The data derived from the Records of Family Faculties will be hereafter distinguished by the letters R.F.F. I was able to extract from them the statures of 205 couples of parents, with those of an aggregate of 930 of their adult children of both sexes. I must repeat that when dealing with the female statures, I transmuted them to their male equivalents, and treated them when thus transmuted, on equal terms with the measures of males,

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except where otherwise expressed. The factor I used was 1·08, which is equivalent to adding a little less than one-twelfth to each female height. It differs slightly from the factors employed by other anthropologists, who, moreover, differ a trifle between themselves; anyhow, it suits my data better than 1·07 or 1·09. I can say confidently that the final result is not of a kind to be sensibly affected by these minute details, because it happened that owing to a mistaken direction, the computer to whom I first entrusted the figures used a somewhat different factor, yet the final results came out closely the same. These R.F.F. data have by no means the precision of the observations to be spoken of in the next paragraph. In many cases there remains considerable doubt whether the measurement refers to the height with the shoes on or off; not a few of the 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, and as there is no sign or suspicion of bias, I have reason to place confidence in the values of the Means that are derived from them. They bear the internal tests that have been applied better than might have been expected, and when checked by the data described in the next paragraph, and cautiously treated, they are very valuable.

Special Data.—A second set of data, distinguished by the name of "Special observations," concern the variations in stature among Brothers. I circulated cards of inquiry among trusted correspondents, stating that I wanted records of the heights of brothers who were more than 24 and less than 60 years of age; that it was not necessary to send the statures of all of the brothers of the same family, but only of as many of them as could be easily and accurately measured, and that the height of even two brothers would be acceptable. The blank forms sent to be filled, were 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 inquirers widened, but I was satisfied when I had obtained returns of 295 families, containing in the aggregate 783 brothers, some few of whom also appear in the R.F.F. data. Though these two sets of returns overlap to a trifling extent, they are practically independent. I look upon the "Special Observations" as being quite as trustworthy as could be expected in any such returns. They bear every internal test that I can apply to them in a very satisfactory manner. The measures are commonly recorded to quarter or half inches.

Measures at my *Anthropometric Laboratory.*—A third set of data have been incidentally of service. They are the large lists of measures, nearly 10,000 in number, made at my Anthropometric Laboratory in the International Health Exhibition of 1884.

4. *Experiments on Sweet Peas.*—The last of the data

that I need specify were the very first that I used; they refer to the sizes of seeds, which are equivalent to the Statures of seeds. I both measured and weighed them, but after assuring myself of the equivalence of the two methods (see Appendix C.), confined myself to ascertaining the weights, as they were much more easily ascertained than the measures. It is more than 10 years since I procured these data. They were the result of an extensive series of experiments on the produce of seeds of different sizes, but of the same species, conducted for the following reasons. I had endeavoured to find a population possessed of some measurable characteristic that was suitable for investigating the causes of the statistical similarity between successive generations of a people, as will hereafter be discussed in Chapter VIII. At last I determined to experiment on seeds, and after much inquiry of very competent advisers, selected sweet-peas for the purpose. They do not cross-fertilize, which is a very exceptional condition among plants; they are hardy, prolific, of a convenient size to handle, and nearly spherical; their weight does not alter perceptibly when the air changes from damp to dry, and the little pea at the end of the pod, so characteristic of ordinary peas, is absent in sweet-peas. I began by weighing thousands of them individually, and treating them as a census officer would treat a large population. Then I selected with great pains several sets for planting. Each set contained seven little packets, numbered K, L, M, N, O, P, and Q, each of the seven packets contained ten seeds of almost exactly the same weight; those in K being the heaviest, L the next heaviest, and so down to Q, which was the lightest. The precise weights are given in Appendix C, together with the corresponding diameters, which I ascertained by laying 100 peas of the same weight in a row. The weights run in an arithmetic series, having a common average difference of 0·172 grain. I do not of course profess to work to thousandths of a grain, though I did work to somewhat less than one hundredth of a grain; therefore the third decimal place represents little more than an arithmetical working value which has to be regarded in multiplications, lest an error of sensible importance should be introduced by its neglect. Curiously enough, the diameters were found also to run approximately in an arithmetic series, owing, I suppose, to the misshape and corrugations of the smaller seeds, which gave them a larger diameter than if they had been plumped out into spheres. All this is shown in the Appendix, where it will be seen that I was justified in sorting the seeds by the convenient method of the balance and weights, and of accepting the weights as directly proportional to the mean diameters.

In each experiment, seven beds were prepared in parallel rows; each was $1\frac{1}{2}$ feet wide and 5 feet long. Ten holes of 1 inch deep were dibbled at equal distances apart along each bed, and a single seed was put into each hole. The beds were then bushed over to keep off the birds. Minute instructions were given to ensure uniformity, which I need not repeat here. The end of all was that the seeds as they became ripe were collected from time to time and put into bags that I had sent, lettered from K to Q, the same letters having been stuck at the ends of the beds. When the crop was coming to an end, the whole remaining produce of each bed, including the foliage, was torn up, tied together, labelled, and sent to me. Many friends and acquaintances had each undertaken the planting and culture of a complete set, so that I had simultaneous experiments going on in various parts of the United Kingdom from Nairn in the North to Cornwall in the South. Two proved failures, but the final result was that I obtained the more or less complete produce of seven sets; that is to say, the produce of $7 \times 7 \times 10$, or of 490 carefully weighed parent seeds. Some ~~further~~ additional account of the results is given in Appendix C.

It would be wholly out of place to enter here into further details of the experiments, or to narrate the numerous little difficulties and imperfections I had to contend with, and how I balanced doubtful cases; how I divided returns into groups to see if they confirmed one another, or how I conducted any other well-known statistical operation. Suffice it to say that I took immense pains, which, if I had understood the general conditions of the problem as clearly as I do now, I should not perhaps have cared to bestow. The results were most satisfactory. They gave me two data, which were all that I wanted in order to understand in its simplest approximate form, the way in which one generation of a people is descended from a previous one; and thus I got at the heart of the problem at once.

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CHAPTER VII.

DISCUSSION OF THE DATA OF STATURE.

Stature as a subject for inquiry.—Marriage Selection.—Issue of unlike Parents.—Description of the Tables of Stature. Mid-Stature of the Population.—Variability of the Population.—Variability of Mid-Parents.—Variability in Co-Fraternities.—Regression: *a*, Filial; *b*, Mid-Parental; *c*, Parental; *d*, Fraternal.—Squadrons of Statures.—Successive Generations of a People.—Natural Selection.—Variability in Fraternities.—Trustworthiness of the Constants.—General view of Kinship.—Separate Contribution from each Ancestor.—Pedigree Moths.

2y.—/

Stature as a Subject for Inquiry.—The first of these inquiries into the laws of human heredity deals with hereditary Stature, which is an excellent subject for statistics. Some of its merits are obvious enough, such as the ease and frequency with which it may be measured, its practical constancy during thirty-five or forty years of middle life, its comparatively small dependence upon differences of bringing up, and its inconsiderable influence on the rate of mortality. Other advantages which are not equally obvious are equally great. One of these is due to the fact that human stature is not a simple element, but a sum of the accumulated lengths or thicknesses of more than a hundred bodily parts, each so distinct from the rest as to have earned a name by which it can be specified. The list includes about fifty separate bones, situated in the skull, the spine, the pelvis, the two legs, and in the two ankles and feet. The bones in both the lower limbs have to be counted, because the Stature depends upon their average length. The two cartilages interposed between adjacent bones, wherever there is a movable joint, and the single cartilage in other cases, are rather more numerous than the bones themselves. The fleshy parts of the scalp of the head and of the soles of the feet conclude the list. Account should also be taken of the shape and set of the many bones which conduce to a more or less arched instep, straight back, or high head. I noticed in the skeleton of O'Brien, the Irish giant, at the College of Surgeons, which is the tallest skeleton in any English museum, that his great stature of about 7 feet 7 inches would have been a trifle increased if the faces of his dorsal vertebræ had been more parallel than they are, and his back consequently straighter.

This multiplicity of elements, whose variations are to some degree independent of one another, some tending to lengthen the total stature, others to shorten it, corresponds to an equal number of sets of rows of pins in the apparatus Fig. 7, p. 63, by which the cause of variability was illustrated. The larger the number of these variable elements, the more nearly does the variability of their sum assume a "Normal" character, though the ~~closeness of~~ approximation ~~varies~~ increases only as the square root of their number. The beautiful regularity in the Statures of a population, whenever they are statistically marshalled in the order of their heights, is due to the number of variable and quasi-independent elements of which Stature is the sum.

Marriage Selection.—Whatever may be the sexual preferences for similarity or for contrast, I find little indication in the average results obtained from a fairly large number of cases, of any single measurable personal peculiarity, whether it be stature, eye-colour, temper, or artistic tastes, in influencing marriage selection to a notable degree. Nor is this extraordinary, for though people may fall in love for trifles, marriage is a serious act, usually determined by the concurrence of numerous motives. Therefore we could hardly expect either shortness or tallness, darkness or lightness in complexion, or any other single quality, to have in the long run a large separate influence.

I was certainly surprised to find how imperceptible was the influence that even good and bad Temper seemed to exert on marriage selection. A list was made (see Appendix D) of the observed frequency of marriages between persons of each of the various classes of Temper, in a group of 111 couples, and I calculated what would have been the relative frequency of intermarriages between persons of the various classes, if the same number of males and females had been paired at random. The result showed that the observed list agreed closely with the calculated list, and therefore that these observations gave no evidence of discriminative selection in respect to Temper. The good-tempered husbands were 46 per cent. in number, and, between them, they married 22 good-tempered and 24 bad-tempered wives; whereas calculation, having regard to the relative proportions of good and bad Temper in the two sexes, gave the numbers as 25 and 21. Again, the bad-tempered husbands, who were 54 per cent. in number, married 31 good-tempered and 23 bad-tempered wives, whereas calculation gave the number as 30 and 24. This rough summary is a just expression of the results arrived at by a more minute analysis, which is described in the Appendix, and need not be repeated here.

Similarly as regards Eye-Colour. If we analyse the marriages between the 78 couples whose eye-colours are described in Chapter VIII., and compare the observed results with those calculated on the supposition that Eye-Colour has no influence whatever in marriage selection, the two lists will be found to be much alike. Thus where both of the parents have eyes of the same colour, whether they be light, or hazel, or dark, the percentage results are almost identical, being 37, 3, and 8 as observed, against 37, 2, and 7 calculated. Where one parent is hazel-eyed and the other dark-eyed, the marriages are as 5 observed against 7 calculated. But the results run much less well together in the other two possible combinations, for where one parent is light and the other hazel-eyed, they give 23 observed against 15 calculated; and where one parent is light and the other dark-eyed, they give 24 observed against 32 calculated.

The effect of Artistic Taste on marriage selection is discussed in Chapter X., and this also is shown to be small. The influence on the race of bias in marriage selection will be discussed in that chapter.

I have taken much trouble at different times to determine whether Stature plays any sensible part in marriage selection. I am not yet prepared to offer complete results, but shall confine my remarks for the present to the particular cases with which we are now concerned. The shrewdest test is to proceed under the guidance of Problem 2 in Chapter V. I find the Q of Stature among the male population to be 1·7 inch, and similarly for the transmuted statures of the female population. Consequently if the men and (transmuted) women married at random so far as stature was concerned, the Q in a group of couples, each couple consisting of a pair of *summed* statures, would be $\sqrt{2} \times 1·7$ inches = 2·41 inches. Therefore the Q in a group of which each element was the *mean* stature of a couple, would be half that amount, or 1·20 inch. This closely corresponds to what I derived from the data contained in the first and in the last column but one of Table 11. The word "Mid-Parent" used in their headings, expresses an ideal person of composite sex whose Stature is half way between the Stature of the father and the transmuted Stature of the mother. I therefore conclude that marriage selection does not pay such regard to Stature, as deserves being taken into account in the cases with which we are concerned.

I tried the question in another but ruder way, by dividing (see Table 9) the male and female parents respectively into three nearly equal groups, of tall, medium, and short. It was impracticable to make them precisely equal, on account of the roughness with which the measurements were recorded, so I framed rules that seemed best adapted to the case. Consequently the numbers of the tall and short proved to be only approximately and not exactly equal, and the two together were only approximately equal to the medium cases. The final results were :—32 instances where one parent was short and the other tall, and 27 where both were short or both were tall. In other words, there were 32 cases of contrast in marriage, to 27 cases of likeness. I do not regard this difference as of consequence, because the numbers are small, and because a slight change in the limiting values assigned to shortness and tallness, would have a sensible effect upon the result. I am therefore content to ignore it, and to regard the Statures of married folk just as if their choice in marriage had been wholly independent of stature. The importance of this supposition in facilitating calculation will be appreciated as we proceed.

Issue of Unlike Parents.—We will next discuss the question whether the Stature of the issue of unlike parents betrays any notable evidence of their unlikeness, or whether the peculiarities of the children do not rather depend on the *average* of two values; one the Stature of the father, and the other the transmuted Stature of the mother; in other words, on the Stature of that ideal personage to whom we have already been introduced under the name of a Mid-Parent. Stature has already been spoken of as a well-marked instance of the heritages that blend freely in the course of hereditary transmission. It now becomes necessary to substantiate the statement, because it is proposed to trace the relationship between the Mid-Parent and the Son. It would not be possible to define the relationship between either parent singly, and the son, in a trustworthy way, without the help of a much larger number of observations than are now at my disposal. They ought to be numerous enough to give good assurance that the cases of tall and short, among the unknown parents, will neutralise one another; otherwise the uncertainty of the stature of the unknown parent would make the results uncertain to a serious degree. I am heartily glad that I shall be able fully to justify the method of dealing with Mid-Parentages instead of with single Parents.

The evidence is as follows:—If the Stature of children depends only upon the *average* Stature of their two Parents, that of the mother having been first transmuted, it will make no difference in a Fraternity whether one of the Parents was tall and the other short, or whether they were alike in Stature. But if some children resemble one Parent in Stature and others resemble the other, the Fraternity will be more diverse when their Parents had differed in Stature than when they were alike. We easily acquaint ourselves with the facts by separating a considerable number of Fraternities into two contrasted groups: (*a*) those who are the progeny of Like Parents; (*b*) those who are the progeny of Unlike Parents. Next we write the statures of the individuals in each Fraternity under the form of $\mathsf{M}+(\pm D)$ (see page), where M is the mean stature of the Fraternity, and D is the deviation of any one of its members from M. Then we marshal all the values of D that belong to the group *a*, into one Scheme of deviations, and all those that belong to the group *b* into another Scheme, and we find the Q of each. If it should be the same, then there is no greater diversity in the *a* Group than there is in the *b* Group, and such proves to be the case. I applied the test (see Table 10) to a total of 525 children, and found that they were no more diverse in the one case than in the other. I therefore conclude that we have only to look to the Stature of the Mid-Parent, and need not care whether the Parents are or are not unlike one another.

The advantages of Stature as a subject from which the simple laws of heredity may be studied, will now be well appreciated. It is nearly constant in the same adult, it is frequently measured and recorded; its discussion need not be entangled with considerations of marriage selection. It is sufficient to consider the Stature of the Mid-Parent and not those of the two Parents separately. Its variability is Normal, so that much use may be made of the curious properties of the law of Frequency of Error in cross-testing the several conclusions, and I may ~~anticipate by saying that hitherto this has always been done with~~ add that in all cases they have borne the test successfully.

The only drawback to the use of Stature in statistical inquiries, is its small variability, one half of the population differing less than 1·7 inch from the average of all of them. In other words, its Q is only 1·7 inch.

Description of the Tables of Stature.—I have arranged and discussed my materials in a great variety of ways, to guard against rash conclusions, but do not think it necessary to trouble the reader with more than a few Tables, which afford sufficient material to determine the more important constants in the formulæ that will be used.

Table 11, R.F.F., refers to the relation between the Mid-Parent and his (or should we say *its?*) Sons or Transmuted Daughters, and records the Statures of 928 adult offspring of 205 Mid-Parents. It shows the distribution of Stature among the Sons of each successive group of Mid-Parents, in which the latter are all of the same Stature, reckoning to the nearest inch. I have calculated the M of each line, chiefly by drawing Schemes from the entries in it. Their values are printed at the ends of the lines and they form the right-hand column of the Table.

Tables 12 and 13 refer to the relation between Brothers. The one is derived from the R.F.F. and the other from the Special data. They both deal with small or moderately sized Fraternities, excluding the larger ones for reasons that will be explained directly, but the R.F.F. Table is the least restricted in this respect, as it only excludes families of 6 brothers and upwards. The data were so few in number that I could not well afford to lop off more. The Tables were constructed by registering the differences between each possible pair of brothers in each family: thus if there were three brothers, A, B, and C, in a particular family, I entered the differences of stature between A and B, A and C, and B and C. four brothers gave rise to 6 entries, and five brothers to 10 entries. The large Fraternities were omitted, as the very large number of different pairs in them would have overwhelmed the influence of the smaller Fraternities. The large Fraternities are separately dealt with in Table 14.

We can derive some of the constants by more than one method; and it is gratifying to find how well the results of different methods confirm one another.

Mid-Stature of the Population.—The Median, Mid-Stature, or M of the general Population is a value of primary importance in this inquiry. Its value will be always designated by the symbol P, and it may be deduced from the bottom lines of any one of the three Tables. I obtain from them respectively the values 68·2, *68·5*, 68·4, but the middle of these, which is printed in italics, is a smoothed result. It is one of the only two smoothed values in the whole of my work, and was justifiably corrected, because the observed values that happen to lie nearest to the Grade of 50° ran out of harmony with the rest of the curve. It is therefore reasonable to consider its discrepancy as fortuitous, although it amounts to more than 0·15 inch. The series in question refers to R.F.F. brothers, who, owing to the principle on which the Table is constructed, are only a comparatively small sample taken out of the R.F.F. Population, and on a principle that gave greater weight to a few large families than to all the rest. Therefore it could not be expected to give rise to so regular a Scheme for the general R.F.F. Population as Table 11, which was fairly based upon the whole of it. Less accuracy was undoubtedly to have been expected in this group than in either of the others.

Variability of the Population.—The value of Q in the Statures of the general Population is to be deduced from the bottom lines of any one of the Tables 11, 12, and 13. The three values of it that I so obtain, are 1·65, 1·7, and 1·7 inch. I should mention that the method of the treatment originally adopted, happened also to make the first of these values 1·7 inch, so I have no hesitation in accepting 1·7 as the value for all my data.

Variability of Mid-Parents.—The value of Q in a Scheme drawn from the Statures of the R.F.F. Mid-Parents according to the data in Table 11, is 1·19 inches. Now it has already been shown that if marriage selection is independent of stature, the value of Q in the Scheme of Mid-parental Statures would be equal to its value in that of the general Population (which we have just seen to be 1·7 inch), divided by the square root of 2; that is by 1·45. This calculation makes it to be

1·21 inch, which agrees excellently with the observed value.[1]

Variability in Co-Fraternities.—As all the Adult Sons and Transmuted Daughters of the same Mid-Parent, form what is called a Fraternity, so all the Adult Sons and Transmuted Daughters of a group of Mid-Parents who have the same Stature (reckoned to the nearest inch) will be termed a Co-Fraternity. Each line in Table 11 refers to a separate Co-Fraternity and expresses the distribution of Stature among them. There are three reasons why Co-Fraternals should be more diverse among themselves than brothers. First, because their Mid-Parents are not of identical height, but may differ even as much as one inch. Secondly, because their grandparents, great-grandparents, and so on indefinitely backwards, may have differed widely. Thirdly, because the nurture or rearing of Co-Fraternals is more various than that of Fraternals. The brothers in a Fraternity of townsfolk do not seem to differ more among themselves than those in a Fraternity of country-folk, but a mixture of Fraternities derived indiscriminately from the two sources, must show greater diversity than either of them taken by themselves. The large differences between town and country-folk, and those between persons of different social classes, are conspicuous in the data contained in the Report of the

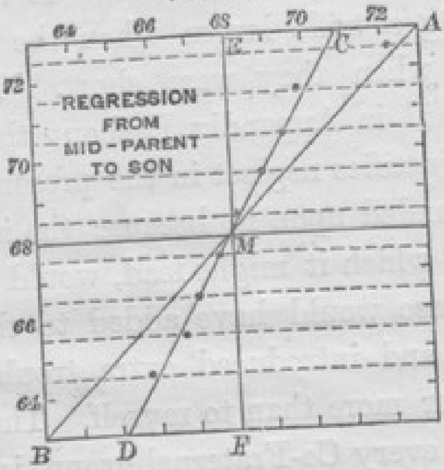
[1] In all my values referring to human stature, the second decimal is rudely approximate. I am obliged to use it, because if I worked only to tenths of an inch, sensible errors might creep in entirely owing to arithmetical operations.

Anthropological Committee to the British Association in 1880, and published in its Journal.

I concluded after carefully studying the chart upon which each of the individual observations from which Table 11 was constructed, had been entered separately in their appropriate places, and not clubbed into Groups as in the Tables, that the value of Q in each Co-Fraternal group was roughly the same, whatever their Mid-Parental value might have been. It was not quite the same, being a trifle larger when the Mid-Parents were tall than when they were short. This justifies what will be said in Appendix E about the Geometric Mean; it also justifies neglect in the present inquiry of the method founded upon it, because the improvement in the results to which it might lead, would be insignificant, while its use would have added to the difficulty of explanation, and introduced extra trouble throughout, to the reader more than to myself. The value that I adopt for Q in every Co-Fraternal group, is 1·5 inch.

Regression.—a. Filial: However paradoxical it may appear at first sight, it is theoretically a necessary fact, and one that is clearly confirmed by observation, that the Stature of the adult offspring must on the whole, be more *mediocre* than the stature of their Parents; that is to say, more near to the M of the general Population. Table 11 enables us to compare the values of the M in different Co-Fraternal groups with the Statures of their respective Mid-Parents. Fig. 10 is a graphical representation of the meaning of the Table so far as it now concerns us. The horizontal dotted lines and the graduations at their sides, correspond to the similarly placed lines of figures and graduations in Table 11. The dot on each line shows the point where its M falls. The value of its M is to be read on the graduations along the top, and is the same as that which is given in the last column of Table 11. It will be perceived that the line drawn

FIG. 10.



through the centres of the dots, admits of being interpreted by the straight line C D, with but a small amount of give and take; and the fairness of this interpretation is confirmed by a study of the MS. chart above mentioned, in which the individual observations were plotted in their right places.

Now if we draw a line A B through every point where the graduations along the top of Fig. 10, are the same as those along the sides, the line will be straight and will run diagonally. It represents what the Mid-

Statures of the Sons would be, if they were on the average identical with those of their Mid-Parents. Most obviously A B does *not* agree with C D; therefore Sons do *not*, on the average, resemble their Mid-Parents. On examining these lines more closely, it will be observed that A B cuts C D at a point M that fairly corresponds to the value of $68\frac{1}{4}$ inches, whether its value be read on the scale at the top or on that at the side. This is the value of P, the Mid-Stature of the population. Therefore it is only when the Parents are mediocre, that their Sons on the average resemble them.

Next draw a vertical line, E M F, through M, and let ECA be any horizontal line cutting ME at E, MC at C, and MA at A. Then it is obvious that the ratio of EA to EC is constant, whatever may be the position of ECA. This is true whether ECA be drawn above or below M. In other words, the proportion between the Mid-Filial and the Mid-Parental deviation is constant, whatever the Mid-Parental stature may be. I reckon this ratio to be as 2 to 3: that is to say, the Filial deviation from P is on the average only two-thirds as wide as the Mid-Parental Deviation. I call this ratio of 2 to 3 the ratio of "Filial Regression." It is the proportion in which the Son is, on the average, less exceptional than his Mid-Parent.

My first estimate of the average proportion between the Mid-Filial and the Mid-Parental deviations, was made from a study of the MS. chart, and I then reckoned it as 3 to 5. The value given above was afterwards substituted, because the data seemed to admit of that interpretation also, in which case the fraction of two-thirds was preferable as being the more simple expression. I am now inclined to think the latter may be a trifle too small, but it is not worth while to make alterations until a new, larger, and more accurate series of observations can be discussed, and the whole work revised. The present doubt only ranges between nine-fifteenths in the first case and ten-fifteenths in the second.

This value of two-thirds will therefore be accepted as the amount of Regression, on the average of many cases, from the Mid-Parental to the Mid-Filial stature, whatever the Mid-Parental stature may be.

As the two Parents contribute equally, the contribution of either of them can be only one half of that of the two jointly; in other words, only one half of that of the Mid-Parent. Therefore the average Regression from the Parental to the Mid-Filial Stature must be the one half of two-thirds, or one-third. I am unable to test this conclusion in a satisfactory manner by direct observation. The data are barely numerous enough for dealing even with questions referring to Mid-Parentages; they are quite insufficient to deal with those that involve the additional large uncertainty introduced owing to an ignorance of the Stature of one of the parents. I have entered the Uni-Parental and the Filial data on a MS. chart, each in its appropriate place, but they are too scattered and irregular to make it useful to give the results in detail. They seem to show a Regression of about two-fifths, which differs from that of one-third in the ratio of 6 to 5. This direct observation is so inferior in value to the inferred result, that I disregard it, and am satisfied to adopt the value given by the latter, that is to say, of one-third, to express the average Regression from either of the Parents to the Son.

b. Mid-Parental: The converse relation to that which we have just discussed, namely the relation between the unknown stature of the Mid-Parent and the known Stature of the Son, is expressed by a fraction that is very far from being the converse of two-thirds. Though the Son deviates on the average from P only $\frac{2}{3}$ as widely as his Mid-parent, it does not in the least follow that the Mid-parent should deviate on the average from P, $\frac{3}{2}$ or $1\frac{1}{2}$, as widely as the Son. The Mid-Parent is not likely to be more exceptional than the son, but quite the contrary. The number of individuals who are nearly mediocre is so preponderant, that an exceptional man is more frequently found to be the exceptional son of mediocre parents than the average son of very exceptional parents. This is clearly shown by Table 11, for the very same observations which give the average value of Filial Regression when it is read in one way, gives that of the Mid-Parental Regression when it is read in another way, namely down the columns, instead of along the horizontal lines. It then shows that the Mid-Parent of a man deviates on the

~~vertical~~ average from P, only one-third as much as the man himself. This value of $\frac{1}{3}$ is four and a half times smaller than the numerical converse of $\frac{3}{2}$, since $4\frac{1}{2}$, or $\frac{9}{2}$, being multiplied into $\frac{1}{3}$, is equal to $\frac{3}{2}$.

c. Parental: As a Mid-Parental deviation is equal to one-half of the two Parental deviations, it follows that the Mid-Parental Regression must be equal to one-half of the sum of the two Parental Regressions. As the latter are equal to one another it follows that all three must have the same value. In other words, the average Mid-Parental Regression being $\frac{1}{3}$, the average Parental Regression must be $\frac{1}{3}$ also.

As there was much appearance of paradox in the above strongly contrasted results, I looked carefully into the run of the figures in Table 11. They were deduced, as already said, from a MS. chart on which the stature of every Son and the transmuted Stature of every Daughter is entered opposite to that of the Mid-Parent, the transmuted Statures being reckoned to the nearest tenth of an inch, and the position of the other entries being in every respect exactly as they were recorded. Then the number of entries in each square inch were counted, and copied in the form in which they appear in the Table. I found it hard at first to catch the full significance of the entries, though I soon discovered curious and apparently very interesting relations between them. These came out distinctly after I had "smoothed" the entries by writing at each intersection between a horizontal line and a ver-

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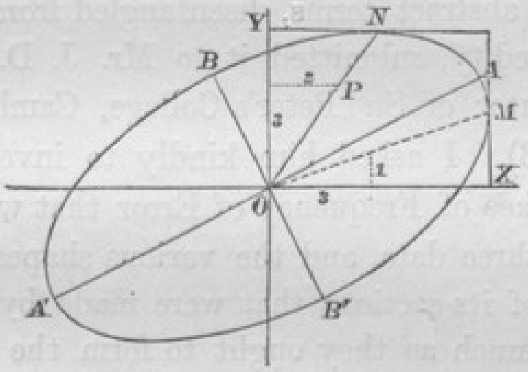
tical one, the sum of the entries in the four adjacent squares. I then noticed (see Fig. 11) that lines drawn through entries of the same value formed a series of concentric and similar ellipses. Their common centre lay at the intersection of those vertical and horizontal lines which correspond to the value of $68\frac{1}{4}$ inches, as read on both the top and on the side scales. Their axes were similarly inclined. The points where each successive ellipse was touched by a horizontal tangent, lay in a straight line that was inclined to the vertical in

FIG. 11.



the ratio of $\frac{2}{3}$, and those where the ellipses were touched by a vertical tangent, lay in a straight line inclined to the horizontal in the ratio of $\frac{1}{3}$. It will be obvious on studying Fig. 11 that the point where each successive horizontal line touches an ellipse is the point at which the greatest value in the line will be found. Therefore these ratios confirm the values of the Ratios of Regression, already obtained by a different method, namely those of $\frac{2}{3}$ from Mid-Parent to Son, and of $\frac{1}{3}$ from Son to Mid-Parent. The same is true in respect

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Tras to last page

to the successive vertical lines. These and other relations were evidently a subject for mathematical analysis and verification. It seemed clear to me that they all depended on three elementary measures, supposing the law of Frequency of Error to be applicable throughout; namely (1) the value of Q in the General Population, which was found to be 1·7 inch; (2) the value of Q in any Co-Fraternity, which was found to be 1·5 inch; (3) the Average Regression of the Stature of the Son from that of the Mid-Parent, which was found to be $\frac{2}{3}$. I wrote down these values, and phrasing the problem in abstract terms, disentangled from all reference to heredity, submitted it to Mr. J. D. Hamilton Dickson, Tutor of St. Peter's College, Cambridge (see Appendix B). I asked him kindly to investigate for me the Surface of Frequency of Error that would result from these three data, and the various shapes and other particulars of its sections that were made by horizontal planes, inasmuch as they ought to form the ellipses of which I spoke.

The problem may not be difficult to an accomplished mathematician, but I certainly never felt such a glow of loyalty and respect towards the sovereignty and wide sway of mathematical analysis as when his answer arrived, confirming, by purely mathematical reasoning, my various and laborious statistical conclusions with far more minuteness than I had dared to hope, because the data ~~from which I had to work~~ ran somewhat roughly, and I had to smooth them with tender caution. His calculation corrected my observed value of Mid-Parental Regression from $\frac{1}{3}$ to $\frac{6}{17\cdot 5}$; the relation between the major and minor axis of the ellipses was changed 3 per cent.; and their inclination to one another was changed less than 2°.[1]

It is obvious from this close accord of calculation with observation, that the law of Error holds throughout with sufficient precision to be of real service, and that the various results of my statistics are not casual and disconnected determinations, but strictly interdependent.

I trust it will have become clear even to the most non-mathematical reader, that the law of Regression in Stature refers primarily to Deviations, that is, to measurements made from the *level of mediocrity* to the

[1] The following is a more detailed comparison between the calculated and the observed results. The latter are enclosed in brackets. The letters refer to Fig. β:—

Given—

The "Probable Error" of each system of Mid-Parentages = 1·22 inch (this was an earlier determination of its value; as already said, the second decimal is to be considered only as approximate).

Ratio of mean filial regression = $\frac{2}{3}$.

"Prob. Error" of each Co-Fraternity = 1·50 inch.

Sections of surface of frequency parallel to XY are true ellipses.

(Obs.—Apparently true ellipses.)

MX : YO = 6 : 17·5, or nearly 1 : 3.

(Obs.—1 : 3.)

Major axes to minor axes = $\sqrt{7} : \sqrt{2}$ = 10 : 5·35.

(Obs.—10 : 5·1.)

Inclination of major axes to OX = 26° 36′.

(Obs. 25°.)

Section of surface parallel to XZ is a true Curve of Frequency.

(Obs.—Apparently so.)

"Prob. Error", the Q of that curve, = 1.07 inch.

(Obs.—1·00, or a little more.)

crown of the head, upwards or downwards as the case may be, ~~but~~ and not from the *ground* to the crown of the head. (In the population with which I am now dealing, the level of mediocrity is $68\frac{1}{4}$ inches (without shoes).) The law of Regression in respect to Stature may be phrased as follows; namely, that the Deviation of the Sons from P are, on the average, equal to one-third of the deviation of the ~~Mid~~ Parent from P, and in the same direction. Or more briefly still:—If P + (± D) be the Stature of the Parent, the Stature of the offspring will on the average be P + (± $\frac{1}{3}$ D).

If this remarkable law of Regression had been based only on those experiments with seeds, in which I first observed it, it might well be distrusted until otherwise confirmed. If it had been corroborated by a comparatively small number of observations on human stature, some hesitation might be expected before its truth could be recognised in opposition to the current belief that the child tends to resemble its parents. But more can be urged than this. It is easily to be shown that we ought to expect Filial Regression, and that it ought to amount to some constant fractional part of the value of the Mid-Parental deviation. All of this will be made clear in a subsequent section, when we shall discuss the cause of the curious statistical constancy in successive generations of a large population. In the meantime, two different reasons may be given for the occurrence of Regression; the one is connected with our notions of stability of type, and of which no more need now be said; the other is as follows:—The child inherits partly from his parents, partly from his ancestry. In every population that intermarries freely, when the genealogy of any man is traced far backwards, his ancestry will be found to consist of such varied elements that they are indistinguishable from a sample taken at haphazard from the general Population. The Mid-Stature M of the remote ancestry of such a man will become identical with P; in other words, it will be mediocre. To put the same conclusion into another form, the most probable value of the Deviation of his Mid-Ancestors in any remote generation, from P, is zero.

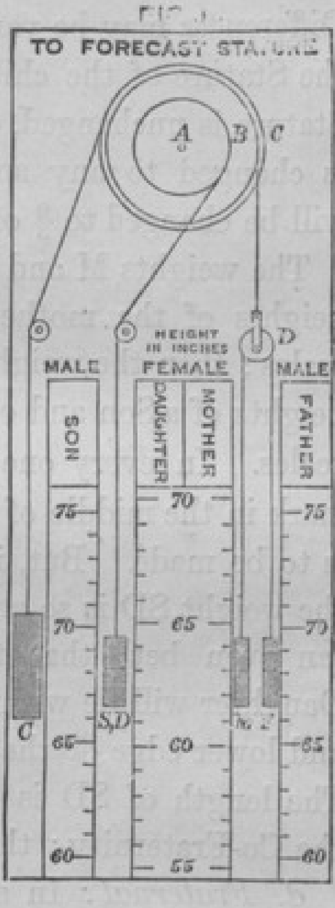
For the moment let us confine our attention to some one generation in the remote ancestry on the one hand, and to the Mid-Parent on the other, and ignore all other generations. The combination of the zero Deviation of the one with the observed Deviation of the other is the combination of nothing with something. Its effect resembles that of pouring a measure of water into a vessel of wine. The wine is diluted to a constant fraction of its alcoholic strength, whatever that strength may have been.

Similarly with regard to every other generation. The Mid-Deviation in any near generation of the ancestors will have a value intermediate between that of the zero Deviation of the remote ancestry, and of the observed Deviation of the Mid-Parent. Its combination with the Mid-Parental Deviation will be as if a mixture of wine and water in some definite proportion, and not pure water, had been poured into the wine. The process throughout is one of proportionate dilutions, and the joint effect of all of them is to weaken the original alcoholic strength in a constant ratio.

The law of Regression tells heavily against the full hereditary transmission of any gift. Only a few out of many children would be likely to differ from mediocrity so widely as their Mid-Parent, and still fewer would differ as widely as the more exceptional of the two Parents. The more bountifully the Parent had been gifted by nature, the more rare will be his good fortune if he begets a son who is as richly endowed as himself, and still more so if he has a son who is endowed yet more largely. But the law is even-handed; it levies an equal succession-tax on the transmission of badness as of goodness. If it discourages the extravagant hopes of a gifted parent that his children will inherit all his powers; it no less discountenances extravagant fears that they will inherit all his weakness and disease.

It must be clearly understood that there is nothing in these statements to invalidate the general doctrine that the children of a gifted pair are much more likely to be gifted than the children of a mediocre pair. They merely express the fact that the ablest of all the children of a few gifted pairs is not likely to be as gifted as the ablest of all the children of a very great many mediocre pairs.

The constancy of the ratio of Regression, whatever may be the amount of the Mid-Parental Deviation, is now seen to be a reasonable law which might have been foreseen. It is so in its relations simple that I have contrived more than one form of apparatus by which the probable stature of the children of known parents can be mechanically reckoned. Fig. 12 is a representation of one of them, that is worked with pulleys and weights. A, B, and C are three thin wheels with grooves round their edges. They are screwed together so as to form a single piece that turns easily on its axis. The weights M and F are attached to either end of a thread that passes over the movable pulley D. The pulley itself hangs from a thread which is wrapped two or three times round the groove of B and is then secured to the wheel. The weight SD hangs from a thread that is wrapped two or three times round the groove of A, and is then secured to the wheel. The diameter of A is to that of B as 2 to 3. Lastly, a thread is wrapped in the opposite direction round the wheel C, which may have any convenient diameter, and is attached to a counterpoise. M refers to the male statures, F to the female ones, S for Sons, D for daughters.



The scale of Female Statures differs from that of the Males, each Female height being laid down in the position which would be occupied by its male equivalent.

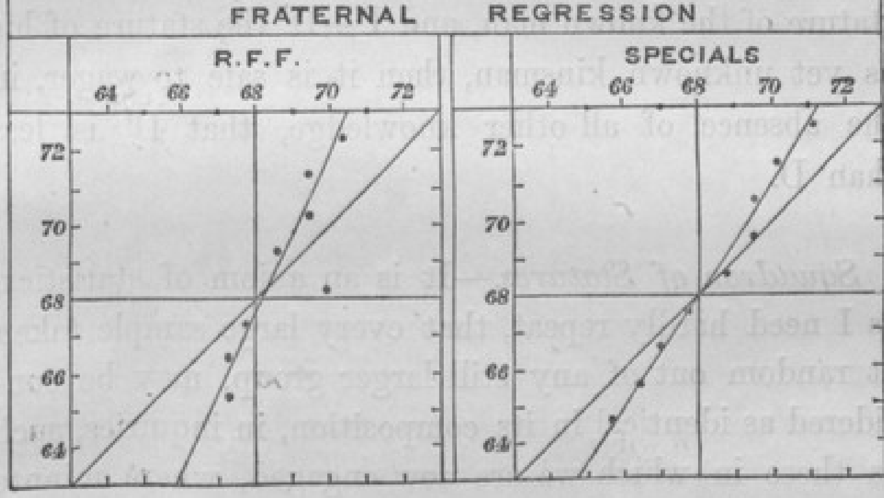
Thus 56 is written in the position of 60·48 inches, which is equal to $56 \times 1·08$. Similarly, 60 is written in the position of 64·80, which is equal to $60 \times 1·08$.

It is obvious that raising M will cause F to fall, and *vice versâ*, without affecting the wheel AB, and therefore without affecting SD; that is to say, the Parental Differences may be varied indefinitely without affecting the Stature of the children, so long as the Mid-Parental Stature is unchanged. But if the Mid-Parental Stature is changed to any specified amount, then that of SD will be changed to $\frac{2}{3}$ of that amount.

The weights M and F have to be set opposite to the heights of the mother and father on their respective scales; then the weight SD will show the most probable heights of a Son and of a Daughter on the corresponding scales. In every one of these cases, it is the fiducial mark in the middle of each weight by which the reading is to be made. But, in addition to this, the length of the weight SD is so arranged that it is an equal chance (an even bet) that the height of each Son or each Daughter will lie within the range defined by the upper and lower edge of the weight, on their respective scales. The length of SD is 3 inches, which is twice the Q of the Co-Fraternity; that is, $2 \times 1·50$ inch.

d. Fraternal: In seeking for the value of Fraternal Regression, it is better to confine ourselves to the Special data given in Table 13, as they are much more trustworthy than the R.F.F. data in Table 12. By treating them in the way shown in Fig. 13, which is constructed on the same principle as Fig. 10, page 96, I obtained the value for Fraternal Regression of $\frac{2}{3}$; that is to say, the unknown brother of a known man is probably only two-thirds as exceptional in Stature as he is. This is the same value as that obtained for the Regression from Mid-Parent to Son. However paradoxical the fact of there being such a thing as Fraternal Regression, may seem at first, a little reflection will show its reasonableness, which will become much clearer later on. In the meantime, we may recollect that the

FIG. 13.



unknown brother has two different tendencies, the one to resemble the known man, and the other to resemble his race. The one tendency is to deviate from P as much as his brother, and the other tendency is not to deviate at all. The result is a compromise.

As the average Regression from either Parent to the Son is twice as great as that from a man to his Brother, a man is, generally speaking, only half as nearly related

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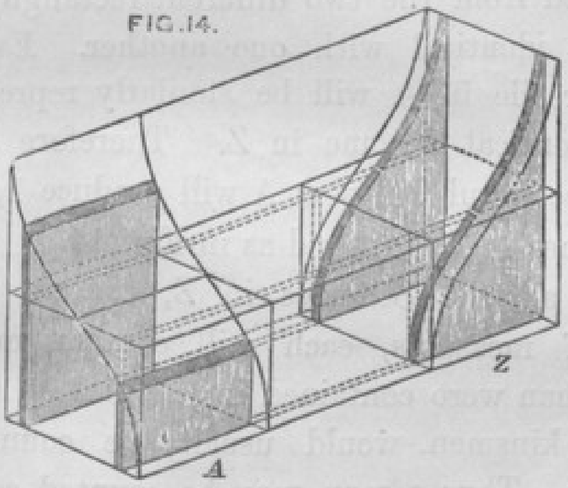
to either of his Parents as he is to his Brother. In other words, the Parental kinship is only half as close as the Fraternal.

We have now seen that there is Regression from the Parent to his Son, from the Son to his Parent, and from the Brother to his Brother. As these are the only three possible lines of kinship, namely, descending, ascending, and collateral, it must be a universal rule that the unknown Kinsman, in any degree, of a known Man, is on the average more mediocre than he. Let $P \pm D$ be the stature of the known man, and $P \pm D'$ the stature of his as yet unknown kinsman, then it is safe to wager, in the absence of all other knowledge, that D' is less than D.

Squadron of Statures.—It is an axiom of statistics, as I need hardly repeat, that every large sample taken at random out of any still larger group, may be considered as identical in its composition, in inquiries such as these in which we are now engaged, where minute accuracy is not desired and where highly exceptional cases are not regarded. Suppose our larger group to consist of a million, that is of 1000×1000 statures, and that we had divided it at random into 1000 samples each containing 1000 statures, and made Schemes of each of them. The 1000 Schemes would be practically identical, and we might marshal them one behind the other in successive ranks, and thereby form a "Squadron," numbering 1000 statures each way, and standing upon a square base. Our Squadron may be divided either into 1000 ranks or into 1000 files. The ranks will form a series of 1000 identical Schemes, the files will form a series of 1000 rectangles, that are of the same breadth, but of dissimilar heights. (See Fig. 14.)

It is easy by this illustration to give a general idea, to be developed as we proceed, of the way in which any large sample, A, of a Population gives rise to a group of Kinsmen, Z, so distant as to retain no family likeness

FIG. 14.



to A, but to be statistically undistinguishable from the Population generally, as regards the distribution of their statures. In this case the samples A and Z would form similar Schemes. I must suppose provisionally, for the purpose of easily arriving at an approximate theory, that tall, short, and mediocre Parents contribute equally to the next generation though this may not strictly be the case.[1]

[1] Oddly enough, the shortest couple on my list have the largest family, namely, sixteen children, of whom fourteen were measured.

Throw A into the form of a Squadron and not of a Scheme, and let us begin by confining our attention to the men who form any two of the rectangular files of A, that we please to select. Then let us trace their connections with their respective Kinsmen in Z. As the number of the Z Kinsmen to each of the A files is considered to be the same, and as their respective Stature-Schemes are supposed to be identical with that of the general Population, it follows that the two Schemes in Z derived from the two different rectangular files in A, will be identical with one another. Every other rectangular file in A will be similarly represented by another identical Scheme in Z. Therefore the 1,000 different rectangular files in A will produce 1,000 identical Schemes in Z, arranged as in Fig. 14.

Though all the Schemes in Z, contain the same number of measures, each will contain many more measures than were contained in the files of A, because the same kinsmen would usually be counted many times over. Thus a man may be counted as uncle to many nephews, and as nephew to many uncles. We will therefore (though it is hardly necessary to do so) suppose each of the files in Z to have been constructed from only a sample consisting of 1,000 persons, taken at random out of the more numerous measures to which it refers. By this treatment Z becomes an exact Squadron, consisting of 1,000 elements, both in rank and in file, and it is identical with A in its constitution, though not in its attitude. The ranks of Z, which are Schemes, have been derived from the files of A, which are rectangles, therefore the two Squadrons must stand at right angles to one another, as in Fig. 14. The upper surface of A is curved in rank, and horizontal in file; that of Z is curved in file and horizontal in rank.

The Kinsmen in nearer degrees than Z will be represented by Squadrons whose forms are intermediate between A and Z. Front views of these are shown in

FIG. 15.

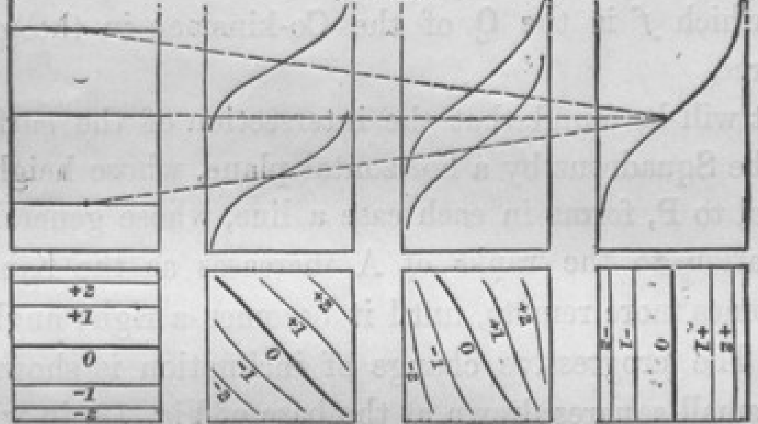


Fig. 15. Consequently they will be somewhat curved both in rank and in file. Also as the Kinsmen of all the members of a Population, in any degree, are themselves a Population having similar characteristics to those of the Population of which they are part, it follows that the elements of every intermediate Squadron when they are broken up and sorted afresh into ordinary Schemes, would form identical Schemes. Therefore, it is clear that a law exists that connects the curvatures in rank and in file, of any Squadron. Both of the curvatures are Curves of Distribution; let us call their Q values respectively r and f. Then if p be the Q of

the general Population, we arrive at a general equation that is true for all degrees of Kinship; namely—

$$r^2 + f^2 = p^2 \qquad (1)$$

but r, the curvature in rank, is a regressed value of p, and may be written wp, w being the value of the regression. Therefore the above equation may be put in the form of

$$w^2p^2 + f^2 = p^2 \qquad (2)$$

in which f is the Q of the Co-kinsmen in the given degree.

It will be found that the intersection of the surfaces of the Squadrons by a horizontal plane, whose height is equal to P, forms in each case a line, whose general inclination to the ranks of A increases as the Kinship becomes more remote, until it becomes a right angle in Z. The progressive change of inclination is shown in the small squares drawn at the base of Fig. 13, in which the lines are the projections of contours drawn on the upper surfaces of the Squadrons, to correspond with the multiples there stated of values of p.

It will be understood from the front views of the four different Squadrons, which form the upper part of Fig. 13, how the Mid-Statures of the Kinsmen to the Men in each of the files of A, gradually becomes more mediocre in the successive stages of kinship until they all reach absolute mediocrity in Z. This figure affords an excellent diagramatic representation, true to scale, of the action of the law of Regression in Descent. I should like to have given in addition, a perspective view of the Squadrons, but failed to draw them clearly, after making many attempts. Their curvatures are so delicate and peculiar that the eye can hardly appreciate them even in a model, without turning it about in different lights and aspects. A plaster model of an intermediate form was exhibited at the Royal Society by Mr. J. D. H. Dickson, when my paper on *Hereditary Stature* was read, together with his solutions of the problems that are given in the Appendix. I also exhibited arrangements of files and ranks that were made of pasteboard. Mr. Dixon mentioned that the mathematical properties of ~~the~~ Surface of ~~a Curve of~~ Frequency, showed that no strictly straight line could be drawn upon it.

Successive Generations of a People.—We are far too apt to regard common events as matters of course, that require no explanation, whereas they may be problems of much interest and of some difficulty, and still await solution.

Why is it, when we compare two large groups of persons selected at random from the same race, but belonging to different generations, that they are usually found to be closely alike? There may be some small statistical dissimilarity due to well understood differences in the general conditions of their lives, but with this I am not concerned. The present question is as to the origin of that statistical resemblance between successive generations which is due to the strict processes of heredity, and which is commonly observed in all forms of life.

In each generation, individuals are found to be tall and short, heavy and light, strong and weak, dark and pale; and the proportions of those who present these several characteristics in their various degrees, tend to be constant. The records of geological history afford striking evidences of this statistical similarity. Fossil remains of plants and animals may be dug out of strata at such different levels, that thousands of generations must have intervened between the periods at which they lived; yet in large samples of such fossils we may seek in vain for peculiarities that distinguish one generation from another, the different sizes, marks, and variations of every kind, occurring with equal frequency in all.

If any are inclined to reply that there is no wonder in the matter, because each individual tends to leave his like behind him, and therefore each generation must, as a matter of course, resemble the one preceding, the patent fact of Regression shows that they utterly misunderstand the case.

We have now reached a stage at which it has become possible to discuss the problem with some exactness, and I will do so by giving mathematical expression to what actually took place in the Statures of that sample of our Population whose life-histories are recorded in the R.F.F. data.

The Males and Females in Generation I. whose **M** has the value of P (viz., 68¼ inches), and whose **Q** is 1·7 inch, were found to group themselves as it were at random, into ~~pairs~~ couples, and thus to form a system of Mid-

Parents. This system had of course the same M as the general Population, but its Q was reduced to $\frac{1}{\sqrt{2}} \times 1{\cdot}7$ inch, or to 1·2 inch. It was next found when the Statures of the Mid-Parents, expressed in the form of $P + (\pm D)$, were sorted into groups in which D was the same (reckoning to the nearest inch), that a Co-fraternity sprung from each group, whose M was of the form $P + (\pm \frac{2}{3}D)$. The next system in which element is one of the Mid-Co-Fraternities must still have the same M as before, of $68\frac{1}{4}$ inches, but its Q will be further reduced from 1·2 inch to $\frac{2}{3} \times 1{\cdot}2$ inch, or to 0·8 inch. Lastly, the individual Co-Fraternals were seen to be dispersed from their respective Mid-Co-Fraternities, with a Q equal in each case to 1·5 inch. The sum of all of the Co-Fraternals forms the Population of Generation II. Consequently the members of Generation II. constitute a system that has an M of $68\frac{1}{4}$ inches and a Q equal to $\sqrt{[(0{\cdot}8)^2 + (1{\cdot}5)^2]}$, = 1·7 inch. These values are identical with those in Generation I.; so the cause of their statistical similarity is tracked out.

There ought to be no misunderstanding as to the character of the evidence or of the reasoning upon which this analysis is based. A small but fair sample of the Population in two successive Generations has been taken, and its conditions as regards Stature have been strictly discussed. It was found that the distribution of Stature was sufficiently Normal to justify our ignoring any shortcomings in that respect. The transmutation of female heights to their male equivalents was justified by the fact that when the individual Statures of a group of females are raised in the proportion of 100 to 108, the Scheme drawn from them fairly coincides with that drawn from male Statures. Marriage selection was found to take no sufficient notice of Stature to be worth consideration; neither was the number of children in Fraternities found to be sensibly affected by the Statures of their Parents. Again, it was seen to be of no consequence when dealing statistically with the offspring, whether their Parents were alike in stature or not, the only datum deserving consideration being the Stature of the Mid-Parent, that is to say, the average value of (1) the Stature of the Father, and of (2) the Transmuted Stature of the Mother. I fully grant that not one of these deductions may be strictly exact, but the error introduced into the conclusions by supposing them to be correct proves not to be worth taking into account in a first approximation.

Precisely the same may be said of the ulterior steps in this analysis. Every one of them is based on the properties of an ideally perfect curve, but in no case has there been need to make any sensible departure from the observed results, except in assigning a uniform value to Q in the different Co-Fraternities. Strictly speaking, that value was found to slightly rise or fall as the Mid-Stature of the Co-Fraternity rose or fell. This suggested the advisability of treating the whole inquiry on the principle of the Geometric Mean, Appendix G. I tried that principle in what seemed to be the most hopeful case among my 18 schemes, but found the gain, if any, to be so small, that I did not care to go on with the experiment. It did not seem to deserve the additional trouble, and I was indisposed to do anything that was not really necessary, which might further confuse the reader. But had I possessed better data, I should have tried the Geometric Mean throughout. In doing so, every measure would be replaced by its logarithm, and these logarithms would be treated just as if they had been the observed values. The conclusions to which they might lead would then be re-transmuted to the numbers of which they were the logarithmic equivalents.

In short, we have dealt mathematically with an ideal population which has similar characteristics to those of a real population, and have seen how closely the behaviour of the ideal population corresponds in every stage to that of the real one. Therefore we have arrived at a closely approximate solution of the problem of statistical constancy, though numerous refinements have been neglected.

Natural Selection.—This hardly falls within the scope of our ~~subject of~~ inquiry into Natural Inheritance, but it will be appropriate to consider briefly the way in which the action of Natural Selection may harmonise with that of pure heredity, and ~~may~~ work together with it in such a manner as not to compromise the normal distribution of faculty. To do this, we must deal with the case that best represents the various possible occurrences, namely that in which the mediocre members of a population are those that are most nearly in harmony with their circumstances. The harmony ought to concern the aggregate of their faculties, combined on the principle adopted in Table 3 after weighting them in the order of their importance. We may deal with any faculty separately, to serve as an example, if its mediocre values happen to be those that are most preservative of life under the majority of circumstances. Such is Stature, in a rudely approximate degree, inasmuch as exceptionally tall or exceptionally short persons have less chance of life than those of moderate size.

It will give more definiteness to the reasoning to take a definite example, even though it be in part an imaginary one. Suppose then, that we are considering the stature of some animal that is liable to be hunted by certain beasts of prey in a particular country. So far as he is big of his kind, he would be better able than the mediocrities to crush through thick grass and foliage whenever he was scampering for his life, to jump over obstacles, and possibly to run somewhat faster than they. So far as he is small of his kind, he would be better able to run through narrower openings, to make quick turns, and to hide himself. Under the general circumstances, it would be found that animals of some particular stature had on the whole a better chance of escape than any other, and if their race is closely adapted to their circumstances in respect to stature, the most favoured stature would be identical with the M of the race. We already know that if we

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call this value P, and write each stature under the form of $P + x$ (in which x includes its sign), and if the number of times with which any value $P + x$ occurs, compared to the number of times in which P occurs, be called y, then x and y are connected by the law of Frequency of Error.

Though the impediments to flight are less unfavourable, on the average, to the stature P than to any other, they differ in different experiences. The course of one animal may chance to pass through denser foliage than usual, or the obstacles in his way may be higher. In that case an animal whose stature exceeded P would have an advantage over mediocrities. Conversely, the circumstances might be more favourable to a small animal.

Each particular line of escape would be most favourable to some particular stature, and whatever the value of x might be, it is possible that the stature $P + x$ might in some cases be more favoured than any other. But the accidents of foliage and soil in a country are characteristic and persistent, and may fairly be considered as approximately of a typical kind. Therefore those that most favour the animals whose stature is P will be more frequently met with than those that favour any other stature of $P + x$, and the frequency of the latter occurrence will diminish rapidly as x increases. If the number of times with which any particular value of $P + x$ is most favoured, as compared with the number of times in which P is most favoured, be called y', we may fairly assume that y' and x are

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connected by the law of Frequency of Error. But though the system of y values and that of y' values may be both subject to the law, it is not for a moment to be supposed that their Q values are necessarily the same.

We have now to show how a large population of animals becomes reduced by the action of natural selection to a smaller one, in which the M value of the statures is unchanged, while the Q value is decreased.

To do this we must first consider the population to have grown up entirely shielded from causes of premature mortality; call their number N. Then suppose them to be assailed by all the lethal influences that have no reference to stature. These would reduce their number to N′, but by the hypothesis, the values of M and of Q would remain unaffected. Next let the influences that act selectively on stature, further reduce the numbers to S; these being the final survivors. We have seen that :—

y = the ~~proportion between the~~ number of individuals who have the stature P $\pm x$, ~~and~~ counting those who have the stature P, as 1.

y' = the ~~proportion between the~~ number of times in which P $\pm x$ is the most favoured stature, ~~and~~ counting those when P is the most favoured, as 1.

Then yy' = the ~~proportion between the~~ number of times that individuals of the stature P $\pm x$ are selected, ~~and~~ counting those in which ~~those~~ individuals of the stature P are selected, as 1.

As the relation between y and x, and between y' and x are severally governed by the law of Frequency of

Error, it follows directly from the formula by which that law is expressed, that the relation between yy' and x is also governed by it. The value of P of course remains the same throughout, but the Q in the system of yy' values is necessarily less than that in the system of y values.

It might well be that natural selection would favour the indefinite increase of numerous separate faculties, if their improvement could be effected without detriment to the rest; then mediocrity in that faculty would not be the safest condition. Thus an increase of fleetness would be a clear gain to an animal liable to be hunted by beasts of prey, if no other useful faculty was thereby diminished.

But a too free use of this "if" would show a jaunty disregard of a real difficulty. Organisms are so knit together that change in one direction involves change in many others; these may not attract attention, but they are none the less existent. Organisms are like ships of war constructed for a particular purpose in warfare, as cruisers, line of battle ships, &c., on the principle of obtaining the utmost efficiency for their special purpose. The result is a compromise between a variety of conflicting desiderata, such as cost, speed, accommodations, stability, weight of guns, thickness of armour, quick steering power, and so on. It is hardly possible in a ~~well thought out~~ ship to make an improvement in any one of these respects, without a sacrifice in other directions. If the fleetness is increased, the engines must be larger, and more space must be given up to coal, and this diminishes the remaining accommodation. Evolution may produce an altogether new type of vessel that shall be more efficient than the old one, but when a vessel of a particular type thoroughly adapted to its functions, it is not possible to produce a mere variety of its class that shall have increased efficiency in some one particular without detriment to the rest. So it is with animals.

Variability in Fraternities.—Human Fraternities are far too small to admit of their Q being satisfactorily measured by the direct method. We are obliged to have recourse to indirect methods, of which no less than four are available. I shall apply each of them to both the Special and to the R.F.F. data; this will give 8 separate estimates of its value, which in the meantime will be called *b*. The four methods are as follow:

First method; by Fraternities each containing the same number of persons:—Let me begin by saying that I had already found in the large Fraternities of Sweet Peas, that the sizes of individuals of whom they consisted were normally distributed, and that their Q was independent of the size, or of the Stature as we may phrase it, of the Mid-Fraternity. We have also seen that the Q is practically the same in all Co-fraternities of men. Therefore it is reasonable to expect that it will also be found to be the same in all their Fraternities, though owing to their small size we cannot assure ourselves of the fact by direct evidence. We will assume this to be the case for the present; it will be seen that the results justify the assumption.

The value of the M of a small Fraternity may be much affected by the addition or subtraction even of a single member, it may therefore be called the *apparent* M, to be distinguished from the *true* M, from which its members would be found to be dispersed, if there had been many more of them. The apparent M approximates towards the true M as the Fraternity increases in size, though at a much slower rate. We have now somehow to get at this *true* M. For distinction and for brevity let us call the *apparent* M of any small Fraternity (MF′), and that of the corresponding *true* M (MF). Then (MF) may be deduced from (MF′) as follows :—

We will begin by allowing ourselves for the moment to imagine the existence of an exceedingly large Fraternity, far more numerous than is physiologically possible, and to suppose that its members vary among themselves just as widely, neither more nor less so, than in the small Fraternities of real life. The (MF′) of our large ideal Fraternity will therefore be identical with its (MF), and its Q will be the same as b. Next, take at random out of this huge ideal Fraternity a large number of small samples, each consisting of the same number, n, of brothers, and call the values of M in the several samples, (MF'_1), (MF'_2), &c. It can easily be shown that (MF'_1), (MF'_2), &c., will be so distributed about the common centre of (MF), that the Prob. Deviation of any one of them from it, that is to say, the Q of their system will $= b \times \frac{1}{\sqrt{n}}$. If $n = 1$, then the Prob. Deviation becomes b, as it should. If $n = 2$, the Prob.

Deviation is determined by the same problem as that which concerned the Q of the Mid-Parentages (page 87), where it was shown to be $b \times \frac{1}{\sqrt{2}}$. By similar reasoning, when $n = 3$, the Prob. Deviation becomes $b \times \frac{1}{\sqrt{3}}$, and so on. When n is infinitely large, the Prob. Deviation is 0; that is to say, the (MF′) values do not differ at all from their common (MF).

Now if we make a collection of human Fraternities, each consisting of the same number, n, of brothers, and note the differences between the (MF′) in each fraternity and the individual brothers, we shall obtain a system of values. By drawing a Scheme from these in the usual way, we are able to find their Q; let us call it d. We ~~can~~ then determine b in terms of d, as follows:— The (MF′) values are distributed about their common (MF), each with the Prob. Deviation of $b \times \frac{1}{\sqrt{n}}$, and the Statures of the individual Brothers are distributed about their respective (MF′) values, each with the Prob. Deviation d. The compound result is the same as if the statures of the individual brothers had been distributed about the common (MF), each with the Prob. Deviation b,

$$\text{consequently } b^2 = d^2 + \frac{b^2}{n}, \text{ or } b^2 = \frac{n}{n-1} d^2.$$

I determined d by observation for four different values of n, having taken four groups of Fraternities, consisting respectively of 4, 5, 6, and 7 brothers, as shown in Table 14. Substituting these four observed values in turns for d in the above formula, I obtained four independent values of b, which are respectively 1·01, 1·01, 1·20, and 1·08; the mean of these is 1·07.

Second Method; from the mean value of Fraternal Regression:—We may look on the Population as composed of a system of Fraternities. Call their respective true centres (see last paragraph) (MF_1), (MF_2), &c. These will be distributed about P with an as yet unknown Prob. Deviation, that we will call c. The individual members of each Fraternity will of course be distributed from their own (MF) with a Q equal to b.

Then $(1\cdot7)^2 = c^2 + b^2$ (1)

Let $P + (\pm F_n)$ be the stature of any individual, and let $P + (\pm M\ F_n)$ be that of the M of his Fraternity, then Problem 4 shows us that:—

the *most probable* value of $\frac{(MF_n)}{F_n}$ is $\sqrt{\frac{c^2}{b^2+c^2}}$ (2)

This is also the value of Fraternal Regression, and therefore equal to $\frac{2}{3}$. Substituting in (2), and replacing c by the value given by (1), we obtain $b = 0\cdot98$ inch.

Third Method; by the Variability in the value of individual cases of Fraternal Regression:—The figures in each line of Table 13 are found to have a Q equal to 1·24 inch, and they are the results of two independent systems of variation. First, the several (MF) values (see last paragraph) are dispersed from the M of all of them with a Q that we will call v. Secondly the individual brothers in each Fraternity are dispersed from their own (MF) with a Q equal to b.

Hence $(1\cdot24)^2 = v^2 + b^2$.

But it is shown Problem 5 that $v = \frac{bc}{\sqrt{(b^2+c^2)}}$;

therefore $(1\cdot24)^2 = b^2 + \frac{b^2c^2}{b^2+c^2}$.

Substituting for c^2 its value of $(1\cdot7)^2 - b^2$ (see last paragraph), we obtain $b = 0\cdot98$ inch.

Fourth Method; from differences between pairs of brothers taken at random:—In the fourth method, Pairs of Brothers are taken at random, and the Differences between the statures in each pair are noted; then, under the following reservation, any one of these differences would have the Prob. value of $\sqrt{2} \times b$. The reservation is, that only as many Differences should be taken out of each Fraternity as are independent. A Fraternity of n brothers admits of $\frac{n(n-1)}{2}$ possible pairs, and the same number of Differences; but as no more than $n-1$ of these are independent, that number only of the Differences 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 sets of Differences from any one Fraternity however large it might be. It was faulty in taking three Differences instead of only two, out of a Fraternity of three brothers, and four Differences, instead of only three, from a Fraternity of four brothers, and therefore giving an increased weight to those Fraternities, but in other respects the system was hardly objectionable. The introduced error must be so slight as to make it scarcely worth while now to go over the work again. By the system adopted, I found the Prob. Difference to be 1·55, which divided by $\sqrt{2}$ gives $b = 1·10$ inch.

Thus far we have dealt with the special data only. The less trustworthy R.F.F. give larger values of b in every case. An epitome of all the results appears in the following table.

Methods and data.	Values of b obtained by different methods and from different data.	
	From Special Data.	From R.F.F. data.[1]
(1) From Fraternities each containing the same number of persons	1·07	1·38
(2) From the mean value of Fraternal Regression............	0·98	1·31
(3) From the Variability of Fraternal Regression..........	1·10	1·14
(4) From Pairs of Brothers taken at random..................	1·10	1·35
Mean...............	1·06	

The data used in the four methods are somewhat different. In (1) I could not deal with small Fraterni-

[1] The R.F.F. results were obtained from brothers only and not from transmuted sisters, except in method (2), where the paucity of the data compelled me to include them.

ties, so all were disregarded that contained fewer than four individuals. In (2) and (3) I could not with safety use large Fraternities. In (4) the method of selection was, as we have seen, quite indifferent. This makes the accordance of the results derived from the Special data all the more gratifying. Those from the R.F.F. data accord less well together. The R.F.F. measures are not sufficiently exact for use in these delicate calculations. Their results, being compounded of b and of their tendency to deviate from exactness, are necessarily too high, and should be discarded. I gather from all this that we may safely consider the value of b to be less than 1·06, and that allowing for some want of precision in the Special data, the very convenient value of 1·00 inch may reasonably be adopted.

Trustworthiness of the Constants.—There is difficulty in correcting the results obtained from the R.F.F. data, though we can make some estimate of their general inaccuracy as compared with the Special data. The reason of the difficulty is that the inaccuracy cannot be ascribed to an uncertainty of equal ± amount in every entry, such as might be due to a doubt of "shoes off" or "shoes on." If it were so, the Prob. Error of a single value of the R.F.F. would be greater than that of one of the Specials, whereas it proves to be the same. It is likely that the inaccuracy is a compound first of the uncertainty above mentioned, whose effect would be to increase the value of the Prob. Error, and secondly of a tendency on the part of my correspondents to record medium statures when they were in doubt, whose effect would be to reduce the value of the Prob. Error. The R.F.F. data in Table 12 run so irregularly that I cannot interpret them with any assurance. The value they give for Fraternal Regression certainly does not exceed $\frac{1}{2}$, and therefore a correction, amounting to no less than $\frac{1}{3}$ of its amount, is required to bring it to a parity with that derived from the Special data (because $\frac{1}{2} + \frac{1}{3} \times \frac{1}{2} = \frac{2}{3}$). Hence it may be argued, that the value of Regression from Mid-Parent to Son, which the R.F.F. data gave as $\frac{2}{3}$, ought to receive a similar correction. If so, it would be raised to $\frac{2}{3} + \frac{2}{9} = \frac{8}{9}$; but I cannot believe this high value to be correct. My first estimate made from the R.F.F. data, was $\frac{3}{5}$, as already mentioned. If this be adopted, the corrected value would be $\frac{4}{5}$, or $\frac{8}{10}$ instead of $\frac{8}{9}$, which might possibly pass. Curiously enough, this value of $\frac{4}{5}$ for Regression from Mid-Parent to Son, coincides with the value of $\frac{2}{5}$ for Regression from a single Parent to Son, which the direct observations showed (see page 99), but which owing to their paucity and to the irregularity of the way in which they ran, I rejected and still reject, at least for the present. While sincerely desirous of obtaining a revised value of average Filial Regression from entirely different and more accurate groups of data, the provisional value already adopted of $\frac{2}{3}$ from Mid-Parent to Son may be accepted as being near enough for the present. It is impossible to revise one datum in the

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R.F.F. series without revising all, as they hang together and support one another.

General View of Kinship.—We are now able to deal with the distribution of statures among the Kinsmen in every near degree, of persons whose statures we know, but whose ancestral statures we either do not know, or do not care to take into account. We are able to calculate Tables for every near degree of Kinship on the form of Table 11, and to reconstruct that same Table in a shape free from irregularities. We must first find the Regression, which we may call w, appropriate to the degree of Kinship in question. Then we have to find a value f for each line of a Table corresponding in form to that of Table 11, in which f was found to be equal to 1·50 inch. We deduce the value of f from that of w by means of the general equation $p^2w^2+f^2=p^2$, p being equal to 1·7 inch. The values to be inserted in the several lines are then calculated from the ordinary table (Table 5) of the "probability integral."

As an example of the first part of the process, let us suppose we are about to construct a table of Uncles and their Nephews, ~~and we wish to~~ we find w and f; ~~we do~~ so as follows: A Nephew is the son of a Brother, therefore in this case we have $w=\frac{1}{3}\times\frac{2}{3}=\frac{2}{9}$; whence $f=1·66$.

The Regression, which we call w, is a convenient and correct measure of family likeness. If the resemblance of the Kinsman to the Man, was on the average as perfect as that of the Man to his own Self, there would be no Regression at all, and the value of w would be 1.

TABLE OF DATA FOR CALCULATING TABLES OF DISTRIBUTION OF STATURE AMONG THE KINSMEN OF PERSONS WHOSE STATURE IS KNOWN.

From group of persons of the same Stature, to their Kinsmen in various near degrees.	Mean regression = w.	$Q = f$ $= p \times \sqrt{(1 - w^2)}$.
Mid-parents to Sons	2 / 3	1·27
Brothers to Brothers	2 / 3	1·27
Fathers or Sons to Sons or Fathers	1 / 3	1·60
Uncles or Nephews to Nephews or Uncles	2 / 9	1·66
Grandsons to Grandparents...	1 / 9	Practically that of Population, or 1·7 inch.
Cousins to Cousins	2 / 27	

On the other hand, if the Kinsmen were on the average no more like the Man than if they had been a group picked at random out of the general Population, then the Regression to P would be complete. The M of the Kinsmen, which is expressed by $P + (\pm wD)$, would in that case become P, whatever might have been the value of D; therefore w must $= 0$. We see by the preceding Table that as a general rule, Fathers or Sons should be held to be only one-half as near in blood as Brothers, and Uncles and Nephews to be one-third as near in blood as Brothers. Cousins are $4\frac{1}{2}$ times as remote as Fathers or as Sons, and 9 times as remote as Brothers. I do not extend the table further, because considerations would have to be taken into account that will be discussed in the next Section.

The remarks made in a previous chapter about

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heritages that blend and those that are mutually exclusive, must be here borne in mind. It would be a poor prerogative to inherit say the fifth part of the peculiarity of some gifted ancestor, but the chance of 1 to 5, of inheriting the whole of it, would be deservedly prized.

Separate Contribution of each Ancestor.—In making the statement that Mid-Parents whose Stature is $P \pm D$ have children whose average stature is $P \pm \frac{2}{3}D$, it is supposed that no separate account has been taken of the previous ancestry. Yet though nothing may be known of them, something is tacitly implied and has been tacitly allowed for, and this requires to be eliminated before we can learn the amount of the Parental bequest, pure and simple. What that something is, we must now try to discover. When speaking of converse Regression, it was shown that a peculiarity in a Man implied a peculiarity of $\frac{1}{3}$ of that amount in his Mid-Parent. Call the peculiarity of the Mid-Parent D, then the implied peculiarity of the Mid-Parent of the Mid-Parent, that is of the Mid-Grand-Parent of the Man, would on the above supposition be $\frac{1}{3}D$, that of the Mid-Great-Grand-Parent would be $\frac{1}{9}D$, and so on. Hence the total bequeathable property would amount to $D(1 + \frac{1}{3} + \frac{1}{9} + \&c.) = D\frac{3}{2}$.

Do the bequests from each of the successive generations reach the child without any, or what, diminution by the way? I have not sufficient data to yield a direct reply, and must therefore try limiting suppositions.

First, suppose the bequests by the various generations

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to be equally taxed; then, as an accumulation of ancestral contributions whose sum amounts to $D\frac{3}{2}$ yields an effective heritage of only $D\frac{2}{3}$, it follows that each piece of heritable property must have been reduced to $\frac{4}{9}$ of its original amount, because $\frac{3}{2} \times \frac{4}{9} = \frac{2}{3}$.

Secondly, suppose the tax not to be uniform, but to be repeated at each successive transmission, and to be equal to $\frac{1}{r}$ of the amount of the property at each stage. In this case the effective heritage would be $D\left(\frac{1}{r} + \frac{1}{3r^2} + \frac{1}{3^2r^3} + -\right) = D\,\frac{3}{3r-1}$, which must, as before, be equal to $D\frac{2}{3}$; whence $\frac{1}{r} = \frac{6}{11}$

Thirdly, it might possibly be supposed that the Mid-Ancestor in a remote generation should on the average contribute more to the child than the Mid-Parent, but this is quite contrary to what is observed. The descendants of what was "pedigree wheat," after being left to themselves for many generations, show little or no trace of the remarkable size of their Mid-Ancestors in the generations just before their time, though their immediate offspring in the first of those generations did so unmistakably.

The results of our only two valid limiting suppositions are therefore, (1) that the Mid-Parental peculiarities, pure and simple, influence the offspring to $\frac{4}{9}$ of their amount; (2) that they influence it to $\frac{6}{11}$ of their 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 $\frac{1}{2}$, and that of the Mid-Grand-Parent as $\frac{1}{4}$, and so on. Consequently the influence of the individual Parent would be $\frac{1}{4}$, and of the individual Grand-Parent $\frac{1}{16}$, and so on. It would, however, be hazardous on the present slender basis, to extend this sequence with confidence to more distant generations.

Pedigree Moths.—I am endeavouring at this moment to obtain data that will enable me to go further, by breeding Pedigree Moths, thanks to the aid of Mr. Frederick Merrifield. The moths *Selenia Illustraria* and *Illunaria* are chosen for the purpose, partly on account of their being what is called double brooded; that is to say, they pass normally through two generations in a single year, which is a great saving of time to the experimenter. They are hardy, prolific and variable, and are found to stand chloroform well, previously to being measured and then paired. Every member of each Fraternity is preserved along three lines of descent—one race of long-winged moths, one of medium-winged, and one of short-winged moths. The three parallel sets are reared under identical conditions, so that the medium series supplies a trustworthy relative base, from which to measure the increasing divergency of the others. No one can be sure of the success of any extensive breeding experiment, but this attempt has been well started and seems to present no peculiar difficulty. Among other reasons for choosing moths for the purpose, is that they are born adults, not changing in stature after they have emerged from the chrysalis and shaken out their wings. Their families are of a convenient size for statistical purposes, say from 50 to 100, neither too few to make satisfactory Schemes nor unmanageably large. They can be mounted as we all know, after their death, with great facility, and be remeasured at leisure. An intelligent and experienced person can carry on a large breeding establishment in a small room, supplemented by a small garden. The methods used and the results up to last spring, have been described by Mr. Merrifield in papers read February and December 1887, and printed in the Transactions of the Entomological Society. I speak of this now, in hopes of attracting the attention of some who are competent and willing to carry on collateral experiments with the same breed, or with altogether different species of moths.

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CHAPTER VIII.

DISCUSSION OF THE DATA OF EYE COLOUR.

Preliminary Remarks.—Data.—Persistence of Eye-Colour in the Population.—Fundamental Eye-Colours.—Principles of Calculation.—Results.

Preliminary Remarks.—In this chapter I will test the conclusions respecting stature by an examination into hereditary eye-colour. Supposing all female measures to have been transmuted to their male equivalents, it has been shown (1) that the possession of each unit of *peculiarity* of stature in a man [that is of each unit of difference from the average of his race] when the man's ancestry is unknown, implies the existence on an average of just one-third of a unit of that peculiarity in his "Mid-Parent," and consequently of the same amount in each of his parents; also just one-third of a unit in his Son; (2) that each unit of peculiarity in each ancestor taken singly, is reduced in transmission according to the following average scale;—a Parent transmits only $\frac{1}{4}$, and a Grand-Parent only $\frac{1}{16}$.

Stature and Eye-colour are not only different as qualities, but they are more contrasted in hereditary behaviour than perhaps any other common qualities. Parents of different Statures usually transmit a blended heritage to their children, but parents of different Eye-colours usually transmit an alternative heritage. If one parent is as much taller than the average of his or her sex as the other parent is shorter, the Statures of their children will be distributed, as we have already seen, in nearly the same way as if the parents had both been of medium height. But if one parent has a light Eye-colour and the other a dark Eye-colour, some of the children will be partly light and ~~some will be~~ dark; they will seldom be medium eye-coloured, like the children of medium eye-coloured parents. The blending in Stature is due to its being the aggregate of the quasi-independent inheritances of many separate parts, while Eye-colour appears to be much less various in its origin. If ~~that~~ notwithstanding this two-fold difference between the qualities of Stature and Eye-colour, the shares of hereditary contribution from the various ancestors are alike in the two cases, as I shall show that they are, we may with some confidence expect that the law by which those hereditary contributions ~~appear~~ to be governed, may be widely, and perhaps universally applicable.

Data.—My data for hereditary Eye-colour are drawn from the same collection of "Records of Family Faculties" ("R.F.F.") as those upon which the inquiries into hereditary Stature were principally based. I have analysed the general value of these data in respect to

Stature, and shown that they were fairly trustworthy. I think they are somewhat more accurate in respect to Eye-colour, upon which family portraits have often furnished direct information, while indirect information has been in other cases obtained from locks of hair that were preserved in the family as mementos.

Persistence of Eye-colour in the Population.—The first subject of our inquiry must be into the existence of any slow change in the statistics of Eye-colour in the English population, or rather in that particular part of it to which my returns apply, that ought to be taken into account before drawing hereditary conclusions. For this purpose I sorted the data, not according to the year of birth, but according to generations, as that method best accorded with the particular form in which all my R.F.F. data are compiled. Those persons who ranked in the Family Records as the "children" of the pedigree, were counted as generation I.; their parents, uncles and aunts, as generation II.; their grandparents, great uncles, and great aunts, as generation III.; their great grandparents, and so forth, as generation IV. No account was taken of the year of birth of the "children," except to learn their age; consequently there is much overlapping of dates in successive generations. We may however safely say, that the persons in generation I. belong to quite a different period to those in generation III., and the persons in II. to those in IV. I had intended to exclude all children under the age of eight years, but in this particular branch of the inquiry, I fear that some cases of young children have been accidentally included. I would willingly have taken a later limit than eight years, but could not spare the data that would in that case have been lost to me.

A great variety of terms are used by the various compilers of the "Family Records" to express Eye-colours. I began by classifying them under the following eight heads;—1, light blue; 2, blue, dark blue; 3, grey, blue-green; 4, dark grey, hazel; 5, light brown; 6, brown; 7, dark brown; 8, black. Then I constructed Table I5.

The accompanying diagram will best convey the significance of the figures in Table I5. Considering that the groups into which the observations are divided are eight in number, the observations are far from being sufficiently numerous to justify us in expecting clean results; nevertheless the curves come out surprisingly well, and in accordance with one another. There can be little doubt that the change, if any, during four successive generations is very small, and much smaller than mere memory is competent to take note of. I therefore disregard a current popular belief in the existence of a gradual darkening of the British population, and shall treat the eye-colours of those classes of our race who have contributed the records, as having been statistically persistent during the period under discussion.

The concurrence of the four curves for the four several generations, affords internal evidence of the trustworthiness of the data. For supposing we had curves that exactly represented the true Eye-colours for the four generations, they would either be concurrent or they would not. If these curves were concurrent, the errors in the R.F.F. data must have been so curiously distributed as to preserve the concurrence. If these curves were not concurrent, then the errors in the R.F.F. data must have been so curiously distributed as to neutralise the non-concurrence. Both of these suppositions are improbable, and we must conclude that the curves really agree, and that the R.F.F. errors are not large enough to spoil the agreement. The close similarity of the two curves, derived respectively from the whole of the male and the whole of the female data, and the more perfect form of the curve derived from the aggregate of all the cases, are additional evidences in favour of the goodness of the data on the whole.

Fundamental Eye-colours.—It is agreed among most writers on the subject (*cf.* A. de Candolle) that the one important division of eye-colours is into the light and the dark. The medium tints are not numerous, but may be derived from any one of four distinct origins. They may be hereditary with no notable variation, they may be varieties of light parentage, they may be varieties of dark parentage, or they may be blends. Medium tints are classed in my list under the heading "4. Dark grey, hazel;" these form only 12·7 per cent. of all the observed cases. In medium tints, the outer portion of the iris is often of a dark grey colour, Percentages of the Various Eye-colours in Four Successive Generations.

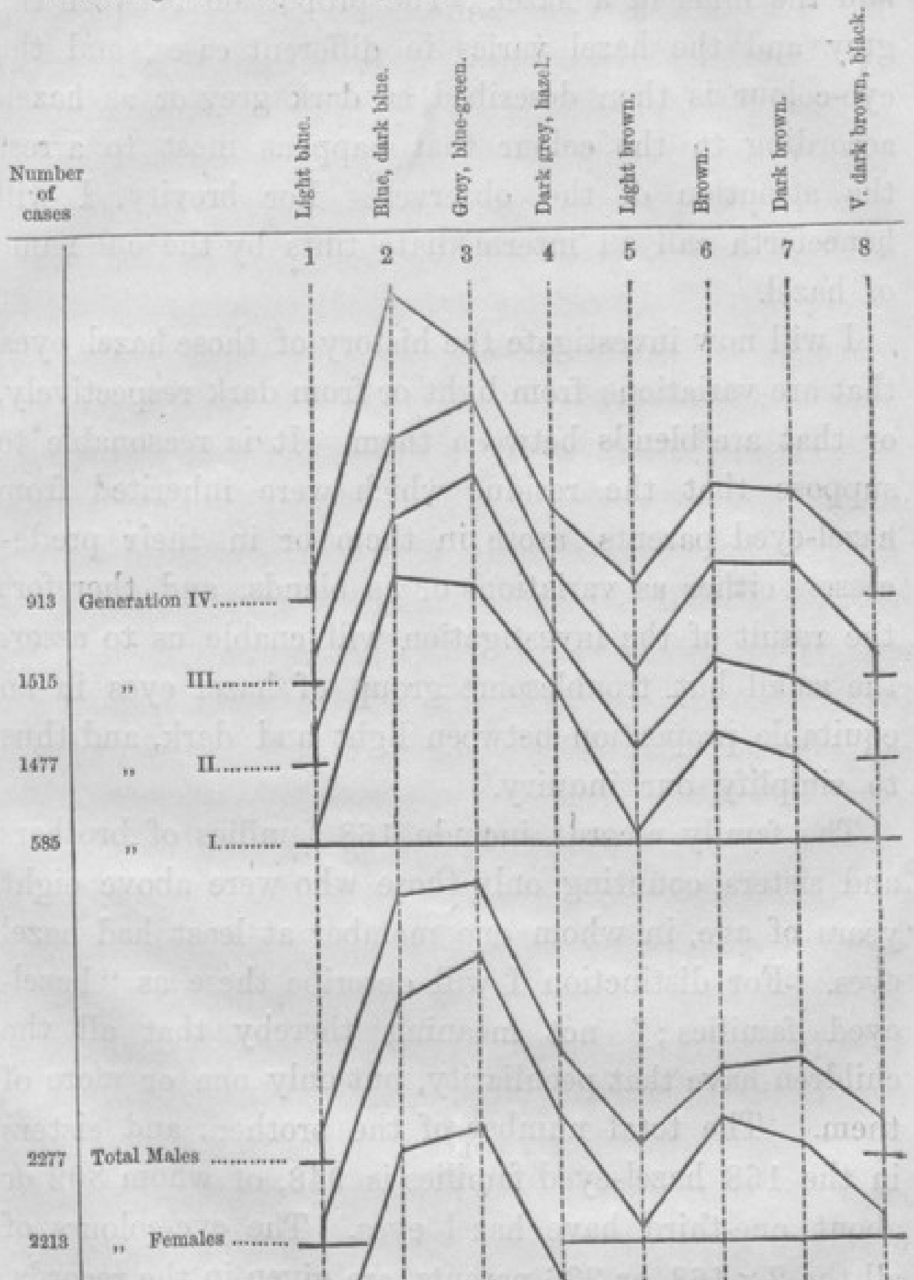

FIG. 16.

and the inner of a hazel. The proportion between the grey and the hazel varies in different cases, and the eye-colour is then described as dark grey or as hazel, according to the colour that happens most to arrest the attention of the observer. For brevity, I will henceforth call all intermediate tints by the one name of hazel.

I will now investigate the history of those hazel eyes that are variations from light or from dark respectively, or that are blends between them. It is reasonable to suppose that the residue which were inherited from hazel-eyed parents, arose in them or in their predecessors either as variations or as blends, and therefore the result of the investigation will enable us to assort the small but troublesome group of hazel eyes in an equitable proportion between light and dark, and thus to simplify our inquiry.

The family records include 168 families of brothers and sisters, counting only those who were above eight years of age, in whom one member at least had hazel eyes. For distinction I will describe these as "hazel-eyed families;" not meaning thereby that all the children have that peculiarity, but only one or more of them. The total number of the brothers and sisters in the 168 hazel-eyed families is 948, of whom 302 or about one-third have hazel eyes. The eye-colours of all the 2×168, or 336 parents, are given in the records, but only those of 449 of the grandparents, whose number would be 672, were it not for a few cases of cousin marriages. Thus I have information concerning about only two-thirds of the grandparents, but this will suffice for our purpose. The results are given in Table 16.

It will be observed that the distribution of eye-colour among the grandparents of the hazel-eyed families is nearly identical with that among the population at large. But among the parents there is a notable difference; they have a decidedly larger percentage of light eye-colour and a slightly smaller proportion of dark, while the hazel element is nearly doubled. A similar change is superadded in the children. The total result in passing from generations III. to I., is that the percentage of the light eyes is diminished from 60 or 61 to 45, therefore by one quarter of its original amount, and that the percentage of the dark eyes is diminished from 26 or 27 to 23, that is to about one-eighth of its original amount, the hazel element in either case absorbing the difference. It follows that the chance of a light-eyed parent having hazel off-spring, is about twice as great as that of a dark-eyed parent. Consequently, since hazel is twice as likely to be met with in any given light-eyed family as in a given dark-eyed one, we may look upon two-thirds of the hazel eyes as being fundamentally light, and one-third of them as fundamentally dark. I shall allot them rateably in that proportion between light and dark, as nearly as may be without using fractions, and so get rid of them. M. Alphonse de Candolle[1] has

[1] Hérédité de la couleur des yeux dans l'espèce humaine," par M. Alphonse de Condolle. "Arch. Sc. Phys. et Nat. Genèva," Aug. 1884, 3rd period. vol. xii. p. 97.

also shown from his data, that *yeux gris* (which I take to be the equivalent of my *hazel*) are referable to a light ancestry rather than to a dark one, but his data are numerically insufficient to warrant a precise estimate of the relative frequency of their derivation from each of these two sources.

In the following discussion I shall deal only with those fraternities in which the Eye-colours are known of the two Parents and of the four Grand-Parents. There are altogether 211 of such groups, containing an aggregate of 1023 children. They do not, however, belong to 211 different family stocks, because each stock which is complete up to the great grand-parents inclusive (and I have fourteen of these) is capable of yielding three such groups. Thus, group 1 contains *a*, the "children;" *b*, the parents; *c*, the grand-parents. Group 2 contains *a*, the father of the "children" and his brothers and his sisters; *b*, the parents of the father; *c*, the grand-parents of the father. Group 3 contains the corresponding selections on the mother's side. Other family stocks furnish two groups. Out of these and other data, Tables 19 and 20 have been made. In Table 19 I have classified the families together whose two parents and four grand-parents present the same combination of Eye-colour, no class, however, being accepted that contains less than twenty children. The data in this table enable us to test the *average* correctness of the law I desire to verify, because many persons and many families appear in the same class, and individual peculiarities

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tend to neutralise each other. In Table 20 I have separately classified on the same system all the families, 78 in number, that consist of six or more children. These data enable us to test the trustworthiness of the law as applied to *individual* families. It will be seen from my way of discussing them, that smaller fraternities than these could not be advantageously dealt with.

It will be noticed that I have not printed the number of dark-eyed children in either of these tables. They are implicitly given, and are instantly to be found by subtracting the number of light-eyed children from the total number of children. Nothing would have been gained by their insertion, while compactness would have been sacrificed.

The entries in the tables are classified, as I said, according to the various combinations of light, hazel, and dark Eye-colours in the Parents and Grand-Parents. There are six different possible combinations among the two Parents, and 15 among the four Grand-Parents, making 6×15, or 90 possible combinations altogether. The number of observations are of course by no means evenly distributed among the classes. I have no returns at all under more than half of them, while the entries of two light-eyed Parents and four light-eyed Grand-Parents are proportionately very numerous.

~~I shall not here discuss~~ The question of marriage selection in respect to Eye-colour, ~~which~~ is a less simple statistical question than at a first sight it may appear to be.

has already been briefly discussed in p. 86. It

Principles of Calculation.—I have next to show how the expectation of Eye-colour among the children of a given family is to be calculated on the basis of the same law that held in respect to stature, so that calculations of the probable distribution of Eye-colours may be made. They are those that fill the three last columns of Tables 19 and 20, which are headed I., II., and III., and are placed in juxtaposition with the observed facts entered in the column headed "Observed." These three columns contain calculations based on data limited in three different ways, in order the more thoroughly to test the applicability of the law that it is desired to verify. Column I. contains calculations based on a knowledge of the Eye-colours of the Parents only; II. contains those based on a knowledge of those of the Grand-Parents only; III. contains those based on a knowledge of those both of the Parents and of the Grand-Parents, and of them only.

I. Eye-colours given of the two Parents—

Let the letter S be used as a symbol to signify the subject (or person) for whom the expected heritage is to be calculated. Let F stand for the words "a parent of S;" G_1 for "a grandparent of S;" G_2 for "a great-grandparent of S," and so on.

We must begin by stating the problem as it would stand if Stature was under consideration, and then modify it so as to apply to Eye-colour. Suppose then, that the amount of the peculiarity of Stature possessed by F is equal to D, and that nothing whatever is known with certainty of any of the ancestors of S except F. We have seen that though nothing may actually be known, yet that something definite is implied about the ancestors of F, namely, that each of his two parents (who will stand in the order of relationship of G_1 to S) will on the average possess $\frac{1}{3}$D. Similarly that each of the four grandparents of F (who will stand in the order of G_2 to S) will on the average possess $\frac{1}{9}$D, and so on. Again we have seen that F, on the average, transmits to S only $\frac{1}{4}$ of his peculiarity; that G_1 transmits only $\frac{1}{16}$; G_2 only $\frac{1}{64}$, and so on. Hence the aggregate of the heritages that may be expected to converge through F upon S, is contained in the following series:—

$$D\left\{\frac{1}{4}+2\left(\frac{1}{3}\times\frac{1}{2^4}\right)+4\left(\frac{1}{9}+\frac{1}{2^6}\right)+\ \&c.\right\}$$

$$=D\left\{\frac{1}{2^2}+\frac{1}{2^3.3}+\frac{1}{2^4.3^2}+\ \&c.\right\}=D\times 0{\cdot}30.$$

That is to say, each parent must in this case be considered as contributing 0·30 to the heritage of the child, or the two parents together as contributing 0·60, leaving an indeterminate residue of 0·40 due to the influence of ancestry about whom nothing is either known or implied, except that they may be taken as members of the same race as S.

In applying this problem to Eye-colour, we must bear in mind that the fractional chance that each member of a family will inherit either a light or a dark Eye-colour, must be taken to mean that that same fraction of the total number of children in the family will probably possess it. Also, as a consequence of this view of the meaning of a fractional chance, it follows that the residue of 0·40 must be rateably assigned between light and dark Eye-colour, in the proportion in which those Eye-colours are found in the race generally, and this was seen to be (see Table 16) as 61·2 : 26·1 ; so I allot 0·28 out of the above residue of 0·40 to the heritage of light, and 0·12 to the heritage of dark. When the parent is hazel-eyed I allot $\frac{2}{3}$ of his total contribution of 0·30, *i.e.*, 0·20 to light, and $\frac{1}{3}$, *i.e.* 0·10 to dark. These chances are entered in the first pair of columns headed I. in Table V.

The pair of columns headed I. in Table 18 shows the way of summing the chances that are given in the columns that have a similar heading in Table 17. By the method there shown, I calculated all the entries that appear in the columns with the heading I. in Tables 19 and 20.

II. Eye-colours given of the four Grand Parents—

Suppose D to be possessed by G_1 and that nothing whatever is known with certainty of any other ancestor of S. Then it has been shown that the child of G_1 (that is F) will possess $\frac{1}{3}$D ; that each of the two parents of G_1 (who stand in the relation of G_2 to S) will also possess $\frac{1}{3}$D ; that each of the four grandparents of G_1 (who stand in the relation of G_3 to S) will possess $\frac{1}{3}$D, and so on. Also it has been shown that the shares of their several peculiarities that will on the average be transmitted by F, G_1, G_2, &c., are $\frac{1}{4}$, $\frac{1}{16}$, $\frac{1}{64}$, &c.,

respectively. Hence the aggregate of the probable heritages from G_1 are expressed by the following series :—

$$D\left\{\frac{1}{3}\times\frac{1}{2^2}+1\times\frac{1}{2^4}+\frac{1}{3}\times 2\times\frac{1}{2^6}+\frac{1}{9}\times 4\times\frac{1}{2^8}+\&c.\right\}$$

$$=D\left\{\frac{1}{12}+\left(\frac{1}{2^4}+\frac{1}{3\times 2^5}+\frac{1}{3^2\times 2^6}+\&c.\right)\right\}=D\times\left(\frac{1}{12}+\frac{3}{40}\right)=D\times 0{\cdot}16.$$

So that each grandparent contributes on the average 0·16 (more exactly 0·1583) of his peculiarity to the heritage of S, and the four grandparents contribute between them 0·64, leaving 36 indeterminate, which when rateably assigned gives 0·25 to light and 0·11 to dark. A hazel-eyed grandparent contributes, according to the ratio described in the last paragraph, 0·10 to light and 0·06 to dark. All this is clearly expressed and employed in the columns II. of Tables 17 and 18.

III. Eye-colours given of the two Parents and four Grand-Parents—

Suppose F to possess D, then F taken alone, and not in connection with what his possession of D might imply concerning the contributions of the previous ancestry, will contribute an average of 0·25 to the heritage of S. Suppose G_1 also to possess D, then his contribution together with what his possession of D may imply concerning the previous ancestry, was calculated in the last paragraph as $D\times\frac{3}{40}=D\times 0{\cdot}075$. For the convenience of using round numbers I take this as $D\times 0{\cdot}08$. So the two parents contribute between them 0·50 of the peculiarity of S, the four grandparents together with what they imply of the previous ancestry contribute 0·32, being an aggregate of 0·82, leaving a residue of 0·18 to be rateably assigned as 0·12 to light, and 0·6 to dark. A hazel-eyed Parent is here reckoned as contributing 0·16 to light and 0·9 to dark; a hazel-eyed Grand-Parent as contributing 0·5 to light and 0·3 to dark. All this is tabulated in Table 17, and its working explained by an example in the columns headed III. of Table 18.

Results.—A mere glance at Tables 19 and 20 will show how surprisingly accurate the predictions are, and therefore how true the basis of the calculations must be. Their *average* correctness is shown best by the totals in Table 19, which give an aggregate of calculated numbers of light-eyed children under Groups I., II., and III. as 623, 601, and 614 respectively, when the observed numbers were 629; that is to say, they are correct in the ratios of 99, 96, and 98 to 100.

Their trustworthiness when applied to *individual* families is shown as strongly in Table 20 whose results are conveniently summarised in Table 18. I have there classified the amounts of error in the several calculations: thus if the estimate in any one family was 3 light-eyed children, and the observed number was 4, I should count the error as 1·0. I have worked to one place of decimals in this table, in order to bring out the different shades of trustworthiness in the three sets of calculations, which thus become very apparent. It will be seen that the calculations in Class III. are by far the most precise. In more than one-half of those calculations the error does not exceed 0·5, whereas in I. and II. more than three-quarters of them are wrong to at least that amount. Only one-quarter of Class III., but somewhere about the half of Classes I. and II., are more than 1·1 in error. In comparing I. with II., we find I. to be slightly but I think distinctly the superior estimate. The relative accuracy of III. as compared with I. and II., is what we should have expected, supposing the basis of the calculations to be true, because the additional knowledge utilised in III., over what is turned to account in I. and II., must be an advantage.

My returns are insufficiently numerous and too subject to uncertainty of observation, to make it worth while to submit them to a more rigorous analysis, but the broad conclusion to which the present results irresistibly lead, is that the same peculiar hereditary relation that was shown to subsist between a man and each of his ancestors in respect to the quality of Stature, also subsists in respect to that of Eye-colour.

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CHAPTER IX.

THE ARTISTIC FACULTY.

Data.—Sexual Distribution.—Marriage Selection.—Regression.—Effect of Bias in Marriage.

Data.—It is many years since I described the family history of the great Painters and Musicians in *Hereditary Genius.* The inheritance of much less exceptional gifts of Artistic Faculty will be discussed in this chapter, and from an entirely different class of data. They are the answers in my R.F.F collection, to the question of "Favourite pursuits and interests? Artistic aptitudes?"

The list of persons who were signalised as being especially fond of music and drawing, no doubt includes many who are artistic in a very moderate degree. Still they form a fairly well defined class, and one that is easy to discuss because their family history is complete. In this respect, they are much more suitable subjects for statistical inquiry than the great Painters and Musicians, whose biographers usually say little or nothing of their non-artistic relatives.

The object of the present chapter is not to give a reply to the simple question, whether or no the Artistic faculty tends to be inherited. A man must be very crotchety or very ignorant, who nowadays seriously doubts the inheritance either of this or of any other faculty. The question is whether or no its inheritance follows a similar law to that which has been shown to govern Stature and Eye-colour, and which has been worked out with some completeness in the foregoing chapters. Before answering this question, it will be convenient to compare the distribution of the Artistic faculty in the two sexes, and to learn the influence it may excerise on marriage selection.

I began by dividing my data into four classes of aptitudes; the first was for music alone; the second for drawing alone; the third for both music and drawing; and the fourth includes all those about whose artistic capacities a discreet silence was observed. After prefatory trials, I found it so difficult to separate aptitude for music from aptitude for drawing, that I determined to throw the three first classes into the single group of Artistic. This and the group of the Non-Artistic are the only two divisions now to be considered.

A difficulty presented itself at the outset in respect to the families that included boys, girls, and young children, whose artistic tastes and capacities can seldom be fairly judged, while they are liable to be appraised too favourably by the compiler of the Family records, especially if he or she was one of their parents. As the practice of picking and choosing is very hazardous in

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statistical inquiries, however fair our intentions may be, and as it in justice always excites suspicion, I decided, though with much regret at their loss, to omit the whole of those who were not adult.

Sexual Distribution.—Men and women, as classes, may differ little in their natural artistic capacity, but such difference as there is in adult life is somewhat in favour of the women. Table 9. *b.* contains 894 cases, 447 of men and 447 of women, divided into three groups according to the rank they hold in the pedigrees. These groups agree fairly well among themselves, and therefore their aggregate results may be freely accepted as trustworthy. They show that 28 per cent. of the males are Artistic and 72 are Not Artistic, and that there are 33 per cent. Artistic females to 67 who are Not Artistic. Part of this female superiority is doubtless to be ascribed to the large share that music and drawing occupy in the education of women, and to the greater leisure that most girls have, or take, for amusing themselves. If the artistic gifts of men and women are naturally the same, as the experience of schools where music and drawing are taught, apparently shows it to be, the small difference observed in favour of women in adult life would be a measure of the smallness of the effect of education compared to that of natural talent. Disregarding the distinction of sex, the figures in Table 9. *b.* show that the number of Artistic to Non-Artistic persons in the general population is in the proportion of $30\frac{1}{2}$ to $69\frac{1}{2}$. The data used in Table 22 refer to a considerably larger number of persons, and do not include more than two-thirds of those employed in Table 9. *b.*, and they make the proportion to be 31 to 69. So we shall be quite correct enough if we reckon that out of ten persons in the families of my R.F.F. correspondents, three on the average are artistic and seven are not.

Marriage Selection.—Table 9. *b.* enables us to ascertain whether there is any tendency, or any disinclination among the Artistic and the Not Artistic, to marry within their respective castes. It shows the observed frequency of their marriages in each of the three possible combinations; namely, both husband and wife artistic; one artistic and one not; and both not artistic. The Table also gives the calculated frequency of the three classes, supposing the pairings to be regulated by the laws of chance. There is I think trustworthy evidence of the existence of some slight disinclination to marry within the same caste, for signs of it appear in each of the three sets of families with which the Table deals. The total result is that there are only 36 per cent. of such marriages observed, whereas if there had been no disinclination but perfect indifference, the number would have been raised to 42. The difference is small and the figures are few, but for the above reasons it is not likely to be fallacious. I believe the facts to be, that highly artistic people keep pretty much to themselves, but that the very much larger body of moderately artistic people do not. A man of highly artistic temperament must look on those who are deficient in it, as barbarians; he would continually crave for a sympathy and response that such persons are incapable of giving. On the other hand, every quiet unmusical man must shrink a little from the idea of wedding himself to a grand piano in constant action, with its vocal and peculiar social accompaniments; but he might anticipate great pleasure in having a wife of a moderately artistic temperament, who would give colour and variety to his prosaic life. On the other hand, a sensitive and imaginative wife would be conscious of needing the aid of a husband who had enough plain common-sense to restrain her too enthusiastic and frequently foolish projects. If wife is read for husband, and husband for wife, the same argument still holds true.

Regression.—Having disposed of these preliminaries, we will now examine into the conditions of the inheritance of the Artistic Faculty. The data that bear upon it are summarised in Table 22, where I have not cared to separate the sexes, as my data are not numerous enough to allow of more subdivision than can be helped. Also, because from such calculations as I have made, the hereditary influences of the two sexes in respect to art appear to be pretty equal: as they are in respect to nearly every other characteristic, exclusive of diseases, that I have examined.

It is perfectly conceivable that the Artistic Faculty in any person might be somehow measured, and its amount determined, just as we may measure Strength, the power of Discrimination of Tints, or the tenacity of Memory. Let us then suppose the measurement of Artistic Faculty to be feasible and to have been often performed, and that the measures of a large number of persons were thrown into a Scheme.

It is reasonable to expect that the Scheme of Artistic Faculty would be approximately Normal in its proportions, like those of the various Qualities and Faculties whose measures were given in Tables 2 and 3.

It is also reasonable to expect that the same law of inheritance might hold good in the Artistic Faculty that was found to hold good both in Stature and in Eye colour; in other words, that value of Filial Regression would in this case also be $\frac{2}{3}$.

We have now to discover whether these assumptions are true without any help from direct measurement. The problem to be solved is a pretty one, and will illustrate the method by which many problems of a similar class have to be worked.

Let the graduations of the scale by which the Artistic Faculty is supposed to be measured, be such that the unit of the scale shall be equal to the Q of the Art-Scheme of the general population. Call the unknown M of the Art-Scheme of the population, P. Then, as explained in page 52, the measure of any individual will be of the form P + (± D), where D is the deviation from P. The first fact we have to deal with is, that only 30 per cent. of the population are Artistic. Therefore no person whose Grade in the Art-Scheme does not exceed 70° can be reckoned as Artistic. Referring to Table 8 we see that the value of D for the Grade of 70° is 0·78; consequently the art-measure of an Artistic person, when reckoned in units of the accepted scale, must exceed P + 0·78.

The average art-measure of all persons whose Grade is higher than 70°, may be obtained with sufficient approximation by taking the average of all the values given in Table 8, for every Grade or more simply, for every odd Grade from 71° to 99° inclusive. It will be found to be 1·71. Therefore an artistic person has, on the average, an art-measure of P + 1·71. We will consider persons of this measure to be representatives of the whole of the artistic portion of the Population. It is not strictly correct to do so, but for approximative purposes this rough and ready method will suffice, instead of the tedious process of making a separate calculation for each Grade.

The M of the Co-Fraternity born of a group of Mid-Parents whose measure is P + 1·71 will be P + ($\frac{2}{3}$ × 1·71) or (P + 1·4). We will call this value C. The Q of this or any other Co-Fraternity may be expected to bear approximately the same ratio to the Q of the general population, that it did in the case of Stature, namely, that of 1·5 to 1·7. Therefore the Q of the Co-Fraternity who are born of Mid-Parents whose Art-measure is C, will be 0·88.

The artistic members of this Co-Fraternity will be those whose measures exceed {P + 0·78}. We may write this value in the form of $\{(P + 1\cdot4) - 0\cdot36\}$, or $\{C - 0\cdot36\}$. Table 8 shows that the Deviation of $-0\cdot36$ is found at the Grade of 40°. Consequently 40 per cent. of this Co-Fraternity will be Non-Artistic and 60 per cent. will be Artistic. Observation Table 23 shows the numbers to be 36 and 64, which is a very happy agreement.

Next as regards the Non-Artistic Parents. The Non-Artistic portion of the Population occupy the 70 first Grades in the Art-Scheme, and may be divided into two groups; one consisting of 40 Grades, and standing between the Grades of 70° and 30°, or between the Grade of 50° and 20 Grades on either side of it, the average Art-measure of whom is P; the other group standing below 30°, whose average measure may be taken to be $P - 1\cdot71$, for the same reason that the group above 70° was taken as $P + 1\cdot71$. Consequently the average measure of the entire Non-Artistic class is

$$\tfrac{1}{70}\{(40 \times P) + 30\,(P - 1\cdot71)\}$$
$$= P - \tfrac{30}{70} \times 1\cdot71 = P - 0\cdot73.$$

Supposing Mid-Parents of this measure, to represent the entire Non-Artistic group, their offspring will be a Co-Fraternity having for their M the value of $P - \{\frac{2}{3} \times 0\cdot73\}$ or $P - 0\cdot49$, which we will call C', and for their Q the value of $0\cdot88$ as before.

Such among them as exceed $\{P - 0\cdot78\}$, which we may write in the form of $\{(P - 0\cdot49) + (1\cdot27)\}$, or $\{C' + 1\cdot27\}$, are Artistic, and they are those who, according to Table 8, rank higher than the Grade 83°. In other words, 83 per cent. of the children of Non-

Artistic parents will be Non-Artistic, and the remainder of 17 per cent. will be Artistic. Observation gives the values of 79 and 21, which is a very fair coincidence.

When one parent is Artistic and the other Not, their joint hereditary influence would be the average of the above two cases; that is to say, $\frac{1}{2}$ (40 + 83), or $61\frac{1}{2}$ per cent. of their children would be Non-Artistic, and $\frac{1}{2}$ (60 + 17), or $38\frac{1}{2}$, would be Artistic. The observed numbers are 61 and 39, which agree excellently well.

We may therefore conclude that the same law of Regression, and all that depends upon it, which governs the inheritance both of Stature and Eye-colour, applies equally to the Artistic Faculty.

Effect of Bias in Marriage.—The slight apparent disinclination of the Artistic and the Not-Artistic to marry in their own caste, is hardly worth regarding, but it is right to clearly understand the extreme effect that might be occasioned by Bias in Marriage. Suppose the attraction of like to like to become paramount, so that each individual in a Scheme married his or her nearest available neighbour, then the Scheme of Mid-Parents would be practically identical with the Scheme drawn from the individual members of the population. In the case of Stature their Q would be 1·7 inch, instead of 1·7 divided by $\sqrt{2}$. The regression and subsequent dispersion remaining unchanged, the Q of the offspring would consequently be increased.

On the other hand, suppose the attraction of contrast to become suddenly paramount, so that Grade 99° paired in an instant with Grade 1°; next 98° with 2°; and so on in order, until the languid desires of 49° and 51° were satisfied last of all. Then every one of the Mid-Parents would be of precisely the same stature P. Consequently their Q would be zero; and that of the system of the Mid-Co-Fraternities would be zero also; hence the Q of the next generation would contract to the Q of a Co-Fraternity, that is to 1·5 inch.

Whatever might be the character or strength of the bias in marriage selection, so long as it remains constant the Q of the population would tend to become constant also, and the statistical resemblance between successive generations of the future Population would be ensured. The stability of the balance between the opposed tendencies of Regression and of Co-Fraternal expansion is due to the Regression increasing with the Deviation. Its effect is like that of a spring acting against a weight; the spring stretches until its gradually increasing resilient force balances the steady pull of the weight, then the two forces of spring and weight are in stable equilibrium. If the weight be lifted by the hand, it will obviously fall down again as soon as the hand is withdrawn; or again, if it be depressed by the hand, the resilience of the spring will become increased, and the weight will rise up again when ~~let go.~~ it is left free to do so.

CHAPTER X.

DISEASE.

Preliminary Problem. [Data.—] —Trustworthiness of R.F.F. Data.—Mixture of Inheritances.—CONSUMPTION: General Remarks; Parent to Child; Distribution of Fraternities; Severely Tainted Fraternities.—Consumptivity.—Data for Hereditary Diseases.

The vital statistics of a population are those of ~~A POPULATION distributed according to age, is like~~ a vast army marching rank behind rank, across the treacherous table-land of life. Some of its members drop out of sight at every step, and a new rank is ever rising up to take the place vacated by the rank that preceded it, and which has already moved on. The population retains its peculiarities although the elements of which it is composed are never stationary, neither are the same individuals present at any two successive epochs. In these respects, a population may be compared to a cloud that seems to repose in calm upon a mountain plateau, while ~~though~~ a gale of wind is ~~may be~~ blowing over it. The outline of the cloud remains ~~is~~ unchanged, although its elements are in violent movement and in a condition of perpetual destruction and renewal. The well understood cause of such clouds is the deflection of a wind laden with invisible vapour, by means of ~~upwards~~ by the sloping flanks of the mountain, up to a level at which the atmosphere is much colder and rarer than below. Part of the invisible vapour with which the wind was charged, becomes thereby condensed into the minute particles of water of which clouds are formed. After a while the process is reversed. The particles of cloud having been carried by the wind across the plateau, are swept down the other side of it again to a lower level, and during their descent they return into invisible vapour. Both in the cloud and in the population, there is on the one hand a continual supply and inrush of new individuals from the unseen; they remain awhile as visible objects, and then disappear. The cloud and the population are composed of elements that resemble each other in the brevity of their existence, while the general features of the cloud and of the population are alike in that they abide.

Preliminary Problem.—The proportion of the population that dies at each age, is well known, and the diseases of which they die are also well known, but the statistics of hereditary disease are as yet for the most part contradictory and untrustworthy.

It is most desirable as a preliminary to more minute inquiries, that the causes of death of a large number of persons should be traced during two successive generations in somewhat the same broad way that Stature and several other peculiarities were traced in the preceding chapters. There are a certain number of recognized groups of disease, which we may call A, B, C, &c., and the proportion of persons who die of these diseases in each of the two generations is the same. The preliminary question to be determined is whether and to what extent those who die of A in the second generation, are more or less often descended from those who died of A in the first generation, than would have been the case if disease were neither hereditarily transmitted nor clung to the same families for any other reason. Similarly as regards B, C, D, and the rest.

This inquiry would be more difficult than those hitherto attempted, because longevity and fertility are both affected by the state of health, and the circumstances of home life and occupation have a great effect in causing and in checking disease. Also because the father and mother are found in some notable cases to contribute disease in very different degrees to their male and female descendants.

I had hoped even to the last moment, that my collection of Family Records would have contributed in some small degree towards answering this question, but after many attempts I find them too fragmentary for the purpose. It ~~is~~ necessary ~~for~~ success to have the completed life-history ~~of two generations; that is~~ of Fraternities who were born some seventy or more years ago, that is, during the earlier part of this century, ~~and that of all~~ their parents, uncles and aunts. My Records contain excellent material of a later date, that will be valuable in future years; but they must bide their time; they are insufficient for the period in question. By attempting to work with incompleted life histories the risk of serious error is incurred.

Data.—The Schedule in Appendix G, which is illustrated in more detail by Tables A and B that follow it, shows the amount of information that I had hoped to obtain from those who were in a position to furnish complete returns. It relates to the "Subject" of the pedigree and to each of his 14 direct ancestors, up to the great-grandparents inclusive, making in all 15 persons. Also, to the Fraternities of which each of these 15 persons was a member. Reckoning the total average number of persons in each fraternity at 5, which is under the mark for my R.F.F collection, questions were thus asked concerning an average of 75 different persons in each family. The total number of the Records that I am able to use, is about 160; so the aggregate of the returns of disease ought to have been about twelve thousand, and should have included the causes of death of perhaps 6,000 of them. As a matter of fact, I have only about one-third of the latter number.

Trustworthiness of R.F.F. data.—The first object was to ascertain the trustworthiness of the medical information sent to me. There is usually much disinclination among families to allude to the serious diseases that they fear to inherit, and it was necessary to learn whether this tendency towards suppression notably vitiated the returns. The test applied was both simple and just.

If consumption, cancer, drink and suicide, appear among the recorded cases of death less frequently than they do in ordinary tables of mortality, then a bias towards suppression could be proved and measured, and would have to be reckoned with; otherwise the returns might be accepted as being on the whole honest and out-spoken. I find the latter to be the case. Sixteen per cent. of the causes of death (or 1 in $6\frac{1}{2}$) are ascribed to consumption, 5 per cent. to cancer, and nearly 2 per cent. to drink and to suicide respectively. Insanity was not specially asked about, as I did not think it wise to put too many disagreeable questions, however it is often mentioned. I dare say that it, or at least eccentricity, is not unfrequently passed over. Careful accuracy in framing the replies appears to have been the rule rather than the exception. In the preface to the blank forms of the *Records of Family Faculties* and elsewhere, I had explained my objects so fully and they were so reasonable in themselves, that my correspondents ~~appear to~~ have entered with interest into what was asked for, and ~~to~~ have shown themselves willing to trust me freely with their family histories. They seem generally to have given all that was known to them, after making much search and many inquiries, and after due references to registers of deaths. The insufficiency of their returns proceeds I feel sure, much less from a desire to suppress unpleasant truths than from pure ignorance, and the latter is in no small part due to the scientific ineptitude of the mass of the members of the medical profession two and more generations ago, when even the stetho-

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scope was unknown. They were then incompetent to name diseases correctly.

Mixture of Inheritances.—The first thing that struck me after methodically classifying the diseases of each family, in the form shown in the Schedule, was their great intermixture. The Tables A and B in Appendix G are offered as ordinary specimens of what is everywhere to be found. They are actual cases, except that I have given fancy names and initials, and for further concealment, have partially transposed the sexes. Imagine an intermarriage between any two in the lower division of these tables, and then consider the variety of inheritable disease to which their children would be liable! The problem is rendered yet more complicated by the metamorphoses of disease. The disease A in the parent does not necessarily appear, even when inherited, as A in the children. We know very little indeed about the effect of a mixture of inheritable diseases, how far they are mutually exclusive and how far they blend; or how far when they blend, they change into a third form. Owing to the habit of free inter-marriage no person can be exempt from the inheritance of a vast variety of of diseases or of special tendencies to them. Deaths by mere old age and the accompanying failure of vital powers without any well defined malady, are very common in my collection, but I do not find as a rule, that the children of persons who die of old age have any marked immunity from specific diseases.

There is a curious double appearance in the Records, the one of an obvious hereditary tendency to disease and the other of the reverse. There are far too many striking instances of coincidence between the diseases of the parents and of the children to admit of reasonable doubt of their being often inherited. On the other hand, when I hide with my hand the lower part of a page such as those in Tables A and B, and endeavour to make a forecast of what I shall find under my hand after studying the upper portion, I am ~~often~~ sometimes greatly mistaken. Very unpromising marriages have often led to fairly good results, especially where the parental disease is one that usually breaks out late in life, as in the case of cancer. The children may then enjoy a fair length of days and die in the end of some other disease; although if that disease had been staved off it is quite possible that the cancer would ultimately have appeared. I have two remarkable instances of this. In one of them, three grandparents out of four died of cancer. In each of the fraternities of which the father and mother were members, one and one person only, died of it. As to the children, although four of them have lived to past seventy years, not one has shown any sign of cancer. The other case differs in details, but is equally remarkable. However diseased the parents may be, it is of course possible that the children may inherit the healthier constitutions of their remoter ancestry. Promising looking marriages are occasionally found to lead to a sickly progeny, but my materials are too scanty to permit of a thorough investigation of these cases.

The general conclusion thus far is, that owing to the hereditary tendencies in each person to disease being usually very various, it is ~~rare~~ not often that useful forecasts can be made concerning the health of the future issue of any newly-married couple.

Consumption.

General Remarks.—The frequency of consumption in England being so great that one in at least every six or seven persons dies of it, and the fact that it usually appears early in life, and is therefore the less likely to be forestalled by any other disease, render it an appropriate subject for statistics. The fact that it may be acquired, although there has been no decided hereditary tendency towards it, introduces no serious difficulty, being more or less balanced by the opposite fact that it may be withstood by sanitary precautions although a strong tendency exists. Neither does it seem worth while to be hypercritical and to dwell overmuch on the different opinions held by experts as to what constitutes consumption. The ordinary symptoms are patent enough, and are generally recognized; so we may be content at first with lax definitions. At the same time, no one can be more strongly impressed than myself with the view that in proportion as we desire to improve our statistical work, so we must be increasingly careful to divide our material into truly homogeneous groups, in order that all the cases contained in the same group shall be alike in every important particular, differing only in petty details. This is far more important than adding to the number

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of cases. My material admits of no such delicacy of division; nevertheless it leads to some results worth mentioning.

In sorting my cases, I included under the head of Consumption all the causes of death described by one or the other following epithets, attention being also paid to the context, and to the phraseology used elsewhere by the same writer:—Consumption; Phthisis; Tubercular disease; Tuberculosis; Decline; Pulmonary, or lung disease; Lost lung; Abscess on lung; Hæmorrhage of lungs (fatal); Lungs affected (here especially the context was considered). All of these were reckoned as actual Consumption.

In addition to these there were numerous phrases of doubtful import that excited more or less reasonable suspicion. It may be that the disease had not sufficiently declared itself to justify more definite language, or else that the phrase employed was a euphemism to veil a harsh truth. Paying still more attention to the context than before, I classed these doubtful cases under three heads:—(1) Highly suspicious; (2) Suspicious; (3) Somewhat suspicious. They were so rated that four cases of the first should be reckoned equivalent to three cases of actual consumption, four cases of the second to two cases, and four of the third to one case.

The following is a list of some of the phrases so dealt with. The occasional appearance of the same phrase under different headings is due to differences in the context:—

1. Highly suspicious:—Consumptive tendency, Con-

sumption feared, and died of bad chill. Chest colds with pleurisy and congestion of lungs. Died of an attack on the chest. Always delicate. Delicate lungs. Hæmorrhage of lungs. Loss of part of lung. Severe pulmonary attacks and chest affections.

2. Suspicious :—Chest complaints. Delicate chest. Cold, cough and bronchitis. Delicate, and died of asthma. Scrofulous tendency.

3. Somewhat suspicious :—Asthma when young. Pulmonary congestion. Not strong; anæmic. Delicate. Colds, coughs. Debility; general weakness. [The context was especially considered in this group.]

Parent to Child.—I have only four cases in which both parents were consumptive; these will be omitted in the following remarks; but whether included or not, the results would be unaltered, for they run parallel to the rest.

There are 66 marriages in which one parent was consumptive; they produced between them 413 children, of whom 70 were actually consumptive; others who being suspiciously so in various degrees, and who when reckoned according to the above method of computation, amounted to 37 cases in addition, forming a total of 107. In other words, 26 per cent. of the children were consumptive. Where neither parent was consumptive, the proportion was as high as 18 or 19 per cent. in the small batch of 23 cases that I tried, but this is clearly too much, because that of the general population is about 16 per cent. Again, by taking each fraternity separately and dividing the quantity of consumption in it by the number of its members, I obtained the average consumptive taint of each fraternity. For instance, if in a fraternity of 10 members there was one actually consumptive member and four "somewhat suspiciously" so, it would count as a fraternity of ten members, of whom two were actually consumptive, and the average taint of the fraternity would be reckoned at one-fifth part of the whole or as 20 per cent.

Treating each fraternity separately in this way, and then averaging the whole of them, the mean taint of the children of one consumptive parent was made out to be 28 per cent.

Distribution of Fraternities.—Next I arranged the fraternities in such way as would show whether, if we reckoned each fraternity as a unit, their respective amounts of consumptive taint were distributed "normally" or not. The results are contained in line A of the following table :—

PERCENTAGE OF CASES HAVING VARIOUS PERCENTAGES OF TAINT.

	Percentages of Taint.					
	0 and under 9	10 and under 19	20 and under 29	30 and under 39	40 and above	Total.
A. 66 cases, one parent consumptive.	27	20	9	15	29	100
B. 84 cases, one brother consumptive.	49	14	10	13	14	100

They struck me as so remarkable, in the way shortly to be explained, that I proceeded to verify them by as different a set of data as my Records could afford. I took every fraternity in which at least one member was consumptive, and treated them in a way that would answer the following question. "One member of a fraternity, whose number is unknown, is consumptive; what is the chance that a named but otherwise unknown brother of that man will be consumptive also?" The fraternity that was taken above as an example, would be now reckoned as one of nine members, of whom one was actually consumptive. There were 84 fraternities available for the present purpose, and the results are given in the line B of the table. The data in A and B somewhat overlap, but for the most part they differ.

They concur in telling the same tale, namely, that it is totally impossible to torture the figures so as to make them yield the single-humped "Curve of Frequency" (Fig. 3 p.38). They make a distinctly double-humped curve, whose outline is no more like the normal curve than the back of a Bactrian camel is to that of an Arabian camel. Consumptive taints reckoned in this way are certainly not "normally" distributed. They depend mainly on one or other of two groups of causes, one of which tends to cause complete immunity and the other to cause severe disease, and these two groups do not blend freely together. Consumption tends to be transmitted strongly or not at all, and in this respect it resembles the baleful influence ascribed to cousin

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marriages, which appears to be very small when statistically discussed, but of whose occasional severity most persons have observed examples.

I interpret these results as showing that consumption is largely acquired, and that the hereditary influence of an acquired attack is small when there is no accompanying "malformation." This last phrase is intended to cover not only a narrow chest and the like, but whatever other abnormal features may supply the physical basis upon which consumptive tendencies depend, and which I presume to be as hereditary as any other malformations.

Severely-tainted Fraternities.—Pursuing the matter further, I selected those fraternities in which consumption was especially frequent, and in which the causes of the deaths both of the Father and of the Mother were given. They were 14 in number, and contained between them a total of 102 children, of whom rather more than half died before the age of 40. Though records of infant deaths were asked for, I doubt if they have been fully supplied. As 102 differs little from 100, the following figures will serve as percentages: 42 died of actual consumption and 11 others of lung disease variously described. Only one case was described as death from heart disease, but weakness of the heart during life was spoken of in a few cases. The remaining causes of death were mostly undescribed, and those that were named present no peculiarity worth notice. I then took out the causes of death of the

Fathers and Mothers and their ages at death, and severally classified them as in the Table below. It must be understood that there is nothing in the Table to show how the persons were paired. The Fathers are treated as a group by themselves, and the Mothers as a separate group, also by themselves.

CAUSES OF DEATH OF THE PARENTS OF THOSE FRATERNITIES IN WHICH CONSUMPTION GREATLY PREVAILED.

Father.	Age at death.	Mother.	Age at death.	Order of ages at death. F.	Order of ages at death. M.
Asthma	70	Consumption	40	51	40
Bronchitis	89	Consumption	43	52	42
Inf. kidneys and bronchitis	73	Consumption	47	59	43
Abscess of liver through lung	(alive)	Consumption	55	62	44
Heart	68	Consumption	65	68	47
Heart	74	Consumption	66	70	50
Apoplexy	62	Water on chest	60	73	58
Apoplexy	75	Weak chest	(alive)	74	60
Apoplexy	78	(1 br. and 2 ss. *d.* of cons.)		74	65
Decay	74	Hæmorrhage of lungs	44	75	66
Cancer	52	Ossification of heart	50	76	73
Senile gangrene	76	Nose bleeding	83	78	74
(2 bros. *d.* of cancer).		Cancer	42	89	83
Mortification of toe	59	Atrophy	73		
Accident	51	Age	74		
(3 bros. and 2 ss. *d.* of cons.)					

Very little account is given of the fraternities to which the fathers and mothers belong, and nothing of interest beyond what is included in the above.

The contrast is here most striking between the tendencies of the Father and Mother to transmit a serious consumptive taint to their children. The cases were selected without the slightest bias in favour of showing this result; in fact, such is the incapacity to see statistical facts clearly until they are pointed out, that I had no idea of the extraordinary tendency on the part of the mother to transmit consumption, as shown in this Table, until I had selected the cases and nearly finished sorting them. Out of the fourteen families, the mother was described as actually dying of consumption in six cases, of lung complaints in two others, and of having highly consumptive tendencies in another, making a total of nine cases out of the fourteen. On the other hand the Fathers show hardly any consumptive taints. One was described as of a very consumptive fraternity, though he himself died of an accident; and another who was still alive had suffered from an abscess of the liver that broke through the lungs. Beyond these there is nothing to indicate consumption on the Fathers' side.

Another way of looking at the matter is to compare the ages at death of the Mothers and of the Fathers respectively, as has been done at the side of the Table, when we see a notable difference between them, the Mid-age of the Mothers being 58, as against 73 of the Fathers.

The only other group of diseases that affords a fair number of instances in my collection, in which fraternities are greatly affected by it, are those of the Heart. The instances are only nine in number, but I give an analysis of them, not for any value of their own, but in order to bring the peculiarities of the consumptive fraternities more strongly into relief by means of comparison. In one of these there was no actual death through it, though three had weak hearts and two others had rheumatic gout and fever. These nine families contained between them sixty-nine children, being at the rate of 7·7 to a family. The number of deaths from heart disease was 24; from ruptured blood vessels, 2; from consumption and lung disease, 8; from dropsy in various forms, 3; from apoplexy, paralysis, and epilepsy, 5; from suicide, 2; from

CAUSES OF DEATH OF THE PARENTS OF THOSE FRATERNITIES IN WHICH HEART DISEASE PREVAILED.

Causes of death.	Ages at death.		Order of ages at death.	
	Father.	Mother.	F.	M.
Heart	59, 70	61, 63, 74	53	61
Apoplexy and paralysis .	74, 78	62, 70, 72	55	62
Consumption	53	...	59	63
Asthma	70	...	70	70
Gout	55	...	70	72
Senile Gangrene	...	81	74	74
Tumour in liver	..	77	75	77
Cancer	75	...	78	81
Living	old.	...	old.	85
Unknown		85		
		2 bros. and 1 sis. *d.* of heart disease and 1 of paralysis *æt.* 40.		

cancer, 1. There is no obvious difference between the diseases of their Fathers and Mothers as shown in the Table, other than the smallness of the number of cases would account for. Their mid-ages at death were closely the same, 70 and 72, and the ages in the two groups run alike.

I must leave it to medical men to verify the amount of truth that may be contained in what I have deduced from these results concerning the distinctly superior power of the mother over that of the father to produce a highly consumptive family. Any physician in large practice among consumptive cases could test the question easily by reference to his note-books. A "highly consumptive" fraternity may conveniently be defined as one in which at least half of its members have actually died of consumption, or else are so stricken that their ultimate deaths from that disease may be reckoned upon. Also to avoid statistical accidents, the fraternities selected for the inquiry should be large, consisting say of ~~five~~ six children and upwards. Of course the numerical proportions given by the above 14 fraternities are very rude indications indeed of the results to which a thorough inquiry might be expected to lead.

Accepting the general truth of the observation that consumptive mothers produce highly consumptive families much more commonly than consumptive fathers, it is easy to offer what seems to be an adequate explanation. Consumption is partly acquired by some form of contagion or infection, and is partly an hereditary malformation. So far as it is due to the latter in the wide sense already given to the word "malformation," it may perhaps be transmissible equally by either parent. But so far as it is contagious or infectious, we must recollect that the child is peculiarly exposed during all the time of its existence before birth, to contagion from its mother. During infancy, it lies perhaps for hours daily in its mother's arms, and afterwards lives much by her side, closely caressed, and breathing the tainted air of her sheltered rooms. The explanation of the fact that we have been discussing appears therefore to be summed up in the single word—Infection.

Consumptivity.—Before abandoning the topic of hereditary consumption, it may be well to discuss it from the same point of view that was taken when discussing the artistic temperament. Consumption being so common in this country that fully one person out of every six or seven die of it, and all forms of hereditary disease being intermixed through marriage, it follows that the whole population must be more or less tainted with consumption. That a condition which we may call "consumptivity," for want of a better word, may exist without showing any outward sign, is proved by the fact that as sanitary conditions worsen by ever so little, more persons are affected by the disease. It seems a fair view to take, that when the amount of consumptivity reaches a certain level, the symptoms of consumption declare themselves; that when it approaches but falls a little short of that level, there are threatening symptoms; that when it falls far below the level, there is a fallacious appearance of perfect freedom from consumptivity. We may reasonably proceed on the hypothesis that consumptivity might somehow be measured, and that if its measurement was made in each of any large group of persons, the measures would be distributed "normally."

So far we are on fairly safe ground, but now uncertainties begin upon which my data fail to throw sufficient light. Longevity, marriage, and fertility must all be affected by the amount of consumptivity, whereas in the case of the faculties hitherto discussed they are not affected to any sensible extent. It however happens that these influences tend to neutralize one another. It is true that consumptive persons die early, and many of them before a marriageable age. On the other hand, they certainly marry earlier as a rule than others, one cause of which lies in their frequent great attractiveness; and again, when they marry, they produce children more quickly than others. Consequently those who die even long before middle age, often contrive to leave large families. The greater rapidity with which the generations follow each other, is also a consideration of some importance. There is therefore a fair doubt whether a group of young persons destined to die of consumption, contribute considerably less to the future population than an equally large group who are destined to die of other diseases. I will at all events assume that consumptivity does not affect the numbers of the adult children, simply as a working hypothesis, and will afterwards compare its results with observed facts.

I should add that the question whether the sexes transmit consumption equally, lies outside the present work, at least for practical purposes; for whether they transmit it equally or not would not affect the results materially. Our list of data is therefore limited to these:—that 16 per cent. of the population die of consumption, that consumptivity is normally distributed, and that the law of hereditary regression from a deviation of three units on the part of either parent to an average of one unit in the child, may be supposed to apply here, just as it did to Stature and to the other subjects of the preceding chapters.

Let the scale by which consumptivity is measured be such that the Q of the general population = 1. Let its M = N, when measured on the same scale; the value of N is and will remain unknown. Let N + C be the number of units of consumptivity that just amount to actual consumption. Our data tell us that 16 per cent. of the population have an amount of consumptivity that exceeds N + C. On referring to Table 8, we find the value of C that corresponds to the Grade of (100°—16°), or of 84°, to be 1·47. Therefore whenever the consumptivity of a person exceeds N + 1·47, he has actual consumption.

Adding together the tabular values in Table 8 at all the odd grades above 84°, we shall find their average value to be 2·23. We may therefore assume that a group of persons each of whom has a consumptivity of N + 2·23 will approximately represent all the grades above 84°. The Co-Fraternity descended from such a group will have an M whose value according to the law of Regression ought to be [N + $\frac{1}{3}$ (2·23)] or [N + 0·74] units.

Those members of the Co-Fraternity are consumptive whose consumptivity exceeds N + 1·47; these are the same as those whose deviation from [N + 0·74] which is the M of the Co-Fraternity, exceeds + 0·73 unit.

Let the Q of the Co-Fraternity be called n. The Grade at which this amount of deviation occurs should be found in Table 8 opposite to the value of 0·73 divided by n.

Next as regards the value to be assigned to n, we may be assured that the Q of a Co-Fraternity cannot exceed that of the general population. Therefore n cannot exceed 1. In the case of Stature the relation between the Q of the Co-Fraternity and that of the Population was found to be as 15 to 17. If the same proportion held good here, its value would be 0·9. This is I think too high an estimate for the following reasons. The variability of the Co-Fraternity depends on two groups of causes. First, on fraternal variability; which itself is due in part to mixed ancestry, and in part to variety of nurture in the same Fraternity, both before as well as after birth. Secondly, it depends upon the variety of ancestry and nurture in different Fraternities. As to the first of the two groups of causes, they seem to affect consumptive fraternities in the same way as others, ~~nothing that is special to consumptivity occurs to me~~, but not so in respect ~~as~~ to the second group. ~~there is a considerable difference.~~ The household arrangements of vigorous, of moderately vigorous, and of invalided parents are not alike. I have already spoken of infection. There is also a tradition in families that are not vigorous, of the necessity of avoiding risks and of never entering professions that involve physical hardship. There is no such tradition in families who are vigorous. Thus there must be much greater variability in the environments of a group of persons taken from the population at large, than there is in a group of consumptive families. It would be quite fair to estimate the value of n at least as low as 0·8.

We have thus three values of n to try; viz. 1, 0·9, and 0·8, of which the first is scarcely possible and the last is much the most suitable of the other two. The corresponding values of 0·73 divided by n, are + 0.73, + 0·81, and + 0·91. Referring to Table 8 we find the Grades corresponding to those deviations to be 69, 71, and 73. We should therefore expect 69, 71, or 73 per cent. of the Co-Fraternity to be non-consumptive, according to the value of n we please to adopt, and the complement to those percentages, viz. 31, 29, or 27, to be consumptive. Observation p. 173, gave the value of 26 by one method of calculation, and 28 by another.

Too much stress must not be laid on this coincidence, because many important points had to be slurred over, as already explained. Still, the *prima facie* result is successful, and enables us to say that so far as this evidence goes, the statistical method we have employed in treating consumptivity seems correct, and that the law of heredity found to govern all the different faculties as yet examined, appears to govern that of consumptivity also, although the constants of the formula differ slightly.

Data for Hereditary Diseases.—The knowledge of the officers of Insurance Companies as to the average value of unsound lives is by the confession of many of them far from being as exact as is desirable. [See, for example, the discussion on a memoir by G. Humphreys, Actuary to the Eagle Insurance Company, read before the Institute of Actuaries, March, 1874.]

Considering the enormous money value concerned, it would seem well worth the while of the higher class of those offices to combine in order to obtain a collection of completed cases for at least two generations, or better still, for three; such as those in Tables *a* and *b*, Appendix G, but much fuller in detail. Being completed and anonymous, there could be little objection on the score of invaded privacy. They would have no perceptible effect on the future insurances of descendants of the families even if they were identified, and they would lay the basis of a very much better knowledge of hereditary disease than we now possess, serving as a step for fresh departures. A main point is that the cases should not be picked and chosen to support any theory, but taken as they come to hand. There must be a vast amount of good material in existence at the command of the medical officers of Insurance Companies. If it were combined and made freely accessible, it would give material for many years' work to competent statisticians, and would be certain, judging from all experience of a like kind, to lead to unexpected results.

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CHAPTER XI.

LATENT ELEMENTS.

Latent Elements not very numerous.—Pure Breed.—Simplification of Hereditary Inquiry.

(Latent Elements not very numerous.—)

It is not possible that more than one half of the varieties and number of each of the parental elements, latent or personal, can on the average subsist in the offspring. For if every variety contributed its representative, each child would on the average contain actually or potentially twice the variety and twice the number of the elements (whatever they may be) that were possessed at the same stage of its life by either of its parents, four times that of any one of its grandparents, 1024 times as many as any one of its ancestors in the 10th degree, and so on, which is absurd. Therefore as regards any variety of the entire inheritance, whether it be dormant or personal, the chance of its dropping out must on the whole be equal to that of its being retained, and only one half of the varieties can on the average be passed on by inheritance. Now we have seen that the *personal* heritage from either Parent is one quarter, therefore as the *total* heritage is one half, it follows that the Latent Elements must follow the same law of inheritance as the Personal ones. In other words, either Parent must contribute on the average only one quarter of the Latent Elements, the remainder of them dropping out and their breed becoming absolutely extinguished.

There seems to be much confusion in current ideas about the extent to which ancestral qualities are transmitted, supposing that what occurs occasionally must occur invariably. If a maternal grandparent be found to contribute some particular quality in one case, and a paternal grandparent in another, it seems to be argued that both contribute elements in every case. This is not a fair inference, as will be seen by the following illustration. A pack of playing cards consists, as we know, of 13 cards of each sort—hearts, diamonds, spades, and clubs. Let these be shuffled together and a batch of 13 cards dealt out from them, forming the deal, No. 1. There is not a single card in the entire pack that may not appear in these 13, but assuredly they do not all appear. Again, let the 13 cards derived from the above pack, which we will suppose to have green backs, be shuffled with another 13 similarly obtained from a pack with blue backs, and that a deal, No. 2, of 13 cards be made from the combined batches. The result will be of the same kind as before. Any card of either of the two original packs may be found in the deal, No. 2, but certainly not all of them. So I conceive it to be with hereditary transmission. No given pair can possibly transmit the whole of their ancestral qualities; on the other hand, there is probably no description of ancestor whose qualities have not been in some cases transmitted to a descendant.

The fact that certain ancestral forms persist in breaking out, such as the zebra-looking stripes on the donkey, is no argument against this view. The reversion may fairly be ascribed to precisely the same cause that makes it almost impossible to wholly destroy the breed of certain weeds in a garden, inasmuch as they are prolific and very hardy, and wage successful battle with their vegetable competitors whenever they are not heavily outmatched in numbers.

If the Personal and Latent Elements are transmitted on the average in equal numbers, it is difficult to suppose that there can be much difference in their variety.

Pure Breed.—In a perfectly pure breed, maintained during an indefinitely long period by careful selection, the tendency to regress towards the M of the general population, would disappear, so far as that tendency may be due to the inheritance of mediocre ancestral qualities, and not to causes connected with the relative stability of different types. The Q of Fraternal Deviations from their respective true Mid-Fraternities which we called b, would also diminish, because it is partly dependent on the children in the same family taking variously after different and unlike progenitors. But the difference between b in a mixed breed such as we have been considering, and the value which we may call β, which it would have in a pure breed, would be very small. Suppose the Prob: Error of the implied Stature of each separate Grand-Parent to be even as great as the Q of the general Population, which is 1·7 inch (it would be less, but we need not stop to discuss its precise value), then the Prob: Error of the implied Mid-Grand-Parental stature would be $\sqrt{\frac{1}{4}} \times 1{\cdot}7$ inch, or say 0·8 inch. The share 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 even than this small value, and we may disregard it. It results that b^2 is greater than $\beta^2 + 0{\cdot}04$. But we have found that $b = 1{\cdot}0$; therefore β is not greater than 0·98.

Simplification of Hereditary Inquiry. — These considerations make it probable that inquiries into human heredity may be much simplified. They assure us that the possibilities of inheritance are not likely to differ much more than those that are actually observed among the members of a large Fraternity. If then we have full life-histories of the Parents and of numerous Uncles and Aunts on both sides, we ought to have a very fair basis for hereditary inquiry. Information of this limited kind is incomparably more easy to obtain than that which I have hitherto striven for, namely, family histories during four successive generations. When the "children" in the pedigree are from 30 to

45 years of age, their own life-histories are sufficiently advanced to be useful, though they are incomplete, and it is still easy for them to compile good histories of their Parents, Uncles, and Aunts. Friends who knew them all would still be alive, and numerous documents such as near relations or personal friends preserve, but which are mostly destroyed at their decease, would still exist. If I were undertaking a fresh inquiry in order to verify and to extend my previous work, it would be on this basis. I should not care to deal with any family that did not number at least six adult children, and the same number of uncles and aunts on both the paternal and maternal sides. Whatever could be learnt about the grandparents and their brothers and sisters, would of course be acceptable, as throwing further light. I should however expect that the peculiarities distributed among any large Fraternity of Uncles and Aunts would fairly indicate the variety of the Latent Elements in the Parent. The complete heritage of the child, on the average of many cases, might then be assigned as follows: One quarter to the personal characteristics of the Father; one quarter to the average of the personal characteristics of the Fraternity as a whole of whom the Father was one member; and similarly as regards the Mother's side.

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CHAPTER XII.

SUMMARY.

THE investigation now concluded is based on the fact that the characteristics of a population that is in harmony with its environment, may remain statistically identical during successive generations. This is true for every characteristic whether it be affected to a great degree by a natural selection, or only so slightly as to be practically independent of it. It was easy to see in a vague way, that an equation admits of being based on this fact; that the equation might serve to suggest a theory of descent, and that no theory of descent that failed to satisfy it could possibly be true.

A large part of the book is occupied with preparations for putting this equation into a working form. Obstacles in the way of doing so, which I need not recapitulate, appeared on every side; they had to be confronted in turns, and then to be either evaded or overcome. The final result was that the higher methods of statistics, which consist in applications of the law of Frequency of Error, were found eminently suitable for expressing the processes of heredity. By their aid, the desired equation was thrown into an exceedingly simple form of approximative accuracy, and it became easy to compare both it and its consequences with the varied results of observation, and thence to deduce numerical results.

A brief account of the chief hereditary processes occupies the first part of the book. It was inserted principally in order to show that a reasonable *à priori* probability existed, of the law of Frequency of Error being found to apply to them. It was not necessary for that purpose to embarrass ourselves with any details of theories of heredity beyond the fact, that descent either was particulate or acted as if it were so. I need hardly say that the idea, though not the phrase of particulate inheritance, is borrowed from Darwin's provisional theory of Pangenesis, but there is no need in the present inquiry to borrow more from it. Neither is it requisite to take Weissmann's views into account, unless I am mistaken as to their scope. It is freely conceded that particulate inheritance is not the only factor to be reckoned with in a complete theory of heredity, but that the stability of the organism has also to be taken into account. This is probably a factor of great importance in regulating the issue of highly bred animals, but it was not found to exercise any sensible influence on the results of the inquiries with which this book is chiefly concerned. Its existence has therefore been only noted, and not otherwise taken into account.

The data on which the results mainly depend had to be collected specially, as no suitable material for the purpose was, so far as I know, in existence. This was done by means of an offer of prizes some years since, that placed in my hands a collection of about 160 useful Family Records. These furnished an adequate though only just an adequate supply of the required data. In order to show the degree of dependence that might be placed on them they were subjected to various analyses, and the result proved to be even more satisfactory than might have been fairly hoped for. Moreover the errors in the Records probably affect different generations in the same way, and would thus be eliminated from the comparative results.

As soon as the character of the problem of Filial descent had become well understood, it was seen that a general equation of the same form as that by which it was expressed, also expressed the connection between kinsmen in every degree. The unexpected law of universal Regression became a theoretical necessity, and on appealing to fact its existence was found to be conspicuous. If the word "peculiarity" be used to signify the difference between the amount of any faculty possessed by a man, and the average of that possessed by the population at large, then the law of Regression may be described as follows. Each peculiarity in a man is shared by his kinsmen, but *on the average* in a less degree. It is reduced to a definite fraction of its amount, quite independently of what its amount might be. The fraction differs in different orders of kinship, becoming smaller as they are more remote. When the kinship is so distant that its effects are not worth taking into account, the peculiarity of the man, however remarkable it may have been, is reduced to zero in his kinsmen. This apparent paradox is fundamentally due to the greater frequency of mediocre deviations than of extreme ones, between deviations separated by equal amounts.

Two causes affect family resemblance; the one is Heredity, the other is Circumstance. That which is transmitted is only a sample taken partly through the operation of "accidents," out of a store of otherwise unused material, and circumstance must always play a large part in the selection of the sample. Circumstance comprises all the additional accidents, and all the peculiarities of nurture both before and after birth, and every influence that may conduce to make the characteristics of one brother differ from those of another. Circumstances are more varied in Co-Fraternities than in Fraternities, and the Grandparents and previous ancestry of members of Co-Fraternities differ; consequently Co-Fraternals differ among themselves more widely than Fraternals.

The average contributions of each ancestor separately to the heritage of the child were determined apparently within narrow limits, for a couple of generations at least. The results proved to be very simple; they assign an average of one quarter from each parent, and one sixteenth from each grandparent. According to this geometrical scale continued indefinitely backwards, the total heritage of the child would be

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accounted for, but the factor of stability of type has to be reckoned with, and this has not yet been adequately discussed.

The ratio of filial Regression is found to be so bound up with co-fraternal variability, that when either is given the other can be calculated. There are no means of deducing the measure of fraternal variability solely from that of co-fraternal. They differ by an element of which the value is thus far unknown. Consequently the measure of fraternal variability has to be calculated separately, and this cannot be done directly, owing to the small size of human families. Four different and indirect methods of attacking the problem suggested themselves, but the calculations were of too delicate a kind to justify reliance on the R.F.F. data. Separate and more accurate measures, suitable for the purpose, had therefore to be collected. The four problems were then solved by their means, and although different groups of these measures had to be used with the different problems, the results were found to agree together.

The problem of expressing the relative nearness of different degrees of kinship, down to the point where kinship is so distant as not to be worth taking into account, was easily solved. It is merely a question of the amount of the Regression that is appropriate to the different degrees of kinship. This admits of being directly observed when a sufficiency of data are accessible, or else of being calculated from the values found in this inquiry. A table of these Regressions was given.

Finally, considerations were offered to show that latent elements probably follow the same law as personal ones, and that though a child may inherit qualities from any one of his ancestors (in one case from this one, and in another case from another), it does not follow that the store of hidden property so to speak, that exists in any parent, is made up of contributions from all or even very many of his ancestry.

Two other topics may be mentioned. Reason was given in p. 16 why experimenters upon the transmission of Acquired Faculty should not be discouraged on meeting with no affirmative evidence of its existence in the first generation, because it is among the grandchildren rather than among the children that it should be looked for. Again, it is hardly to be expected that an acquired faculty, if transmissible at all, would be transmitted without dilution. It could at the best be no more than a variation liable to Regression, which would probably so much diminish its original amount on passing to the grand children as to render it barely recognizable. The difficulty of devising experiments on the transmission of acquired faculties is much increased by these considerations.

The other subject to be alluded to is the fundamental distinction that may exist between two couples whose personal faculties are naturally alike. If one of the couples consist of two gifted members of a poor stock, and the other of two ordinary members of a gifted stock, the difference between

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them will betray itself in their offspring. The children of the former will tend to regress; those of the latter will not. The value of a good stock to the well-being of future generations is therefore obvious, and it is well to recall attention to an early sign by which we may be assured that a new and gifted variety possesses the necessary stability to easily originate a new stock. It is its refusal to blend freely with other forms. Some among the members of the same fraternity might possess the characteristics in question with much completeness, and the remainder hardly or not at all. If this alternative tendency was also witnessed among cousins, there could be little doubt that the new variety was of a stable ~~form~~ character, and therefore capable of being easily developed by interbreeding into a pure and durable ~~variety~~ race.

TABLES.

Table 1.

Strength of Pull.
519 Males aged 23–26.
From measures made at the International Health Exhibition in 1884.

Strength of Pull.	No. of cases observed.	Percentages.	
		No. of cases observed.	Sums from beginning.
Under 50 lbs.	10	2	2
,, 60 ,,	42	8	10
,, 70 ,,	140	27	37
,, 80 ,,	168	33	70
,, 90 ,,	113	21	91
,, 100 ,,	22	4	95
Above 100 ,,	24	5	100
Total	519	100	

TABLE 2.

DATA FOR SCHEMES OF DISTRIBUTION of various qualities and faculties among the persons measured at the Anthropometric Laboratory in the International Exhibition of 1884.

Subject of measurement.	Age.	Unit of measurement.	Sex.	No. of persons in the group.	Values at the undermentioned Grades, from 0° to 100°.										
					5°	10°	20°	30°	40°	50°	60°	70°	80°	90°	95°
Height, standing, without shoes .	23–51	Inches	M.	811	63·2	64·5	65·8	66·5	67·3	67·9	68·5	69·2	70·0	71·3	72·4
			F.	770	58·9	59·9	61·3	62·1	62·7	63·3	63·9	64·6	65·3	66·4	67·3
Height, sitting, from seat of chair	23–51	Inches	M.	1013	33·6	34·2	34·9	35·3	35·4	36·0	36·3	36·7	37 1	37·7	38·2
			F.	775	31·8	32·3	32·9	33·3	33·6	33·9	34·2	34·6	34·9	35·6	36·0
Span of arms . . .	23–51	Inches	M.	811	65·0	66·1	67·2	68·2	69·0	69·9	70·6	71·4	72·3	73·6	74·8
			F.	770	58·6	59·5	60·7	61·7	62·4	63·0	63·7	64·5	65·4	66·7	68·0
Weight in ordinary indoor clothes .	23–26	Pounds	M.	520	121	125	131	135	139	143	147	150	156	165	172
			F.	276	102	105	110	114	118	122	129	132	136	142	149
Breathing capacity.	23–26	Cubic Inches	M.	212	161	177	187	199	211	219	226	236	248	277	290
			F.	277	92	102	115	124	131	138	144	151	164	177	186
Strength of pull as archer with bow.	23–26	Pounds	M.	519	56	60	64	68	71	74	77	80	82	89	96
			F.	276	30	32	34	36	38	40	42	44	47	51	54
Strength of squeeze with strongest hand	23–26	Pounds	M.	519	67	71	76	79	82	85	88	91	95	100	104
			F.	276	36	39	43	47	49	52	55	58	62	67	72
Swiftness of blow .	23–26	Ft. per second	M.	516	13·2	14·1	15·2	16·2	17·3	18·1	19·1	20·0	20·9	22·3	23·6
			F.	271	9·2	10·1	11·3	12·1	12·8	13·4	14·0	14·5	15·1	16·3	16·9
Sight, keenness of — by distance of reading diamond test-type .	23–26	Inches	M.	398	13	17	20	22	23	25	26	28	30	32	34
			F.	433	10	12	16	19	22	24	26	27	29	31	32

TABLE 3.

DEVIATIONS from M in each of the series in Table 2, after reduction to a Scale in which $Q' = 1$, where Q' is the *Mean* of the observed Deviations at the Grades 20°, 30°, 70°, and 80°.

Subject of measurement.	Values of Q'	Unit of measurement in Table 2.	Sex.	No. of persons	Deviations reckoned in units of Q'.										
					5°	10°	20°	30°	40°	50°	60°	70°	80°	90°	95°
Height, standing, without shoes	1·72	Inches	M.	811	2·73	1·98	1·22	0·81	0·35	0	0·35	·76	1·22	1·98	2·61
	1·62		F.	770	2 71	2·10	1·23	·74	·37	0	·37	·80	1·23	1·91	2·46
Height, sitting, from seat of chair	0·95	Inches	M.	1013	2·52	1·89	1·15	·73	·63	0	·31	·73	1·15	1·79	2·31
	0·82		F.	775	2·55	1·95	1·22	·73	·36	0	·36	·85	1·22	2·07	2·55
Span of arms	2·07	Inches	M.	811	2·36	1·83	1·30	·82	·43	0	·33	·72	1·16	1·79	2 36
	1·87		F.	770	2·35	1·87	1·23	·69	·32	0	·37	·80	1·28	1·98	2·67
Weight in ordinary indoor clothes	10·00	Pounds	M.	520	2 20	1·80	1·20	·80	·40	0	·40	·70	1·30	2·20	2·90
	11·00		F.	276	1·80	1·60	1·10	·70	·40	0	·60	·90	1·30	1·80	2·40
Breathing capacity	24·50	Cubic Inches	M.	212	2·32	1·68	1·28	·80	·32	0	·28	.68	1·16	2·32	2·84
	19·00		F.	277	2·39	1·87	1·20	·73	·36	0	·31	·67	1·35	2·03	2·49
Strength of pull as archer with bow	7·50	Pounds	M.	519	2·39	1·86	1·33	·80	·40	0	·40	·80	1·06	1·99	2·92
	5·22		F.	276	1·92	1·06	·80	·53	·27	0	·27	·53	·93	1·46	1·86
Strength of squeeze with strongest hand	7·75	Pounds	M.	519	2·32	1·81	1·16	·77	·39	0	·39	·77	1·29	1·93	2·45
	7·50		F.	276	2 12	1·73	1·20	·66	·40	0	·40	·80	1·33	1·99	2·66
Swiftness of blow	2·37	Ft. per second	M.	516	2·06	1·68	1·22	·80	·34	0	·42	·80	1·18	1·77	2·31
	1·55		F.	271	2·71	2·13	1·35	·84	·38	0	·38	·71	1·10	1·87	2·26
Sight, keenness of — by distance of reading diamond test-type	4·00	Inches	M.	398	3·00	2·00	1·25	·75	·50	0	·25	·75	1·25	1·75	2·25
	5·22		F.	433	2·66	2·28	1·52	·95	·38	0	·38	·57	·95	1·33	1 52
SUMS					43·11	33·12	21·96	13·65	7·00	0	6·57	13·34	21·46	33·96	43·82
MEANS					2·40	1·84	1·22	0·76	0·39	0	0·37	0·74	1·19	1·89	2·43
MEANS multiplied by 1·015, to change unit to $Q = 1$					2·44	1·87	1·24	0·77	0·40	0	0·38	0·75	1·21	1·92	2·47
Normal Values, when $Q = 1$					2·44	1·90	1·25	0·78	0·38	0	0·38	0·78	1·25	1·90	2·44

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Tables 4 to 8 inclusive give data for drawing Normal Curves of Frequency and Distribution. They also show the way in which the latter is derived from the values of the Probability Integral.

The equation for the Probablity Curve[1] is $y = k\,e^{-h^2x^2}$ in which h is "the Measure of Precision." By taking k and h each as unity, the values in Table 4 are computed.

TABLE 4.

Data for a Normal Curve of Frequency.

$$y = e^{-x^2}$$

x	y	x	y	x	y	x	y
0	1·00	± 1·0	0·37	± 2·0	0·0183	± 3·0	0·0001
± 0·2	0·96	± 1·2	0·23	± 2·2	0·0079		
± 0·4	0·85	± 1·4	0·14	± 2·4	0·0032	± infinity	0·0000
± 0·6	0·70	± 1·6	0·78	± 2·6	0·0012		
± 0·8	0·53	± 1·8	0·40	± 2·8	0·0004		

TABLE 5.

Values of the Probability Integral, $\frac{2}{\sqrt{\pi}}\int_0^t e^{-t^2}dt$, for Argument t.

$t\ (=hx)$	·0	·1	·2	·3	·4	·5	·6	·7	·8	·9
0	0·00	0·11	0·22	0·33	0·43	0·52	0·60	0·68	0·74	0·80
1·0	0·843	0·880	0·910	0·934	0·952	0·966	0·976	0·984	0·989	0·923
2·0	·9953	·9970	·9981	·9989	·9993	·9996	·9998	·9999	·9999	·9999
infinite	1·0000									

When $t = ·4769$ the corresponding tabular is ·50; therefore, ·4769 is the value of the "Probable Error."

[1] See Merriman, *On the Method of Least Squares* (Macmillan, 1885), pp. 26, 186, where fuller Tables than 4, 5, and 6 will be found.

TABLE 6.

Values of the Probability Integral for Argument $\frac{t}{0.4769}$; that is, when the unit of measurement = the Probable error.

Multiples of the Probable Error.	·0	·1	·2	·3	·4	·5	·6	·7	·8	·9
0	0·00	0·65	0·11	0·16	0·21	0·26	0·31	0·36	0·41	0·46
1·0	·50	·54	·58	·62	·66	·69	·72	·75	·78	·80
2·0	·82	·84	·86	.88	·89	·91	·92	·93	·94	·95
3·0	·957	.964	·969	·974	·978	·982	·985	·987	·990	·992
4·0	·9930	.9943	·9954	·9963	·9970	·9976	·9981	·9985	·9988	·9990
5·0	·9993	·9994	·9996	·9997	·9997	·9998	·9998	·9999	·9999	·9999
infinite	1·000									

Tables 5 and 6 show the proportion of cases in any Normal system, in which the amount of Error lies within various extreme values, the total number of cases being reckoned as 1·0. Here no regard is paid to the sign of the Error, whether it be *plus* or *minus*, but its amount is alone considered. The unit of the scale by which the Errors are measured, differs in the two Tables. In Table 5 it is the "Modulus," and the result is that the Errors in one half of the cases, that is in 0·50 of them lie within the extreme value (found by interpolation) of 0·4769, while the other half exceed that value. In Table 6 the unit of the scale is 0·4769. It is derived from Table 5 by dividing all the tabular entries by that amount. Consequently one half of the cases have Errors that do not exceed 1·0 in terms of the new unit, and that unit is the Probable Error of the System. It will he seen in Table 6 that the entry of ·50 stands opposite to the argument of 1·0.

If it be desired to transform Tables 5 and 6 into others that shall show the proportion of cases in which the *plus* Errors and the *minus* Errors respectively lie within various extreme limits, their entries would have to be halved.

Let us suppose this to have been done to Table 6, and that a new Table, which it is not necessary to print, has been thereby produced and which we will call 6*a*. Next multiply all the entries in the new Table by 100 in order to make them refer to a total number of 100 cases, and call this second Table 6*b*. Lastly make a converse Table to 6*b*; one in which the arguments of 6*b* become the entries, and the entries of 6*b* become the arguments. From this Table 7 is made. For example, in Table 6, opposite to the argument 1·0 the entry of ·50 is found; that entry becomes ·25 in 6*a*, and 25 in 6*b*. In Table 7 25 is the argument, and the corresponding entry is 25. The meaning of this is, that in 25 per cent. of the cases the greatest of the Errors just attains to ± 1·0. Similarly the Table shows that in 30 per cent. of the cases, the greatest of the Errors just attains to ± 1·25; in 40 per cent. to 1·90, and so on. These various percentages correspond to the centesimal Grades in a Curve of Distribution, when the Grade 0° is placed at the middle of the axis, which is the point where it is cut by the Curve, and where the other Grades are reckoned outwards on either hand, up to + 50° on the one side, and to — 50° on the other.

To recapitulate:—In order to obtain Table 7 from the primary Table 5, we first halve each of the entries in the body of Table 5, then we multiply each of the arguments by 100, and divide it by ·4769. Then we expand the Table by interpolations, so as to include among its entries every whole number from 1 to 99 inclusive. Selecting these and disregarding the rest, we turn them into the arguments of Table 7, and their corresponding arguments into the entries in Table 7.

TABLE 7.

ORDINATES TO NORMAL CURVE OF DISTRIBUTION

on a scale whose unit = the Probable Error; and in which the 100 Grades run from 0° to +50° on the one side, and to −50° on the other.

Grades.	0	1	2	3	4	5	6	7	8	9
0	0·00	0·04	0·07	0·11	0·15	0.19	0·22	0·26	0·30	0·34
10	0·38	0·41	0·45	0·49	0·53	0·57	0·61	0·65	0·69	0·74
20	0·78	0·82	0·86	0·97	0·95	1·00	1·05	1·10	1·15	1·20
30	1·25	1·30	1·36	1·42	1·47	1·54	1·60	1·67	1·74	1·82
40	1·90	1·99	2·08	2·19	2·31	2·44	2·60	2·79	3·05	3·45

But in the Schemes, the 100 Grades do not run from—50° through 0° to + 50°, but from 0° to 100°. It is therefore convenient to modify Table 7 in a manner that will admit of its being used directly for drawing Schemes without troublesome additions or subtractions. This is done in Table 8, where the values from 50° onwards, and those from 50° backwards are identical with those from 0° to ± 50° in Table 7, but the first half of those in Table 8 are positive and the latter half are negative.

Table 8.

Ordinates to Normal Curve of Distribution on a scale whose unit = the Probable Error, and in which the 100 Grades run from 0° to 100°.

Grades	0	1	2	3	4	5	6	7	8	9
0	−∞	−3·45	−3·05	−2·79	−2·60	−2·44	−2·31	−2·19	−2·08	−1·99
10	−1·90	−1·82	−1·74	−1·67	−1·60	−1·54	−1·47	−1·42	−1·36	−1·30
20	−1·25	−1·20	−1·15	−1·10	−1·05	−1·00	−0·95	−0·91	−0·86	−0·82
30	−0·78	−0·74	−0.69	−0·65	−0·61	−0·57	−0·53	−0·49	−0·45	−0·41
40	−0·38	−0·34	−0·30	−0·26	−0·22	−0·19	−0·15	−0·11	−0·07	−0·04
50	0·00	+0·04	+0·07	+0·11	+0·15	+0·19	+0·22	+0·26	−0·30	+0·34
60	+0·38	+0·41	+0·45	+0·49	+0·53	+0·57	+0·61	+0·65	+0·69	+0·74
70	+0·78	+0·82	+0·86	+0·91	+0·95	+1·00	+1·05	+1·10	+1·15	+1·20
80	+1·25	+1·30	+1·36	+1·42	+1·47	+1·54	+1·60	+1·67	+1·74	+1·82
90	+1·90	+1·99	+2·08	+2·19	+2·31	+2·44	+2·60	+2·79	+3·05	+3·45

Examples of the way in which Table 8 is to be read :—

The ordinate at 0° is − infinity ; at 10° it is −1·90 ; at 11° it is −1·82 ; at 25° it is −1·00 ; at 75° it is +1·00. The Table does not go beyond Grade 99°. At the Grade 100°, the ordinate would be + infinity.

TABLE 9.

MARRIAGE SELECTION IN RESPECT TO STATURE.

The 205 male parents and the 205 female parents are each divided into three groups—T, M, and S, and *t*, *m*, and *s*, respectively—that is, Tall, Medium, and Short (medium male measurements being taken as 67 inches, and upwards to 70 inches). The number of marriages in each possible combination between them ~~(see Table 8)~~ were then counted, with the result that men and women of contrasted heights, Short and Tall, or Tall and Short, married about as frequently as men and women of similar heights, both Tall or both Short; there were 32 cases of the one to 27 of the other.

S., t. 12 cases.	M., t. 20 cases.	T., t. 18 cases.
S., m. 25 cases.	M., m. 51 cases.	T., m. 28 cases.
S., s. 9 cases.	M., s. 28 cases.	T., s. 14 cases.

Short and tall, 12 + 14 = 32 cases.
Short and short, 9 } = 27 cases.
Tall and tall, 18 }

We may therefore regard the married folk as couples picked out of the general population at haphazard when applying the law of probabilities to heredity of stature.

TABLE 9A.

MARRIAGE SELECTION IN RESPECT TO EYE-COLOUR in 78 Parental Couples.

Eye Colour of		No. of cases observed.	Per Cents.				Eye Colour of Husband and Wife.
Husband	Wife.		Obs.	Chance.	Observed.	Chance.	
Light Hazel Dark	Light Hazel Dark	29 2 6	37 3 8	37 2 7	48	46	Alike
Light Hazel	Hazel Light	18	23	15	28	22	Half-contrasted
Hazel Dark	Dark Hazel	4	5	7			
Light Dark	Dark Light	19	24	32	24	32	Contrasted

The chance combinations in pairs are calculated for a population containing 61·2 per cent. of Light Eye-colour, 12·7 of Hazel, and 26·1 of Dark.

TABLE 9B.

MARRIAGES OF THE ARTISTIC AND THE NOT ARTISTIC.

Rank in Pedigrees.	No. of persons.	Percentages.									
		Males.		Females.		Pairs of artistic and not artistic persons.					
						Marriages observed.			Chance combinations.		
		art.	not.	art.	not.	both art.	1 art. 1 not.	both not.	both art.	1 art. 1 not.	both not.
Parents	326	32	68	39	61	14	31	50	12	46	42
Paternal grandparents ..	280	27	73	30	70	12	31	57	8	41	51
Maternal grandparents..	288	24	76	28	72	9	41	50	7	39	54
Totals and means...	894	28	72	33	67	12	36	52	9	42	49
Tastes of Husband and Wife—alike						12 + 52 = 64			9 + 49 = 58		
,, ,, ,, contrasted.......						36			42		

TABLE 10.

EFFECT UPON ADULT CHILDREN OF DIFFERENCES IN HEIGHT OF THEIR PARENTS.

Difference in inches between the Heights of the Parents.	Proportion per 50 of cases in which the Heights[1] of the Children deviated to various amounts from the Mid-filial Stature of their respective families.					Number of Children whose Heights were observed. (Total 525.)
	Less than 1 inch.	Less than 2 inches.	Less than 3 inches.	Less than 4 inches.	Less than 5 inches.	
Under 1 inch	21	35	43	46	48	105
1 and under 2	23	37	46	49	50	122
2 ,, 3	16	34	41	45	49	112
3 ,, 5	24	35	41	47	49	108
5 and above...........	18	30	40	47	49	78

[1] Every female height has been transmuted to its male equivalent by multiplying it by 1·08, and only those families have been included in which the number of adult children amounted to six, at least.

NOTE.—When these figures are protracted into curves, it will be seen—(1) that they run much alike; (2) that their peculiarities are not in sequence; and (3) that the curve corresponding to the first line occupies a medium position. It is therefore certain that differences in the heights of the Parents have on the whole an inconsiderable effect on the heights of their Offspring.

TABLE 11 (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 or Values of M.
	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 72·5...	...	...	...	...	...	...	...	...	...	...	...	1	3	...	4[1]	5[1]	
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.

[1] I have reprinted this Table without alteration from that published in the *Proc. Roy. Soc.*, notwithstanding a small blunder since discovered in sorting the entries between the first and second lines. It is obvious that 4 children cannot have 5 Mid-Parents. The first line is not considered at all, on account of the paucity of the numbers it contains. The bottom line, which looks suspicious, is correct.

TABLE 12 (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 73·2		
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	68·9
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	

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TABLE 13 (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	

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TABLE 14 (Special Data).

DEVIATIONS OF INDIVIDUAL BROTHERS FROM THEIR MID-FRATERNAL STATURES.

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	...

TABLE 15.

FREQUENCY OF DIFFERENT EYE-COLOURS IN FOUR SUCCESSIVE GENERATIONS.

Sex and the No. of the (ascending) generation.	No. of cases of eye-colour observed.									Percentages.								
	1. Light blue.	2. Blue. Dark blue.	3. Grey. Blue-green.	4. Dark grey. Hazel.	5. Light brown.	6. Brown.	7. Dark Brown.	8. Very dark brown. Black.	Totals.	1. Light blue.	2. Blue. Dark blue.	3. Grey. Blue-green.	4. Dark Grey. Hazel.	5. Light brown.	6. Brown.	7. Dark brown.	8. Very dark brown. Black.	Totals.
Males IV.	13	177	136	40	2	39	44	12	463	2·8	38·2	29·4	8·6	0·4	8·4	9·5	2·6	99·9
Males III.	19	234	233	84	3	79	97	24	773	2·4	30·3	30·1	10·9	0·4	10·1	12·6	3·1	99·9
Males II.	30	167	236	108	8	83	74	36	742	4·0	22·5	31·8	14·6	1·1	11·2	10·0	4·8	100·0
Males I.	3	89	82	47	1	37	31	9	299	1·0	28·9	27·4	15·7	0·3	12·4	10·4	3·0	100·0
General	65	687	687	279	14	238	246	81	2277	2·9	29·3	30·2	12·3	0·6	10·4	10·8	3·6	100·0
Females IV.	7	132	114	48	2	70	58	19	450	1·5	29·3	25·3	10·7	0·4	15·6	12·9	4·2	99·9
Females III.	22	173	241	89	7	100	98	17	742	2·9	23·3	32·5	12·1	0·9	13·5	12·5	2·3	100·0
Females II.	21	210	241	98	3	78	60	24	735	2·9	28·6	32·8	13·3	0·4	10·6	8·2	3·3	100·1
Females I.	6	78	82	55	5	33	22	5	286	2·1	27·3	28·7	19·2	1·7	11·5	7·7	1·7	99·0
General	56	593	678	290	17	281	233	65	2213	2·5	26·8	30·6	13·1	0·8	12·7	10·5	2·9	99·9
Males and Females IV.	20	309	240	88	4	109	102	31	913	2	34	27	10	1	12	11	3	100
Males and Females III.	41	407	474	173	10	179	190	41	1515	3	27	31	11	1	12	12	3	100
Males and Females II.	51	377	477	206	11	161	134	60	1477	3	26	32	14	1	11	9	4	100
Males and Females I.	9	167	164	102	6	70	53	14	585	1	29	28	18	1	12	9	2	100
General	121	1260	1365	569	31	519	479	146	4490	2·7	28·1	30·4	12·7	0·7	11·6	10·7	3·3	100·2

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TABLE 16.

THE DESCENT OF HAZEL-EYED FAMILIES.

	Total cases.	Observed.			Percentages.		
		Light.	Hazel.	Dark.	Light.	Hazel.	Dark.
General population	4490	2746	569	1175	61·2	12·7	26·1
III. Grandparents...	449	267	61	121	60	13	27
II. Parents	336	165	85	86	49	25	26
I. Children..............	948	430	302	216	45	32	23

TABLE 17.

CALCULATED CONTRIBUTIONS OF EYE-COLOUR.

Contribution to the heritage from each.	Data limited to the eye-colours of the					
	2 parents.		4 grandparents.		2 parents and 4 grandparents.	
	I.		II.		III.	
	Light.	Dark.	Light.	Dark.	Light.	Dark.
Light-eyed parent.........	0·30	...	...	...	0·25	...
Hazel-eyed parent.........	0·20	0·10	...	...	0·16	0·09
Dark-eyed parent	...	0·30	...	...	...	0·25
Light-eyed grandparent..	...	...	0·16	...	0·08	...
Hazel-eyed grandparent.	...	...	0·10	0·06	0·05	0·03
Dark-eyed grandparent...	...	...	...	0·16	...	0·08
Residue, rateably assigned	0·28	0·12	0·25	0·11	0·12	0·06

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TABLE 18.

EXAMPLE OF ONE CALCULATION IN EACH OF THE THREE CASES.

Ancestry and their eye-colours.	I. No. about whom data exist.	I. Contribute to Light.	I. Contribute to Dark.	II. No. about whom data exist.	II. Contribute to Light.	II. Contribute to Dark.	III. No. about whom data exist.	III. Contribute to Light.	III. Contribute to Dark.
Light-eyed parents.	2	0·60	...	...	...	...	...	...	...
Hazel-eyed parents.	...	...	...	...	...	...	1	0·16	0·09
Dark-eyed parents.	...	...	...	...	...	...	1	...	0·25
Light-eyed grand-parents.	...	...	...	1	0·16	...	1	0·08	...
Hazel-eyed grand-parents.	...	...	...	2	0·20	0·12	2	0·10	0·06
Dark-eyed grand-parents.	...	...	...	1	...	0·16	1	...	0·08
Residue, rateably as signed	...	0·28	0·12		0·25	0·11		0·12	0·06
Total contributions	...	0·88	0·12		0·61	0·39		0·46	0·54
		1·00			1·00			1·00	

Table 19.

Observed and Calculated Eye-colours in 16 Groups of Families.

Those families are grouped together in whom the distribution of Light, Hazel, and Dark Eye-colour among the Parents and Grandparents is alike. Each group contains at least Twenty Brothers or Sisters.

Eye-colours of the						Total children.	Number of the light eye-coloured children.			
Parents.			Grandparents.				Observed.	Calculated.		
Light.	Hazel.	Dark.	Light.	Hazel.	Dark.			I.	II.	III.
2	...	...	4	...	...	183	174	161	163	172
2	...	...	3	1	...	53	46	47	44	48
2	...	...	3	...	1	92	88	81	67	79
2	...	...	2	1	1	27	26	24	18	22
...	...	2	2	...	2	22	11	6	12	6
1	1	...	3	1	...	62	52	48	51	51
1	1	...	3	...	1	42	30	33	31	32
1	1	...	2	2	...	31	28	24	24	20
1	1	...	2	...	2	49	35	38	28	34
1	1	...	2	1	1	31	25	24	21	23
1	...	1	3	...	1	76	45	44	55	46
1	...	1	2	...	2	66	30	38	38	35
1	...	1	2	...	1	27	15	16	18	16
1	...	1	1	...	3	20	9	12	8	9
1	...	1	1	1	2	22	8	13	11	11
...	1	1	1	1	2	24	9	14	12	10
							629	623	601	614

TABLE 20.

OBSERVED AND CALCULATED EYE-COLOURS IN 78 SEPARATE FAMILIES, EACH OF NOT LESS THAN SIX BROTHERS OR SISTERS.

Eye-colours of the						Total children.	Number of the light eye-coloured children.			
Parents.			Grandparents.				Observed.	Calculated.		
Light.	Hazel.	Dark.	Light.	Hazel.	Dark.			I.	II.	III.
2	...	...	4	...	...	6	6	5·3	5·3	5·6
2	...	...	4	...	...	6	6	5·3	5·3	5·6
2	...	...	4	...	...	6	6	5·3	5·3	5·6
2	...	...	4	...	...	6	5	5·3	5·3	5·6
2	...	...	4	...	...	7	7	6·2	6·2	6·6
2	...	...	4	...	...	7	7	6·2	6·2	6·6
2	...	...	4	...	...	7	7	6·2	6·2	6·6
2	...	...	4	...	...	7	7	6·2	6·2	6·6
2	...	...	4	...	...	7	7	6·2	6·2	6·6
2	...	...	4	...	...	8	8	7·0	7·1	7·5
2	...	...	4	...	...	8	8	7·0	7·1	7·5
2	...	...	4	...	...	8	8	7·0	7·1	7·5
2	...	...	4	...	...	8	8	7·0	7·1	7·5
2	...	...	4	...	...	8	7	7·0	7·1	7·5
2	...	...	4	...	...	8	7	7·0	7·1	7·5
2	...	...	4	...	...	12	12	10·6	10·7	11·3
2	...	...	3	1	...	7	7	6·2	5·8	6·4
2	...	...	3	1	...	10	4	8·8	8·3	9·1
2	...	...	3	1	...	12	12	10·6	10·0	10·9
2	...	...	3	...	1	7	6	6·2	5·1	6·0
2	...	...	3	...	1	8	8	7·0	5·8	6·9
2	...	...	3	...	1	9	9	7·9	6·6	7·7
2	...	...	3	...	1	9	9	7·9	6·6	7·7
2	...	...	3	...	1	9	7	7·9	6·6	7·7
2	...	...	3	...	1	10	10	8·8	7·3	8·6
2	...	...	2	2	...	7	7	6·2	5·4	6·2
2	...	...	2	2	...	10	9	8·8	7·7	8·8
2	...	...	2	1	1	6	6	5·3	4·0	5·0
2	...	...	2	1	1	10	10	8·8	6·7	8·3
...	2	...	2	1	1	7	4	6·2	4·7	4·6
...	2	...	2	...	2	8	5	5·4	4·6	4·8
...	...	2	3	...	1	6	2	1·7	4·4	2·2
...	...	2	2	...	2	9	1	2·5	5·1	2·5
...	...	2	1	...	3	6	1	2·7	2·5	1·2
...	...	2	1	...	3	11	3	3·1	4·5	2·2
...	...	2	1	1	2	6	...	1·7	3·0	1·5
...	...	2	1	1	2	7	4	2·0	3·6	1·8
1	1	...	3	1	...	6	6	4·7	5·0	4·9
1	1	...	3	1	...	7	6	5·5	5·7	5·7
1	1	...	3	1	...	8	6	6·2	6·6	6·6
1	1	...	3	1	...	9	7	7·0	7·5	7·4
1	1	...	3	1	...	11	10	8·6	9·1	9·2

TABLE 20—*continued.*

Eye-colours of the						Total children.	Number of the light eye-coloured children.			
Parents.			Grandparents.				Observed.	Children.		
Light.	Hazel.	Dark.	Light.	Hazel.	Dark.			I.	II.	III.
1	1	...	3	...	1	9	6	7·0	6·6	6·9
1	1	...	3	...	1	11	7	8·6	8·0	8·5
1	1	...	2	2	...	7	6	5·5	5·4	4·4
1	1	...	2	2	...	9	9	7·0	6·9	5·7
1	1	...	2	2	...	11	1	8·6	8·5	6·9
1	1	...	2	...	2	6	6	4·7	3·4	4·1
1	1	...	2	...	2	6	4	4·7	3·4	4·1
1	1	...	2	...	2	8	5	6·2	4·6	5·5
1	1	...	2	...	2	9	7	7·0	5·1	6·2
1	1	...	2	1	1	6	6	4·7	4·0	4·4
1	1	...	2	1	1	10	9	7·8	6·7	7·4
1	1	...	1	3	...	9	4	7·0	5·5	6·8
1	1	...	1	1	2	8	5	6·2	4·1	5·3
1	...	1	4	...	...	7	3	4·1	6·2	4·8
1	...	1	3	...	1	6	4	3·5	4·4	3·7
1	...	1	3	...	1	7	3	4·1	5·1	4·3
1	...	1	3	...	1	8	6	4·6	5·8	4·9
1	...	1	3	...	1	8	5	4·6	5·8	4·9
1	...	1	3	...	1	8	4	4·6	5·8	4·9
1	...	1	3	...	1	9	6	5·2	6·6	5·5
1	...	1	3	...	1	9	5	5·2	6·6	5·5
1	...	1	2	...	2	6	5	3·5	3·4	3·2
1	...	1	2	...	2	6	3	3·5	3·4	3·2
1	...	1	2	...	2	8	4	4·6	4·6	4·2
1	...	1	2	...	2	10	2	5·8	5·7	5·3
1	...	1	2	...	2	14	9	8·1	8·0	7·4
1	...	1	2	1	1	7	5	4·1	4·7	4·1
1	...	1	1	2	1	7	3	4·1	4·3	3·9
1	...	1	1	1	2	7	4	4·1	3·6	3·5
1	...	1	1	...	3	8	4	4·6	3·3	3·6
1	...	1	1	...	3	8	3	4·6	3·3	3·6
1	...	1	...	1	3	6	3	3·5	2·1	2·6
...	1	1	2	...	2	6	3	4·8	3·4	2·6
...	1	1	2	1	1	9	4	7·0	6·0	4·4
...	1	1	1	...	3	13	8	10·1	5·3	4·7
...	1	1	...	4	...	7	2	5·5	4·6	3·4

TABLE 21.

ERROR IN CALCULATIONS.

Numbers of Errors of various Amounts in the 3 Calculations, Table 20, of the Number of Light Eye-coloured Children in the 78 Families.

Data employed referring to	Amount of Errors. 0·0 to 0·5.	0·6 to 1·1	1·2 to 1·7	1·8 to 2·3	2·4 and above.	Total Cases.
I. The 2 parents only............	19	30	18	5	6	78
II. The 4 grandparents only.....	16	28	10	10	14	78
III. The 2 parents and 4 grandparents........................	41	17	8	4	8	78

TABLE 22.

INHERITANCE OF THE ARTISTIC FACULTY.

Parents.	Children. Observed. Number of Fraternities.	Observed. Total children.	Observed. Of whom are artistic.	Per cents. Observed. art.	Per cents. Observed. not art.	Per cents. Calculated. art.	Per cents. Calculated. not art.
Both artistic............	30	148	95	64	36	60	40
One artistic; one not..	101	520	201	39	61	39	61
Neither artistic.........	150	839	173	21	79	17	83
Totals................	281	1507	469	100	100	100	100

The "parents" and the "children" in this Table usually rank respectively as Grandparents and Parents in the R.F.F. pedigrees.

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APPENDIX.

A.

The following memoirs by the author, bearing on Heredity, have been variously utilised in this volume:

Experiments in Pangenesis. *Proc. Royal Soc.*, No. 127, 1871, p. 393.
Blood Relationship. *Proc. Royal Soc.*, No. 136, 1872, p. 394.
A Theory of Heredity. *Journ. Anthropol. Inst.*, 1875, p. 329.
Statistics by Intercomparison. *Phil. Mag.*, Jan. 1875.
*On the Probability of the Extinction of Families. *Journ. Anthropol. Inst.*, 1875.
Typical laws of Heredity. *Journ. Royal Inst.*, Feb. 1877.
*Geometric Mean in Vital and Social Statistics. *Proc. Royal Soc.*, No. 198, 1879. See subsequent memoir by Dr. Macalister.
Address to Anthrop. Section British Association at Aberdeen. *Journ. Brit. Assoc.*, 1885.
Regression towards Mediocrity in Hereditary Stature. *Journ. Anthropol. Inst.*, 1885.
Presidential Addresses to Anthropol. Inst., 1885, 6 and 7.
Family Likeness in Stature. *Proc. Royal Soc.*, No. 242, 1886.
Family Likeness in Eye-colour. *Proc. Royal Soc.*, No. 245, 1886.
Good and Bad Temper in English Families. *Fortnightly Review*, July, 1887.
Pedigree Moth Breeding. *Trans. Entomolog. Soc.*, 1887. See also subsequent memoir by Mr. Merrifield, and another read by him, Dec. 1887.

Those marked with an asterisk (*) are reprinted with slight revision in the Appendix. es F, D, & E.

Ig. Appendices.

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WORKS BY THE AUTHOR.

(Published by Messrs. Macmillan & Co.)

Hereditary Genius. 1869. [Out of print.]
English Men of Science. 1874.
Inquiries into Human Faculty. 1883.
Record of Family Faculties.[1] 1884. 2*s.* 6*d.*
Life History Album [2] (edited by F. Galton). 1884. 3*s.* 6*d.* and 4*s.* 6*d.*

[1] The Record of Family Faculties consists of Tabular Forms and Directions for entering Data, with an Explanatory Preface. It is a large thin quarto book of seventy pages, bound in limp cloth. The first part of it contains a preface, with explanation of the object of the work and of the way in which it is to be used. The rest consists of blank forms, with printed questions and blank spaces to be filled with writing. The Record is designed to facilitate the orderly collection of such data as are important to a family from an hereditary point of view. It allots equal space to every direct ancestor in the nearer degrees, and is supposed to be filled up in most cases by a parent, say the father of a growing family. If he takes pains to make inquiries of elderly relatives and friends, and to seek in registers, he will be able to ascertain most of the required particulars concerning not only his own parents, but also concerning his four grandparents; and he can ascertain like particulars concerning those of his wife. Therefore his children will be provided with a large store of information about their two parents, four grandparents, and eight great-grandparents, which form the whole of their fourteen nearest ancestors. A separate schedule is allotted to each of them. Space is afterwards provided for the more important data concerning many at least, of the brothers and sisters of each direct ancestor. The schedules are followed by Summary Tables, in which the distribution of any characteristic throughout the family at large may be compendiously exhibited.

[2] The Life History Album was prepared by a Sub-Committee of the Collective Investigation Committee of the British Medical Association. It is designed to serve as a continuous register of the principal biological facts in the life of its owner. The book begins with a few pages of explanatory remarks, followed by tables and charts. The first table is to contain a brief medical history of each member of the near ancestry of the owner. This is followed by printed forms on which the main facts of the owner's growth and development from birth onwards may be registered, and by charts on which measurements may be laid down at appropriate intervals and compared with the curves of normal growth. Most of the required data are such as any intelligent person is capable of recording; those that refer to illnesses should be brief and technical, and ought to be filled up by the medical attendant. Explanations are given of the most convenient tests of muscular force, of keenness of eyesight and hearing, and of the colour sense. The 4*s.* 6*d.* edition contains a card of variously coloured wools to test the sense of colour.

*** These two works pursue similar objects of personal and scientific utility, along different paths. The Album is designed to lay the foundation of a practice of maintaining trustworthy life-histories that shall be of medical service in after-life to the person who keeps them. The Record shows how the life histories of members of the same family may be collated and used to forecast the development in mind and body of the younger generation of that family. Both works are intended to promote the registration of a large amount of information that has hitherto been allowed to run to waste in oblivion, instead of accumulating and forming stores of recorded experience for future personal use, and from which future inquirers into heredity may hope to draw copious supplies.

B.

PROBLEMS BY J. D. HAMILTON DICKSON, FELLOW AND TUTOR OF ST. PETER'S COLLEGE, CAMBRIDGE.

(*Reprinted from Proc. Royal Soc., No.* 242, 1886, *p.* 63.)

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,

$$\frac{y^2}{(1\cdot22)^2}+\frac{(3x-2y)^2}{9(1\cdot50)^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

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,

$$\frac{y}{(1\cdot22)^2}+\frac{(3x-2y)^2}{9(1\cdot50)^2}=\text{C, a constant} \quad . \quad . \quad . \quad (2)$$

2. Tangents to these ellipses parallel to the axis of y are found,

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by differentiating (2) and putting the coefficient of dy equal to zero, to meet the ellipses on the line,

$$\left.\begin{aligned} &\frac{y}{(1\cdot22)^2} - 2\frac{3x-2y}{9(1\cdot50)^2} = 0, \\ \text{that is} \quad &\frac{y}{x} = \frac{\frac{6}{9(1\cdot50)^2}}{\frac{1}{(1\cdot22)^2} + \frac{4}{9(1\cdot50)^2}} = \frac{6}{17\cdot6} \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (3)$$

or, approximately, on the line $y = \frac{1}{3}x$. Let this be the line OM. (See Fig. 11, p. 101.)

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 z OP.

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)

$$\frac{y_1}{(1\cdot22)^2} = 2\frac{3x_1 - 2y_1}{9(1\cdot50)^2} = 0$$

the exponential takes the form

$$\left\{\frac{1}{(1\cdot22)^2} + \frac{4}{9(1\cdot50)^2}\right\}y'^2 + \left\{\frac{y_1^2}{(1\cdot22)^2} + \frac{(3x_1-2y_1)^2}{9(1\cdot50)^2}\right\} \quad . \quad . \quad (4)$$

whence, if e be the probable error of this section,

$$\left.\begin{aligned} &\frac{1}{e^2} = \frac{1}{(1\cdot22)^2} + \frac{4}{9(1\cdot50)^2} \\ \text{or [on referring to (3)]} \quad &e = 1\cdot50\sqrt{\frac{9}{17\cdot6}} \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (5)$$

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 co-ordinate plane have the same probable error.

4. The ellipses (2) when referred to their principal axes become, after some arithmetical simplification,

$$\frac{x'^2}{20\cdot 68}+\frac{y'^2}{5\cdot 92}=\text{constant}, \quad . \quad . \quad . \quad . \quad (6)$$

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,

$$\frac{y^2}{a^2}+\frac{(x-y\tan\theta)^2}{b^2}=\text{C.} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

equation (3) becomes

$$\left.\begin{aligned}\frac{y^2}{a^2}+\tan\theta\frac{x-y\tan\theta}{b^2}&=0\\ \text{or}\qquad \frac{y}{x}=\tan\phi&=\frac{a^2\tan\theta}{b^2+a^2\tan^2\theta}\end{aligned}\right\} \quad . \quad . \quad . \quad (8)$$

and (5) becomes

$$\frac{1}{e^2}=\frac{1}{a^2}+\frac{\tan^2\theta}{b^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

whence

$$\frac{\tan\phi}{\tan\theta}=\frac{e^2}{b^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

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

$$\frac{\tan\phi}{\tan\theta}=\frac{a^2}{b^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

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

$$\frac{x^2}{c^2} + \frac{y^2}{b^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

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',

$$\frac{x'}{c^2} = \frac{y'}{b^2} = \frac{a}{b^2 + c^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

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

$$\frac{1}{v^2} = \frac{1}{c^2} + \frac{1}{b^2}, \text{ or } v = \frac{bc}{\sqrt{b^2 + c^2}} \quad . \quad . \quad . \quad . \quad (14)$$

which it is important to notice, is the same for all values of a.

C.

EXPERIMENTS ON SWEET PEAS BEARING ON THE LAW OF REGRESSION.

The reason why Sweet Peas were chosen, and the methods of selecting and planting them are described in Chapter VI., p.79. The following Table justifies their selection by the convenient and accurate method of weighing, as equivalent to that of measuring them. It will be seen that within the limits of observed variation a difference of 0·172 grain in weight corresponds closely to an average difference of 0·01 inch in diameter.

TABLE 1.

COMPARISON OF WEIGHTS OF SWEET PEAS WITH THEIR DIAMETERS.

Distinguishing letter of seed.	Weight of one seed in grains. Common difference = 0·172 grain.	Length of row of 100 seeds in inches.	Diameter of one seed in hundredths of inch. Common difference = 0·01 inch.
K	1·750	21·0	21
L	1·578	20·2	20
M	1·406	19·2	19
N	1·234	17·9	18
O	1·062	17·0	17
P	·890	16·1	16
Q	·718	15·2	15

The results of the experiment are given in Table 2; its first and last columns are those that especially interest us; the remaining columns showing how these two were obtained.

It will be seen that for each increase of one unit on the part of the parent seed, there is a mean increase of only one-third of a unit in the filial seed; and again that the mean filial seed resembles the parental when the latter is about 15·5 hundredths of an inch in diameter. Taking 15·5 as the point towards which Filial Regression points, whatever may be the parental deviation from that point, the mean Filial Deviation will be in the same direction, but only one-third as much.

TABLE 2.

PARENT SEEDS AND THEIR PRODUCE.

The proportionate number of sweet peas of different sizes, produced by parent seeds also of different sizes, are given below. The measurements are those of their mean diameters, in hundredths of an inch.

Diameter of Parent Seed.	Diameters of Filial Seeds.								Total.	Mean Diameter of Filial Seeds.	
	Under 15.	15–	16–	17–	18–	19–	20–	Above 21–		Observed	Smoothed
21	22	8	10	18	21	13	6	2	100	17·5	17·3
20	23	10	12	17	20	13	3	2	100	17·3	17·0
19	35	16	12	13	11	10	2	1	100	16·0	16·6
18	34	12	13	17	16	6	2	0	100	16·3	16·3
17	37	16	13	16	13	4	1	0	100	15·6	16·0
16	34	15	18	16	13	3	1	0	100	16·0	15·7
15	46	14	9	11	14	4	2	0	100	15·3	15·4

This point is so low in the scale, that I possess less evidence than I desired to prove the bettering of the produce of very small seeds. The seeds smaller than Q were such a miserable set that I could hardly deal with them. Moreover, they were very infertile. It did, however, happen that in a few of the sets some of the Q seeds turned out very well.

If I desired to lay much stress on these experiments, I could make my case considerably stronger by going minutely into other details, including confirmatory measurements of the foliage and length of pod, but I do not care to do so.

D.

GOOD AND BAD TEMPER IN ENGLISH FAMILIES.[1]

[1] Reprinted after slight revision from *Fortnightly Review*, July, 1887.

ONE of the questions put to the compilers of the Family Records spoken of in page 72, referred to the "Character and Temperament" of the persons described. These were distributed through three and sometimes four generations, and consisted of those who lay in the main line of descent, together with their brothers and sisters.

Among the replies, I find that much information has been incidentally included concerning what is familiarly called the "temper" of no less than 1,981 persons. As this is an adequate number to allow for many inductions, and as temper is a strongly marked characteristic in all animals; and again, as it is of social interest from the large part it plays in influencing domestic happiness for good or ill, it seemed a proper subject for investigation.

The best explanation of what I myself mean by the word "temper" will be inferred from a list of the various epithets used by the compilers of the Records, which I have interpreted as expressing one or other of its qualities or degrees. The epithets are as follows, arranged alphabetically in the two main divisions of good and bad temper:—

Good temper.—Amiable, buoyant, calm, cool, equable, forbearing, gentle, good, mild, placid, self-controlled, submissive, sunny, timid, yielding. (15 epithets in all.)

Bad temper.—Acrimonious, aggressive, arbitrary, bickering, capricious, captious, choleric, contentious, crotchety, decisive, despotic, domineering, easily offended, fiery, fits of anger, gloomy, grumpy, harsh, hasty, headstrong, huffy, impatient, imperative, impetuous, insane temper, irritable, morose, nagging, obstinate, odd-tempered, passionate, peevish, peppery, proud, pugnacious, quarrelsome, quick-tempered, scolding, short, sharp, sulky, sullen, surly, uncertain, vicious, vindictive. (46 epithets in all.)

I also grouped the epithets as well as I could, into the following five classes: 1, mild; 2, docile; 3, fretful; 4, violent; 5, masterful.

Though the number of epithets denoting the various kinds of bad temper is three times as large as that used for the good, yet the number of persons described under the one general head is about the same as that described under the other. The first set of data that I tried, gave the proportion of the good to the bad-tempered as 48 to 52; the second set as 47 to 53. There is little difference between the two sexes in the frequency of good and bad temper, but that little is in favour of the women, since about 45 men are recorded as good-tempered for every 55 who are bad, and conversely 55 women as good-tempered for 45 who are bad.

I will not dwell on the immense amount of unhappiness, ranging from family discomfort down to absolute misery, or on the breaches of friendship that must have been occasioned by the cross-grained, sour, and savage dispositions of those who are justly labelled by some of the severer epithets; or on the comfort, peace, and good-will diffused through domestic circles by those who are rightly described by many of the epithets in the first group. We can hardly, too, help speculating uneasily upon the terms that our own relatives would select as most appropriate to our particular selves. But these considerations, interesting as they are in themselves, lie altogether outside the special purpose of this inquiry.

In order to ascertain the facts of which the above statistics are a brief summary, I began by selecting the larger families out of my lists, namely, those that consisted of not less than four brothers or sisters, and by noting the persons they included who were described as good or bad-tempered; also the remainder about whose temper nothing was said either one way or the other, and whom perforce I must call neutral. I am at the same time well aware that, in some few cases a tacit refusal to describe the temper should be interpreted as reticence in respect to what it was thought undesirable even to touch upon.

I found that out of a total of 1,361 children, 321 were described as good-tempered, 705 were not described at all, and 342 were described as bad-tempered. These numbers are nearly in the proportion of 1, 2, and 1, that is to say, the good are equal in number to the bad-tempered, and the neutral are just as numerous as the good and bad-tempered combined.

The equality in the total records of good and bad tempers is an emphatic testimony to the correct judgments of the compilers in the choice of their epithets, for whenever a group has to be divided into three classes, of which the second is called neutral, or medium, or any other equivalent term, its nomenclature demands that it should occupy a strictly middlemost position, an equal number of contrasted cases flanking it on either hand. If more cases were recorded of good temper than of bad, the compilers would have laid down the boundaries of the neutral zone unsymmetrically, too far from the good end of the scale of temper, and too near the bad end. If the number of cases of bad temper exceeded that of the good, the error would have been in the opposite direction. But it appears, on the whole, that the compilers of the records have erred neither to the right hand nor to the left. So far, therefore, their judgments are shown to be correct.

Next as regards the proportion between the number of those who rank as neutrals to that of the good or of the bad. It was recorded as 2 to 1; is that the proper poportion? Whenever the nomenclature is obliged to be somewhat arbitrary, a doubtful term should be interpreted in the sense that may have the widest suitability. Now a large class of cases exist in which the interpretation of the word neutral is fixed. It is that in which the three grades of magnitude are conceived to result from the various possible combinations of two elements, one of which is positive and the other negative, such as good and bad, and which are supposed to occur on each occasion at haphazard, but in the long run with equal frequency. The number of possible combinations of the two elements is only four, and each of these must also in the long run occur with equal frequency. They are: 1, both positive; 2, the first positive, the second negative; 3, the first negative, the second positive; 4, both negative. In the second and third of these combinations the negative counterbalances the positive, and the result is neutral. Therefore the proportions in which the several events of good, neutral, and bad would occur is as 1, 2, and 1. These proportions further commend themselves on the ground that the whole body of cases is thereby divided into two main groups, equal in number, one of which includes all neutral or medium cases, and the other all that are exceptional. Probably it was this latter view that was taken, it may be half unconsciously, by the compilers of the Records. Anyhow, their entries conform excellently to the proportions specified, and I give them credit for their practical appreciation of what seems theoretically to be the fittest standard. I speak, of course, of the Records taken as a whole; in small groups of cases the proportion of the neutral to the rest is not so regular.

The results shown in Table I. are obtained from all my returns. It is instructive in many ways, and not least in showing to a statistical eye how much and how little value may reasonably be

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attached to my materials. It was primarily intended to discover whether any strong bias existed among the compilers to spare the characters of their nearest relatives. In not a few cases they have written to me, saying that their records had been drawn up with perfect frankness, and earnestly reminding me of the importance of not allowing their remarks to come to the knowledge of the persons described. It is almost needless to repeat what I have published more than once already, that I treat the Records quite confidentially. I have left written instructions that in case of my death they should all be destroyed unread, except where I have left a note to say that the compiler wished them returned. In some instances I know that the Records were compiled by a sort of family council, one of its members acting as secretary; but I doubt much whether it often happened that the Records were known to many of the members of the family in their complete form. Bearing these and other considerations in mind, I thought the best test for bias would be to divide the entries into two contrasted groups, one including those who figured in the pedigrees as either father, mother, son, or daughter—that is to say, the compiler and those who were very nearly related to him—and the other including the uncles and aunts on both sides.

TABLE 1.

DISTRIBUTION OF TEMPER IN FAMILIES (per cents.)

Relationships.	1. Mild.	2. Docile.	3. Fretful.	4. Violent.	5. Masterful.	Total.	No. of cases observed.
a. Fathers and Sons	35	12	32	12	9	100	188
b. Mothers and Daughters	39	18	31	8	4	100	179
c. Uncles....................	32	13	25	18	12	100	272
d. Aunts	39	14	29	9	9	100	238
a + b. Direct line..........	74	30	63	20	13	200	367
c + d. Collaterals..........	71	27	54	27	21	200	510
	Good.		Bad Temper.				
a + b. Direct line..........	104		96			200	367
c + d. Collaterals..........	98		102			200	510

On comparing the entries, especially the summaries in the lower lines of the Table, it does not seem that the characters of near relatives are treated much more tenderly than those of the more remote. There is little indication of the compilers having been biassed by affection, respect, or fear. More cases of a record being left blank when a bad temper ought to have been recorded, would probably occur in the direct line, but I do not see how this could be tested. An omission may be due to pure ignorance; indeed I find it not uncommon for compilers to know very little of some of their uncles or aunts. The Records seem to be serious and careful compositions, hardly ever used as vehicles for personal animosity, but written in much the same fair frame of mind that most people force themselves into when they write their wills.

TABLE 2.

COMBINATIONS OF TEMPER IN MARRIAGE (per cents.).

Tempers of Husbands.	A.—Observed Pairs.					B.—Haphazard Pairs.				
	Tempers of Wives.					Tempers of Wives.				
	Good.		Bad Tempers.			Good.		Bad Tempers.		
	1	2	3	4	5	1	2	3	4	5
Good 1	6	10	9	6	2	13	5	10	3	2
,, ... 2	4	2	5	2	—	5	2	4	1	1
Bad 3	14	4	9	3	2	11	5	8	2	2
,, ... 4	7	3	3	2	1	6	2	5	1	1
,, ... 5	3	—	2	—	1	4	2	3	1	1
Good..........	22		24			25		21		
Bad..........	31		23			30		24		

The sexes are separated in the Table, to show the distribution of the five classes of temper among them severally. There is a large proportion of the violent and masterful among the men, of the fretful, the mild, and the docile among the women. On adding the entries it will be found that the proportion of those who fall within the several classes are 36 per cent. of mild-tempered, 15 per cent. of docile, 29 per cent. of fretful, 12 per cent of violent, 8 per cent. of masterful.

The importance assigned in marriage-selection to good and bad temper is an interesting question, not only from its bearing on domestic happiness, but also from the influence it may have in promoting or retarding the natural good temper of our race, assuming, as we may do for the moment, that temper is hereditary. I cannot deal with the question directly, but will give some curious facts in Table II. that throw indirect light upon it. There a comparison is made of (A) the actual frequency of marriage between persons each of the various classes of temper, with (B) the calculated frequency according to the laws of chance, on the supposition that there had been no marriage-selection at all, but that the pairings, so far as temper is concerned, had been purely at haphazard. There are only 111 marriages in my lists in which the tempers of both parents are recorded. On the other hand, the number of possible combinations in couples of persons who belong to the five classes of temper is very large, so I make the two groups comparable by reducing both to percentages.

It will be seen that with two apparent exceptions in the upper left-hand corners of either Table (of 6 against 13, and of 10 against 5), there are no indications of predilection for, or avoidance of marriage between persons of any of the five classes, but that the figures taken from observation run as closely with those derived through calculation, as could be expected from the small number of observations. The apparent exceptions are that the percentage of mild-tempered men who marry mild-tempered women is only 6, as against 13 calculated by the laws of chance, and that those who marry docile wives are 10, as against a calculated 5. There is little difference between mildness and docility, so we may throw these entries together without much error, and then we have 6 and 10, or 16, as against 13 and 5, or 18, which is a close approximation. We may compare the frequency of marriages between persons of like temper in each of the five classes by reading the Table diagonally. They are as (6), 2, 9, 2, 1, in the observed cases, against (13), 2, 8, 1, 1, in the calculated ones; here the irregularity of the 6 and 13, which are put in brackets for distinction sake, is conspicuous. Elsewhere there is not the slightest indication of a dislike in persons of similar tempers, whether mild, docile, fretful, violent, or masterful, to marry one another. The large initial figures 6 and 13 catch the eye, and at a first glance impress themselves unduly on the imagination, and might lead to erroneous speculations about mild-tempered persons, perhaps that they find one another rather insipid; but the reasons I have given, show conclusively that the recorded rarity of the marriages between mild-tempered persons is only apparent. Lastly, if we disregard the five smaller classes and attend only to the main divisions of good and bad temper, there does not appear to be much bias for, or against, the marriage of good or bad-tempered persons in their own or into the opposite division.

The admixture of different tempers among the brothers and sisters of the same family is a notable fact, due to various causes which act in different directions. It is best to consider them before we proceed to collect evidence and attempt its interpretation. It becomes clear enough, and may be now taken for granted, that the tempers of progenitors do not readily blend in the offspring, but that some of the children take mainly after one of them, some after another, but with a few threads, as it were, of various ancestral tempers woven in, which occasionally manifest themselves. If no other influences intervened, the tempers of the children in the same family would on this account be almost as varied as those of their ancestors; and these, as we have just seen, married at haphazard, so far as their tempers were concerned; therefore the numbers of good and bad children in families would be regulated by the same laws of chance that apply to a gambling-table. But there are other influences to be considered. There is a well-known tendency to family likeness among brothers and sisters, which is due, not to the blending of ancestral peculiarities, but to the prepotence of one of the progenitors, who stamped more than his or her fair share of qualities upon the descendants. It may be due also to a familiar occurrence that deserves but has not yet received a distinctive name, namely, where all the children are alike and yet their common likeness cannot be traced to their progenitors. A new variety has come into existence through a process that affects the whole Frater-

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nity and may result in an unusually stable variety (see Chapter III.). The most strongly marked family type that I have personally met with, first arose simultaneously in the three brothers of a family who transmitted their peculiarities with unusual tenacity to numerous descendants through at least four generations. Other influences act in antagonism to the foregoing; they are the events of domestic life, which instead of assimilating tempers tend to accentuate slight differences in them. Thus if some members of a family are a little submissive by nature, others who are naturally domineering are tempted to become more so. Then the acquired habit of dictation in these reacts upon the others and makes them still more submissive. In the collection I made of the histories of twins who were closely alike, it was most commonly said that one of the twins was guided by the other. I suppose that after their many childish struggles for supremacy, each finally discovered his own relative strength of character, and thenceforth the stronger developed into the leader, while the weaker contentedly subsided into the position of being led. Again, it is sometimes observed that one member of an otherwise easygoing family, discovers that he or she may exercise considerable power by adopting the habit of being persistently disagreeable whenever he or she does not get the first and best of everything. Some wives contrive to tyrannise over husbands who are mild and sensitive, who hate family scenes and dread the disgrace attending them, by holding themselves in readiness to fly into a passion whenever their wishes are withstood. They thus acquire a habit of "breaking out," to use a term familiar to the warders of female prisons and lunatic asylums; and though their relatives and connections would describe their tempers by severe epithets, yet if they had married masterful husbands their characters might have developed more favourably.

To recapitulate briefly, one set of influences tends to mix good and bad tempers in a family at haphazard; another set tends to assimilate them, so that they shall all be good or all be bad; a third set tends to divide each family into contrasted portions. We have now to ascertain the facts and learn the results of these opposing influences.

In dealing with the distribution of temper in Fraternities,[1] we

[1] A Fraternity consists of the brothers of a family, and of the sisters after the

can only make use of those in which at least two cases of temper are recorded; they are 146 in number. I have removed all the cases of neutral temper, treating them as if they were non-existent, and dealing only with the remainder that are good or bad. We have next to eliminate the haphazard element. Beginning with Fraternities of two persons only, either of whom is just as likely to be good as bad tempered, there are, as we have already seen, four possible combinations, resulting in the proportions of 1 case of both good, 2 cases one good and one bad, and one case of both bad. I have 42 such Fraternities, and the observed facts are that in 10 of them both are good tempered, in 20 one is good and one bad, and in 12 both are bad tempered. Here only a trifling and untrustworthy difference is found between the observed and the haphazard distribution, the other conditions having neutralised each other. But when we proceed to larger Fraternities the test becomes shrewder, and the trifling difference already observed becomes more marked, and is at length unmistakable. Thus the successive lines of Table III. show a continually increasing diverg-

TABLE 3.

DISTRIBUTION OF TEMPER IN FRATERNITIES.

		A.—Observed.			B.—Haphazard.		
Number in each Fraternity.	Number of Fraternities.	All good-tempers.	Intermediate cases.	All bad-tempers.	All good-tempers.	Intermediate cases.	All bad-tempers.
2	42	10	20	12	10	21	11
3	55	11	15 21	8	7	20 21	7
4	29	5	6 9 8	1	2	8 12 8	2
5	6	1	0 2 1 0	2	0	1 2 2 1	0
6	14	1	0 1 3 3 2	4	0	2 4 5 4 2	0
4 to 6	49	7		7	2		2

qualities of the latter have been transmuted to their Male Equivalents; but as no change in the Female values seems really needed, so none has been made in respect to Temper.

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ence between the observed and the haphazard distribution of temper, as the Fraternities increase in size. A compendious comparison is made in the bottom line of the Table by adding together the instances in which the Fraternities are from 4 to 6 in number, and in taking only those in which all the members of the Fraternity were alike in temper, whether good or bad. There are 7 + 7, or 14, observed cases of this against 2 + 2, or 4, haphazard cases, found in a total of 49 Fraternities. Hence it follows that the domestic influences that tend to differentiate temper wholly fail to overcome the influences, hereditary and other, that tend to make it uniform in the same Fraternity.

As regards direct evidences of heredity of temper, we must frame our inquiries under a just sense of the sort of materials we have to depend upon. They are but coarse portraits scored with white or black, and sorted into two heaps, irrespective of the gradations of tint in the originals. The processes I have used in discussing the heredity of stature, eye-colour, and artistic faculty, cannot be employed in dealing with the heredity of temper. I must now renounce those refined operations and set to work with ruder tools on my rough material.

The first inquiry will be, Do good-tempered parents have, on the whole, good-tempered children, and do bad-tempered parents have bad-tempered ones? I have 43 cases where both parents are recorded as good-tempered, and 25 where they were both bad-tempered. Out of the children of the former, 30 per cent. were good-tempered and 10 per cent. bad; out of the latter, 4 per cent. were good and 52 per cent. bad-tempered. This is emphatic testimony to the heredity of temper. I have worked out the other less contrasted combinations of parental temper, but the results are hardly worth giving. There is also much variability in the proportions of the neutral cases.

I then attempted, with still more success, to answer the converse question, Do good-tempered Fraternities have, on the whole, good-tempered ancestors, and bad-tempered Fraternities bad-tempered ones? After some consideration of the materials, I defined—rightly or wrongly—a good-tempered Fraternity as one in which at least two members were good-tempered and none were bad, and a bad-tempered Fraternity as one in which at least two members were bad-tempered, whether or no any cases of good temper were said to be associated with them. Then, as regards the ancestors, I thought by far the most trustworthy group was that which consisted of the two parents and of the uncles and aunts on both sides. I have thus 46 good-tempered Fraternities with an aggregate of 333 parents, uncles, and aunts; and 71 bad-tempered, with 633 parents, uncles, and aunts. In the former group, 26 per cent. were good tempered and 18 bad; in the latter group, 18 were good-tempered and 29 were bad, the remainder being neutral. These results are almost the exact counterparts of one another, so I seem to have made good hits in framing the definitions. More briefly, we may say that when the Fraternity is good-tempered as above defined, the number of good-tempered parents, uncles, and aunts, exceeds that of the bad-tempered in the proportion of 3 to 2; and that when the Fraternity is bad-tempered, the proportions are exactly reversed.

I have attempted in other ways to work out the statistics of hereditary tempers, but none proved to be of sufficient value for publication. I can trace no prepotency of one sex over the other in transmitting their tempers to their children. I find clear indications of strains of bad temper clinging to families for three generations, but I cannot succeed in putting them into a numerical form.

It must not be thought that I have wished to deal with temper as if it were an unchangeable characteristic, or to assign more trustworthiness to my material than it deserves. Both these views have been discussed; they are again alluded to to show that they are not dismissed from my mind, and partly to give the opportunity of adding a very few further remarks.

Persons highly respected for social and public qualities may be well-known to their relatives as having sharp tempers under strong but insecure control, so that they "flare up" now and then. I have heard the remark that those who are over-suave in ordinary demeanour have often vile tempers. If this be the case—and I have some evidence of its truth—I suppose they are painfully conscious of their infirmity, and through habitual endeavours to subdue it, have insensibly acquired an exaggerated suavity at the times when their temper is unprovoked. Illness, too, has much influence in affecting the temper. Thus I sometimes come across entries to the effect of, "not naturally ill-tempered, but peevish through illness. Overwork and worry will make even mild-tempered men exceedingly touchy and cross.

The accurate discernment and designation of character is almost beyond the reach of any one, but, on the other hand, a rough knowledge and description of its prominent features is easily obtainable; and it seems to me that the testimony of a member of a family who has seen and observed a person in his unguarded moments and under very varied circumstances for many years, is a verdict deserving of much confidence. I shall have fulfilled my object in writing this paper if it leaves a clear impression of the great range and variety of temper among persons of both sexes in the upper and middle classes of English society; of its disregard in Marriage Selection; of the great admixture of its good and bad varieties in the same family; and of its being, nevertheless, as hereditary as any other quality. Also, that although it exerts an immense influence for good or ill on domestic happiness, it seems that good temper has not been especially looked for, nor ill temper especially shunned, as it ought to be in marriage-selection.

E.

[1] THE GEOMETRIC MEAN, IN VITAL AND SOCIAL STATISTICS.

My purpose is to show that an assumption which lies at the basis of the well-known law of "Frequency of Error" is incorrect when applied to many groups of vital and social phenomena, although that law has been applied to them by statisticians with partial success. Next, I will point out the correct hypothesis upon which a Law of Error suitable to these cases ought to be calculated; and subsequently I will communicate a memoir by Mr. (now Dr.) Donald McAlister, who, at my suggestion, has mathematically investigated the subject.

The assumption to which I refer is, that errors in excess or in

[1] Reprinted, with slight revision, from the *Proceedings of the Royal Society*, No. 198, 1879.

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deficiency of the truth are equally probable ; or conversely, that if two fallible measurements have been made of the same object, their arithmetical mean is more likely to be the true measurement than any other quantity that can be named.

This assumption cannot be justified in vital phenomena. For example, suppose we endeavour to match a tint; Weber's law, in its approximative and simplest form of Sensation varying as the logarithym of the Stimulus, tells us that a series of tints, in which the quantities of white scattered on a black ground are as 1, 2, 4, 8, 16, 32, &c., will appear to the eye to be separated by equal intervals of tint. Therefore, in matching a grey that contains 8 portions of white, we are just as likely to err by selecting one that has 16 portions as one that has 4 portions. In the first case there would be an error in excess, of 8 units of absolute tint; in the second there would be an error in deficiency, of 4. Therefore, an error of the same magnitude in excess or in deficiency is not equally probable in the judgment of tints by the eye. Conversely, if two persons, who are equally good judges, describe their impressions of a certain tint, and one says that it contains 4 portions of white and the other that it contains 16 portions, the most reasonable conclusion is that it really contains 8 portions. The arithmetic mean of the two estimates is 10, which is *not* the most probable value; it is the geometric mean 8 (4 : 8 : : 8 : 16) which is the most probable.

Precisely the same condition characterises every determination by each of the senses; for example, in judging of the weight of bodies or of their temperatures, of the loudness and of the pitches of tones, and of estimates of lengths and distances *as wholes*. Thus, three rods of the lengths a, b, c, when taken successively in the hand, appear to differ by equal intervals when $a : b :: b : c$, and not when $a - b = b - c$. In all physiological phenomena, where there is on the one hand a stimulus and on the other a response to that stimulus, Weber's or some other geometric law may be assumed to prevail; in other words, the true mean is geometric rather than arithmetic.

The geometric mean appears to be equally applicable to the majority of the influences, which, combined with those of purely vital phenomena, give rise to the events with which sociology deals. It is difficult to find terms sufficiently general to apply to the varied topics of sociology, but there are two categories which are of common oc-

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currence in which the geometric mean is certainly appropriate. The one is increase, as exemplified by the growth of population, where an already large nation tends to receive larger accessions than a small one under similar circumstances, or when a capital employed in a business increases in proportion to its size. The other category is the influences of circumstances or of "milieux" as they are often called, such as a period of plenty in which a larger field or a larger business yields a greater excess over its mean yield than a smaller one. Most of the causes of those differences with which sociology are concerned, and which are not purely vital phenomena, such as those previously discussed, may be classified under one or other of these two categories, or under such as are in principle almost the same. In short, sociological phenomena, like vital phenomena are, as a general rule, subject to the condition of the geometric mean.

The ordinary law of Frequency of Error, based on the arithmetic mean, corresponds, no doubt, sufficiently well with the observed facts of vital and social phenomena, to be very serviceable to statisticians, but it is far from satisfying their wants, and it may lead to absurdity when applied to wide deviations. It asserts that deviations in excess must be balanced by deviations of equal magnitude in deficiency; therefore, if the former be greater than the mean itself, the latter must be less than zero, that is, must be negative. This is an impossibility in many cases, to which the law is nevertheless applied by statisticians with no small success, so long as they are content to confine its application within a narrow range of deviation. Thus, in respect of Stature, the law is very correct in respect to ordinary measurements, although it asserts that the existence of giants, whose height is more than double the mean height of their race, implies the possibility of the existence of dwarfs, whose stature is less than nothing at all.

It is therefore an object not only of theoretical interest but of practical use, to thoroughly investigate a Law of Error, based on the geometric mean, even though some of the expected results may perhaps be apparent at first sight. With this view I placed the foregoing remarks in Mr. Donald McAlister's hands, who contributed a memoir that will be found in the *Proc. Royal Soc.*, No. 198, 1879, following my own. It should be referred to by the mathematicians who may read this book.

F.

PROBABLE EXTINCTION OF FAMILIES.[1]

THE decay of the families of men who occupied conspicuous positions in past times has been a subject of frequent remark, and has given rise to various conjectures. It is not only the families of men of genius or those of the aristocracy who tend to perish, but it is those of all with whom history deals, in any way, even such men as the burgesses of towns, concerning whom Mr. Doubleday has inquired and written. The instances are very numerous in which surnames that were once common have since become scarce or have wholly disappeared. The tendency is universal, and, in explanation of it, the conclusion has been hastily drawn that a rise in physical comfort and intellectual capacity is necessarily accompanied by diminution in "fertility"—using that phrase in its widest sense and reckoning abstinence from marriage as one cause of sterility. If that conclusion be true, our population is chiefly maintained through the "proletariat," and thus a large element of degradation is inseparably connected with those other elements which tend to ameliorate the race. On the other hand, M. Alphonse de Candolle has directed attention to the fact that, by the ordinary law of chances, a large proportion of families are continually dying out, and it evidently follows that, until we know what that proportion is, we cannot estimate whether any observed diminution of surnames among the families whose history we can trace, is or is not a sign of their diminished "fertility." I give extracts from M. De Candolle's work in a foot-note,[2] and may add that, although I have not hitherto published anything on the matter, I took considerable pains some years ago to obtain numerical results in respect to this

[1] Reprinted, with slight revision, from the *Journ. Anthropol. Inst.* 18 .

[2] "Au milieu des renseignements précis et des opinions très-sensées de MM. Benoiston de Châteauneuf, Galton, et autres statisticiens, je n'ai pas rencontré la réflexion bien importante qu'ils auraient dû faire de l'extinction *inévitable* des noms de famille. Évidemment tous les noms doivent s'éteindre . . . . Un mathématicien pourrait calculer comment la réduction des noms ou titres aurait lieu, d'après la probabilité des naissances toutes féminines ou toutes masculines ou mélangées et la probabilité d'absence de naissances dans un couple quelconque," &c.—ALPHONSE DE CANDOLLE, *Histoire des Sciences et des Savants*, 1873.

very problem. I made certain very simple, ~~but~~ and yet very inaccurate, suppositions, concerning average fertility, and I worked to the nearest integer, starting with 10,000 persons, but the computation became intolerably tedious after a few steps, and I had to abandon it. The Rev. H. W. Watson kindly, at my request, took the problem in hand, and his results form the subject of the following paper. They do not give what can properly be called a general solution, but they do give certain general results. They show (1) how to compute, though with great labour, any special case; (2) a remarkably easy way of computing those special cases in which the law of fertility approximates to a certain specified form; and (3) how all surnames tend to disappear.

The form in which I originally stated the problem is as follows. I purposely limited it in the hope that its solution might be more practicable if unnecessary generalities were excluded :—

A large nation, of whom we will only concern ourselves with the adult males, N in number, and who each bear separate surnames, colonise a district. Their law of population is such that, in each generation, a_0 per cent. of the adult males have no male children who reach adult life; a_1 have one such male child; a_2 have two; and so on up to a_5 who have five. Find (1) what proportion of the surnames will have become extinct after r generations; and (2) how many instances there will be of the same surname being held by m persons.

Discussion of the problem by the Rev. H. W. Watson, D.Sc. F.R.S. *formerly Fellow of Trinity College, Cambridge.*

Suppose that at any instant all the adult males of a large nation have different surnames, it is required to find how many of these surnames will have disappeared in a given number of generations upon any hypothesis, to be determined by statistical investigations, of the law of male population.

Let, therefore, a_0 be the percentage of males in any generation who have no sons reaching adult life, let a_1 be the percentage that have one such son, a_2 the percentage that have two, and so on up to a_q, the percentage that have q such sons, q being so large that it is not worth while to consider the chance of any man having more than q adult sons—our first hypothesis will be that the numbers

a_0, a_1, a_2, etc., remain the same in each succeeding generation. We shall also, in what follows, neglect the overlapping of generations—that is to say, we shall treat the problem as if all the sons born to any man in any generation came into being at one birth, and as if every man's sons were born and died at the same time. Of course it cannot be asserted that these assumptions are correct. Very probably accurate statistics would discover variations in the values of a_0, a_1, etc., as the nation progressed or retrograded; but it is not at all likely that this variation is so rapid as seriously to vitiate any general conclusions arrived at on the assumption of the values remaining the same through many successive generations. It is obvious also that the generations must overlap, and the neglect to take account of this fact is equivalent to saying, that at any given time we leave out of consideration those male descendants, of any original ancestor who are more than a certain average number of generations removed from him, and compensate for this by giving credit for such male descendants, not yet come into being, as are not more than that same average number of generations removed from the original ancestors.

Let then $\frac{a_0}{100}$, $\frac{a_1}{100}$, $\frac{a_2}{100}$, etc., up to $\frac{a_q}{100}$ be denoted by the symbols t_0, t_1, t_2, etc., up to t_q, in other words, let t_0, t_1, etc., be the chances in the first and each succeeding generation of any individual man, in any generation, having no son, one son, two sons, and so on, who reach adult life. Let N be the original number of distinct surnames, and let ${}_rm_s$ be the fraction of N which indicates the number of such surnames with s representatives in the rth generation.

Now, if any surname have p representatives in any generation, it follows from the ordinary theory of chances that the chance of that same surname having s representatives in the next succeeding generation is the coefficient of x^s in the expansion of the multinomial

$$(t_0 + t_1x + t_2x^2 + \text{, etc. } + t_qx^q)^p$$

Let then the expression $t_0 + t_1x + t_2x^2 + \text{ etc. } + t_qx^q$ be represented by the symbol T.

Then since, by the assumption already made, the number of surnames with no representative in the r–1th generation is ${}_{r-1}m_0$ N, the number with one representative ${}_{r-1}m_1.\mathrm{N}$, the number with two ${}_{r-1}m_2.$ N and so on, it follows, from what we last stated, that the number of surnames with s representatives in the rth generation must be the coefficient of x^s in the expression

$$\left\{ {}_{r-1}m_0 + {}_{r-1}m_1\mathrm{T} + {}_{r-1}m_2\mathrm{T}^2 + \text{ etc. } + {}_{r-1}m_{q^{r-1}}\mathrm{T}^{q^{r-1}} \right\} \mathrm{N}$$

If, therefore, the coefficient of N in this expression be denoted by $f_r\,(x)$ it follows that ${}_{r-1}m_1$, ${}_{r-1}m_2$ and so on, are the coefficients of x, x^2 and so on, in the expression $f_{r-1}\,(x)$.

If, therefore, a series of functions be found such that

$$f_1(x) = t_0 + t_1x + \text{ etc. } + t_qx^q \text{ and } f_r\,(x) = f_{r-1}\,(t_0 + t_1x \text{ etc. } + t^qx^q)$$

then the proportional number of groups of surnames with s representatives in the rth generation will be the coefficient of x^s in $f_r\,(x)$ and the actual number of such surnames will be found by multiplying this coefficient by N. The number of surnames unrepresented or become extinct in the rth generation will be found by multiplying the term independent of x in $f_r\,(x)$ by the number N.

The determination, therefore, of the rapidity of extinction of surnames, when the statistical data, t_0, t_1, etc., are given, is reduced to the mechanical, but generally laborious process of successive substitution of $t_0 + t_1x + t_2x^2 +$ etc., for x in successively determined values of $f_r\,(x)$, and no further progress can be made with the problem until these statistical data are fixed; the following illustrations of the application of our formula are, however, not without interest.

(1) The very simplest case by which the formula can be illustrated is when $q = 2$ and t_0, t_1, t_2 are each equal to $\frac{1}{3}$.

Here $f_1\,(x) = \dfrac{1 + x + x^2}{3} f_2(x) = \dfrac{1}{3}\left\{ 1 + \dfrac{1}{3}\,(1 + x + x^2) + \dfrac{1}{9}\,1 + x + x^2)^2 \right\}^2$

and so on.

Making the successive substitutions, we obtain

$$f_2\,(x) = \frac{1}{3}\left\{ \frac{13}{9} + \frac{5x}{9} + \frac{6x^2}{9} + \frac{2x}{9} + \frac{x}{9} \right\}$$

$$f_3(x) = \frac{1249}{2187} + \frac{265x}{2187} + \frac{343x^2}{2187} + \frac{166x^3}{2187} + \frac{109x^4}{2187} + \frac{34x^5}{2187} + \frac{16x^6}{2187} + \frac{4x^7}{2187} + \frac{x^8}{2187}$$

$$\begin{aligned} f_4(x) = {}& \cdot63183 + \cdot08306x + \cdot10635x^2 + \cdot07804x^3 + \cdot06489x^4 + \cdot05443x^5 + \cdot01437x^6 \\ & + \cdot01692x^7 + \cdot01144x^8 + \cdot00367x^9 + \cdot00104x^{10} + \cdot00015x^{11} + \cdot00005x^{12} \\ & + \cdot00001x^{13} + \cdot00000x^{14} + \cdot00000x^{15} + \cdot00000x^{16} \end{aligned}$$

and the constant term in $f_5(x)$ or ${}_5m_0$ is therefore

$$\cdot 63183+\frac{\cdot 08306}{3}+\frac{\cdot 10635}{9}+\frac{\cdot 07804}{27}+\frac{\cdot 06489}{81}+\frac{\cdot 05443}{243}+\frac{\cdot 01437}{729}+\frac{\cdot 01692}{2187}+\frac{\cdot 01144}{6561}$$
$$+\frac{\cdot 00367}{19683}+\frac{\cdot 00104}{59049}+\frac{\cdot 00015}{177147}+$$

The value of which to five places of decimals is ·67528.

The constant terms, therefore, in f_1, f_2 up to f_5 when reduced to decimals, are in this case ·33333, ·48148, ·57110, ·64113, and ·65628 respectively. That is to say, out of a million surnames at starting, there have disappeared in the course of one, two, etc., up to five generations, 333333, 481480, 571100, 641130, and 675280 respectively.

The disappearances are much more rapid in the earlier than in the later generations. Three hundred thousand disappear in the first generation, one hundred and fifty thousand more in the second, and so on, while in passing from the fourth to the fifth, not more than thirty thousand surnames disappear.

All this time the male population remains constant. For it is evident that the male population of any generation is to be found by multiplying that of the preceding generation, by t_1+2t_2, and this quantity is in the present case equal to one.

If axes Ox and Oy be drawn, and equal distances along Ox represent generations from starting, while two distances are marked along every ordinate, the one representing the total male population in any generation, and the other the number of remaining surnames in that generation, of the two curves passing through the extremities of these ordinates, the *population* curve will, in this case, be a straight line parallel to Ox, while the *surname* curve will intersect the population curve on the axis of y, will proceed always convex to the axis of x, and will have the positive part of that axis for an asymptote.

The case just discussed illustrates the use to be made of the general formula, as well as the labour of successive substitutions, when the expressions $f_1(x)$ does not follow some assigned law. The calculation may be infinitely simplified when such a law can be found; especially if that law be the expansion of a binomial, and only the extinctions are required.

For example, suppose that the terms of the expression t_0+t_1x+ etc. $+t_qx_q$ are proportional to the terms of the expanded binomial

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$(a+bx)^q$ *i.e.* suppose that $t_0 = \frac{a^q}{(a+b)^q}$, $t_1 = q\frac{a^{q-1}b}{(a+b)^q}$ and so on.

$$\text{Here } f_1(x) = \frac{(a+bx)^q}{(a+b)^q} \text{ and } {}_1m_0 = \frac{a^q}{(a+b)^q}$$

$$f_2(x) = \frac{1}{(a+b)^q}\left\{ a + b\frac{(a+bx)^q}{(a+b)^q} \right\}^q$$

$${}_2m_0 = \frac{1}{(a+b)^q}\left\{ a + b\,{}_1m_0 \right\}^q$$

$$\text{Generally } {}_rm_0 = \frac{1}{(a+b)^q}\left\{ a + b\,{}_{r-1}m_0 \right\}^q = \frac{b_q}{(a+b)_q}\left\{ \frac{a}{b} + {}_{r-1}m_0 \right\}^q$$

If, therefore, we wish to find the number of extinctions in any generation, we have only to take the number in the preceding generation, add it to the constant fraction $\frac{a}{b}$, raise the sum to the power of q, and multiply by $\frac{b^q}{(a+b)^q}$

With the aid of a table of logarithms, all this may be effected for a great number of generations in a very few minutes. It is by no means unlikely that when the true statistical data t_0, t_1, etc., t_q are ascertained, values of a, b, and q may be found, which shall render the terms of the expansion $(a+bx)^q$ approximately proportionate to the terms of $f_1(x)$. If this can be done, we may *approximate* to the determination of the rapidity of extinction with very great ease, for any number of generations, however great.

For example, it does not seem very unlikely that the value of q might be 5, while $t_0, t_1 \ldots t_q$ might be ·237, ·396, ·264, ·088, ·014, ·001, or nearly, $\frac{1}{4}$, $\frac{1}{3}$, $\frac{7}{24}$, $\frac{1}{23}$, $\frac{1}{138}$, and $\frac{1}{1000}$.

Should that be the case, we have, $f_1(x) = \frac{(3+x)^5}{4^5}$ ${}_1m_0 = \frac{3^5}{4^5}$

and generally ${}_rm_0 = \frac{1}{4^5}\left\{ 3 + {}_{r-1}m_0 \right\}^5$

Thus we easily get for the number of extinctions in the first ten generations respectively.

·237, ·346, ·410, ·450, ·477, ·496, ·510, ·520, ·527, ·533.

We observe the same law noticed above in the case of $\frac{1+x+x^2}{3}$ viz., that while 237 names out of a thousand disappear in the first step, and an additional 109 names in the second step, there are only 27 disappearances in the fifth step, and only six disappearances in the tenth step.

If the curves of surnames and of population were drawn from this case, the former would resemble the corresponding curve in the case last mentioned, while the latter would be a curve whose distance from the axis of x increased indefinitely, inasmuch as the expression

$$t_1+2t_2+3t_3+4t_4+5t_5$$

is greater than one.

Whenever $f_1\ (x)$ can be represented by a binomial, as above suggested, we get the equation

$$_rm_0=\frac{1}{(a+b)^q}\left\{\ a+b_{r\text{-}1}m\ \right\}^q$$

whence it follows that as r increases indefinitely the value of $_rm_0$ approaches indefinitely to the value y where

$$y=\frac{1}{(a+b)}\left\{a+by\right\}$$

that is where $y=1$.

All the surnames, therefore, tend to extinction in an indefinite time, and this result might have been anticipated generally, for a surname once lost can never be recovered, and there is an additional chance of loss in every successive generation. This result must not be confounded with that of the extinction of the male population; for in every binomial case where q is greater than 2, we have t_1+2t_2+ etc. $+qt_q>1$, and, therefore an indefinite increase of male population.

The true interpretation is that each of the quantities, $_rm_1$, $_rm_2$, etc., tends to become zero, as r is indefinitely increased, but that it does not follow that the product of each by the infinitely large number N is also zero.

As, therefore, time proceeds indefinitely, the number of surnames extinguished becomes a number of the *same order of magnitude* as the total number at first starting in N, while the number of surnames represented by one, two, three, etc., representatives is some infinitely smaller but finite number. When the finite numbers are multiplied by the corresponding number of representatives, sometimes infinite in number, and the products added together, the sum will generally exceed the original number N. In point of fact, just as in the cases calculated above to five generations, we had a continual, and indeed

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at first, a rapid extinction of surnames, combined in the one case with a stationary, and in the other case an increasing population, so is it when the number of generations is increased indefinitely. We have a continual extinction of surnames going on, combined with constancy, or increase of population, as the case may be, until at length the number of surnames remaining is absolutely insensible, as compared with the number at starting; but the total number of representatives of those remaining surnames is infinitely greater than the original number.

We are not in a position to assert from *actual calculation* that a corresponding result is true for every form of $f_1(x)$, but the reasonable inference is that such is the case, seeing that it holds whenever $f_1(x)$ may be compared with $\frac{(a+bx)^q}{(a+b)^q}$ whatever a, b, or q may be.

G.

ORDERLY ARRANGEMENT OF HEREDITARY DATA.

THERE are many methods both of drawing pedigrees and of describing kinship, but for my own purposes I still prefer those that I designed myself. The chief requirements that have to be fulfilled are compactness, an orderly and natural arrangement, and clearly intelligible symbols.

Nomenclature.—A symbol ought to be suggestive, consequently the initial letter of a word is commonly used for the purpose. But this practice would lead to singular complications in symbolizing the various ranks of kinship, and it must be applied sparingly. The letter F is equally likely to suggest any one of the three very different words of Father, Female, and Fraternal. The letter M suggests both Mother and Male; S would do equally for Son and for Sister. Whether they are English, French, or German words, much the same complexity prevails. The system employed in *Hereditary Genius* had the merit of brevity, but was apt to cause mistake; it was awkward in manuscript and difficult to the printer, and I have now abandoned it in favour of the method employed in the *Records* *of Family Faculties.* This will now be briefly described again. Each kinsman can be described in two ways, either by letters or by a number. In ordinary cases both the letter and number are intended to be used simultaneously, thus FF.8. means the Father's Father of the person described, though either FF or 8, standing by themselves, would have the same meaning. The double nomenclature has great practical advantages. It is a check against mistake and makes reference and orderly arrangement easy.

As regards the letters, F stands for Father and M for Mother, whenever no letter succeeds them; otherwise they stand for Father's and for Mother's respectively. Thus F is Father; FM is Father's Mother; FMF is Father's Mother's Father.

As regards the principle upon which the numbers are assigned, arithmeticians will understand me when I say that it is in accordance with the binary system of notation, which runs parallel to the binary distribution of the successive ranks of ancestry, as two parents, four grandparents, eight great-grandparents, and so on. The "subject" of the pedigree is of generation O; that of his parents, of generation 1; that of his grandparents, of generation 2, &c. This is clearly shown in the following table :—

Kinship.	Order of Generation.	Numerical Values							
		in Binary Notation.				in Decimal Notation.			
Child...............	0	1				1			
Parents	1	10		11		2		3	
Gr. Par.............	2	100	101	110	111	4	5	6	7
G. Gr. Par.	3	1000	1010	1100	1110	8	10	12	14
		1001	1011	1100	1111	9	11	13	15

All the male ancestry are thus described by even numbers and the female ancestry by odd ones. They run as follows :—

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F, 2.		M, 3.	
FF, 4.	FM, 5.	MF, 6.	MM, 7.
FFF, 8.	FMF, 10.	MFF, 12.	MMF, 14.
FFM, 9.	FMM, 11.	MFM, 13.	MMM, 15.

It will be observed that the double of the number of any ancestor is that of his or her Father; and that the double of the number *plus* 1 is that of his or her Mother; thus FM 5 has for her father FMF 10, and for her mother FMM 11.

When the word Brother or Sister has to be abbreviated it is safer not to be too stingy in assigning letters, but to write *br*, *sr*, and in the plural *brs*, *srs*, and for the long phrase of "brothers and sisters," to write *brss*.

All these symbols are brief enough to save a great deal of space, and they are perfectly explicit. When such a phrase has to be expressed as "the Fraternity of whom FF is one" I write in my own notes simply FF', but there has been no occasion to adopt this symbol in the present book.

I have not satisfied myself as to any system for expressing descendants. Theoretically, the above binary system admits of extension by the use of negative indices, but the practical application of the idea seems cumbrous.

We and the French sadly want a word that the Germans possess to stand for Brothers and Sisters. Fraternity refers properly to the brothers only, but its use has been legitimately extended here to mean the brothers and the sisters, after the qualities of the latter have been reduced to their male equivalents. The Greek word *adelphic* would do for an adjective.

Pedigrees.—The method employed in the *Record of Family Faculties* for entering all the facts concerning each kinsman in a methodical manner was fully described in that book, and could not easily be epitomised here; but a description of the method in which the manuscript extracts from the records have been made for my own use will be of service to others when epitomising their own family characteristics. It will be sufficient to describe the quarto books that contain the medical extracts. Each page is ten and a half inches high and eight and a half wide, and the two opposite pages that are

SCHEDULE.

<table>
<tr><td colspan="4">JAMES LIPHOOK.</td><td colspan="4">JAMES LIPHOOK.</td></tr>
<tr><td colspan="2">Father's Father's Father and his fraternity.</td><td colspan="2">Father's Mother's Father and his fraternity.</td><td colspan="2">Mother's Father's Father and his fraternity.</td><td colspan="2">Mother's Mother's Father and his fraternity.</td></tr>
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<tr><td>Spare space.</td><td colspan="2">Father and his fraternity.</td><td>Spare space.</td><td>Spare space.</td><td colspan="2">Mother and her fraternity.</td><td>Spare space.</td></tr>
<tr><td colspan="4">Spare space.</td><td colspan="4">Children.</td></tr>
</table>

EXAMPLE A.

Father's name . . . . JAMES GLADDING.
Mother's maiden name . . . . MARY CLAREMONT.

Initials.	Kin.	Principal illnesses and cause of death.	Age at death.
J. G.	Father	Bad rheum. fever; agonising headaches; diarrhœa; bronchitis; pleurisy . . *Heart disease*	54
R. G.	bro.	Rheum. gout . . . . *Apoplexy*	56
W. G.	bro.	Good health except gout; paralysis later *Apoplexy*	83
F. L.	sis.	Rheum. fever and rheum. gout . . . *Apoplexy*	73
C. G.	sis.	Delicate . . . . (inoculated) *Small pox*	
M. G.	Mother	Tendency to lung disease; biliousness; frequent heart attacks . *Heart disease and dropsy*	63
A. C.	bro.	Good health . . . . *Accident*	46
W. C.	bro.	Led a wild life . . . . . . *Premature old age*	62
E. C.	bro.	Always delicate . . . . *Consumption*	19
F. R.	sis.	Small-pox three times . . . . *General failure*	85
R. N.	sis.	Bilious; weak health . . . . *Cancer*	50
L. C.	sis.	. . . . *Fever*	21
M. G.	bro.	Inflam. lungs; rheum. fever . . *Heart disease*	17
K. G.	bro.	Debility; heart disease; colds . *Consumption*	40
G. L.	sis.	Bad headaches; coughs; weak spine; hysteria; apoplexy . . . . *Paralysis*	50
F. S.	sis.	Bad colds; inflam. lungs; hysteria	living
R. F.	sis.	Infantile paralysis; colds; nervous depression .	living
L. G.	sis.	Inflam. brain, also lungs; neuralgia; nervous fever	living

(Space left for remarks.)

EXAMPLE B.

Father's name . . . . . JULIUS FITZROY.
Mother's maiden name . . . AMELIA MERRYWEATHER.

Initials.	Kin.	Principal illnesses and cause of death.	Age at death.
R. F.	Father	Gouty Habit . . *Weak Heart and Congest. Liver*	73
L. F.	bro.	. . . . *Gout and Decay*	88
A. G. F.	bro.	. . . . *Accident*	4
W. F.	bro.	. . . . *Typhoid*	16
	Mother	Gall stones . . . . *Internal Malady (?) Cancer*	55
P. M.	bro.	. . . . *Paralysis*	86
A. M.	bro.	. . . . *Paralysis*	85
N. M.	bro.	Still living.	
R. B.	sis.	. . . . *Consumption*	33
C. M.	sis.	. . . . *Rheum. in Head*	88
F. L.	sis.	. . . . *Softening of Brain*	76
		1 died an infant.	
G. F.	bro.	Gout: tendency to mesenteric disease; eruptive disorders . . . *Blood poisoning after a cut*	46
H. F.	bro.	Liver deranged; bad headaches; once supposed consumptive . . . . . *Gradual Paralysis*	45
S. T. F.	bro.	Eruptive disorder; mesentery disease; inflammation of liver . . *Inflammation of Lungs*	42
H. G.	sis.	Eruptive disorder; liver . . . *Inflam. of Lungs*	47
H. B. R.	sis.	Delicate; tend. to mesent. disease . *Consumption*	29
N. F.	sis.	Colds; liver disorder . . . . . . *Consumption*	30
E. L. F.	sis.	Mesenteric disease; grandular swellings . *Atrophy*	16
		2 died infants.	

(Space left for remarks.)

found wherever the book is opened, relate to the same family. The open book is ruled so as to resemble the accompanying schedule, which is drawn on a reduced scale in page 257. The printing within the compartments of the schedule does not appear in the MS. books, it is inserted here merely to show to whom each compartment refers. It will be seen that the paternal ancestry are described in the left page, the maternal in the right. The method of arrangement is quite orderly, but not altogether uniform. To avoid an unpleasing arrangement like a tree with branches, and which is very wasteful of space, each grandparent and his own two parents are arranged in a set of three compartments one above the other. There are, of course, four grandparents and therefore four such sets in the schedule. Reference to the examples A and B pages 252-5 will show how these compartments are filled up. The rest of the Schedule explains itself. The children of the pedigree are written below the compartment assigned to the mother and her brothers and sisters; the vacant spaces are of much occasional service, to receive the overflow from some of the already filled compartments as well as for notes. It is astonishing how much can be got into such a schedule by writing on ruled paper with the lines one-sixth of an inch apart, which is not too close for use. Of course the writing must be small, but it may be bold, and the figures should be written very distinctly.

For a less ambitious attempt, including the grandparents and their fraternity, but not going further back, the left-hand page would suffice, placing 'Children' where 'Father' now stands, 'Father's Father' for 'Father,' and so on throughout.

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THE END.

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NATURAL INHERITANCE. By Francis Galton, F.R.S. Author of "Hereditary Genius," &c. London: Macmillan & Co.

REMEMBERING the former works of Mr Galton, full of interesting inquiries and very curious, if not always conclusive, results, we opened this volume with pleasing expectations. We expected, of course, to find further valuable and fruitful or suggestive investigations into the laws of heredity; into the manner in which, and the extent to which, qualities and faculties become transmitted, and traits or types are fixed in the family and the race. These anticipations were doomed to disappointment, and we find instead certain technical chapters chiefly on the methods of calculation of averages, introductory to some tables of statistics on matters of no great importance. These tables give returns and averages on such points as the "strength of pull," from measures taken at the Health Exhibition; on stature, breathing capacity, "strength of squeeze with the strongest hand"; on marriage selection in relation to stature, to eye-colour, and to artistic and artistic qualities. Now, these subjects of ...

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of hazel-eyed families, or experiments on regression in sweet peas. We know that Mr Galton has been indefatigable in his efforts to get facts and to give figures; that he has enlisted in his researches volunteers, who have filled up schedules of family conditions, and he has urged them to keep family records and "life albums," in which are noted, for scientific purposes, the height and temper, diseases and views of their fathers, mothers, uncles, cousins, and aunts, all to form material for the study of nature's law of inheritance. But what strikes us most in going over these chapters and tables is the poverty of the results. On the measurements of strength the main conclusion seems to be regarding the inequality of the sexes as estimated by Salter's instrument; and this result is so far highly comforting to the male sex, for "out of 1657 adult women of all ages measured at the laboratory, the strongest could only exert a squeeze of 86 lbs., or about that of a medium man." This is really gratifying. Mr Galton has, further, inquired diligently into the qualities which might influence selection in marriage; but the qualities he has specially examined—such as stature, temper, eye-colour, artistic tastes—have not proved potent factors, and he finds, after all his labour, that "these have not determined choice to any notable degree."

It might have been thought that temper would have been an important consideration, but it turns out that it counts for little, probably owing to the well-known fact that the exhibit on of temper is usually a postnuptial incident. Returns, for example, give 46 per cent. of good-tempered husbands, and of these 22 marry good-tempered wives, and 24 marry bad-tempered wives; while the 54 per cent. who are bad-tempered marry 31 good-tempered and 23 bad-tempered wives. This seems, surely, not a very valuable field of inquiry; while, we must say, it seems a great waste of time, ingenuity, and scientific calculation to inquire into and form elaborate tables of the number of persons who marry possessors of eyes of the same colour as themselves, or to reckon how many folk with dark eyes espouse those with hazel, and so on. The futility of the exertion is shown by the fact, discovered by the returns, that eye-colour has no apparent influence whatever. The author has gone into laborious schemes to test whether stature, high or low, is an appreciable influence in determining matrimonial choice, and he is after all obliged to "conclude that marriage selection does not pay such regard to stature as deserves to be taken into account in the cases with which we are concerned." Tall people marry tall, short people marry short, evidently as often as people marry those of contrasting stature. As Mr Galton properly remarks, the frequency of consumption in England being so great that every sixth or seventh person dies of it, renders it an appropriate subject for statistics. Yet the results arrived at are not striking—not even the discovery "of the extraordinary tendency on the part of the mother to transmit consumption" as compared with the father; for what can be more natural than this "extraordinary tendency"? It shows how carefully one requires to weigh the evidence of returns, when Mr Galton has found from records of a certain number of cases, where neither parent was consumptive, that the proportion of consumptive children in the cases sent was 18 or 19 per cent., which is, of course, far too high, seeing that the proportion in the general population is only 16 per cent. This volume consists really of a series of illustrative tables, prefaced by a series of brief, ably wrought-out chapters explanatory of them, and of the methods, geometrical and physiological, in which averages should be struck, returns made and tested, together with the schemes of deviation and frequency, the laws of regression and variability. In short, this is a volume setting forth the scientific principles of calculation, all useful for the accurate testing of facts, very valuable for the statistician and scientist as an apparatus for estimating the weight and force of returns on hereditary influences; but the work does not fulfil its comprehensive title of "Natural Inheritance," because it gives only the methods of examination, and not any results of investigation or any discussion of the most interesting and important subject of heredity which we are led naturally to expect, both from the name of this volume and the character of Mr Galton's previous works.

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NATURAL INHERITANCE

NATURAL INHERITANCE

BY

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AUTHOR OF

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The words by which the various Tables are here described, have been chosen for the sake of quick reference and are not often identical with those used for their actual headings.

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NATURAL INHERITANCE.

NATURAL INHERITANCE.

CHAPTER I.

INTRODUCTORY.

I HAVE long been engaged upon certain problems that lie at the base of the science of heredity, and during several years have published technical memoirs concerning them, a list of which is given in Appendix A. This volume contains the more important of the results, set forth in an orderly way, with more completeness than has hitherto been possible, together with a large amount of new matter.

The inquiry relates to the inheritance of moderately exceptional qualities by brotherhoods and multitudes rather than by individuals, and it is carried on by more refined and searching methods than those usually employed in hereditary inquiries.

One of the problems to be dealt with, refers to the curious regularity commonly observed in the statistical peculiarities of great populations during a long series of generations. The large do not always beget the large, nor the small the small, and yet the observed proportions between the large and the small in each degree of size and in every quality, hardly varies from one generation to another.

A second problem regards the average share contributed to the personal features of the offspring by each ancestor severally. Though one half of every child may be said to be derived from either parent, yet he may receive a heritage from a distant progenitor that neither of his parents possessed as *personal* characteristics. Therefore the child does not on the average receive so much as one half of his *personal* qualities from each parent, but something less than a half. The question I have to solve, in a reasonable and not merely in a statistical way, is, how much less?

The last of the problems that I need mention now, concerns the nearness of kinship in different degrees. We are all agreed that a brother is nearer akin than a nephew, and a nephew than a cousin, and so on, but how much nearer are they in the precise language of numerical statement?

These and many other problems are all fundamentally connected, and I have worked them out to a first degree of approximation, with some completeness. The conclusions cannot however be intelligibly presented in an introductory chapter. They depend on ideas that must first be well comprehended, and which are now novel to the large majority of readers and unfamiliar to all. But those who care to brace themselves to a sustained effort, need not feel much regret that the road to be travelled over is indirect, and does not admit of being mapped beforehand in a way they can clearly understand. It is full of interest of its own. It also familiarizes us with the measurement of variability, and with curious laws of chance that apply to a vast diversity of social subjects. This part of the inquiry may be said to run along a road at a high level, that affords wide views in unexpected directions, and from which easy descents may be made to totally different goals to those we have now to reach. I have a great subject to write upon, but feel keenly my literary incapacity to make it easily intelligible without sacrificing accuracy and thoroughness.

CHAPTER II.

PROCESSES IN HEREDITY.

Natural and Acquired Peculiarities.—Transmutation of Female into Male Measures.—Particulate Inheritance.—Family Likeness and Individual Variation.—Latent Characteristics.—Heritages that Blend and those that are Mutually Exclusive.—Inheritance of Acquired Faculties.—Variety of Petty Influences.

A CONCISE account of the chief processes in heredity will be given in this chapter, partly to serve as a reminder to those to whom the works of Darwin especially, and of other writers on the subject, are not familiar, but principally for the sake of presenting them under an aspect that best justifies the methods of investigation about to be employed.

Natural and Acquired Peculiarities.—The peculiarities of men may be roughly sorted into those that are natural and those that are acquired. It is of the former that I am about to speak in this book. They are noticeable in every direction, but are nowhere so remarkable as in those twins[1] who have been dissimilar

[1] See *Human Faculty*, p. 237.

in features and disposition from their earliest years, though brought into the world under the same conditions and subsequently nurtured in an almost identical manner. It may be that some natural peculiarity does not appear till late in life, and yet may justly deserve to be considered natural, for if it is decidedly exceptional in its character its origin could hardly be ascribed to the effects of nurture. If it was also possessed by some ancestor, it must be considered to be hereditary as well. But "Natural" is an unfortunate word for our purpose; it implies that the moment of birth is the earliest date from which the effects of surrounding conditions are to be reckoned, although nurture begins much earlier than that. I therefore must ask that the word "Natural" should not be construed too literally, any more than the analogous phrases of inborn, congenital, and innate. This convenient laxity of expression for the sake of avoiding a pedantic periphrase need not be accompanied by any laxity of idea.

Transmutation of Female into Male Measures.—We shall have to deal with the hereditary influence of parents over their offspring, although the characteristics of the two sexes are so different that it may seem impossible to speak of both in the same terms. The phrase of "Average Stature" may be applied to two men without fear of mistake in its interpretation; neither can there be any mistake when it is applied to two women, but what meaning can we attach to the word "Average" when it is applied to the stature of two such different beings as the Father and the Mother? How can we appraise the hereditary contributions of different ancestors whether in this or in any other quality, unless we take into account the sex of each ancestor, in addition to his or her characteristics? Again, the same group of progenitors transmits qualities in different measure to the sons and to the daughters; the sons being on the whole, by virtue of their sex, stronger, taller, hardier, less emotional, and so forth, than the daughters. A serious complexity due to sexual differences seems to await us at every step when investigating the problems of heredity. Fortunately we are able to evade it altogether by using an artifice at the outset, else, looking back as I now can, from the stage which the reader will reach when he finishes this book, I hardly know how we should have succeeded in making a fair start. The artifice is never to deal with female measures as they are observed, but always to employ their male equivalents in the place of them. I transmute all the observations of females before taking them in hand, and thenceforward am able to deal with them on equal terms with the observed male values. For example: the statures of women bear to those of men the proportion of about twelve to thirteen. Consequently by adding to each observed female stature at the rate of one inch for every foot, we are enabled to compare their statures so increased and transmuted, with the observed statures of males, on equal terms. If the observed stature of a woman is 5 feet, it will count by this rule as 5 feet + 5 inches; if it be

6 feet, as 6 feet + 6 inches; if $5\frac{1}{2}$ feet, as $5\frac{1}{2}$ feet + $5\frac{1}{2}$ inches; that is to say, as 5 feet + $11\frac{1}{2}$ inches.[1]

Similarly as regards sons and daughters; whatever may be observed or concluded concerning daughters will, if transmuted, be held true as regarding sons, and whatever is said concerning sons, will if re-transmuted, be held true for daughters. We shall see further on that it is easy to apply this principle to all measurable qualities.

Particulate Inheritance.—All living beings are individuals in one aspect and composite in another. They are stable fabrics of an inconceivably large number of cells, each of which has in some sense a separate life of its own, and which have been combined under influences that are the subjects of much speculation, but are as yet little understood. We seem to inherit bit by bit, this element from one progenitor that from another, under conditions that will be more clearly expressed as we proceed, while the several bits are themselves liable to some small change during the process of transmission. Inheritance may therefore be described as largely if not wholly "particulate," and as such it will be treated in these pages. Though this word is good English and accurately expresses its own meaning, the application

[1] The proportion I use is as 100 to 108; that is, I multiply every female measure by 108, which is a very easy operation to those who possess that most useful book to statisticians, *Crelle's Tables* (G. Reimer, Berlin). It gives the products of all numbers under 1000, each into each; so by referring to the column headed 108, the transmuted values of the female statures can be read off at once.

now made of it will be better understood through an illustration. Thus, many of the modern buildings in Italy are historically known to have been built out of the pillaged structures of older days. Here we may observe a column or a lintel serving the same purpose for a second time, and perhaps bearing an inscription that testifies to its origin, while as to the other stones, though the mason may have chipped them here and there, and altered their shapes a little, few, if any, came direct from the quarry. This simile gives a rude through true idea of the exact meaning of Particulate Inheritance, namely, that each piece of the new structure is derived from a corresponding piece of some older one, as a lintel is derived from a lintel, a column from a column, a piece of wall from a piece of wall.

I will pursue this rough simile just one step further, which is as much as it will bear. Suppose we were building a house with second-hand materials carted from a dealer's yard, we should often find considerable portions of the same old houses to be still grouped together. Materials derived from various structures might have been moved and much shuffled together in the yard, yet pieces from the same source would frequently remain in juxtaposition and it may be entangled. They would lie side by side ready to be carted away at the same time and to be re-erected together anew. So in the process of transmission by inheritance, elements derived from the same ancestor are apt to appear in large groups, just as if they had clung together in the pre-embryonic stage, as perhaps they did. They form what is well expressed by the word "traits," traits of feature and character—that is to say, continuous features and not isolated points.

We appear, then, to be severally built up out of a host of minute particles of whose nature we know nothing, any one of which may be derived from any one progenitor, but which are usually transmitted in aggregates, considerable groups being derived from the same progenitor. It would seem that while the embryo is developing itself, the particles more or less qualified for each new post wait, as it were in competition to obtain it. Also that the particle that succeeds, must owe its success partly to accident of position and partly to being better qualified than any equally well placed competitor to gain a lodgment. Thus the step by step development of the embryo cannot fail to be influenced by an incalculable number of small and mostly unknown circumstances.

Family Likeness and Individual Variation.—Natural peculiarities are apparently due to two broadly different causes, the one is Family Likeness and the other is Individual Variation. They seem to be fundamentally opposed, and to require independent discussion, but this is not the case altogether, nor indeed in the greater part. It will soon be understood how the conditions that produce a general resemblance between the offspring and their parents, must at the same time give rise to a considerable amount of individual differences. Therefore I need not discuss Family Likeness and Individual Varia-

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tion under separate heads, but as different effects of the same underlying causes.

The origin of these and other prominent processes in heredity is best explained by illustrations. That which will be used was suggested by those miniature gardens, self-made and self-sown, that may be seen in crevices or other receptacles for drifted earth, on the otherwise bare faces of quarries and cliffs. I have frequently studied them through an opera glass, and have occasionally clambered up to compare more closely their respective vegetations. Let us then suppose the aspect of the vegetation, not of one of these detached little gardens, but of a particular island of substantial size, to represent the features, bodily and mental, of some particular parent. Imagine two such islands floated far away to a desolate sea, and anchored near together, to represent the two parents. Next imagine a number of islets, each constructed of earth that was wholly destitute of seeds, to be reared near to them. Seeds from both of the islands will gradually make their way to the islets through the agency of winds, currents, and birds. Vegetation will spring up, and when the islets are covered with it, their several aspects will represent the features of the several children. It is almost impossible that the seeds could ever be distributed equally among the islets, and there must be slight differences between them in exposure and other conditions, corresponding to differences in pre-natal conditions. All of these would have some influence upon the vegetation; hence there would be a corresponding variety in the results. In some islets one plant would prevail, in others another; nevertheless there would be many traits of family likeness in the vegetation of all of them, and no plant would be found that had not existed in one or other of the islands.

Though family likeness and individual variations are largely due to a common cause, some variations are so large and otherwise remarkable, that they seem to belong to a different class. They are known among breeders as "sports"; I will speak of these later on.

Latent Characteristics.—Another fact in heredity may also be illustrated by the islands and islets; namely, that the child often resembles an ancestor in some feature or character that neither of his parents personally possessed. We are told that buried seeds may lie dormant for many years, so that when a plot of ground that was formerly cultivated is again deeply dug into and upturned, plants that had not been known to grow on the spot within the memory of man, will frequently make their appearance. It is easy to imagine that some of these dormant seeds should find their way to an islet, through currents that undermined the island cliffs and drifted away their *débris*, after the cliffs had tumbled into the sea. Again, many plants on the islands may maintain an obscure existence, being hidden and half smothered by successful rivals; but whenever their seeds happened to find their way to any one of the islets, while those of their rivals did not, they would sprout freely and assert themselves. This

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illustration partly covers the analogous fact of diseases and other inheritances skipping a generation, which by the way I find to be by no means so usual an occurrence as seems popularly to be imagined.

Heritages that Blend and those that are Mutually Exclusive.—As regards heritages that blend in the offspring, let us take the case of human skin colour. The children of the white and the negro are of a blended tint; they are neither wholly white nor wholly black, neither are they piebald, but of a fairly uniform mulatto brown. The quadroon child of the mulatto and the white has a quarter tint; some of the children may be altogether darker or lighter than the rest, but they are not piebald. Skin-colour is therefore a good example of what I call blended inheritance. It need be none the less "particulate" in its origin, but the result may be regarded as a fine mosaic too minute for its elements to be distinguished in a general view.

Next as regards heritages that come altogether from one progenitor to the exclusion of the rest. Eye-colour is a fairly good illustration of this, the children of a light-eyed and of a dark-eyed parent being much more apt to take their eye-colours after the one or the other than to have intermediate and blended tints.

There are probably no heritages that perfectly blend or that absolutely exclude one another, but all heritages have a tendency in one or the other direction, and the tendency is often a very strong one. This is paralleled by what we may see in plots of wild vegetation, where two varieties of a plant mix freely, and the general aspect of the vegetation becomes a blend of the two, or where individuals of one variety congregate and take exclusive possession of one place, and those of another variety congregate in another.

A peculiar interest attaches itself to mutually exclusive heritages, owing to the aid they afford to the establishment of incipient races. A solitary peculiarity that blended freely with the characteristics of the parent stock, would disappear in hereditary transmission, as quickly as the white tint imported by a solitary European would disappear in a black population. If the European mated at all, his spouse must be black, and therefore in the very first generation the offspring would be mulattoes, and half of his whiteness would be lost to them. If these mulattoes did not interbreed, the whiteness would be reduced in the second generation to one quarter; in a very few more generations all recognizable trace of it would have gone. But if the whiteness refused to blend with the blackness, some of the offspring of the white man would be wholly white and the rest wholly black. The same event would occur in the grandchildren, mostly but not exclusively in the children of the white offspring, and so on in subsequent generations. Therefore, unless the white stock became wholly extinct, some undiluted specimens of it would make their appearance during an indefinite time, giving it repeated chances of holding its own in the struggle for existence, and of establishing itself if its qualities were superior to those of the black stock under any one of many different conditions.

Inheritance of Acquired Faculties.—~~Before closing the chapter~~ I am unprepared to say more than a few words ~~must be said~~ on the obscure, unsettled, and much discussed subject of the possibility of transmitting acquired faculties. The main evidence in its favour is the gradual change of the instincts of races at large, in conformity with changed habits, and through their ~~self~~ increased adaptation to their surroundings, otherwise apparently than through the influence of Natural Selection. There is very little direct evidence of its influence in the course of a single generation, if the phrase of Acquired Faculties is used in perfect strictness and all inheritance is excluded that could be referred to some form of Natural Selection, or of Infection before birth, or of peculiarities of Nurture and Rearing. Moreover, a large deduction from the collection of rare cases must be made on the ground of their being accidental coincidences. When this is done, the remaining instances of acquired disease or faculty, or of any mutilation being transmitted from parent to child, are very few. Some apparent evidence of a positive kind, that was formerly relied upon, has been since found capable of being interpreted in another way, and is no longer adduced. On the other hand there exists such a vast mass of distinctly negative evidence, that every instance offered to prove the transmission of acquired faculties requires to be closely criticized. For example, a woman who was sober becomes a drunkard. Her children born during the period of her sobriety are said to be quite healthy; her subsequent children are said to be neurotic. The objections to accepting this as a valid instance in point are many. The woman's tissues must have been drenched with alcohol, and the unborn infant alcoholised during all its existence in that state. The quality of the mother's milk would be bad. The surroundings of a home under the charge of a drunken woman would be prejudicial to the health of a growing child. No wonder that it became neurotic. Again, a large number of diseases are conveyed by germs capable of passing from the tissues of the mother into those of the unborn child otherwise than through the blood. Moreover it must be recollected that the connection betwen the ~~mother and her~~ unborn child and the mother is hardly more intimate than that between ~~a~~ some parasites and the animals on which ~~it~~ they live~~s~~. Not a single nerve has been traced between them, not a drop of blood[1] has been found to pass from the mother to the child. The unborn child together with the growth to which it is attached, and which is afterwards thrown off, have their own vascular system to themselves, entirely independent of that of the mother. If in an anatomical preparation the veins of the mother are injected with a coloured fluid, none of it enters the veins of the child; conversely, if the veins of the child

[1] See *Lectures* by William O. Priestley, M.D. (Churchill, London, 1860), pp. 50, 52, 55, 59, and 64.

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are injected, none of the fluid enters those of the mother. Again, not only is the unborn child a separate animal from its mother, that obtains its air and nourishment from her purely through soakage, but its constituent elements are of very much less recent growth than is popularly supposed. The ovary of the mother is as old as the mother herself; it was well developed in her own embryonic state. The ova it contains in her adult life were actually or potentially present before she was born, and they grew as she grew. There is more reason to look on them as collateral with the mother, than as parts of the mother. The same may be said with little reservation concerning the male elements. It is therefore extremely difficult to see how acquired faculties can be inherited by the children. It would be less difficult to conceive of their inheritance by the grandchildren. Well devised experiment into the limits of the power of inheriting acquired faculties and mutilations, whether in plants or animals, is one of the present desiderata in hereditary science. Fortunately for us, our ignorance of the subject will not introduce any special difficulty in the inquiry on which we are now engaged.

Variety of Petty Influences.—The incalculable number of petty accidents that concur to produce variability among brothers, make it impossible to predict the exact qualities of any individual from ~~those of his parents.~~ hereditary data. But we may predict average results with great certainty, as will be seen further on, and we can also obtain precise information concerning the penumbra of uncertainty that attaches itself to single predictions. It would be premature to speak further of this at present; what has been said is enough to give a clue to the chief motive of this chapter. Its intention has been to show the large part that is always played by chance in the course of hereditary transmission, and to establish the importance of an intelligent use of the laws of chance and of the statistical methods that are based upon them, in ~~establishing~~ [expressing] the conditions under which heredity acts.

I may here point out that, as the processes of statistics are themselves processes of intimate blendings, their results are the same, whether ~~or no~~ the materials had [been] partially ~~gone through the process of~~ blend~~ing~~[ed] [or not,] before they were statistically taken in hand.

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CHAPTER III.

ORGANIC STABILITY.

Structure.—Filial relation.—Stable Forms.—Subordinate positions of Stability.—Model.—Stability of Sports.—Evolution not by ~~small~~ minute steps only. (Infertility of mixed Types.—)

Structure.—The total heritage of each man must include a greater variety of material than was utilised in forming his personal structure. The existence in some latent form of an unused portion is proved by his power, already alluded to, of transmitting ancestral characters that he did not personally exhibit. Therefore the organised structure of each individual should be viewed as the fulfilment of only one out of an indefinite number of mutually exclusive possibilities. His structure is the coherent and more or less stable development of what is no more than an imperfect sample of a large variety of elements.

The precise conditions under which each several element or particle (whatever may be its nature) finds its way into the sample are, it is needless to repeat, unknown, but we may provisionally classify them under one or other of the following three categories, as they apparently exhaust all reasonable possibilities: first, that in which each element selects its most suitable immediate neighbourhood, in accordance with the guiding idea in Darwin's theory of Pangenesis; secondly, that of more or less general co-ordination of the influences exerted on each element, not only by its immediate neighbours, but by many or most of the others as well; finally, that of accident or chance, under which name a group of agencies are to be comprehended, diverse in character and alike only in the fact that their influence on the settlement of each particle was not immediately directed towards that end. In philosophical language we say that such agencies are not purposive, or that they are not teleological; in popular language they are called accidents or chances.

Filial Relation.—A conviction that inheritance is mainly particulate and much influenced by chance, greatly affects our idea of kinship and makes us consider the parental and filial relation to be curiously circuitous. It appears that there is no direct hereditary relation between the personal parents and the personal child, except perhaps through little-known channels of secondary importance, but that the main line of hereditary connection unites the sets of elements out of which the personal parents had been evolved with the set out of which the personal child was evolved. The main line may be rudely likened to the chain of a necklace, and the personalities to pendants attached to its links. We are unable to see the particles and watch their grouping, and we know nothing directly about them, but we may gain some idea of the various possible results by noting the differences between the brothers in any large fraternity (as will be done further on with much minuteness), whose total heritages must have been much alike, but whose personal structures are often very dissimilar. This is why it is so important in hereditary inquiry to deal with fraternities rather than with individuals, and with large fraternities rather than small ones. We ought, for example, to compare the group containing both parents and all the uncles and aunts, with that containing all the children. The relative weight to be assigned to the uncles and aunts is a question of detail to be discussed in its proper place further on. (see Chap. XI).

Stable Forms.—The changes in the substance of the newly-fertilised ova of all animals, of which more is annually becoming known,[1] indicate segregations as well as aggregations, and it is reasonable to suppose that repulsions concur with affinities in producing them. We know nothing as yet of the nature of these affinities and repulsions, but we may expect them to act in great numbers and on all sides in a space of three dimensions, just as the personal likings and dis-

[1] A valuable memoir on the state of our knowledge of these matters up to the end of 1887 is published in Vol. XIX. of the *Proceedings of the Philosophical Society of Glasgow*, and reprinted under the title of *The Modern Cell Theory, and theories as to the Physiological Basis of Heredity*, by Prof. John Gray McKendrick, M.D., F.R.S., &c. (R. Anderson, Glasgow, 1888.)

likings of each individual insect in a flying swarm may be supposed to determine the position that he occupies in it. Every particle must have many immediate neighbours. Even a sphere surrounded by other spheres of equal sizes, like a cannon-ball in the middle of a heap, when they are piled in the most compact form, is in actual contact with no less than twelve others. We may therefore feel assured that the particles which are still unfixed must be affected by very numerous influences acting from all sides and varying with slight changes of place, and that they may occupy many positions of temporary and unsteady equilibrium, and be subject to repeated unsettlement, before they finally assume the positions into which they severally remain at rest.

The whimsical effects of chance in producing stable results are common enough. Tangled strings variously twitched, soon get themselves into tight knots. Rubbish thrown down a sink is pretty sure in time to choke the pipe; no one bit may be so large as its bore, but several bits in their numerous chance encounters will at length so come into collision as to wedge themselves into a sort of arch across the tube, and effectually plug it. Many years ago there was a fall of large stones from the ruinous walls of Kenilworth Castle. Three of them, if I recollect rightly, or possibly four, fell into a very peculiar arrangement, and bridged the interval between the jambs of an old window. There they stuck fast, showing clearly against the sky. The oddity of the structure attracted continual attention, and its stability was much commented on. These hanging stones, as they were called, remained quite firm for many years; at length a storm shook them down.

In every congregation of mutually reacting elements, some characteristic groupings are usually recognised that have become familiar through their frequent recurrence and partial persistence. Being less evanescent than other combinations, they may be regarded as temporarily Stable Forms. No demonstration is needed to show that their number must be greatly smaller than that of all the possible combinations of the same elements. I will briefly give as great a diversity of instances as I can think of, taken from Governments, Crowds, Landscapes, and even from Cookery, and shall afterwards draw some illustrations from mechanical inventions, to illustrate what is meant by characteristic and stable groupings. From some of them it will also be gathered that secondary and other orders of stability exist besides the primary ones.

In Governments, the primary varieties of stable forms are very few in number, being such as autocracies, constitutional monarchies, oligarchies, or republics. The secondary forms are far more numerous; still it is hard to meet with an instance of one that cannot be pretty closely paralleled by another. A curious evidence of the small variety of possible ~~constitutions~~ governments is to be found in the constitutions of the governing bodies of the Scientific Societies of London and the Provinces, which are numerous and independent. Their development seems to follow a single course that has many stages, and invariably tends towards the following staff of officers: President, vice-Presidents, a Council, Honorary Secretaries, a paid Secretary, Trustees, and a Treasurer. As Britons are not unfrequently servile to rank, some seek a purely ornamental Patron as well.

Every variety of Crowd has its own characteristic features. At a national pageant, an evening party, a race-course, a marriage, or a funeral, the groupings in each case recur so habitually that it sometimes appears to me as if time had no existence, and that the ceremony in which I am taking part is identical with others at which I had been present one year, ten years, twenty years, or any other time ago.

The frequent combination of the same features in Landscape Scenery, justifies the use of such expressions as "true to nature," when applied to a pictorial composition or to the descriptions of a novel writer. The experiences of travel in one part of the world may curiously resemble those in another. Thus the military expedition by boats up the Nile was planned from experiences gained on the Red River of North America, and carried out with the aid of Canadian *voyageurs*. The snow mountains all over the world present the same peculiar difficulties to the climber, so that Swiss experiences and in many cases Swiss guides have been used for the exploration of the Himalaya, the Caucasus, the lofty mountains of New Zealand, the Andes, and Greenland. Whenever the general conditions of the country and climate are alike, we recognise characteristic and familiar features at every turn, whether we are walking by the brookside, along the seashore, in the woods, or on the hills.

Even in Cookery it seems difficult to invent a new and good dish, though the current recipes are few, and the proportions of the flour, sugar, butter, eggs, &c., used in making them might be indefinitely varied and be still eatable. I consulted cookery books to learn the facts authoritatively, and found the following passage: "I have constantly kept in view the leading principles of this work, namely, to give in these domestic recipes *the most exact quantities.* . . . I maintain that one cannot be too careful; it is the only way to put an end to those approximations and doubts which will beset the steps of the inexperienced, and which account for so many people eating indifferent meals at home."[1]

It is the triteness of these experiences that makes the most varied life monotonous after a time, and many old men as well as Solomon have frequent occasion to lament that there is nothing new under the sun.

The object of these diverse illustrations is to impress the meaning I wish to convey, by the phrase of stable forms or groupings, which, however ~~vague~~ uncertain it may be in outline, is perfectly distinct in substance.

Every one of the meanings that have been attached by writers to the vague but convenient word "type" has for its central idea the existence of a limited number

[1] *The Royal Cookery Book.* By Jules Gouffé, Chef de Cuisine of the Paris Jockey Club; translated by Alphonse Gouffé, Head Pastry Cook to H.M. the Queen. Sampson Low. 1869. Introduction, p. 9.

f. 20r

of frequently recurrent forms. The word etymologically compares these forms to the identical medals that may be struck by one or other of a set of dies. The central idea on which the phrase "stable forms" is based is of the same kind, while the phrase further accounts for their origin, vaguely it may be, but still significantly, by showing that though we know little or nothing of details, the result of organic groupings is analogous to much that we notice elsewhere on every side.

Subordinate positions of Stability.—Of course there are different degrees of stability. If the same structural form recurs in successively descending generations, its stability must be great, otherwise it could not have withstood the effects of the admixture of equal doses of alien elements in successive generations. Such a form well deserves to be called typical. A breeder would always be able to establish it. It tends of itself to become a new and stable variety; therefore all the breeder has to attend to is to give fair play to its tendency, by weeding out from among its offspring such reversions to other forms as may crop up from time to time, and by preserving the breed from rival admixtures until it has become confirmed, and adapted in every minute particular to its surroundings.

Personal Forms may be compared to Human Inventions, as these also may be divided into types, sub-types, and deviations from them. Every important invention is a new type, and of such a definite kind as to admit of clear verbal description, and so of becoming the subject of patent rights; at the same time it need not be so minutely defined as to exclude the possibility of small improvements or of deviations from the main design, any of which may be freely adopted by the inventor without losing the protection of his patent. But the range of protection is by no means sharply distinct, as most inventors know to their cost. Some other man, who may or may not be a plagiarist, applies for a separate patent for himself, on the ground that he has introduced modifications of a fundamental character; in other words, that he has created a fresh type. His application is opposed, and the question whether his plea be valid or not, becomes a subject for legal decision.

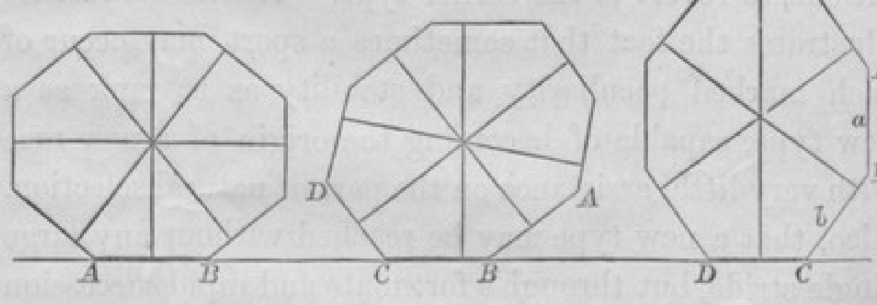
Whenever a patent is granted subsidiary to another, and lawful to be used only by those who have acquired rights to work the primary invention, then we should rank the new patent as a secondary and not as a primary type. Thus we see that mechanical inventions offer good examples of types, sub-types, and mere deviations.

The three kinds of public carriages that characterise the streets of London; namely, omnibuses, hansoms, and four-wheelers, are specific and excellent illustrations of what I wish to express by mechanical types, as distinguished from sub-types. Attempted improvements in each of them are yearly seen, but none have as yet superseded the old familiar patterns, which cannot, as it thus far appears, be changed with advantage, taking the circumstances of London as they are. Yet there have been numerous subsidiary and patented contrivances, each a distinct step in the improvement of one or other of the three primary types, and there are or may be in each of the three an indefinite number of varieties in details, too unimportant to be subjects of patent rights.

The broad classes, of primary or subordinate types, and of mere deviations from them, are separated by no well-defined frontiers. Still the distinction is very serviceable, so much so that the whole of the laws of patent and copyright depend upon it, and it forms the only foundation for the title to a vast amount of valuable property. Corresponding forms of classification must be equally appropriate to the organic structure of all living things.

Model.—The distinction between primary and subordinate positions of stability will be made clearer by the

FIG. 1.



help of Fig 1, which is drawn from a model I made. The model has more sides, but Fig. 1 suffices for illustration. It is a polygonal slab that can be made to stand on any one of its edges when set upon a level table, and is intended to illustrate the meaning of primary and subordinate stability in organic structures, although the conditions of these must be far more complex than anything we have wits to imagine. The model and the organic structure have the cardinal fact in common, that if either is disturbed without transgressing the range of its stability, it will tend to re-establish itself, but if the range is overpassed it will topple over into a new position; also that ~~they~~ both of them are more likely to topple over towards the position of primary stability, than away from it.

The ultimate point to be illustrated is this. Though a long established race habitually breeds true to its kind, subject to small unstable deviations, yet every now and then the offspring of these deviations do not tend to revert, but possess some small stability of their own. They therefore have the character of sub-types, always, however, with a reserved tendency under strained conditions, to revert to the earlier type. The model further illustrates the fact that sometimes a sport may occur of such marked peculiarity and stability as to rank as a new type, capable of becoming the origin of a new race with very little assistance on the part of natural selection. Also, that a new type may be reached without any large single stride, but through a fortunate and rapid succession of many small ones.

The model is a polygonal slab, the polygon being one that might have been described within an oval, and it is so shaped as to stand on any one of its edges. When the slab rests as in Fig. 1, on the edge A B, corresponding to the shorter diameter of the oval, it stands in its most stable position, and in one from which it is equally difficult to dislodge it by a tilt either forwards or backwards. So long as it is merely tilted it will fall back on being left alone, and its position when merely tilted corresponds to a simple deviation. But when it is pushed with sufficient force, it will tumble on to the next edge, B C, into a new position of stability. It will rest there, but less securely than in its first position; moreover its range of stability will no longer be disposed symmetrically. A comparatively slight push from the front will suffice to make it tumble back, a comparatively heavy push from behind is needed to make it tumble forward. If it be tumbled over into a third position (not shown in the Fig.), the process just described may recur with exaggerated effect, and similarly for many subsequent ones. If, however, the slab is at length brought to rest on the edge C D, most nearly corresponding to its longest diameter, the next onward push, which may be very slight, will suffice to topple it over into an entirely new system of stability; in other words, a "sport" comes suddenly into existence. Or the figure might have been drawn with its longest diameter passing into a projecting spur, so that a push of extreme strength would be required to topple it entirely over.

If the first position, A B, is taken to represent a type, the other portions will represent sub-types. All the stable positions on the same side of the longer diameter are subordinate to the first position. On whichever of of them the polygon may stand, its principal tendency on being seriously disturbed will be to fall back towards the first position; yet each position is stable within certain limits.

Consequently the model illustrates how the following conditions may co-exist: (1) Variability within narrow limits without prejudice to the purity of the breed. (2) Partly stable sub-types. (3) Tendency, when much disturbed, to revert from a sub-type to an earlier form. (4) Occasional sports which may give rise to new types.

Stability of Sports.—Experience does not show that those wide varieties which are called "sports" are unstable. On the contrary, they are often transmitted to successive generations with curious persistence. Neither is there any reason for expecting otherwise. While we can well understand that a strained modification of a type would not be so stable as one that approximates more nearly to the typical centre, the variety may be so wide that it falls into different conditions of stability, and ceases to be a strained modification of the original type.

The hansom cab was originally a marvellous novelty. In the language of breeders it was a sudden and remarkable "sport," yet the suddenness of its appearance has been no bar to its unchanging hold on popular favour. It is not a monstrous anomaly of incongruous parts, and therefore unstable, but quite the contrary. Many other instances of very novel and yet stable inventions could be quoted. One of the earliest electrical batteries was that which is still known as a Grove battery, being the invention of Sir William Grove. Its principle was quite new at the time, and it continues in use without alteration.

The persistence in inheritance of trifling characteristics, such as a mole, a white tuft of hair, or multiple fingers, has often been remarked. The reason of it is, I presume, that such characteristics have inconsiderable influence upon the general organic stability; they are mere excrescences, that may be associated with very different types, and are therefore inheritable without let or hindrance.

It seems to me that stability of type, about which we as yet know very little, must be an important factor in the general theory of heredity, when the theory is applied to cases of high breeding. It will be shown later on, at what point a separate allowance requires to be made for it. But in the earlier and principal part of the inquiry, which deals with the inheritance of qualities that are only exceptional in a small degree, a separate allowance does not appear to be required.

Infertility of Mixed Types.—It is not difficult to see in a general way why very different types should refuse to coalesce, and it is scarcely possible to explain the reason why, more clearly than by an illustration. Thus a useful blend between a four-wheeler and a hansom would be impossible; it would have to run on three wheels and the half-way position for the driver would be upon its roof. A blend would be equally impossible between an omnibus and a hansom, and it would be difficult between an omnibus and a four-wheeler.

Evolution not by Minute Steps Only.—The theory of Natural Selection might dispense with a restriction, for which it is difficult to see either the need or the justification, namely, that the course of evolution always proceeds by steps that are severally minute, and that become effective only through accumulation. That the steps *may* be small and that they *must* be small are very different views; it is only to the latter that I object, and only when the indefinite word "small" is used in the sense of "barely discernible," or as small compared with such large sports as are known to have been the origins of new races. An apparent ground for the common belief is founded on the fact that whenever search is made for intermediate forms between widely divergent varieties, whether they be of plants or of animals, of weapons or utensils, of customs, religion or language, or of any other product of evolution, a long and orderly series can usually be made out, each member of which differs in an almost imperceptible degree from the adjacent specimens. But it does not at all follow because these intermediate forms have been found to exist, that they are the very stages that were passed through in the course of evolution. Counter evidence exists in abundance, not only of the appearance of considerable sports, but of their remarkable stability in hereditary transmission. Many of the specimens of intermediate forms may have been unstable varieties, whose descendants had reverted; they might be looked upon as tentative and faltering steps taken along parallel courses of evolution, and afterwards retracted. Affiliation from each generation to the next requires to be proved before any apparent line of descent can be accepted as the true one. The history of inventions fully illustrates this view. It is a most common experience that what an inventor knew to be original, and believed to be new, had been invented independently by others many times before, ~~and~~ but had never become established. Even when it has new features, the inventor usually finds, on consulting lists of patents, that other inventions closely border on his own. Yet we know that inventors often proceed by strides, their ideas originating in some sudden happy thought suggested by a chance occurrence, though their crude ideas may have to be laboriously worked out afterwards. If, however, all the varieties of any machine that had ever been invented, were collected and arranged in a Museum in the apparent order of their Evolution, each would differ so little from its neighbour as to suggest the fallacious inference that the successive inventors of that machine had progressed by means of a very large number of hardly discernible steps.

The object of this and of the preceding chapter has been first to dwell on the fact of inheritance being "particulate," secondly to show how this fact is compatible with the existence of various types, some of which are subordinate to others, and thirdly to argue that Evolution need not proceed by small steps only. I have largely used metaphor and illustration to explain the facts, wishing to avoid entanglements with theory as far as possible, inasmuch as no complete theory of inheritance has yet been propounded that meets with general acceptation.

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f. 25r

CHAPTER IV.

SCHEMES OF DISTRIBUTION AND OF FREQUENCY.

Fraternities and Populations to be treated as Units.—Schemes of Distribution and their Grades.—The Shape of Schemes is independent of the number of observations.—Data for Eighteen Schemes.—Application of the method of Schemes to inexact Measures.—Schemes of Frequency.

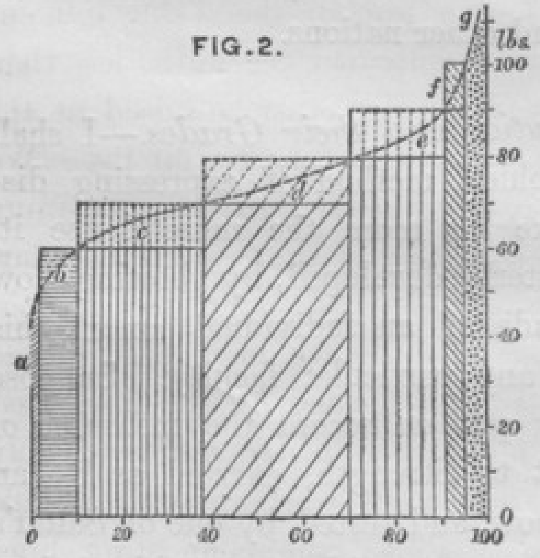
Fraternities and Populations to be Treated as Units.—The science of heredity is concerned with Fraternities and large Populations rather than with individuals, and must treat them as units. A compendious method is therefore requisite by which we may express the distribution of each faculty among the members of any large group, whether it be a Fraternity or an entire Population.

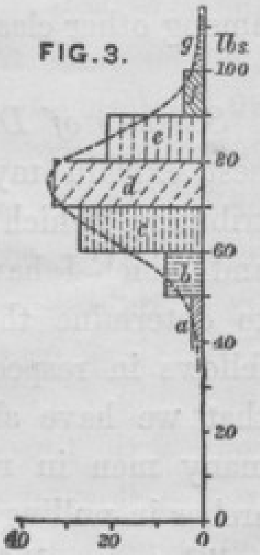
The knowledge of an average value is a meagre piece of information. How little is conveyed by the bald statement that the average income of English families is 100*l.* a year, compared with what we should learn if we were told how English incomes were distributed; what proportion of our countrymen had just and only just enough means to ward off starvation, and what were the proportions of those who had incomes in each and every other degree, up to the huge annual receipts of a few great speculators, manufacturers, and landed proprietors. So in respect to the distribution of any human quality or faculty, a knowledge of mere averages tells but little; we want to learn how the quality is distributed among the various members of the Fraternity or of the Population, and to express what we know in so compact a manner that it can be easily grasped and dealt with. A parade of great accuracy is foolish, because precision is unattainable in biological and social statistics; their results being never strictly constant. Over-minuteness is mischievous, because it overwhelms the mind with more details than can be compressed into a single view. We require no more than a fairly just and comprehensive method of expressing the way in which each measurable quality is distributed among the members of any group, whether the group consists of brothers or of members of any particular social, local, or other body of persons, or whether it is co-extensive with an entire nation or race.

A knowledge of the distribution of any quality enables us to ascertain the Rank that each man holds among his fellows, in respect to that quality. This is a valuable piece of knowledge in this struggling and competitive world, where success is to the foremost, and failure to the hindmost, irrespective of absolute efficiency. A blurred vision would be above all price to an individual man in a nation of blind men, though it would hardly enable him to earn his bread elsewhere. When the distribution of any faculty has been ascertained, we can tell from the measurement, say of our child, how he ranks among other children in respect to that faculty, whether it be a physical gift, or one of health, or of intellect, or of morals. As the years go by, we may learn by the same means whether he is making his way towards the front, whether he just holds his place, or whether he is falling back towards the rear. Similarly as regards the position of our class, or of our nation, among other classes and other nations.

Schemes of Distribution and their Grades.—I shall best explain my graphical method of expressing Distribution, which I like the more, the more I use it, and which I have latterly developed, by showing how to determine the Grade of an individual among his fellows in respect to any particular faculty. Suppose that we have already put on record the measures of many men in respect to Strength, exerted as by an archer in pulling his bow, and tested by one of Salter's well-known dial instruments with a movable index. Some men will have been found strong and others weak, how can we picture in a compendious diagram, or how can we define by figures, the distribution of this faculty of Strength throughout the group? How shall we determine and specify the Grade that any particular person would occupy in the group? The first step is to marshal our measures in the orderly way familiar to statisticians, which is shown in Table I. I usually work to about twice its degree of minuteness, but enough has been entered in the Table for the purpose of illustration, while its small size makes it all the more intelligible.

The fourth column of the Table headed "Percentages" of "Sums from the beginning," is pictorially translated into Fig. 2, and the third column headed "Percentages" of "No. of cases observed," into Fig. 3. The scale of





lbs. is given at the side of both Figs.: and the compartments *a* to *g*, that are shaded with *broken* lines, have the same meaning in both, but they are differently disposed in the two Figs. We will now consider Fig. 2 only, which is the one that principally concerns us. The percentages in the last column of Table I. have been marked off on the bottom line of Fig. 2, where they are called (centesimal) Grades. The number of lbs. found in the first column of the Table determines

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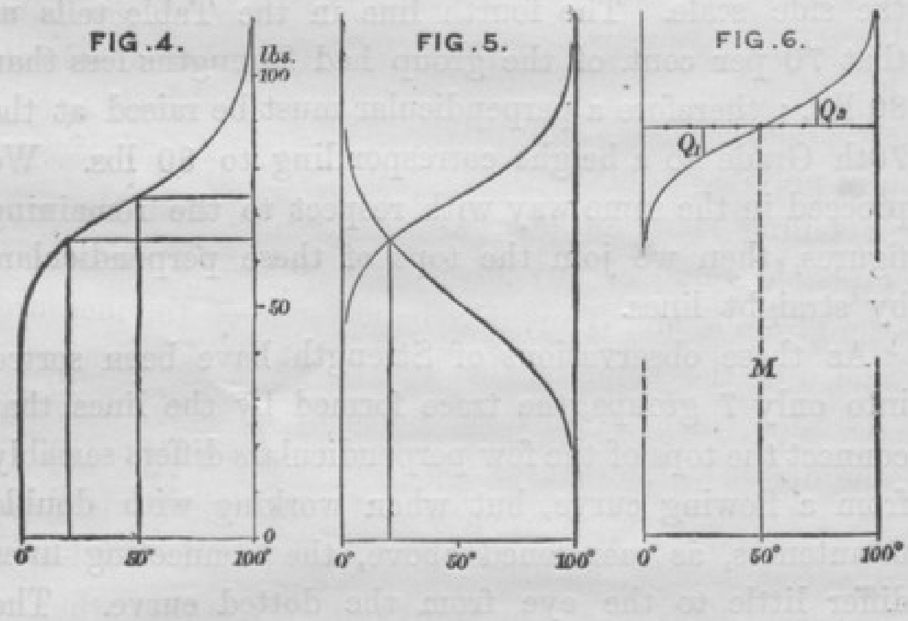
the height of the vertical lines to be erected at the corresponding Grades when we are engaged in constructing the Fig.

Let us begin with the third line in the Table for illustration: it tells us that 37 per cent. of the group had Strengths less than 70 lbs. Therefore, when drawing the figure, a perpendicular must be raised at the 37th grade to a height corresponding to that of 70 lbs. on the side scale. The fourth line in the Table tells us that 70 per cent. of the group had Strengths less than 80 lbs.; therefore a perpendicular must be raised at the 70th Grade to a height corresponding to 80 lbs. We proceed in the same way with respect to the remaining figures, then we join the tops of these perpendiculars by straight lines.

As these observations of Strength have been sorted into only 7 groups, the trace formed by the lines that connect the tops of the few perpendiculars differs sensibly from a flowing curve, but when working with double minuteness, as mentioned above, the connecting lines differ little to the eye from the dotted curve. The dotted curve may then be accepted as that which would result if a separate perpendicular had been drawn for every observation, and if permission had been given to slightly smooth their irregularities. I call the figure that is bounded by such a curve as this, a Scheme of Distribution, the perpendiculars ~~being~~ first rubbed out, that formed the scaffolding by which it was constructed. having been (See Fig 4.)

A Scheme enables us in a moment to find the Grade of Rank (on a scale reckoned from 0° to 100°) of any person in the group to which he belongs. The measured strength of the person is to be looked for in the side scale of the Scheme ; a horizontal line is thence drawn until it meets the curve ; from the point of meeting a perpendicular is dropped upon the scale of Grades at the base ; then the Grade on which it falls is

FIG. 4. FIG. 5. FIG. 6.



the one required. For example: let us suppose the Strength of Pull of a man to have been 74 lbs., and that we wish to determine his Rank in Strength among the large group of men who were measured at the Health Exhibition in 1884. We find by Fig. 4 that his centesimal Grade is 50°; in other words, that 50 per cent. of the group will be weaker than he is, and 50 per cent. will be stronger. His position will be exactly Middlemost, after the Strengths of all the men in the group have been marshalled in the order of their magnitudes. In other words, he is of mediocre strength. The accepted term to express the value that occupies the Middlemost position is "Median," which may be used either as an adjective or as a substantive, but it will be usually replaced in this book by the abbreviated form M. I also use the word "Mid" in a few combinations, such as "Mid-Fraternity," to express the same thing. The Median, M, has three properties. The first follows immediately from its construction, namely, that the chance is an equal one, of any previously unknown measure in the group exceeding or falling short of M. The second is, that the most probable value of any previously unknown measure in the group is M. Thus if N be any one of the measures, and u be the value of the unit in which the measure is recorded, such as an inch, tenth of an inch, &c., then the number of measures that fall between $(N - \frac{1}{2}u)$ and $(N + \frac{1}{2}u)$, is greatest when $N = M$. Mediocrity is always the commonest condition, for reasons that will become apparent later on. The third property is that whenever the curve of the Scheme is symmetrically disposed on either side of M, except that one half of it is turned upwards, and the other half downwards, then M is identical with the ordinary Arithmetic Mean or Average. This is closely the condition of all the curves I have to discuss. The reader may look on the Median and on the Mean as being practically the same things, throughout this book.

It must be understood that M, like the Mean or the Average, is almost always an interpolated value, corresponding to no real measure. If the observations were infinitely numerous its position would not differ more than infinitesimally from that of some one of them; even in an ~~ordinary~~ series of ~~a few~~ one or two hundred in number, the difference is insignificant.

Now let us make our Scheme answer another question. Suppose we want to know the percentage of men in the group of which we have been speaking, whose Strength lies between any two specified limits, as between 74 lbs. and 64 lbs. We draw horizontal lines (Fig. 4) from points on the side scale corresponding to either limit, and drop perpendiculars upon the base, from the points where those lines meet the curve. Then the number of Grades in the intercept, is the answer. The Fig. shows that the number in the present case is 30; therefore 30 per cent. of the group have Strengths of Pull ranging between 74 and 64 lbs.

We learn how to transmute female measures of any characteristic into male ones, by comparing their respective schemes, and devising a formula that will change the one into the other. In the case of Stature, the the simple multiple of 1·08 was found to do this with sufficient precision.

If we wish to compare the average Strengths of two different groups of persons, say one consisting of men and the other of women, we have simply to compare the values at the 50th Grades in the two schemes. For even if the Medians differ considerably from the Means,

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both the ratios and the differences between either pair of values would be sensibly the same.

A different way of comparing two Schemes is sometimes useful. It is to draw them in opposed directions, as in Fig. 5, p. 40. Their curves will then cut each other at some point, whose Grade when referred to either of the two Schemes (whichever of them may be preferred), determines the point at which the same values are to be found. In Fig. 5, the Grade in the one Scheme is 20°; therefore in the other Scheme it is 100° − 20°, or 80°. In respect to the Strength of Pull of men and women, it appears that the woman who occupies the Grade of 96° in her Scheme, has the same strength as the man who occupies the Grade of 4° in his Scheme.

I should add that this great inequality in Strength between the sexes, is confirmed by other measurements made at the same time in respect to the Strength of their Squeeze, as tested by another of Salter's instruments. Then the woman in the 93rd and the man in the 7th Grade of their resective Schemes, proved to be of equal strength. In my paper[1] on the results obtained at the laboratory, I remarked: "Very powerful women exist, but happily perhaps for the repose of the other sex such gifted women are rare. Out of 1,657 adult women of all ages measured at the laboratory, the strongest could only exert a squeeze of 86 lbs., or about that of a medium man."

[1] *Journ. Anthropol. Inst.* 1885. *Mem.:* There is a blunder in the paragraph, p. 23, headed "Height Sitting and Standing." The paragraph should be struck out.

The Shape of Schemes is Independent of the Number of Observations.—When Schemes are drawn from different samples of the same large group of measurements, though the number in the several samples may differ greatly, we can always so adjust the horizontal scales that the breadth of the several Schemes shall be uniform. Then the shapes of the Schemes drawn from different samples will be little affected by the number of observations used in each, supposing of course that the numbers are never too small for ordinary statistical purposes. The only recognisable differences between the Schemes will be, that, if the number of observations in the sample is very large, the upper margin of the Scheme will fall into a more regular curve, especially towards either of its limits. Some irregularity will be found in the above curve of the Strength of Pull; but if the observations had been ten times more numerous, it is probable, judging from much experience of such curves, that the irregularity would have been less conspicuous, and perhaps would have disappeared altogether.

However numerous the observations may be, the curve will always be uncertain and incomplete at its extreme ends, because the next value may happen to be greater or less than any one of those that preceded it. Again, the position of the first and the last observation, supposing each observation to have been laid down separately, can never coincide with the adjacent limit. The more numerous the observations, and therefore the closer the perpendiculars by which they are represented, the nearer will the two extreme perpendiculars approach the limits, but they will never actually touch them. A chess board has eight squares in a row, and eight pieces may be arranged in order on any one row, each piece occupying the centre of a square. Let the divisions in the row be graduated, calling the boundary to the extreme left, 0°. Then the successive divisions between the squares will be 1°, 2°, 3°, up to 7°, and the boundary to the extreme right will be 8°. It is clear that the position of the first piece lies half-way between the grades (in a scale of eight grades) of 0° and 1°; therefore the grade occupied by the first piece would be counted on that scale as 0·5°; also the grade of the last piece as 7·5°. Or again, if we had 800 pieces, and the same number of class-places, the grade of the first piece, in a scale of 800 grades, would exceed the grade 0°, by an amount equal to the width of one half-place on that scale, while the last of them would fall short of the 800th grade by an equal amount. This half-place has to be attended to and allowed for when schemes are constructed from comparatively few observations, and always when values that are very near to either of the centesimal grades 0° or 100° are under observation; but between the centesimal grades of 5° and 95° the influence of a half class-place upon the value of the corresponding observation is insignificant, and may be disregarded. It will not henceforth be necessary to repeat the word centesimal. It will be always implied when nothing is said to the contrary, and nothing henceforth will be said to the contrary except once in the next paragraph.

Data for Eighteen Schemes.—Sufficient data for reconstructing any Scheme, with much correctness, may be printed in a single line of a Table, and according to a uniform plan that is suitable for any kind of values. The measures to be recorded are those at a few definite Grades, beginning say at 5°, ending at 95°, and including every intermediate tenth Grade from 10° to 90°. It is convenient to add those at the Grades 25° and 75°, if space permits. The former values are given for eighteen different Schemes, in Table 2. In the memoir from which this table is reprinted, the values at what I now call (centesimal) Grades, were termed Percentiles. Thus the values at the Grades 5° and 10° would be respectively the 5th and the 10th percentile. It still seems to me that the word percentile is a useful and expressive abbreviation, but it will not be necessary to employ it in the present book. It is of course unadvisable to use more technical words than is absolutely necessary, and it will be possible to get on without it, by the help of the new and more important word "Grade."

A series of Schemes that express the distribution of various faculties, is valuable in an anthropometric laboratory, for they enable every person who is measured to find his Rank or Grade in each of them.

Diagrams may also be constructed by drawing parallel lines, each divided into 100 Grades, and entering each round number of inches, lbs., &c., at their proper places. A diagram of this kind is very convenient for reference, but it does not admit of being printed; it must be drawn or lithographed. I have constructed one of these from the 18 Schemes, and find it is easily understood and much used at my laboratory.

Application of Schemes to Inexact Measures.—Schemes of Distribution may be constructed from observations that are barely exact enough to deserve to be called measures.

I will illustrate the method of doing so by marshalling the data contained in a singularly interesting little memoir written by Sir James Paget, into the form of such a Scheme. It is published in vol. v. of St. Bartholomew's Hospital Reports, and is entitled "What Becomes of Medical Students." He traced with great painstaking the career of no less than 1,000 pupils who had attended his classes at that Hospital during various periods and up to a date 15 years previous to that at which his memoir was written. He thus did for St. Bartholomew's Hospital what has never yet been done, so far as I am aware, for any University or Public School, whose historians count the successes and are silent as to the failures, giving to inquirers no adequate data for ascertaining the real value of those institutions in English Education. Sir J. Paget divides the successes of his pupils in their profession into five grades, all of which he carefully defines; they are *distinguished; considerable; moderate; very limited success;* and *failures.* Several of the students had left the profession either before or after taking their degrees, usually owing to their unfitness to succeed, so after analysing the accounts of them given in the memoir, I drafted several into the list of failures and distributed the rest, with the result that the number of cases in the successive classes, amounting now to the full total of 1,000, became 28, 80, 616, 151, and 125. This differs, I should say, a little from the inferences of one author, but the matter is here of small importance, so I need not go further into details.

If a Scheme is drawn from these figures, in the way described in page 39, it will be found to have the characteristic shape of our familiar curve of Distribution. If we wished to convey the utmost information that this Scheme is capable of giving, we might record in much detail the career of two or three of the men who are clustered about each of a few selected Grades, selecting those that are used in Table II., or fewer of them. I adopted this method when estimating the variability of the Visualising Power (*Inquiries into Human Faculty*). My data were very lax, but this method of treatment got all the good out of them that they possessed. In the present case, it appears that towards the summit of the successes achieved within fifteen years of taking their degrees, stand the three Professors of Anatomy at Oxford, Cambridge, and Edinburgh; that towards the bottom of the failures, lie two men who committed suicide under circumstances of great disgrace, and lowest of all Palmer, the Rugeley murderer, who was hung.

We are able to compare any two such Schemes as the above, with numerical precision. The want of exactness in the data from which they are drawn, will of course cling to the result, but no new error will be introduced by the process of comparison. Suppose the second Scheme to refer to the successes of students from another hospital, we should draw the two Schemes in opposed directions, just as was done in the Strength of Pull of Males and Females, Fig. 5, and determine the Grade in either of the Schemes at which success was equal.

Schemes of Frequency.—The method of arranging observations in an orderly manner that is generally employed by statisticians, is shown in Fig. 3, page 38, which expresses the same facts as Fig. 2 under a different aspect, and so gives rise to the well-known Curve of "Frequency of Error," though in Fig. 3 the curve is turned at right angles to the position in which it is usually drawn. It is so placed in order to show more clearly its relation to the Curve of Distribution. The Curve of Frequency is far less convenient than that of Distribution, for the purposes just described and for most of those to be hereafter spoken of. But the Curve of Frequency has other uses, of which advantage will be taken later on, but to which it is unnecessary now to refer.

A Scheme as explained thus far, is nothing more than a compendium of a mass of observations which, on being marshalled in an orderly manner, fall into a diagram whose contour is so regular, simple, and bold, as to admit of being described by a few numerals (Table 2.), from which it can at any time be drawn afresh. The regular distribution of the several faculties among a large population is little disturbed by the fact that its members are varieties of different types and sub-types. So the distribution of a heavy mass of foliage gives little indication of its growth from separate twigs, of separate branches, of separate trees.

The application of theory to Schemes, their approximate description by only two values, and the properties of their bounding Curves, will be described in the next chapter.

CHAPTER V.

NORMAL VARIABILITY.

Schemes of Deviation.—Normal Curve of Distribution.—Comparison of the observed with the Normal Curve.—The value of a single Deviation at a known Grade determines a Normal Scheme of Deviations.—Two Measures at two known Grades determine a Normal Scheme of Measures.—The Charms of Statistics.—Mechanical illustration of the Cause of the Curve of Frequency.—Order in apparent Chaos.—Problems in the Law of Error.

Schemes of Deviations.—We have now seen how easy it is to represent the distribution of any quality among a multitude of men, either by a simple diagram or by a line containing few figures. In this chapter it will be shown that a considerably briefer description is approximately sufficient.

Every measure in a Scheme is equal to its Middlemost, or Median value, or M, a certain *plus* or *minus* Deviation from M. The Deviation, or "Error" as it is technically called, is *plus* for all grades above 50°, zero for 50°, and *minus* for all grades below 50°. Thus if (±D) be the deviation from M in any particular case, every measure in a Scheme may be expressed in the form of $M+(\pm D)$. If $M=0$, or if it is subtracted from every measure, the residues which are the different values of $(\pm D)$ will form a Scheme by themselves. Schemes may therefore be made of Deviations as well as of Measures, and one of the former is seen in the upper part of Fig. 6, page 40. It is merely the upper portion of the corresponding Scheme of Measures, in which the axis of the curve plays the part of the base.

A strong family likeness runs between the 18 different Schemes of Deviations that may be respectively derived from the data in the 18 lines of Table 2. If the slope of the curve in one Scheme is steeper than that of another, we need only to fore-shorten the steeper Scheme, by inclining it away from the line of sight, in order to reduce its apparent steepness and to make it look almost identical with the other. Or, better still, we may select appropriate vertical scales that will enable all the Schemes to be drawn afresh with a uniform slope, and be made strictly comparable.

Suppose that we have only two Schemes, A. and B., that we wish to compare. Let $L._1$, $L._2$ be the lengths of the perpendiculars at two specified grades in Scheme A., and $K._1$ $K._2$ the lengths of those at the same grades in Scheme B.; then if every one of the data from which Scheme B. was drawn be multiplied by $\frac{L._1-L._2}{K._1-K._2}$, a series of transmuted data will be obtained for drawing a new Scheme B., on such a vertical scale that its general slope between the selected grades shall be the same as in Scheme A. For practical convenience the selected Grades will be always those of 25° and 75°. They stand at the first and third quarterly divisions of the base, and are therefore easily found by a pair of compasses. They are also well placed to afford a fair criterion of the general slope of the Curve. If we call the perpendicular at 25°, $Q._1$; and that at 75°, $Q._2$, then the unit by which every Scheme will be defined is the value in it of $\frac{1}{2}(Q._2 - Q._1)$, and will be called its Q. As the M measures the Average Height of the curved boundary of a Scheme, so the Q measures its general slope. When we wish to transformn many different Schemes, numbered I., II., III., &c., whose respective values of Q are q_1, q_2, q_3, &c., to others whose values of Q are in each case equal to q_0, then the data from which Scheme I. was drawn must be multiplied by $\frac{q_0}{q_1}$; those from which Scheme II. was drawn, by $\frac{q_0}{q_2}$, and so on, and new Schemes have to be constructed from these transmuted values.

Our Q has the further merit of being practically the same as the value which mathematicians call the "Probable Error," of which we shall speak further on.

Want of space in Table 2 prevented the insertion of the measures at the Grades 25° and 75°, but those at 20° and 30° are given on the one hand, and those at 70° and 80° on the other, whose respective averages differ but little from the values at 25° and 75°. I therefore will use those four measures to obtain a value for our unit, which we will call Q′, to distinguish it from Q.

These are not identical in value, because the outline of the Scheme is a curved and not a straight line, but the difference between them is small, and is approximately the same in all Schemes. It will shortly be seen that $Q' = 1{\cdot}015 \times Q$ approximately; therefore a series of Deviations measured in terms of the large unit Q' are numerically smaller than if they had been measured in terms of the small unit (for the same reason that the numerals in 2, 3, &c., *feet* are smaller than those in the corresponding values of 24, 36, &c., *inches*), and they must be multiplied by 1.015 when it is desired to change them into a series having the smaller value of Q for their unit.

All the 18 Schemes of Deviation that can be derived from Table 2 have been treated on these principles, and the results are given in Table 3. Their general accordance with one another, and still more with the mean of all of them, is obvious.

Normal Curve of Distribution.—The values in the bottom line of Table 3, which is headed "Normal Values when $Q = 1$," and which correspond with minute precision to those in the line immediately above them, are not derived from observations at all, but from the well-known Tables of the "Probability Integral" in a way that mathematicians will easily understand by comparing the Tables 4 to 8 inclusive. I need hardly remind the reader that the Law of Error upon which these Normal Values are based, was excogitated for the use of astronomers and others who are concerned with extreme

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accuracy of measurement, and without the slightest idea until the time of Quetelet that they might be applicable to human measures. But Errors, Differences, Deviations, Divergencies, Dispersions, and individual Variations, all spring from the same kind of causes. Objects that bear the same name, or can be described by the same phrase, are thereby acknowledged to have common points of resemblance, and to rank as members of the same species, class, or whatever else we may please to call the group. On the other hand, every object has Differences peculiar to itself, by which it is distinguished from others.

This general statement is applicable to thousands of instances. The Law of Error finds a footing wherever the individual peculiarities are wholly due to the combined influence of a multitude of "accidents," in the sense in which that word has already been defined. All persons conversant with statistics are aware that this supposition brings Variability within the grasp of the laws of Chance, with the result that the relative frequency of Deviations of different amounts admits of being calculated, when those amounts are measured in terms of any self-contained unit of variability, such as our Q. The Tables 7, 8, and 9 give the results of these purely mathematical calculations, and the Curves based upon them may with propriety be distinguished as "Normal." Tables 7 and 8 are based upon the familiar Table of the Probability Integral, given in Table 5, *viâ* that in Table 6, in which the unit of variability is taken to be the "Probable Error" or our Q, and not the "Modulus." Then I turn Table 6

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inside out, as it were, deriving the "arguments" for Tables 7 and 8 from the entries in the body of Table 6, and making other easily intelligible alterations.

Comparison of the Observed with the Normal Curve.—I confess to having been amazed at the extraordinary coincidence between the two bottom lines of Table 3, considering the great variety of faculties contained in the 18 Schemes; namely, three kinds of linear measurement, besides one of weight, one of capacity, two of strength, one of vision, and one of swiftness. It is obvious that weight cannot really vary at the same rate as height, even allowing for the fact that tall men are often lanky, but the theoretical impossibility is of the less practical importance, as the variations in weight are small compared to the weight itself. Thus we see from the value of Q in the first column of Table 3, that half of the persons deviated from their M by no more than 10 or 11 lbs., which is about one-twelfth part of its value of M. Although the several series in Table 3 run fairly well together, I should not have dared to hope that their irregularities would have balanced one another so beautifully as they have done. It has been objected to some of my former work, especially in *Hereditary Genius*, that I pushed the applications of the Law of Frequency of Error somewhat too far. I may have done so, rather by incautious phrases than in reality; but I am sure that, with the evidence now before us, the applicability of that law is more than justified within the reasonable limits asked for in the present book. I am satisfied to claim that the Normal Curve is a fair average representation of the Observed Curves during nine-tenths of their course; that is, for so much of them as lies between the grades of 5° and 95°. In particular, the agreement of the Curve of Stature with the Normal Curve is very fair, and forms a mainstay of my inquiry into the laws of Natural Inheritance.

It has already been said that mathematicians laboured at the law of Error for one set of purposes, and we are entering into the fruits of their labours for another. Hence there is no ground for surprise that their Nomenclature is often cumbrous and out of place, when applied to problems in heredity. This is especially the case with regard to their term of "Probable Error," by which they mean the value that one half of the Errors exceed and the other half fall short of. This is practically the same as our Q.[1] It is strictly the same whenever the two halves of the Scheme of Deviations to which it applies are symmetrically disposed about their common axis.

The term Probable Error, in its plain English interpretation of the *most* Probable Error, is quite misleading, for it is *not* that. The most Probable Error # (as Dr. Venn has pointed out, in his *Logic of Chance*)

[1] The following little Table may be of service:—

Values of the different Constants when the Prob. Error is taken as unity, and their corresponding Grades.

Prob. Error	1·000 ;	corresponding Grades	25°·0, 75°·0
Modulus	2·097 ;	,, ,,	7°·9, 92°·1
Mean Error	1·183 ;	,, ,,	21°·2, 78°·8
Error of Mean Squares	1·483 ;	,, ,,	16°·0, 84°·0

is zero. This results from what was said a few pages back about the most probable measure in a Scheme being its M. In a Scheme of Errors the M is equal to 0, therefore the most Probable Error in such a Scheme is 0 also. It is astonishing that mathematicians, who are the most precise and perspicacious of men, have not long since revolted against this cumbrous, slip-shod, and misleading phrase. They really mean what I should call the Mid-Error, but their phrase is too firmly established for me to uproot it. I shall however always write the word Probable when used in this sense, in the form of "Prob."; thus "Prob. Error," as a continual protest against its illegitimate use, and as some slight safeguard against its misinterpretation. Moreover the term Probable Error is absurd when applied to the subjects now in hand, such as Stature, Eye-colour, Artistic Faculty, or Disease. I shall therefore usually speak of Prob. Deviation.

Though the value of our Q is the same as that of the Prob. Deviation, Q is not a convertible term with Prob. Deviation. We shall often have to speak of the one without immediate reference to the other, just as we speak of the diameter of the circle without reference to any of its properties, such as, if lines are drawn from its ends to any point in the circumference, they will meet at a right angle. The Q of a Scheme is as definite a phrase as the Diameter of a Circle, but we cannot replace Q even in that phrase by the words Prob. Deviation, and speak of the Prob. Deviation of a Scheme, without doing some violence to language. We should have to express ourselves from another point of view, and at much greater length, and say "the Prob. Deviation of any, as yet unknown measure in the Scheme, from the Mean of all the measures from which the Scheme was constructed."

The primary idea of Q has no reference to the existence of a Mean value from which Deviations take place. It is half the difference between the measures found at the 25th and 75th Centesimal Grades. In this definition there is not the slightest allusion, direct or indirect, to the measure at the 50th Grade, which is the value of M. When a Scheme is normal, the measure at Grade 25° is M—Q, and that at Grade 75° is M + Q, but all this is superimposed upon the primary conception. Q stands essentially on its own basis, and has nothing to do with M. It will often happen that we shall have to deal with Prob: Deviations, but that is no reason why we should not use Q whenever it suits our purposes better, especially as statistical statements tend to be so cumbrous that every abbreviation is welcome.

The stage to which we have now arrived is this. It has been shown that the distribution of very different human qualities and faculties is approximately Normal, and it is inferred that with reasonable precautions we may treat them as if they were wholly so, in order to obtain approximate results. We shall thus deal with an entire Scheme of Deviations in terms of its Q, and with an entire Scheme of Measures in terms of its M and Q, just as we deal with an entire Circle in terms of its

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radius, or with an entire Ellipse in terms of its major and minor axes. We can also apply the various beautiful properties of the Law of Frequency of Error to the observed values of Q. In doing so, we act like woodsmen who roughly calculate the cubic contents of the trunk of a tree, by measuring its length, and girth at either end, and submitting their measures to formulæ that have been deduced from the properties of ideally perfect straight lines and circles. Their results prove serviceable, although the trunk is only rudely straight and circular. I trust that my results will be yet closer approximations to the truth than those usually arrived at by the woodsmen.

The value of a single Deviation at a known Grade determines a Normal Scheme of Deviations.—When Normal Curves of Distribution are drawn within the same limits, they differ from each other only in their general slope; and the slope is determined, if the value of the Deviation is given at any one specified Grade. It must be borne in mind that the width of the limits between which the Scheme is drawn, has no influence on the values of the Deviations at the various Grades, because the latter are proportionate parts of the base. As the limits vary in width, so do the intervals between Grades. When measuring the Deviation at a specified Grade for the purpose of determining the whole Curve, it is of course convenient to adhere to the same Grade in all cases. It will be recollected that when dealing with the observed curves a few pages back, I used not one Grade but two Grades for the purpose, namely 25° and 75°; but in the Normal Curve, the *plus* and *minus* Deviations are equal in amount at all pairs of symmetrical distances on either side of grade 50°; therefore the Deviation at either of the Grades 25° or 75° is equal to Q, and suffices to define the entire Curve, ~~as completely as a radius defines an entire circle~~. The reason why a certain value Q′ was stated a few pages back to be equal to 1·015 Q, is that the Normal Deviations at 20° and at 30°, (whose average we called Q′) are found in Table 8, to be 1·25 and 0·78; and similarly those at 70° and 60°. The average of 1·25 and 0·78 is 1·015, whereas the Deviation at 25° or at 75° is 1·000.

Two Measures at known Grades determine a Normal Scheme of Measures.—If we know the value of M as well as that of Q we know the entire Scheme. M expresses the mean value of all the objects contained in the group, and Q defines their variability. But if we know the Measures at any two specified Grades, we can deduce M and Q from them, and so determine the entire Scheme. The method of doing this is explained in the foot-note.[1]

[1] The following is a fuller description of these ~~two leading~~ propositions ~~in the~~ this and in the preceeding paragraph:—

(1) In any Normal Scheme, and therefore approximately in an observed one, if the value of the Deviation is given at any *one* specified Grade the whole Curve is determined. Let D be the given Deviation, and d the tabular Deviation at the same Grade, as found in Table 8; then multiply every entry in Table 8 by $\frac{D}{d}$. As the tabular value of Q is 1, it will be changed by the multiplication into $\frac{D}{d}$.

f. 38v

The Charms of Statistics.—It is difficult to understand why statisticians commonly limit their inquiries to Averages, and do not revel in more comprehensive views. Their souls seem as dull to the charm of variety as that of the native of one of our flat English counties, whose retrospect of Switzerland was that, if its mountains could be thrown into its lakes, two nuisances would be got rid of at once. An Average is but a solitary fact, whereas if a single other fact be added to it, an entire Normal Scheme, which nearly corresponds to the observed one, starts potentially into existence.

Some people hate the very name of statistics, but I find them full of beauty and interest. Whenever they are not brutalised, but delicately handled by the higher methods, and are warily interpreted, their power of dealing with complicated phenomena is extraordinary. They are the only tools by which an opening can be cut

(2) If the Measures at any *two* specified Grades are given, the whole Scheme of Measures is thereby determined. Let A, B be the two given Measures of which A is the larger, and let a, b be the values of the tabular Deviations for the same Grades, as found in Table 8, not omitting their signs of *plus* or *minus* as the case may be.

Then the Q of the Scheme $= \pm \frac{A-B}{a-b}$. (The sign of Q is not to be regarded; it is merely a magnitude.)

$$M = A - a\,Q \text{; or } M = B - b\,Q.$$

Example: A, situated at Grade 55°, = 14·38
B, situated at Grade 5°, = 9·12

The corresponding tabular Deviations are :—a = +0·19 ; b = −2·44.

$$\text{Therefore } Q = \frac{14{\cdot}38 - 9{\cdot}12}{0{\cdot}19 + 2{\cdot}44} = \frac{5{\cdot}26}{2{\cdot}63} = 2{\cdot}0$$

$$M = 14.38 - 0{\cdot}19 \times 2 = 14{\cdot}0$$

$$\text{or } = 9{\cdot}12 + 2{\cdot}44 \times 2 = 14{\cdot}0$$

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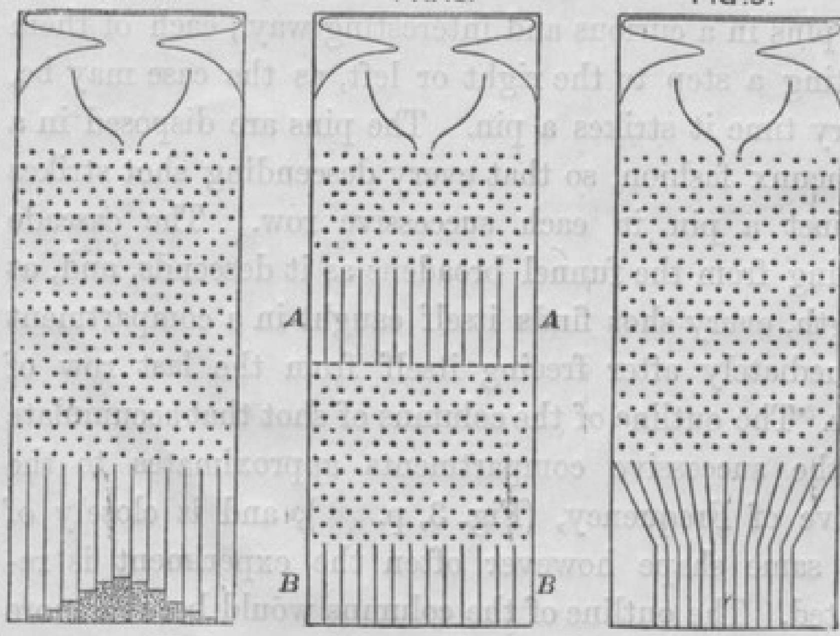
through the formidable thicket of difficulties that bars the path of those who pursue the Science of man.

Mechanical Illustration of the Cause of the Curve of Frequency.—The Curve of Frequency, and that of Distribution, are convertible : therefore if the genesis of either of them can be made clear, that of the other becomes also intelligible. I shall now illustrate the origin of the Curve of Frequency, by means of an apparatus shown in Fig. 7. that mimics in a very pretty way the conditions

FIG. 7. FIG. 8. FIG. 9.



on which Deviation depends. It is a frame glazed in front, leaving a depth of about a quarter of an inch behind the glass. Strips are placed in the upper part to act as a funnel. Below the outlet of the funnel stand a succession of rows of pins stuck squarely into the back-board, and below these again are a series of vertical compartments. A charge of small shot is enclosed. When the frame is held topsy-turvy, all the shot runs to the upper end; then, when it is turned back into its working position, the desired action commences. Lateral strips, shown in the diagram, have the effect of directing all the shot that had collected at the upper end of the frame to run into the wide mouth of the funnel. The shot passes through the funnel and issuing from its narrow end, scampers deviously down through the pins in a curious and interesting way; each of them darting a step to the right or left, as the case may be, every time it strikes a pin. The pins are disposed in a quincunx fashion, so that every descending shot strikes against a pin in each successive row. The cascade issuing from the funnel broadens as it descends, and, at length, every shot finds itself caught in a compartment immediately after freeing itself from the last row of pins. The outline of the columns of shot that accumulate in the successive compartments approximates to the Curve of Frequency, (Fig. 3, p. 38), and is closely of the same shape however often the experiment is repeated. The outline of the columns would become more nearly identical with the Normal Curve of Frequency, if the rows of pins were much more numerous, the shot smaller, and the compartments narrower; also if a larger quantity of shot was used.

The principle on which the action of the apparatus depends is, that a number of small and independent accidents befall each shot in its career. In rare cases, a long run of luck continues to favour the course of a particular shot towards either outside place, but in the large majority of instances the number of accidents that cause Deviation to the right, balance in a greater or less degree those that cause Deviation to the left. Therefore most of the shot finds its way into the compartments that are situated near to a perpendicular line drawn from the outlet of the funnel, and the Frequency with which shots stray to different distances to the right or left of that line diminishes in a much faster ratio than those distances increase. This illustrates and explains the reason why mediocrity is so common.

If a larger quantity of shot is put inside the apparatus, the resulting curve will be more humped, but one half of the shot will still fall within the same distance as before, reckoning to the right and left of the perpendicular line that passes through the mouth of the funnel. This distance, which does not vary with the quantity of the shot, is the "Prob: Error," or "Prob: Deviation," of any single shot, and has the same value as our Q. But a Scheme of Frequency is unsuitable for finding the values of either M or Q. To do so, we must divide its strangely shaped *area* into four equal parts by vertical lines, which is hardly to be effected except by a tedious process of "Trial and Error." On the other hand M and Q can be derived from Schemes of Distribution with no more trouble than is needed to divide a *line* into four equal parts.

Order in Apparent Chaos.—I know of scarcely anything so apt to impress the imagination as the wonderful form of cosmic order expressed by the "Law of Frequency of Error." The law would have been personified by the Greeks and deified, if they had known of it. It reigns with serenity and in complete self-effacement amidst the wildest confusion. The huger the mob, and the greater the apparent anarchy, the more perfect is its sway. It is the supreme law of Unreason. Whenever a large sample of chaotic elements are taken in hand and marshalled in the order of their magnitude, an unsuspected and most beautiful form of regularity proves to have been latent all along. The tops of the marshalled row form a flowing curve of invariable proportions; and each element, as it is sorted into place, finds, as it were, a pre-ordained niche, accurately adapted to fit it. If the measurement at any two specified Grades in the row are known, those that will be found at every other Grade, except towards the extreme ends, can be predicted in the way already explained, and with much precision.

Problems in the Law of Error.—All the properties of the Law of Frequency of Error can be expressed in terms of Q, or of the Prob: Error, just as those of a circle can be expressed in terms of its radius. The visible Schemes are not, however, to be removed too soon from our imagination. It is always well to retain a clear geometric view of the facts when we are dealing with statistical problems, which abound with dangerous pitfalls, easily overlooked by the unwary, while they are cantering gaily along upon their arithmetic. The Laws of Error are beautiful in themselves and exceedingly fascinating to inquirers, owing to the thoroughness and simplicity with which they deal with masses of materials that appear at first sight to be entanglements on the largest scale, and of a hopelessly confused description. I will mention five of the laws.

(1) The following is a mechanical illustration of the first of them. In the apparatus already described, let q stand for the Prob: Error of any one of the shots that are dispersed among the compartments BB at its base. Now cut the apparatus in two parts, horizontally through the rows of pins. Separate the parts and interpose a row of vertical compartments AA, as in Fig. 8, page 65, where the bottom compartments, BB, corresponding to those shown in Fig. 7, are reduced to half their depth, in order to bring the whole figure within the same sized outline as before. The compartments BB are still deep enough for their purpose. It is clear that the interpolation of the AA compartments can have no ultimate effect on the final dispersion of the shot into those at BB. Now close the bottoms of all the AA compartments; then the shot that falls from the funnel will be retained in them, and will be comparatively little dispersed. Let the Prob: Error of a shot in the AA compartments be called a. Next, open the bottom of any one of the AA compartments; then the shot it contains will cascade downwards and disperse themselves among the BB compartments on either side of the perpendicu-

lar line drawn from its starting point, and each shot will have a Prob: Error that we will call b. Do this for all the AA compartments in turn; b will be the same for all of them, and the final result must be to reproduce the identically same system in the BB compartments that was shown in Fig. 7, and in which each shot had a Prob: Error of q.

The dispersion of the shot at BB may therefore be looked upon as compounded of two superimposed and independent systems of dispersion. In the one, when acting singly, each shot has a Prob: Error of a; in the other, when acting singly, each shot has a Prob: Error of b, and the result of the two acting together is that each shot has a Prob: Error of q. What is the relation between a, b, and q? Calculation shows that $q^2 = a^2 + b^2$. In other words, q corresponds to the hypothenuse of a right-angled triangle of which the other two sides are a and b respectively.

(2) It is a corollary of the foregoing that a system Z, in which each element is the Sum of a couple of independent Errors, of which one has been taken at random from a Normal system A and the other from a Normal system B, will itself be Normal.[1] Calling the Q of the Z system q, and the Q of the A and B systems respectively, a and b, then $q^2 = a^2 + b^2$.

[1] We may see the rationale of this corollary if we invert part of the statement of the problem. Instead of saying that an A element deviates from its M, and that a B element also deviates independently from its M, we may phrase it thus: An A element deviates from its M, and its M deviates from the B element. Therefore the deviation of the B element from the A element is compounded of two independent deviations, as in Problem 1.

(3) Suppose that a row of compartments, whose upper openings are situated like those in Fig. 7, page 63, are made first to converge towards some given point below, but that before reaching it their sloping course is checked and they are thenceforward allowed to drop vertically as in Fig. 9. The effect of this will be to compress the heap of shot laterally; its outline will still be a Curve of Frequency, but its Prob: Error will be diminished.

The foregoing three properties of the Law of Error are well known to mathematicians and require no demonstration here, but two other properties that are not familiar will be of use also; proofs of them by Mr. J. Hamilton Dickson are given in Appendix B. They are as follows. I purposely select a different illustration to that used in the Appendix, for the sake of presenting the same general problem under more than one of its applications.

(4) Bullets are fired by a man who aims at the centre of a target, which we will call its M, and we will suppose the marks that the bullets make to be painted red, for the sake of distinction. The system of lateral deviations of these red marks from the centre M will be approximately Normal, whose Q we will call c. Then another man takes aim, not at the centre of the target, but at one or other of the red marks, selecting these at random. We will suppose his shots to be painted green. The lateral distance of any green shot from the red mark at which it was aimed will have a Prob: Error that we

will call b. Now, if the lateral distance of a particular green mark from M is given, what is the *most probable* distance from M of the red mark at which it was aimed? It is $\sqrt{\frac{c^2}{c^2+b^2}}$.

(5) What is the Prob: Error of this determination? In other words, if estimates have been made for a great many distances founded upon the formula in (4), they would be correct on the average, though erroneous in particular cases. The errors thus made would form a normal system whose Q it is desired to determine. Its value is $\frac{bc}{\sqrt{(b^2+c^2)}}$.

By the help of these five problems the statistics of heredity become perfectly manageable. It will be found that they enable us to deal with Fraternities, Populations, or other Groups, just as if they were units. The largeness of the number of individuals in any of our groups is so far from scaring us, that they are actually welcomed as making the calculations more sure and none the less simple.

CHAPTER VI.

DATA.

Records of Family Faculties, or R. F. F. data.—Special Data.—Measures at Anthropometric Laboratory.—Experiments on Sweet Peas.

I had to collect all my data for myself, as nothing existed, so far as I know, that was suitable even to my primary purpose. This was to obtain records of at least two successive generations of some population of considerable size. They must have lived under conditions that were of a usual kind, and in which no great varieties of nature were to be found. Natural selection must have had little influence on the characteristics that were to be examined. These must be variable, measurable, and fairly constant in the same individual. The result of numerous inquiries, made of the most competent persons, was that I began my experiments many years ago on the seeds of sweet peas, and that at the present time I am breeding moths, as will be explained in a later chapter, but this book refers to a human population, which, take it all in all, is the easiest to work with when the data are once obtained, to say nothing of its being more interesting by far than one of sweet peas or of moths.

Record of Family Faculties, or R.F.F. data.—The source from which the larger part of my data is derived consists of a large collection of "Records of Family Faculties," obtained through the offer of prizes. They have been much tested and cross-tested, and have borne the ordeal very fairly, so far as it has been applied. It is well to reprint the terms of the published offer, in order to give a just idea of the conditions under which they were compiled. It was as follows:

"Mr. Francis Galton offers 500*l.* in prizes to those British Subjects resident in the United Kingdom who shall furnish him before May 15, 1884, with the best Extracts from their own Family Records.

"These Extracts will be treated as confidential documents, to be used for statistical purposes only, the insertion of names of persons and places being required solely as a guarantee of authenticity and to enable Mr. Galton to communicate with the writers in cases where further question may be necessary.

"The value of the Extracts will be estimated by the degree in which they seem likely to facilitate the scientific investigations described in the preface to the "Record of Family Faculties."

"More especially:

"(*a*) By including every direct ancestor who stands within the limits of kinship there specified.

"(*b*) By including brief notices of the brothers and sisters (if any) of each of those ancestors. (Importance will be attached both to the completeness with which each family of brothers and sisters is described, and also to the number of persons so described.)

"(*c*) By the character of the evidence upon which the information is based.

"(*d*) By the clearness and conciseness with which the statements and remarks are made.

"The Extracts must be legibly entered either in the tabular forms contained in the copy of the "Record of Family Faculties" (into which, if more space is wanted, additional pages may be stitched), or they may be written in any other book with pages of the same size as those of the Record, provided that the information be arranged in the same tabular form and order. (It will be obvious that uniformity in the arrangement of documents is of primary importance to those who examine and collate a large number of them).

"Each competitor must furnish the name and address of a referee of good social standing (magistrate, clergyman, lawyer, medical practitioner, &c.), who is personally acquainted with his family, and of whom inquiry may be made, if desired, as to the general trustworthiness of the competitor.

"The Extracts must be sent prepaid and by post, addressed to Francis Galton, 42 Rutland Gate, London, S.W. It will be convenient if the letters 'R.F.F.' (Record of Family Faculties) be written in the left-hand corner of the parcel, below the address.

"The examination will be conducted by the donor of the prizes, aided by competent examiners.

"The value of the individual prizes cannot be fixed beforehand. No prize will, however, exceed 50*l.*, nor be less than 5*l.*, and 500*l.* will on the whole be awarded.

"A list of the gainers of the prizes will be posted to each of them. It will be published in one or more of the daily newspapers, also in at least one clerical, and one medical Journal."

The number of Family Records sent in reply to this offer, that deserved to be seriously considered before adjudging the prizes, barely reached 150; 70 of these being contributed by males, 80 by females. The remainder were imperfect, or they were marked "not for competition," but at least 10 of these have been to some degree utilised. The 150 Records were contributed by persons of very various ranks. After classing the female writers according to the profession of their husbands, if they were married, or according to that of their fathers, if they were unmarried, I found that each of the following 7 classes had 20 or somewhat fewer representatives: (1) Titled persons and landed gentry; (2) Army and Navy; (3) Church (various denominations); (4) Law; (5) Medicine; (6) Commerce, higher class; (7) Commerce, lower class. This accounts for nearly 130 of the writers of the Records; the remainder are land agents, farmers, artisans, literary men, schoolmasters, clerks, students, and one domestic servant in a family of position.

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Three cases occurred in which the Records sent by different contributors overlapped. The details are complicated, and need not be described here, but the result is that five persons have been adjudged smaller prizes than they individually deserved.

Every one of the replies refers to a very large number of persons, as will easily be understood if the fact is borne in mind that each individual has 2 parents, 4 grandparents, and 8 great parents; also that he and each of those 14 progenitors had usually brothers and sisters, who were included in the inquiry. The replies were unequal in merit, as might have been expected, but many were of so high an order that I could not justly select a few as recipients of large prizes to the exclusion of the rest. Therefore I divided the sum into two considerable groups of small prizes, all of which were well deserved, regretting much that I had none left to award to a few others of nearly equal merit to some of those who had been successful. The list of winners is reproduced below, the four years that have elapsed have of course made not a few changes in the addresses, which are not noticed here.

LIST OF AWARDS.

A PRIZE OF £7 WAS AWARDED TO EACH OF THE 40 FOLLOWING CONTRIBUTORS.

Amphlett, John, Clent, Stourbridge; Batchelor, Mrs. Jacobstow Rectory, Stratton, N. Devon; Bathurst, Miss K., Vicarage, Biggleswade, Bedfordshire; Beane, Mrs. C. [F., 3 Portland Place, Venner Road, Sydenham; Berisford, Samuel, Park Villas, Park Lane, Macclesfield; Carruthers, Mrs., Brightside, North Finchley; Carter, Miss Jessie E., Hazelwood, The Park, Cheltenham; Cay, Mrs., Eden House, Holyhead; Clark, J. Edmund,

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Feversham Terrace, York; Cust, Lady Elizabeth, 13 Eccleston Square, S.W.; Fry, Edward, Portsmouth, 5 The Grove, Highgate, N.; Gibson, G. A., M.D., 1 Randolph Cliff, Edinburgh; Gidley, B. Courtenay, 17 Ribblesdale Road, Hornsey, N.; Gillespie, Franklin, M.D., 1 The Grove, Aldershot; Griffith-Boscawen, Mrs., Trevalyn Hall, Wrexham; Hardcastle, Henry, 38 Eaton Square, S.W.; Harrison, Miss Edith, 68 Gloucester Place, Portman Square, W.; Hobhouse, Mrs. 4 Kensington Square, W.; Holland, Miss, Ivymeath, Snodland, Kent; Hollis, George, Dartmouth House, Dartmouth Park Hill, N.; Ingram, Mrs. Ades, Chailey, Lewis, Sussex; Johnstone, Miss C. L., 3 Clarendon Place, Leamington; Lane-Poole, Stanley, 6 Park Villas East, Richmond, Middlesex; Leathley, D. W. B., 59 Lincoln's Inn Fields (in trust for a competitor who desires her name not to be published); Lewin, Lieutenant-Colonel T. H., Colway Lodge, Lyme Regis; Lipscomb, R. H., East Budleigh, Budleigh Salterton, Devon; Malden, Henry C., Windlesham House, Brighton; Malden, Henry Elliot, Kitland, Holmwood, Surrey; McCall, Hardy Bertram, 5 St. Augustine's Road, Edgbaston, Birmingham; Moore, Miss Georgina M., 45 Chepstow Place, Bayswater, W.; Newlands, Mrs., Raeden, near Aberdeen; Pearson, David R., M.D., 23 Upper Phillimore Place, Kensington, W.; Pearson, Mrs., The Garth, Woodside Park, North Finchley: Pechell, Hervey Charles, 6 West Chapel Street, Curzon Street, W.; Roberts, Samuel, 21 Roland Gardens, S.W.; Smith, Mrs. Archibald, Riverbank, Putney, S.W.; Strachey, Mrs. Fowey Lodge, Clapham Common, S.W.; Sturge, Miss Mary C., Chilliswood, Tyndall's Park, Bristol; Sturge, Mrs. R. F., 101 Pembroke Road, Clifton; Wilson, Edward T., M.D., Westall, Cheltenham.

A PRIZE OF £5 WAS AWARDED TO EACH OF THE 44 FOLLOWING CONTRIBUTORS.

Allan, Francis J., M.D., 1 Dock Street, E.; Atkinson, Mrs., Clare College Lodge, Cambridge; Bevan, Mrs., Plumpton House, Bury St. Edmunds; Browne, Miss, Maidenwell House, Louth, Lincolnshire; Cash, Frederick Goodall, Gloucester; Chisholm, Mrs., Church Lane House, Haslemere, Surrey; Collier, Mrs. R., 7 Thames Embankment, Chelsea; Croft, Sir Herbert G. D., Lugwardine Court, Hereford; Davis, Mrs. (care of Israel Davis, 6 King's Bench Walk, Temple, E.C.); Drake, Henry H., The Firs, Lee, Kent; Ercke, J. J. G., 13, Brownhill Road, Catford, S.E.; Flint, Fenner Ludd, 83 Brecknock Road, N.; Ford, William, 4 South Square, Gray's Inn, W.C.; Foster, Rev. A. J., The Vicarage, Wootton, Bedford; Glanville-Richards, W. V. S., 23 Endsleigh Place, Plymouth; Hale, C. D. Bowditch, 8 Sussex Gardens, Hyde Park, W.; Horder, Mrs. Mark, Rothenwood, Ellen Grove, Salisbury; Jackson, Edwin, 79 Withington Road, Whalley Range, Manchester; Jackson, George, 1 St. George's Terrace, Plymouth; Kesteven, W. H., 401 Holloway Road, N.; Lawrence, Mrs.

Alfred, 16 Suffolk Square, Cheltenham ; Lawrie, Mrs., 1 Chesham Place, S.W. ; Leveson-Gower, G. W. G., Titsey Place, Limpsfield, Surrey ; Lobb, H. W., 66 Russell Square, W. ; McConnell, Miss M. A. Brooklands, Prestwich, Manchester ; Marshall, Mrs., Fenton Hall, Stoke-upon-Trent ; Meyer, Mrs., 1 Rodney Place, Clifton, Bristol; Milman, Mrs., The Governor's House, H.M. Prison, Camden Road ; Olding, Mrs. W. 4 Brunswick Road, Brighton, Sussex ; Passingham, Mrs., Milton, Cambridge ; Pringle, Mrs. Fairnalie, Fox Grove Road, Beckenham, Kent ; Reeve, Miss, Foxholes, Christchurch, Hants ; Scarlett, Mrs., Boscomb Manor, Bournemouth ; Shand, William, 57 Caledonian Road, N. ; Shaw, Cecil E., Wellington Park, Belfast ; Sizer, Miss Kate T., Moorlands, Great Huntley, Colchester ; Smith, Miss A. M. Carter, Thistleworth, Stevenage ; Smith, Rev. Edward S., Viney Hall Vicarage, Blakeney, Gloucestershire ; Smith, Mrs. F. P., Cliffe House, Sheffield ; Staveley, Edw. S. R., Mill Hill School, N.W. ; Sturge, Miss Mary W., 17 Frederick Road, Edgbaston, Birmingham ; Terry, Mrs., Tostock, Bury St. Edmunds, Suffolk ; Utley, W. H. Alliance Hotel, Cathedral Gates, Manchester ; Weston, Mrs. Ensleigh, Lansdown, Bath ; Wodehouse, Mrs. E. R. 56 Chester Square, S.W.

The material in these Records is sufficiently varied to be of service in many inquiries. The chief subjects to which allusion will be made in this book concern Stature, Eye colour, Temper, the Artistic Faculty, and some forms of Disease, but others are utilized that refer ~~also~~ to Marriage Selection and Fertility, &c.

The following remarks in this chapter refer almost wholly to the data of Stature.

The data derived from the Records of Family Faculties will be hereafter distinguished by the letters R.F.F. I was able to extract from them the statures of 205 couples of parents, with those of an aggregate of 930 of their adult children of both sexes. I must repeat that when dealing with the female statures, I transmuted them to their male equivalents; and treated them when thus transmuted on equal terms with the measures of males, except where otherwise expressed. The factor I used was 1·08, which is equivalent to adding a little less than one-twelfth to each female height. It differs slightly from the factors employed by other anthropologists, who, moreover, differ a trifle between themselves; anyhow, it suits my data better than 1·07 or 1·09. I can say confidently that the final result is not of a kind to be sensibly affected by these minute details, because it happened that owing to a mistaken direction, the computer to whom I first entrusted the figures used a somewhat different factor, yet the final results came out closely the same. These R.F.F. data have by no means the precision of the observations to be spoken of in the next paragraph. In many cases there remains considerable doubt whether the measurement refers to the height with the shoes on or off; not a few of the 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, and as there is no sign or suspicion of bias, I have reason to place confidence in the values of the Means that are derived from them. They bear the internal tests that have been applied better than might have been expected, and when checked by the data described in the next paragraph, and cautiously treated, they are very valuable.

Special Data.—A second set of data, distinguished by the name of "Special observations," concern the variations in stature among Brothers. I circulated cards of inquiry among trusted correspondents, stating that I wanted records of the heights of brothers who were more than 24 and less than 60 years of age; that it was not necessary to send the statures of all of the brothers of the same family, but only of as many of them as could be easily and accurately measured, and that the height of even two brothers would be acceptable. The blank forms sent to be filled, were 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 inquirers widened, but I was satisfied when I had obtained returns of 295 families, containing in the aggregate 783 brothers, some few of whom also appear in the R.F.F. data. Though these two sets of returns overlap to a trifling extent, they are practically independent. I look upon the "Special Observations" as being quite as trustworthy as could be expected in any such returns. They bear every internal test that I can apply to them in a very satisfactory manner. The measures are commonly recorded to quarter or half inches.

Measures at my Anthropometric Laboratory.—A third set of data have been incidentally of service. They are the large lists of measures, nearly 10,000 in number, made at my Anthropometric Laboratory in the International Health Exhibition of 1884.

4. *Experiments on Sweet Peas.*—The last of the data

that I need specify were the very first that I used; they refer to the sizes of seeds, which are equivalent to the Statures of seeds. I both measured and weighed them, but after assuring myself of the equivalence of the two methods (see Appendix C.), confined myself to ascertaining the weights, as they were much more easily ascertained than the measures. It is more than 10 years since I procured these data. They were the result of an extensive series of experiments on the produce of seeds of different sizes, but of the same species, conducted for the following reasons. I had endeavoured to find a population possessed of some measurable characteristic that was suitable for investigating the causes of the statistical similarity between successive generations of a people, as will hereafter be discussed in Chapter VIII. At last I determined to experiment on seeds, and after much inquiry of very competent advisers, selected sweet-peas for the purpose. They do not cross-fertilize, which is a very exceptional condition among plants; they are hardy, prolific, of a convenient size to handle, and nearly spherical; their weight does not alter perceptibly when the air changes from damp to dry, and the little pea at the end of the pod, so characteristic of ordinary peas, is absent in sweet-peas. I began by weighing thousands of them individually, and treating them as a census officer would treat a large population. Then I selected with great pains several sets for planting. Each set contained seven little packets, numbered K, L, M, N, O, P, and Q, each of the seven packets contained ten seeds of almost exactly the same weight; those in K being the heaviest, L the next heaviest, and so down to Q, which was the lightest. The precise weights are given in Appendix C, together with the corresponding diameters, which I ascertained by laying 100 peas of the same weight in a row. The weights run in an arithmetic series, having a common average difference of 0·172 grain. I do not of course profess to work to thousandths of a grain, though I did work to somewhat less than one hundredth of a grain; therefore the third decimal place represents little more than an arithmetical working value which has to be regarded in multiplications, lest an error of sensible importance should be introduced by its neglect. Curiously enough, the diameters were found also to run approximately in an arithmetic series, owing, I suppose, to the misshape and corrugations of the smaller seeds, which gave them a larger diameter than if they had been plumped out into spheres. All this is shown in the Appendix, where it will be seen that I was justified in sorting the seeds by the convenient method of the balance and weights, and of accepting the weights as directly proportional to the mean diameters.

In each experiment, seven beds were prepared in parallel rows; each was $1\frac{1}{2}$ feet wide and 5 feet long. Ten holes of 1 inch deep were dibbled at equal distances apart along each bed, and a single seed was put into each hole. The beds were then bushed over to keep off the birds. Minute instructions were given to ensure uniformity, which I need not repeat here. The end of all was that the seeds as they became ripe were collected from time to time and put into bags that I had sent, lettered from K to Q, the same letters having been stuck at the ends of the beds. When the crop was coming to an end, the whole remaining produce of each bed, including the foliage, was torn up, tied together, labelled, and sent to me. Many friends and acquaintances had each undertaken the planting and culture of a complete set, so that I had simultaneous experiments going on in various parts of the United Kingdom from Nairn in the North to Cornwall in the South. Two proved failures, but the final result was that I obtained the more or less complete produce of seven sets; that is to say, the produce of $7 \times 7 \times 10$, or of 490 carefully weighed parent seeds. Some ~~further~~ additional account of the results is given in Appendix C.

It would be wholly out of place to enter here into further details of the experiments, or to narrate the numerous little difficulties and imperfections I had to contend with, and how I balanced doubtful cases; how I divided returns into groups to see if they confirmed one another, or how I conducted any other well-known statistical operation. Suffice it to say that I took immense pains, which, if I had understood the general conditions of the problem as clearly as I do now, I should not perhaps have cared to bestow. The results were most satisfactory. They gave me two data, which were all that I wanted in order to understand in its simplest approximate form, the way in which one generation of a people is descended from a previous one; and thus I got at the heart of the problem at once.

CHAPTER VII.

DISCUSSION OF THE DATA OF STATURE.

Stature as a subject for inquiry.—Marriage Selection.—Issue of unlike Parents.—Description of the Tables of Stature. Mid-Stature of the Population.—Variability of the Population.—Variability of Mid-Parents.—Variability in Co-Fraternities.—Regression: *a*, Filial; *b*, Mid-Parental; *c*, Parental; *d*, Fraternal.—Squadrons of Statures.—Successive Generations of a People.—Natural Selection.—Variability in Fraternities.—Trustworthiness of the Constants.—General view of Kinship. Separate Contribution from each Ancestor.—Pedigree Moths.

Stature as a Subject for Inquiry.—The first of these inquiries into the laws of human heredity deals with hereditary Stature, which is an excellent subject for statistics. Some of its merits are obvious enough, such as the ease and frequency with which it may be measured, its practical constancy during thirty-five or forty years of middle life, its comparatively small dependence upon differences of bringing up, and its inconsiderable influence on the rate of mortality. Other advantages which are not equally obvious are equally great. One of these is due to the fact that human stature is not a simple element, but a sum of the accumulated lengths or thicknesses of more than a hundred bodily parts, each so distinct from the rest as to have earned a name by which it can be specified. The list includes about fifty separate bones, situated in the skull, the spine, the pelvis, the two legs, and in the two ankles and feet. The bones in both the lower limbs have to be counted, because the Stature depends upon their average length. The two cartilages interposed between adjacent bones, wherever there is a movable joint, and the single cartilage in other cases, are rather more numerous than the bones themselves. The fleshy parts of the scalp of the head and of the soles of the feet conclude the list. Account should also be taken of the shape and set of the many bones which conduce to a more or less arched instep, straight back, or high head. I noticed in the skeleton of O'Brien, the Irish giant, at the College of Surgeons, which is the tallest skeleton in any English museum, that his great stature of about 7 feet 7 inches would have been a trifle increased if the faces of his dorsal vertebræ had been more parallel than they are, and his back consequently straighter.

This multiplicity of elements, whose variations are to some degree independent of one another, some tending to lengthen the total stature, others to shorten it, corresponds to an equal number of sets of rows of pins in the apparatus Fig. 7, p.63, by which the cause of variability was illustrated. The larger the number of these variable elements, the more nearly does the variability of their sum assume a "Normal" character, though the ~~closeness of~~ approximation ~~varies~~ increases only as the square root of their number. The beautiful regularity in the Statures of a population, whenever they are statistically marshalled in the order of their heights, is due to the number of variable and quasi-independent elements of which Stature is the sum.

Marriage Selection.—Whatever may be the sexual preferences for similarity or for contrast, I find little indication in the average results obtained from a fairly large number of cases, of any single measurable personal peculiarity, whether it be stature, eye-colour, temper, or artistic tastes, in influencing marriage selection to a notable degree. Nor is this extraordinary, for though people may fall in love for trifles, marriage is a serious act, usually determined by the concurrence of numerous motives. Therefore we could hardly expect either shortness or tallness, darkness or lightness in complexion, or any other single quality, to have in the long run a large separate influence.

I was certainly surprised to find how imperceptible was the influence that even good and bad Temper seemed to exert on marriage selection. A list was made (see Appendix D) of the observed frequency of marriages between persons of each of the various classes of Temper, in a group of 111 couples, and I calculated what would have been the relative frequency of intermarriages between persons of the various classes, if the same number of males and females had been paired at random. The result showed that the observed list agreed closely with the calculated list, and therefore that these observations gave no evidence of discriminative selection in respect to Temper. The good-tempered husbands were 46 per cent. in number, and, between them, they married 22 good-tempered and 24 bad-tempered wives; whereas calculation, having regard to the relative proportions of good and bad Temper in the two sexes, gave the numbers as 25 and 21. Again, the bad-tempered husbands, who were 54 per cent. in number, married 31 good-tempered and 23 bad-tempered wives, whereas calculation gave the number as 30 and 24. This rough summary is a just expression of the results arrived at by a more minute analysis, which is described in the Appendix, and need not be repeated here.

Similarly as regards Eye-Colour. If we analyse the marriages between the 78 couples whose eye-colours are described in Chapter VIII., and compare the observed results with those calculated on the supposition that Eye-Colour has no influence whatever in marriage selection, the two lists will be found to be much alike. Thus where both of the parents have eyes of the same colour, whether they be light, or hazel, or dark, the percentage results are almost identical, being 37, 3, and 8 as observed, against 37, 2, and 7 calculated. Where one parent is hazel-eyed and the other dark-eyed, the marriages are as 5 observed against 7 calculated. But the results run much less well together in the other two possible combinations, for where one parent is light and the other hazel-eyed, they give 23 observed against 15 calculated; and where one parent is light and the other dark-eyed, they give 24 observed against 32 calculated.

The effect of Artistic Taste on marriage selection is discussed in Chapter X., and this also is shown to be small. The influence on the race of Bias in Marriage Selection will be discussed in that chapter.

I have taken much trouble at different times to determine whether Stature plays any sensible part in marriage selection. I am not yet prepared to offer complete results, but shall confine my remarks for the present to the particular cases with which we are now concerned. The shrewdest test is to proceed under the guidance of Problem 2 in Chapter V. I find the Q of Stature among the male population to be 1·7 inch, and similarly for the transmuted statures of the female population. Consequently if the men and (transmuted) women married at random so far as stature was concerned, the Q in a group of couples, each couple consisting of a pair of *summed* statures, would be $\sqrt{2} \times 1{\cdot}7$ inches $= 2{\cdot}41$ inches. Therefore the Q in a group of which each element was the *mean* stature of a couple, would be half that amount, or 1·20 inch. This closely corresponds to what I derived from the data contained in the first and in the last column but one of Table 11. The word "Mid-Parent" used in their headings, expresses an ideal person of composite sex, whose Stature is half way between the Stature of the father and the transmuted Stature of the mother. I therefore conclude that marriage selection does not pay such regard to Stature, as deserves being taken into account in the cases with which we are concerned.

I tried the question in another but ruder way, by dividing (see Table 9) the male and female parents respectively into three nearly equal groups, of tall, medium, and short. It was impracticable to make them precisely equal, on account of the roughness with which the measurements were recorded, so I framed rules that seemed best adapted to the case. Consequently the numbers of the tall and short proved to be only approximately and not exactly equal, and the two together were only approximately equal to the medium cases. The final results were :—32 instances where one parent was short and the other tall, and 27 where both were short or both were tall. In other words, there were 32 cases of contrast in marriage, to 27 cases of likeness. I do not regard this difference as of consequence, because the numbers are small, and because a slight change in the limiting values assigned to shortness and tallness, would have a sensible effect upon the result. I am therefore content to ignore it, and to regard the Statures of married folk just as if their choice in marriage had been wholly independent of stature. The importance of this supposition in facilitating calculation will be appreciated as we proceed.

Issue of Unlike Parents.—We will next discuss the question whether the Stature of the issue of unlike parents betrays any notable evidence of their unlikeness, or whether the peculiarities of the children do not rather depend on the *average* of two values; one the Stature of the father, and the other the transmuted Stature of the mother; in other words, on the Stature of that ideal personage to whom we have already been introduced under the name of a Mid-Parent. Stature has already been spoken of as a well-marked instance of the heritages that blend freely in the course of hereditary transmission. It now becomes necessary to substantiate the statement, because it is proposed to trace the relationship between the Mid-Parent and the Son. It would not be possible to define the relationship between either parent singly, and the son, in a trustworthy way, without the help of a much larger number of observations than are now at my disposal. They ought to be numerous enough to give good assurance that the cases of tall and short, among the unknown parents, will neutralise one another; otherwise the uncertainty of the stature of the unknown parent would make the results uncertain to a serious degree. I am heartily glad that I shall be able fully to justify the method of dealing with Mid-Parentages instead of with single Parents.

The evidence is as follows :—If the Stature of children depends only upon the *average* Stature of their two Parents, that of the mother having been first transmuted, it will make no difference in a Fraternity whether one of the Parents was tall and the other short, or whether they were alike in Stature. But if some children resemble one Parent in Stature and others resemble the other, the Fraternity will be more diverse when their Parents had differed in Stature than when they were alike. We easily acquaint ourselves with the facts by separating a considerable number of Fraternities into two contrasted groups: (*a*) those who are the progeny of Like Parents; (*b*) those who are the progeny of Unlike Parents. Next we write the statures of the individuals in each Fraternity under the form of M + (±D) (see page), where M is the mean stature of the Fraternity, and D is the deviation of any one of its members from M. Then we marshal all the values of D that belong to the group *a*, into one Scheme of deviations, and all those that belong to the group *b* into another Scheme, and we find the Q of each. If it should be the same, then there is no greater diversity in the *a* Group than there is in the *b* Group, and such proves to be the case. I applied the test (see Table 10) to a total of 525 children, and found that they were no more diverse in the one case than in the other. I therefore conclude that we have only to look to the Stature of the Mid-Parent, and need not care whether the Parents are or are not unlike one another.

The advantages of Stature as a subject from which the simple laws of heredity may be studied, will now be well appreciated. It is nearly constant in the same adult, it is frequently measured and recorded; its discussion need not be entangled with considerations of marriage selection. It is sufficient to consider the Stature of the Mid-Parent and not those of the two Parents separately. Its variability is Normal, so that much use may be made of the curious properties of the law of Frequency of Error in cross-testing the several conclusions, and I may ~~anticipate by saying that hitherto this has always been done with~~ add that in all cases they have borne the test successfully.

The only drawback to the use of Stature in statistical inquiries, is its small variability, one half of the population differing less than 1·7 inch from the average of all of them. In other words, its Q is only 1·7 inch.

Description of the Tables of Stature.—I have arranged and discussed my materials in a great variety of ways, to guard against rash conclusions, but do not think it necessary to trouble the reader with more than a few Tables, which afford sufficient material to determine the more important constants in the formulæ that will be used.

Table 11, R.F.F., refers to the relation between the Mid-Parent and his (or should we say *its?*) Sons or Transmuted Daughters, and records the Statures of 928 adult offspring of 205 Mid-Parents. It shows the distribution of Stature among the Sons of each successive group of Mid-Parents, in which the latter are all of the same Stature, reckoning to the nearest inch. I have calculated the M of each line, chiefly by drawing Schemes from the entries in it. Their values are printed at the ends of the lines and they form the right-hand column of the Table.

Tables 12 and 13 refer to the relation between Brothers. The one is derived from the R.F.F. and the other from the Special data. They both deal with small or moderately sized Fraternities, excluding the larger ones for reasons that will be explained directly, but the R.F.F. Table is the least restricted in this respect, as it only excludes families of 6 brothers and upwards. The data were so few in number that I could not well afford to lop off more. The Tables were constructed by registering the differences between each possible pair of brothers in each family: thus if there were three brothers, A, B, and C, in a particular family, I entered the differences of stature between A and B, A and C, and B and C. four brothers gave rise to 6 entries, and five brothers to 10 entries. The larger Fraternities were omitted, as the very large number of different pairs in them would have overwhelmed the influence of the smaller Fraternities. The large Fraternities are separately dealt with in Table 14.

We can derive some of the constants by more than one method; and it is gratifying to find how well the results of different methods confirm one another.

Mid-Stature of the Population.—The Median, Mid-Stature, or M of the general Population is a value of primary importance in this inquiry. Its value will be always designated by the symbol P, and it may be deduced from the bottom lines of any one of the three Tables. I obtain from them respectively the values 68·2, *68·5*, 68·4, but the middle of these, which is printed in italics, is a smoothed result. It is one of the only two smoothed values in the whole of my work, and was justifiably corrected, because the observed values that happen to lie nearest to the Grade of 50° ran out of harmony with the rest of the curve. It is therefore reasonable to consider its discrepancy as fortuitous, although it amounts to more than 0·15 inch. The series in question refers to R.F.F. brothers, who, owing to the principle on which the Table is constructed, are only a comparatively small sample taken out of the R.F.F. Population, and on a principle that gave greater weight to a few large families than to all the rest. Therefore it could not be expected to give rise to so regular a Scheme for the general R.F.F. Population as Table 11, which was fairly based upon the whole of it. Less accuracy was undoubtedly to have been expected in this group than in either of the others.

Variability of the Population.—The value of Q in the Statures of the general Population is to be deduced from the bottom lines of any one of the Tables 11, 12, and 13. The three values of it that I so obtain, are 1·65, 1·7, and 1·7 inch. I should mention that the method of the treatment originally adopted, happened also to make the first of these values 1·7 inch, so I have no hesitation in accepting 1·7 as the value for all my data.

Variability of Mid-Parents.—The value of Q in a Scheme drawn from the Statures of the R.F.F. Mid-Parents according to the data in Table 11, is 1·19 inches. Now it has already been shown that if marriage selection is independent of stature, the value of Q in the Scheme of Mid-parental Statures would be equal to its value in that of the general Population (which we have just seen to be 1·7 inch), divided by the square root of 2; that is by 1·45. This calculation makes it to be

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1·21 inch, which agrees excellently with the observed value.[1]

Variability in Co-Fraternities.—As all the Adult Sons and Transmuted Daughters of the same Mid-Parent, form what is called a Fraternity, so all the Adult Sons and Transmuted Daughters of a group of Mid-Parents who have the same Stature (reckoned to the nearest inch) will be termed a Co-Fraternity. Each line in Table 11 refers to a separate Co-Fraternity and expresses the distribution of Stature among them. There are three reasons why Co-Fraternals should be more diverse among themselves than brothers. First, because their Mid-Parents are not of identical height, but may differ even as much as one inch. Secondly, because their grandparents, great-grandparents, and so on indefinitely backwards, may have differed widely. Thirdly, because the nurture or rearing of Co-Fraternals is more various than that of Fraternals. The brothers in a Fraternity of townsfolk do not seem to differ more among themselves than those in a Fraternity of country-folk, but a mixture of Fraternities derived indiscriminately from the two sources, must show greater diversity than either of them taken by themselves. The large differences between town and country-folk, and those between persons of different social classes, are conspicuous in the data contained in the Report of the

[1] In all my values referring to human stature, the second decimal is rudely approximate. I am obliged to use it, because if I worked only to tenths of an inch, sensible errors might creep in entirely owing to arithmetical operations.

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Anthropological Committee to the British Association in 1880, and published in its Journal.

I concluded after carefully studying the chart upon which each of the individual observations from which Table 11 was constructed, had been entered separately in their appropriate places, and not clubbed into Groups as in the Tables, that the value of Q in each Co-Fraternal group was roughly the same, whatever their Mid-Parental value might have been. It was not quite the same, being a trifle larger when the Mid-Parents were tall than when they were short. This justifies what will be said in Appendix E about the Geometric Mean; it also justifies neglect in the present inquiry of the method founded upon it, because the improvement in the results to which it might lead, would be insignificant, while its use would have added to the difficulty of explanation, and introduced extra trouble throughout, to the reader more than to myself. The value that I adopt for Q in every Co-Fraternal group, is 1·5 inch.

Regression.—a. Filial: However paradoxical it may appear at first sight, it is theoretically a necessary fact, and one that is clearly confirmed by observation, that the Stature of the adult offspring must on the whole, be more *mediocre* than the stature of their Parents; that is to say, more near to the M of the general Population. Table 11 enables us to compare the values of the M in different Co-Fraternal groups with the Statures of their respective Mid-Parents. Fig. 10 is a graphical representation of the meaning of

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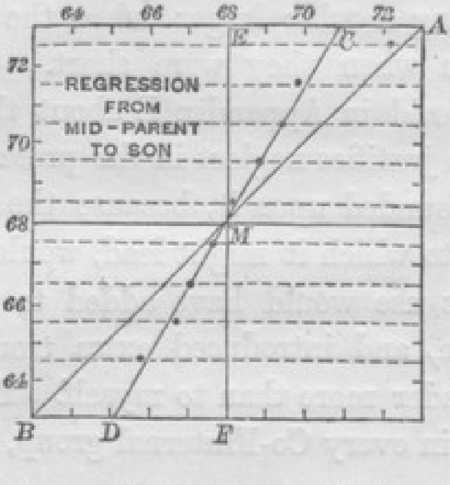
the Table so far as it now concerns us. The horizontal dotted lines and the graduations at their sides, correspond to the similarly placed lines of figures and graduations in Table 11. The dot on each line shows the point where its M falls. The value of its M is to be read on the graduations along the top, and is the same as that which is given in the last column of Table 11. It will be perceived that the line drawn

FIG. 10.



through the centres of the dots, admits of being interpreted by the straight line C D, with but a small amount of give and take; and the fairness of this interpretation is confirmed by a study of the MS. chart above mentioned, in which the individual observations were plotted in their right places.

Now if we draw a line A B through every point where the graduations along the top of Fig. 10, are the same as those along the sides, the line will be straight and will run diagonally. It represents what the Mid-

Statures of the Sons would be, if they were on the average identical with those of their Mid-Parents. Most obviously A B does *not* agree with C D; therefore Sons do *not*, on the average, resemble their Mid-Parents. On examining these lines more closely, it will be observed that A B cuts C D at a point M that fairly corresponds to the value of $68\frac{1}{4}$ inches, whether its value be read on the scale at the top or on that at the side. This is the value of P, the Mid-Stature of the population. Therefore it is only when the Parents are mediocre, that their Sons on the average resemble them.

Next draw a vertical line, E M F, through M, and let ECA be any horizontal line cutting ME at E, MC at C, and MA at A. Then it is obvious that the ratio of EA to EC is constant, whatever may be the position of ECA. This is true whether ECA be drawn above or below M. In other words, the proportion between the Mid-Filial and the Mid-Parental deviation is constant, whatever the Mid-Parental stature may be. I reckon this ratio to be as 2 to 3: that is to say, the Filial deviation from P is on the average only two-thirds as wide as the Mid-Parental Deviation. I call this ratio of 2 to 3 the ratio of "Filial Regression." It is the proportion in which the Son is, on the average, less exceptional than his Mid-Parent.

My first estimate of the average proportion between the Mid-Filial and the Mid-Parental deviations, was made from a study of the MS. chart, and I then reckoned it as 3 to 5. The value given above was afterwards substituted, because the data seemed to admit of that interpretation also, in which case the fraction of two-thirds was preferable as being the more simple expression. I am now inclined to think the latter may be a trifle too small, but it is not worth while to make alterations until a new, larger, and more accurate series of observations can be discussed, and the whole work revised. The present doubt only ranges between nine-fifteenths in the first case and ten-fifteenths in the second.

This value of two-thirds will therefore be accepted as the amount of Regression, on the average of many cases, from the Mid-Parental to the Mid-Filial stature, whatever the Mid-Parental stature may be.

As the two Parents contribute equally, the contribution of either of them can be only one half of that of the two jointly; in other words, only one half of that of the Mid-Parent. Therefore the average Regression from the Parental to the Mid-Filial Stature must be the one half of two-thirds, or one-third. I am unable to test this conclusion in a satisfactory manner by direct observation. The data are barely numerous enough for dealing even with questions referring to Mid-Parentages; they are quite insufficient to deal with those that involve the additional large uncertainty introduced owing to an ignorance of the Stature of one of the parents. I have entered the Uni-Parental and the Filial data on a MS. chart, each in its appropriate place, but they are too scattered and irregular to make it useful to give the results in detail. They seem to show a Regression of about two-fifths, which differs from that of one-third in the ratio of 6 to 5. This direct observation is so inferior in value to the inferred result, that I disregard it, and am satisfied to adopt the value given by the latter, that is to say, of one-third, to express the average Regression from either of the Parents to the Son.

b. Mid-Parental: The converse relation to that which we have just discussed, namely the relation between the unknown stature of the Mid-Parent and the known Stature of the Son, is expressed by a fraction that is very far from being the converse of two-thirds. Though the Son deviates on the average from P only $\frac{2}{3}$ as widely as his Mid-parent, it does not in the least follow that the Mid-parent should deviate on the average from P, $\frac{3}{2}$ or $1\frac{1}{2}$, as widely as the Son. The Mid-Parent is not likely to be more exceptional than the son, but quite the contrary. The number of individuals who are nearly mediocre is so preponderant, that an exceptional man is more frequently found to be the exceptional son of mediocre parents than the average son of very exceptional parents. This is clearly shown by Table 11, ~~for~~ where the very same observations which give the average value of Filial Regression when it is read in one way, gives that of the Mid-Parental Regression vertical when it is read in another way, namely down the columns, instead of along the horizontal lines. It then shows that the Mid-Parent of a man deviates on the

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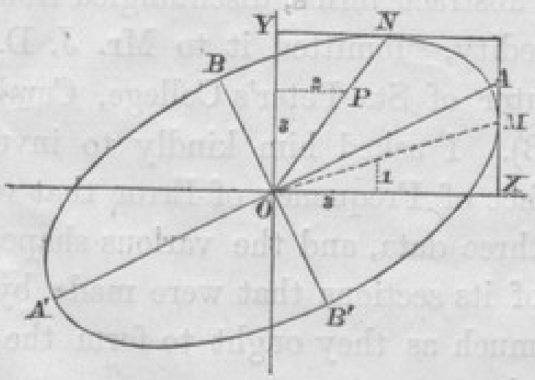
vertical average from P, only one-third as much as the man himself. This value of $\frac{1}{3}$ is four and a half times smaller than the numerical converse of $\frac{3}{2}$, since $4\frac{1}{2}$, or $\frac{9}{2}$, being multiplied into $\frac{1}{3}$, is equal to $\frac{3}{2}$.

c. Parental: As a Mid-Parental deviation is equal to one-half of the two Parental deviations, it follows that the Mid-Parental Regression must be equal to one-half of the sum of the two Parental Regressions. As the latter are equal to one another it follows that all three must have the same value. In other words, the average Mid-Parental Regression being $\frac{1}{3}$, the average Parental Regression must be $\frac{1}{3}$ also.

As there was much appearance of paradox in the above strongly contrasted results, I looked carefully into the run of the figures in Table 11. They were deduced, as already said, from a MS. chart on which the stature of every Son and the transmuted Stature of every Daughter is entered opposite to that of the Mid-Parent, the transmuted Statures being reckoned to the nearest tenth of an inch, and the position of the other entries being in every respect exactly as they were recorded. Then the number of entries in each square inch were counted, and copied in the form in which they appear in the Table. I found it hard at first to catch the full significance of the entries, though I soon discovered curious and apparently very interesting relations between them. These came out distinctly after I had "smoothed" the entries by writing at each intersection between a horizontal line and a vertical one, the sum of the entries in the four adjacent squares. I then noticed (see Fig. 11) that lines drawn through entries of the same value formed a series of concentric and similar ellipses. Their common centre lay at the intersection of those vertical and horizontal lines which correspond to the value of $68\frac{1}{4}$ inches, as read on both the top and on the side scales. Their axes were similarly inclined. The points where each successive ellipse was touched by a horizontal tangent, lay in a straight line that was inclined to the vertical in

FIG. 11.



the ratio of $\frac{2}{3}$, and those where the ellipses were touched by a vertical tangent, lay in a straight line inclined to the horizontal in the ratio of $\frac{1}{3}$. It will be obvious on studying Fig. 11 that the point where each successive horizontal line touches an ellipse is the point at which the greatest value in the line will be found. Therefore these ratios confirm the values of the Ratios of Regression, already obtained by a different method, namely those of $\frac{2}{3}$ from Mid-Parent to Son, and of $\frac{1}{3}$ from Son to Mid-Parent. The same is true in respect

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tree to last page

to the successive vertical lines. These and other relations were evidently a subject for mathematical analysis and verification. It seemed clear to me that they all depended on three elementary measures, supposing the law of Frequency of Error to be applicable throughout; namely (1) the value of Q in the General Population, which was found to be 1·7 inch; (2) the value of Q in any Co-Fraternity, which was found to be 1·5 inch; (3) the Average Regression of the Stature of the Son from that of the Mid-Parent, which was found to be $\frac{2}{3}$. I wrote down these values, and phrasing the problem in abstract terms, disentangled from all reference to heredity, submitted it to Mr. J. D. Hamilton Dickson, Tutor of St. Peter's College, Cambridge (see Appendix B). I asked him kindly to investigate for me the Surface of Frequency of Error that would result from these three data, and the various shapes and other particulars of its sections that were made by horizontal planes, inasmuch as they ought to form the ellipses of which I spoke.

The problem may not be difficult to an accomplished mathematician, but I certainly never felt such a glow of loyalty and respect towards the sovereignty and wide sway of mathematical analysis as when his answer arrived, confirming, by purely mathematical reasoning, my various and laborious statistical conclusions with far more minuteness than I had dared to hope, because the data ~~from which I had to work~~ ran somewhat roughly, and I had to smooth them with tender caution. His calculation corrected my observed value of Mid-Parental Regression

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from $\frac{1}{3}$ to $\frac{6}{17.5}$; the relation between the major and minor axis of the ellipses was changed 3 per cent.; and their inclination to one another was changed less than 2°.[1]

It is obvious from this close accord of calculation with observation, that the law of Error holds throughout with sufficient precision to be of real service, and that the various results of my statistics are not casual and disconnected determinations, but strictly interdependent.

I trust it will have become clear even to the most non-mathematical reader, that the law of Regression in Stature refers primarily to Deviations, that is, to measurements made from the *level of mediocrity* to the

[1] The following is a more detailed comparison between the calculated and the observed results. The latter are enclosed in brackets. The letters refer to Fig. 8:—

Given—

The "Probable Error" of each system of Mid-Parentages = 1·22 inch, (this was an earlier determination of its value; as already said, the second decimal is to be considered only as approximate).

Ratio of mean filial regression = $\frac{2}{3}$.

"Prob. Error" of each Co-Fraternity = 1·50 inch.

Sections of surface of frequency parallel to XY are true ellipses.

(Obs.—Apparently true ellipses.)

MX : YO = 6 : 17·5, or nearly 1 : 3.

(Obs.—1 : 3.)

Major axes to minor axes = $\sqrt{7} : \sqrt{2}$ = 10 : 5·35.

(Obs.—10 : 5·1.)

Inclination of major axes to OX = 26° 36′.

(Obs. 25°.)

Section of surface parallel to XZ is a true Curve of Frequency.

(Obs.—Apparently so.)

"Prob. Error", the Q of that curve, = 1.07 inch.

(Obs.—1·00, or a little more.)

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crown of the head, upwards or downwards as the case may be, ~~but~~ and not from the *ground* to the crown of the head. (In the population with which I am now dealing, the level of mediocrity is $68\frac{1}{4}$ inches (without shoes).) The law of Regression in respect to Stature may be phrased as follows; namely, that the Deviation of the Sons from P are, on the average, equal to one-third of the deviation of the ~~Mid~~ Parent from P, and in the same direction. Or more briefly still:—If $P + (\pm D)$ be the Stature of the Parent, the Stature of the offspring will on the average be $P + (\pm \frac{1}{3} D)$.

If this remarkable law of Regression had been based only on those experiments with seeds, in which I first observed it, it might well be distrusted until otherwise confirmed. If it had been corroborated by a comparatively small number of observations on human stature, some hesitation might be expected before its truth could be recognised in opposition to the current belief that the child tends to resemble its parents. But more can be urged than this. It is easily to be shown that we ought to expect Filial Regression, and that it ought to amount to some constant fractional part of the value of the Mid-Parental deviation. All of this will be made clear in a subsequent section, when we shall discuss the cause of the curious statistical constancy in successive generations of a large population. In the meantime, two different reasons may be given for the occurrence of Regression; the one is connected with our notions of stability of type, and of which no more need now be said; the other is as follows:—The child inherits partly from his parents, partly from his ancestry. In every population that intermarries freely, when the genealogy of any man is traced far backwards, his ancestry will be found to consist of such varied elements that they are indistinguishable from a sample taken at haphazard from the general Population. The Mid-Stature M of the remote ancestry of such a man will become identical with P; in other words, it will be mediocre. To put the same conclusion into another form, the most probable value of the Deviation of his Mid-Ancestors in any remote generation, from P, is zero.

For the moment let us confine our attention to some one generation in the remote ancestry on the one hand, and to the Mid-Parent on the other, and ignore all other generations. The combination of the zero Deviation of the one with the observed Deviation of the other is the combination of nothing with something. Its effect resembles that of pouring a measure of water into a vessel of wine. The wine is diluted to a constant fraction of its alcoholic strength, whatever that strength may have been.

Similarly with regard to every other generation. The Mid-Deviation in any near generation of the ancestors will have a value intermediate between that of the zero Deviation of the remote ancestry, and of the observed Deviation of the Mid-Parent. Its combination with the Mid-Parental Deviation will be as if a mixture of wine and water in some definite proportion, and not pure water, had been poured into the wine. The process throughout is one of proportionate dilutions, and the joint effect of all of them is to weaken the original alcoholic strength in a constant ratio.

The law of Regression tells heavily against the full hereditary transmission of any gift. Only a few out of many children would be likely to differ from mediocrity so widely as their Mid-Parent, and still fewer would differ as widely as the more exceptional of the two Parents. The more bountifully the Parent ~~had been~~ is gifted by nature, the more rare will be his good fortune if he begets a son who is as richly endowed as himself, and still more so if he has a son who is endowed yet more largely. But the law is even-handed; it levies an equal succession-tax on the transmission of badness as of goodness. If it discourages the extravagant hopes of a gifted parent that his children will inherit all his powers; it no less discountenances extravagant fears that they will inherit all his weakness and disease.

It must be clearly understood that there is nothing in these statements to invalidate the general doctrine that the children of a gifted pair are much more likely to be gifted than the children of a mediocre pair. They merely express the fact that the ablest of all the children of a few gifted pairs is not likely to be as gifted as the ablest of all the children of a very great many mediocre pairs.

The constancy of the ratio of Regression, whatever may be the amount of the Mid-Parental Deviation, is now seen to be a reasonable law which might have been foreseen. It is so in its relations simple that I have

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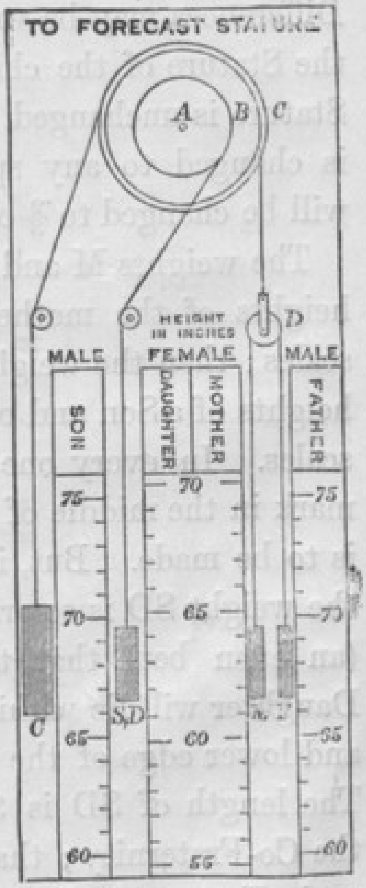
contrived more than one form of apparatus by which the probable stature of the children of known parents can be mechanically reckoned. Fig. 12 is a representation of one of them, that is worked with pulleys and weights. A, B, and C are three thin wheels with grooves round their edges. They are screwed together so as to form a single piece that turns easily on its axis. The weights M and F are attached to either end of a thread that passes over the movable pulley D. The pulley itself hangs from a thread which is wrapped two or three times round the groove of B and is then secured to the wheel. The weight SD hangs from a thread that is wrapped two or three times round the groove of A, and is then secured to the wheel. The diameter of A is to that of B as 2 to 3. Lastly, a thread is wrapped in the opposite direction round the wheel C, which may have any convenient diameter, and is attached to a counterpoise. M refers to the male statures, F to the female ones, S to the Sons, D to the daughters.



The scale of Female Statures differs from that of the Males, each Female height being laid down in the position which would be occupied by its male equivalent.

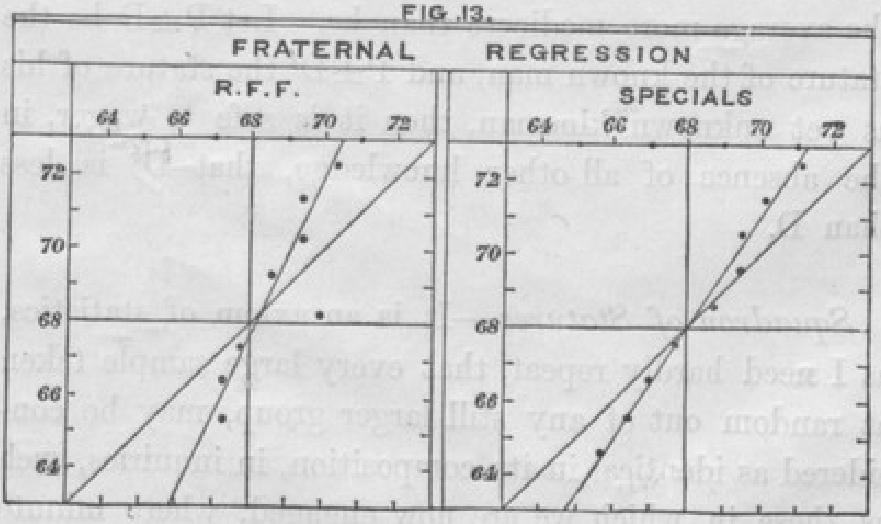
Thus 56 is written in the position of 60·48 inches, which is equal to 56 × 1·08. Similarly, 60 is written in the position of 64·80, which is equal to 60 × 1·08.

It is obvious that raising M will cause F to fall, and *vice versâ*, without affecting the wheel AB, and therefore without affecting SD; that is to say, the Parental Differences may be varied indefinitely without affecting the Stature of the children, so long as the Mid-Parental Stature is unchanged. But if the Mid-Parental Stature is changed to any specified amount, then that of SD will be changed to $\frac{2}{3}$ of that amount.

The weights M and F have to be set opposite to the heights of the mother and father on their respective scales; then the weight SD will show the most probable heights of a Son and of a Daughter on the corresponding scales. In every one of these cases, it is the fiducial mark in the middle of each weight by which the reading is to be made. But, in addition to this, the length of the weight SD is so arranged that it is an equal chance (an even bet) that the height of each Son or each Daughter will lie within the range defined by the upper and lower edge of the weight, on their respective scales. The length of SD is 3 inches, which is twice the Q of the Co-Fraternity; that is, 2 × 1·50 inch.

d. Fraternal: In seeking for the value of Fraternal Regression, it is better to confine ourselves to the Special data given in Table 13, as they are much more trustworthy than the R.F.F. data in Table 12. By treating them in the way shown in Fig. 13, which is constructed on the same principle as Fig. 10, page 96, I obtained the value for Fraternal Regression of $\frac{2}{3}$; that is to say, the unknown brother of a known man is probably only two-thirds as exceptional in Stature as he is. This is the same value as that obtained for the Regression from Mid-Parent to Son. However paradoxical the fact of there being such a thing as Fraternal Regression, may seem at first, a little reflection will show its reasonableness, which will become much clearer later on. In the meantime, we may recollect that the

FIG. 13.



unknown brother has two different tendencies, the one to resemble the known man, and the other to resemble his race. The one tendency is to deviate from P as much as his brother, and the other tendency is not to deviate at all. The result is a compromise.

As the average Regression from either Parent to the Son is twice as great as that from a man to his Brother, a man is, generally speaking, only half as nearly related

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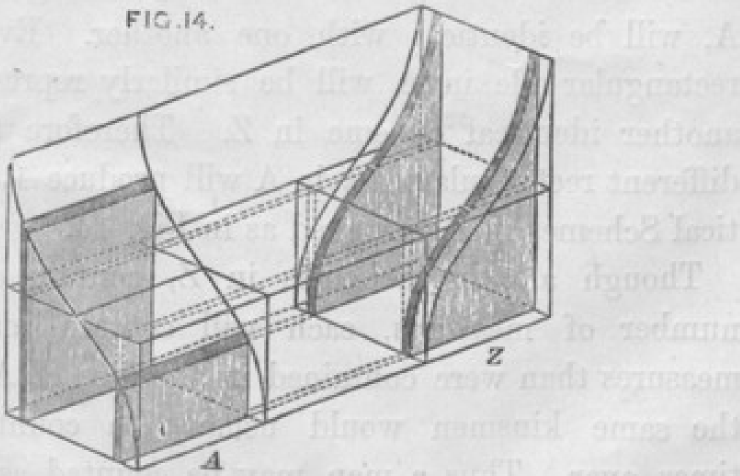
to either of his Parents as he is to his Brother. In other words, the Parental kinship is only half as close as the Fraternal.

We have now seen that there is Regression from the Parent to his Son, from the Son to his Parent, and from the Brother to his Brother. As these are the only three possible lines of kinship, namely, descending, ascending, and collateral, it must be a universal rule that the unknown Kinsman, in any degree, of a known Man, is on the average more mediocre than he. Let $P \pm D$ be the stature of the known man, and $P \pm D'$ the stature of his as yet unknown kinsman, then it is safe to wager, in the absence of all other knowledge, that D' is less than D.

Squadron of Statures.—It is an axiom of statistics, as I need hardly repeat, that every large sample taken at random out of any still larger group, may be considered as identical in its composition, in inquiries such as these in which we are now engaged, where minute accuracy is not desired and where highly exceptional cases are not regarded. Suppose our larger group to consist of a million, that is of 1000×1000 statures, and that we had divided it at random into 1000 samples each containing 1000 statures, and made Schemes of each of them. The 1000 Schemes would be practically identical, and we might marshal them one behind the other in successive ranks, and thereby form a "Squadron," numbering 1000 statures each way, and standing upon a square base. Our Squadron may be divided either into 1000 ranks or into 1000 files. The ranks will form a series of 1000 identical Schemes, the files will form a series of 1000 rectangles, that are of the same breadth, but of dissimilar heights. (See Fig. 14.)

It is easy by this illustration to give a general idea, to be developed as we proceed, of the way in which any large sample, A, of a Population gives rise to a group of Kinsmen, Z, so distant as to retain no family likeness

FIG. 14.



to A, but to be statistically undistinguishable from the Population generally, as regards the distribution of their statures. In this case the samples A and Z would form similar Schemes. I must suppose provisionally, for the purpose of easily arriving at an approximate theory, that tall, short, and mediocre Parents contribute equally to the next generation though this may not strictly be the case.[1]

[1] Oddly enough, the shortest couple on my list have the largest family, namely, sixteen children, of whom fourteen were measured.

Throw A into the form of a Squadron and not of a Scheme, and let us begin by confining our attention to the men who form any two of the rectangular files of A, that we please to select. Then let us trace their connections with their respective Kinsmen in Z. As the number of the Z Kinsmen to each of the A files is considered to be the same, and as their respective Stature-Schemes are supposed to be identical with that of the general Population, it follows that the two Schemes in Z derived from the two different rectangular files in A, will be identical with one another. Every other rectangular file in A will be similarly represented by another identical Scheme in Z. Therefore the 1,000 different rectangular files in A will produce 1,000 identical Schemes in Z, arranged as in Fig. 14.

Though all the Schemes in Z, contain the same number of measures, each will contain many more measures than were contained in the files of A, because the same kinsmen would usually be counted many times over. Thus a man may be counted as uncle to many nephews, and as nephew to many uncles. We will therefore (though it is hardly necessary to do so) suppose each of the files in Z to have been constructed from only a sample consisting of 1,000 persons, taken at random out of the more numerous measures to which it refers. By this treatment Z becomes an exact Squadron, consisting of 1,000 elements, both in rank and in file, and it is identical with A in its constitution, though not in its attitude. The ranks of Z, which are Schemes, have been derived from the files of A, which are rect-

r.W.B.
f. 64r

angles, therefore the two Squadrons must stand at right angles to one another, as in Fig. 14. The upper surface of A is curved in rank, and horizontal in file; that of Z is curved in file and horizontal in rank.

The Kinsmen in nearer degrees than Z will be represented by Squadrons whose forms are intermediate between A and Z. Front views of these are shown in

FIG. 15.

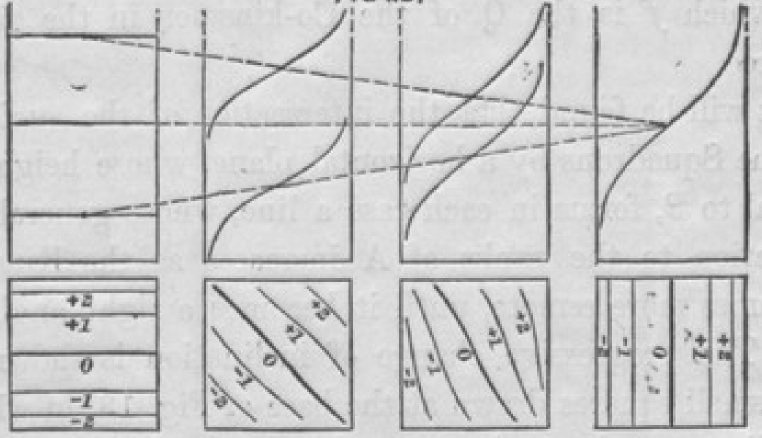


Fig. 15. Consequently they will be somewhat curved both in rank and in file. Also as the Kinsmen of all the members of a Population, in any degree, are themselves a Population having similar characteristics to those of the Population of which they are part, it follows that the elements of every intermediate Squadron when they are broken up and sorted afresh into ordinary Schemes, would form identical Schemes. Therefore, it is clear that a law exists that connects the curvatures in rank and in file, of any Squadron. Both of the curvatures are Curves of Distribution; let us call their Q values respectively r and f. Then if p be the Q of

the general Population, we arrive at a general equation that is true for all degrees of Kinship; namely—

$$r^2 + f^2 = p^2 \qquad (1)$$

but r, the curvature in rank, is a regressed value of p, and may be written wp, w being the value of the Regression. Therefore the above equation may be put in the form of

$$w^2 p^2 + f^2 = p^2 \qquad (2)$$

in which f is the Q of the Co-kinsmen in the given degree.

It will be found that the intersection of the surfaces of the Squadrons by a horizontal plane, whose height is equal to P, forms in each case a line, whose general inclination to the ranks of A increases as the Kinship becomes more remote, until it becomes a right angle in Z. The progressive change of inclination is shown in the small squares drawn at the base of Fig. 13, in which the lines are the projections of contours drawn on the upper surfaces of the Squadrons, to correspond with the multiples there stated of values of p.

It will be understood from the front views of the four different Squadrons, which form the upper part of Fig. 13, how the Mid-Statures of the Kinsmen to the Men in each of the files of A, gradually become more mediocre in the successive stages of kinship until they all reach absolute mediocrity in Z. This figure affords an excellent diagramatic representation, true to scale, of the action of the law of Regression in Descent. I should like to have given in addition, a perspective view of the Squadrons, but failed to draw them clearly, after making many attempts. Their curvatures are so delicate and peculiar that the eye can hardly appreciate them even in a model, without turning it about in different lights and aspects. A plaster model of an intermediate form was exhibited at the Royal Society by Mr. J. D. H. Dickson, when my paper on *Hereditary Stature* was read, together with his solutions of the problems that are given in the Appendix. I also exhibited arrangements of files and ranks that were made of pasteboard. Mr. Dixon mentioned that the mathematical properties of ~~the~~ Surface of ~~a Curve of~~ Frequency, showed that no strictly straight line could be drawn upon it.

Successive Generations of a People.—We are far too apt to regard common events as matters of course, that require no explanation, whereas they may be problems of much interest and of some difficulty, and still await solution.

Why is it, when we compare two large groups of persons selected at random from the same race, but belonging to different generations, that they are usually found to be closely alike? There may be some small statistical dissimilarity due to well understood differences in the general conditions of their lives, but with this I am not concerned. The present question is as to the origin of that statistical resemblance between successive generations which is due to the strict processes of heredity, and which is commonly observed in all forms of life.

In each generation, individuals are found to be tall and short, heavy and light, strong and weak, dark and pale; and the proportions of those who present these several characteristics in their various degrees, tend to be constant. The records of geological history afford striking evidences of this statistical similarity. Fossil remains of plants and animals may be dug out of strata at such different levels, that thousands of generations must have intervened between the periods at which they lived; yet in large samples of such fossils we may seek in vain for peculiarities that distinguish one generation from another, the different sizes, marks, and variations of every kind, occurring with equal frequency in all.

If any are inclined to reply that there is no wonder in the matter, because each individual tends to leave his like behind him, and therefore each generation must, as a matter of course, resemble the one preceding, the patent fact of Regression shows that they utterly misunderstand the case.

We have now reached a stage at which it has become possible to discuss the problem with some exactness, and I will do so by giving mathematical expression to what actually took place in the Statures of that sample of our Population whose life-histories are recorded in the R.F.F. data.

The Males and Females in Generation I. whose **M** has the value of P (viz., $68\frac{1}{4}$ inches), and whose **Q** is 1·7 inch, were found to group themselves as it were at random, into ~~pairs~~ couples, and thus to form a system of Mid-

Parents. This system had of course the same M as the general Population, but its Q was reduced to $\sqrt{\frac{1}{2}} \times 1{\cdot}7$ inch, or to 1·2 inch. It was next found when the Statures of the Mid-Parents, expressed in the form of $P + (\pm D)$, were sorted into groups in which D was the same (reckoning to the nearest inch), that a Co-fraternity sprung from each group, whose M was of the form $P + (\pm \frac{2}{3} D)$. The next system in which element is one of the Mid-Co-Fraternity must still have the same M as before, of $68\frac{1}{4}$ inches, but its Q will be further reduced from 1·2 inch to $\frac{2}{3} \times 1{\cdot}2$ inch, or to 0·8 inch. Lastly, the individual Co-Fraternals were seen to be dispersed from their respective Mid-Co-Fraternities, with a Q equal in each case to 1·5 inch. The sum of all of the Co-Fraternals forms the Population of Generation II. Consequently the members of Generation II. constitute a system that has an M of $68\frac{1}{4}$ inches and a Q equal to $\sqrt{[(0{\cdot}8)^2 + (1{\cdot}5)^2]}$, = 1·7 inch. These values are identical with those in Generation I.; so the cause of their statistical similarity is tracked out.

There ought to be no misunderstanding as to the character of the evidence or of the reasoning upon which this analysis is based. A small but fair sample of the Population in two successive Generations has been taken, and its conditions as regards Stature have been strictly discussed. It was found that the distribution of Stature was sufficiently Normal to justify our ignoring any shortcomings in that respect. The transmutation

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of female heights to their male equivalents was justified by the fact that when the individual Statures of a group of females are raised in the proportion of 100 to 108, the Scheme drawn from them fairly coincides with that drawn from male Statures. Marriage selection was found to take no sufficient notice of Stature to be worth consideration; neither was the number of children in Fraternities found to be sensibly affected by the Statures of their Parents. Again, it was seen to be of no consequence when dealing statistically with the offspring, whether their Parents were alike in stature or not, the only datum deserving consideration being the Stature of the Mid-Parent, that is to say, the average value of (1) the Stature of the Father, and of (2) the Transmuted Stature of the Mother. I fully grant that not one of these deductions may be strictly exact, but the error introduced into the conclusions by supposing them to be correct proves not to be worth taking into account in a first approximation.

Precisely the same may be said of the ulterior steps in this analysis. Every one of them is based on the properties of an ideally perfect curve, but in no case has there been need to make any sensible departure from the observed results, except in assigning a uniform value to Q in the different Co-Fraternities. Strictly speaking, that value was found to slightly rise or fall as the Mid-Stature of the Co-Fraternity rose or fell. This suggested the advisability of treating the whole inquiry on the principle of the Geometric Mean, Appendix G. I tried that principle in what seemed to be the most hopeful case among my 18 schemes, but found the gain, if any, to be so small, that I did not care to go on with the experiment. It did not seem to deserve the additional trouble, and I was indisposed to do anything that was not really necessary, which might further confuse the reader. But had I possessed better data, I should have tried the Geometric Mean throughout. In doing so, every measure would be replaced by its logarithm, and these logarithms would be treated just as if they had been the observed values. The conclusions to which they might lead would then be re-transmuted to the numbers of which they were the logarithmic equivalents.

In short, we have dealt mathematically with an ideal population which has similar characteristics to those of a real population, and have seen how closely the behaviour of the ideal population corresponds in every stage to that of the real one. Therefore we have arrived at a closely approximate solution of the problem of statistical constancy, though numerous refinements have been neglected.

Natural Selection.—This hardly falls within the scope of our ~~subject of~~ inquiry into Natural Inheritance, but it will be appropriate to consider briefly the way in which the action of Natural Selection may harmonise with that of pure heredity, and ~~may~~ work together with it in such a manner as not to compromise the normal distribution of faculty. To do this, we must deal with the case that best represents the various possible occurrences, namely that in which the mediocre members of a population are those that are most nearly in harmony with their circumstances. The harmony ought to concern the aggregate of their faculties, combined on the principle adopted in Table 3, after weighting them in the order of their importance. We may deal with any faculty separately, to serve as an example, if its mediocre values happen to be those that are most preservative of life under the majority of circumstances. Such is Stature, in a rudely approximate degree, inasmuch as exceptionally tall or exceptionally short persons have less chance of life than those of moderate size.

It will give more definiteness to the reasoning to take a definite example, even though it be in part an imaginary one. Suppose then, that we are considering the stature of some animal that is liable to be hunted by certain beasts of prey in a particular country. So far as he is big of his kind, he would be better able than the mediocrities to crush through thick grass and foliage whenever he was scampering for his life, to jump over obstacles, and possibly to run somewhat faster than they. So far as he is small of his kind, he would be better able to run through narrowest openings, to make quick turns, and to hide himself. Under the general circumstances, it would be found that animals of some particular stature had on the whole a better chance of escape than any other, and if their race is closely adapted to their circumstances in respect to stature, the most favoured stature would be identical with the M of the race. We already know that if we call this value P, and write each stature under the form of $P+x$ (in which x includes its sign), and if the number of times with which any value $P+x$ occurs, compared to the number of times in which P occurs, be called y, then x and y are connected by the law of Frequency of Error.

Though the impediments to flight are less unfavourable, on the average, to the stature P than to any other, they differ in different experiences. The course of one animal may chance to pass through denser foliage than usual, or the obstacles in his way may be higher. In that case an animal whose stature exceeded P would have an advantage over mediocrities. Conversely, the circumstances might be more favourable to a small animal.

Each particular line of escape would be most favourable to some particular stature, and whatever the value of x might be, it is possible that the stature $P+x$ might in some cases be more favoured than any other. But the accidents of foliage and soil in a country are characteristic and persistent, and may fairly be considered as approximately of a typical kind. Therefore those that most favour the animals whose stature is P will be more frequently met with than those that favour any other stature of $P+x$, and the frequency of the latter occurrence will diminish rapidly as x increases. If the number of times with which any particular value of $P+x$ is most favoured, as compared with the number of times in which P is most favoured, be called y', we may fairly assume that y' and x are connected by the law of Frequency of Error. But though the system of y values and that of y' values may be both subject to the law, it is not for a moment to be supposed that their Q values are necessarily the same.

We have now to show how a large population of animals becomes reduced by the action of natural selection to a smaller one, in which the M value of the statures is unchanged, while the Q value is decreased.

To do this we must first consider the population to have grown up entirely shielded from causes of premature mortality; call their number N. Then suppose them to be assailed by all the lethal influences that have no reference to stature. These would reduce their number to N′, but by the hypothesis, the values of M and of Q would remain unaffected. Next let the influences that act selectively on stature, further reduce the numbers to S; these being the final survivors. We have seen that:—

y = the ~~proportion between the~~ number of individuals who have the stature $P \pm x$, ~~and~~ counting those who have the stature P, as 1.

y' = the ~~proportion between the~~ number of times in which $P \pm x$ is the most favoured stature, ~~and~~ counting those when P is the most favoured, as 1.

Then yy' = the ~~proportion between the~~ number of times that individuals of the stature $P \pm x$ are selected, ~~and~~ counting those in which ~~those~~ individuals of the stature P are selected, as 1.

As the relation between y and x, and between y' and x are severally governed by the law of Frequency of

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Error, it follows directly from the formula by which that law is expressed, that the relation between yy' and x is also governed by it. The value of P of course remains the same throughout, but the Q in the system of yy' values is necessarily less than that in the system of y values.

It might well be that natural selection would favour the indefinite increase of numerous separate faculties, if their improvement could be effected without detriment to the rest; then mediocrity in that faculty would not be the safest condition. Thus an increase of fleetness would be a clear gain to an animal liable to be hunted by beasts of prey, if no other useful faculty was thereby diminished.

But a too free use of this "if" would show a jaunty disregard of a real difficulty. Organisms are so knit together that change in one direction involves change in many others; these may not attract attention, but they are none the less existent. Organisms are like ships of war constructed for a particular purpose in warfare, as cruisers, line of battle ships, &c., on the principle of obtaining the utmost efficiency for their special purpose. The result is a compromise between a variety of conflicting desiderata, such as cost, speed, accommodations, stability, weight of guns, thickness of armour, quick steering power, and so on. It is hardly possible in a ~~well thought out~~ ship of any long established type to make an improvement in any one of these respects, without a sacrifice in other directions. If the fleetness is increased, the engines must be larger, and more space must be given up to coal, and this diminishes the remaining accommodation. Evolution may produce an altogether new type of vessel that shall be more efficient than the old one, but when a vessel of a particular type has become thoroughly adapted through long experience to its functions, it is not possible to produce a mere variety of its class that shall have increased efficiency in some one particular without detriment to the rest. So it is with animals.

Variability in Fraternities.—Human Fraternities are far too small to admit of their Q being satisfactorily measured by the direct method. We are obliged to have recourse to indirect methods, of which no less than four are available. I shall apply each of them to both the Special and to the R.F.F. data; this will give 8 separate estimates of its value, which in the meantime will be called *b*. The four methods are as follow:

First method; by Fraternities each containing the same number of persons:—Let me begin by saying that I had already found in the large Fraternities of Sweet Peas, that the sizes of individuals of whom they consisted were normally distributed, and that their Q was independent of the size, or of the Stature as we may phrase it, of the Mid-Fraternity. We have also seen that the Q is practically the same in all Co-fraternities of men. Therefore it is reasonable to expect that it will also be found to be the same in all their Fraternities, though owing to their small size we cannot assure ourselves of the fact by direct evidence. We will assume this to be the case for the present; it will be seen that the results justify the assumption.

four independent values of b, which are respectively 1·01, 1·01, 1·20, and 1·08; the mean of these is 1·07.

Second Method; from the mean value of Fraternal Regression:—We may look on the Population as composed of a system of Fraternities. Call their respective true centres (see last paragraph) (MF_1), (MF_2), &c. These will be distributed about P with an as yet unknown Prob. Deviation, that we will call c. The individual members of each Fraternity will of course be distributed from their own (MF) with a Q equal to b.

Then $(1·7)^2 = c^2 + b^2$ (1)

Let $P + (\pm F_n)$ be the stature of any individual, and let $P + (\pm M\,F_n)$ be that of the **M** of his Fraternity, then Problem 4 shows us that:—

the *most probable* value of $\frac{(MF_n)}{F_n}$ is $\sqrt{\frac{c^2}{b^2+c^2}}$ (2)

This is also the value of Fraternal Regression, and therefore equal to $\frac{2}{3}$. Substituting in (2), and replacing c by the value given by (1), we obtain $b = 0·98$ inch.

Third Method; by the Variability in the value of individual cases of Fraternal Regression:—The figures in each line of Table 13 are found to have a Q equal to 1·24 inch, and they are the results of two independent systems of variation. First, the several (MF) values (see last paragraph) are dispersed from the M of all of them with a Q that we will call v. Secondly the individual brothers in each Fraternity are dispersed from their own (MF) with a Q equal to b.

Hence $(1\cdot24)^2 = v^2 + b^2$.

But it is shown Problem 5 that $v = \frac{bc}{\sqrt{(b^2+c^2)}}$;

therefore $(1\cdot24)^2 = b^2 + \frac{b^2c^2}{b^2+c^2}$.

Substituting for c^2 its value of $(1\cdot7)^2 - b^2$ (see last paragraph), we obtain $b = 0\cdot98$ inch.

Fourth Method; from differences between pairs of brothers taken at random:—In the fourth method, Pairs of Brothers are taken at random, and the Differences between the statures in each pair are noted; then, under the following reservation, any one of these differences would have the Prob. value of $\sqrt{2} \times b$. The reservation is, that only as many Differences should be taken out of each Fraternity as are independent. A Fraternity of n brothers admits of $\frac{n(n-1)}{2}$ possible pairs, and the same number of Differences; but as no more than $n-1$ of these are independent, that number only of the Differences 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 sets of Differences from any one Fraternity however large it might be. It was faulty in taking three Differences instead of only two, out of a Fraternity of three brothers, and four Differences, instead of only three, from a Fraternity of four brothers, and therefore giving an increased weight to those Fraternities, but in other respects the system was hardly objectionable. The introduced error must be so slight as to make it scarcely worth while now to go over the work again. By the system adopted, I found the Prob. Difference to be 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 in every case. An epitome of all the results appears in the following table.

Methods and data.	Values of b obtained by different methods and from different data.	
	From Special Data.	From R.F.F. data.[1]
(1) From Fraternities each containing the same number of persons	1·07	1·38
(2) From the mean value of Fraternal Regression............	0·98	1·31
(3) From the Variability of Fraternal Regression......... ..	1·10	1·14
(4) From Pairs of Brothers taken at random.................	1·10	1·35
Mean..............	1·06	

The data used in the four methods are somewhat different. In (1) I could not deal with small Fraterni-

[1] The R.F.F. results were obtained from brothers only and not from transmuted sisters, except in method (2), where the paucity of the data compelled me to include them.

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ties, so all were disregarded that contained fewer than four individuals. In (2) and (3) I could not with safety use large Fraternities. In (4) the method of selection was, as we have seen, quite indifferent. This makes the accordance of the results derived from the Special data all the more gratifying. Those from the R.F.F. data accord less well together. The R.F.F. measures are not sufficiently exact for use in these delicate calculations. Their results, being compounded of *b* and of their tendency to deviate from exactness, are necessarily too high, and should be discarded. I gather from all this that we may safely consider the value of *b* to be less than 1·06, and that allowing for some want of precision in the Special data, the very convenient value of 1·00 inch may reasonably be adopted.

Trustworthiness of the Constants.—There is difficulty in correcting the results obtained from the R.F.F. data, though we can make some estimate of their general inaccuracy as compared with the Special data. The reason of the difficulty is that the inaccuracy cannot be ascribed to an uncertainty of equal ± amount in every entry, such as might be due to a doubt of "shoes off" or "shoes on." If it were so, the Prob. Error of a single value of the R.F.F. would be greater than that of one of the Specials, whereas it proves to be the same. It is likely that the inaccuracy is a compound first of the uncertainty above mentioned, whose effect would be to increase the value of the Prob. Error, and secondly of a tendency on the part of my correspondents to record medium statures when they were in doubt, whose effect would be to reduce the value of the Prob. Error. The R.F.F. data in Table 12 run so irregularly that I cannot interpret them with any assurance. The value they give for Fraternal Regression certainly does not exceed $\frac{1}{2}$, and therefore a correction, amounting to no less than $\frac{1}{3}$ of its amount, is required to bring it to a parity with that derived from the Special data (because $\frac{1}{2} + \frac{1}{3} \times \frac{1}{2} = \frac{2}{3}$). Hence it might be argued, that the value of Regression from Mid-Parent to Son, which the R.F.F. data gave as $\frac{2}{3}$, ought to receive a similar correction. If so, it would be raised to $\frac{2}{3} + \frac{2}{9} = \frac{8}{9}$; but I cannot believe this high value to be correct. My first estimate made from the R.F.F. data, was $\frac{3}{5}$, as already mentioned. If this be adopted, the corrected value would be $\frac{4}{5}$, or $\frac{8}{10}$ instead of $\frac{8}{9}$, which might possibly pass. Curiously enough, this value of $\frac{4}{5}$ for Regression from Mid-Parent to Son, coincides with the value of $\frac{2}{5}$ for Regression from a single Parent to Son, which the direct observations showed (see page 99), but which owing to their paucity and to the irregularity of the way in which they ran, I rejected and still reject, at least for the present. While sincerely desirous of obtaining a revised value of average Filial Regression from entirely different and more accurate groups of data, the provisional value already adopted of $\frac{2}{3}$ from Mid-Parent to Son may be accepted as being near enough for the present. It is impossible to revise one datum in the

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R.F.F. series without revising all, as they hang together and support one another.

General View of Kinship.—We are now able to deal with the distribution of statures among the Kinsmen in every near degree, of persons whose statures we know, but whose ancestral statures we either do not know, or do not care to take into account. We are able to calcu late Tables for every near degree of Kinship on the form of Table 11, and to reconstruct that same Table in a shape free from irregularities. We must first find the Regression, which we may call w, appropriate to the degree of Kinship in question. Then we have to find a value f for each line of a Table corresponding in form to that of Table 11, in which f was found to be equal to 1·50 inch. We deduce the value of f from that of w by means of the general equation $p^2w^2+f^2=p^2$, p being equal to 1·7 inch. The values to be inserted in the several lines are then calculated from the ordinary table (Table 5) of the "probability integral."

As an example of the first part of the process, let us suppose we are about to construct a table of Uncles and their Nephews, and we wish to find w and f. We do so as follows: A Nephew is the son of a Brother, therefore in this case we have $w=\frac{1}{3}\times\frac{2}{3}=\frac{2}{9}$; whence $f=1·66$.

The Regression, which we call w, is a convenient and correct measure of family likeness. If the resemblance of the Kinsman to the Man, was on the average as perfect as that of the Man to his own Self, there would be no Regression at all, and the value of w would be 1.

TABLE OF DATA FOR CALCULATING TABLES OF DISTRIBUTION OF STATURE AMONG THE KINSMEN OF PERSONS WHOSE STATURE IS KNOWN.

From group of persons of the same Stature, to their Kinsmen in various near degrees.	Mean regression $= w$.	$Q = f$ $= p \times \sqrt{(1 - w^2)}$.
Mid-parents to Sons	2 / 3	1·27
Brothers to Brothers	2 / 3	1·27
Fathers or Sons to Sons or Fathers	1 / 3	1·60
Uncles or Nephews to Nephews or Uncles	2 / 9	1·66
Grandsons to Grandparents...	1 / 9	Practically that of Population, or 1·7 inch.
Cousins to Cousins	2 / 27	

On the other hand, if the Kinsmen were on the average no more like the Man than if they had been a group picked at random out of the general Population, then the Regression to P would be complete. The M of the Kinsmen, which is expressed by P+(±wD), would in that case become P, whatever might have been the value of D; therefore w must$=0$. We see by the preceding Table that as a general rule, Fathers or Sons should be held to be only one-half as near in blood as Brothers, and Uncles and Nephews to be one-third as near in blood as Brothers. Cousins are $4\frac{1}{2}$ times as remote as Fathers or as Sons, and 9 times as remote as Brothers. I do not extend the table further, because considerations would have to be taken into account that will be discussed in the next Section.

The remarks made in a previous chapter about heritages that blend and those that are mutually exclusive, must be here borne in mind. It would be a poor prerogative to inherit say the fifth part of the peculiarity of some gifted ancestor, but the chance of 1 to 5, of inheriting the whole of it, would be deservedly prized.

Separate Contribution of each Ancestor.—In making the statement that Mid-Parents whose Stature is $P \pm D$ have children whose average stature is $P \pm \frac{2}{3}D$, it is supposed that no separate account has been taken of the previous ancestry. Yet though nothing may be known of them, something is tacitly implied and has been tacitly allowed for, and this requires to be eliminated before we can learn the amount of the Parental bequest, pure and simple. What that something is, we must now try to discover. When speaking of converse Regression, it was shown that a peculiarity in a Man implied a peculiarity of $\frac{1}{3}$ of that amount in his Mid-Parent. Call the peculiarity of the Mid-Parent D, then the implied peculiarity of the Mid-Parent of the Mid-Parent, that is of the Mid-Grand-Parent of the Man, would on the above supposition be $\frac{1}{3}D$, that of the Mid-Great-Grand-Parent would be $\frac{1}{9}D$, and so on. Hence the total bequeathable property would amount to $D(1 + \frac{1}{3} + \frac{1}{9} + \text{\&c.}) = D\frac{3}{2}$.

Do the bequests from each of the successive generations reach the child without any, or what, diminution by the way? I have not sufficient data to yield a direct reply, and must therefore try limiting suppositions.

First, suppose the bequests by the various generations to be equally taxed; then, as an accumulation of ancestral contributions whose sum amounts to $D\frac{3}{2}$ yields an effective heritage of only $D\frac{2}{3}$, it follows that each piece of heritable property must have been reduced to $\frac{4}{9}$ of its original amount, because $\frac{3}{2}\times\frac{4}{9}=\frac{2}{3}$.

Secondly, suppose the tax not to be uniform, but to be repeated at each successive transmission, and to be equal to $\frac{1}{r}$ of the amount of the property at each stage. In this case the effective heritage would be $D\left(\frac{1}{r}+\frac{1}{3r^2}+\frac{1}{3^2r^3}+-\right)=D\,\frac{3}{3r-1}$, which must, as before, be equal to $D\frac{2}{3}$; whence $\frac{1}{r}=\frac{6}{11}$.

Thirdly, it might possibly be supposed that the Mid-Ancestor in a remote generation should on the average contribute more to the child than the Mid-Parent, but this is quite contrary to what is observed. The descendants of what was "pedigree wheat," after being left to themselves for many generations, show little or no trace of the remarkable size of their Mid-Ancestors in the generations just before their time, though their immediate offspring in the first of those generations did so unmistakably.

The results of our only two valid limiting suppositions are therefore, (1) that the Mid-Parental peculiarities, pure and simple, influence the offspring to $\frac{4}{9}$ of their amount; (2) that they influence it to $\frac{6}{11}$ of their 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 $\frac{1}{2}$, and that of the Mid-Grand-Parent as $\frac{1}{4}$, and so on. Consequently the influence of the individual Parent would be $\frac{1}{4}$, and of the individual Grand-Parent $\frac{1}{16}$, and so on. It would, however, be hazardous on the present slender basis, to extend this sequence with confidence to more distant generations.

Pedigree Moths.—I am endeavouring at this moment to obtain data that will enable me to go further, by breeding Pedigree Moths, thanks to the aid of Mr. Frederick Merrifield. The moths *Selenia Illustraria* and *Illunaria* are chosen for the purpose, partly on account of their being what is called double brooded; that is to say, they pass normally through two generations in a single year, which is a great saving of time to the experimenter. They are hardy, prolific and variable, and are found to stand chloroform well, previously to being measured and then paired. Every member of each Fraternity is preserved along three lines of descent—one race of long-winged moths, one of medium-winged, and one of short-winged moths. The three parallel sets are reared under identical conditions, so that the medium series supplies a trustworthy relative base, from which to measure the increasing divergency of the others. No one can be sure of the success of any extensive breeding experiment, but this attempt has been well started and seems to present no peculiar difficulty. Among other reasons for choosing moths for the purpose, is that they are born adults, not changing in stature after they have emerged from the chrysalis and shaken out their wings. Their families are of a convenient size for statistical purposes, say from 50 to 100, neither too few to make satisfactory Schemes, nor unmanageably large. They can be mounted as we all know, after their death, with great facility, and be remeasured at leisure. An intelligent and experienced person can carry on a large breeding establishment in a small room, supplemented by a small garden. The methods used and the results up to last spring, have been described by Mr. Merrifield in papers read February and December 1887, and printed in the Transactions of the Entomological Society. I speak of this now, in hopes of attracting the attention of some who are competent and willing to carry on collateral experiments with the same breed, or with altogether different species of moths.

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CHAPTER VIII.

DISCUSSION OF THE DATA OF EYE COLOUR.

Preliminary Remarks.—Data.—Persistence of Eye-Colour in the Population.—Fundamental Eye-Colours.—Principles of Calculation.—Results.

Preliminary Remarks.—In this chapter I will test the conclusions respecting stature by an examination into hereditary eye-colour. Supposing all female measures to have been transmuted to their male equivalents, it has been shown (1) that the possession of each unit of *peculiarity* of stature in a man [that is of each unit of difference from the average of his race] when the man's ancestry is unknown, implies the existence on an average of just one-third of a unit of that peculiarity in his "Mid-Parent," and consequently of the same amount in each of his parents; also just one-third of a unit in his Son; (2) that each unit of peculiarity in each ancestor taken singly, is reduced in transmission according to the following average scale;—a Parent transmits only $\frac{1}{4}$, and a Grand-Parent only $\frac{1}{16}$.

Stature and Eye-colour are not only different as qualities, but they are more contrasted in hereditary behaviour than perhaps any other common qualities. Parents of different Statures usually transmit a blended heritage to their children, but parents of different Eye-colours usually transmit an alternative heritage. If one parent is as much taller than the average of his or her sex as the other parent is shorter, the Statures of their children will be distributed, as we have already seen, in nearly the same way as if the parents had both been of medium height. But if one parent has a light Eye-colour and the other a dark Eye-colour, some of the children will be partly light and some will be dark; they will seldom be medium eye-coloured, like the children of medium eye-coloured parents. The blending in Stature is due to its being the aggregate of the quasi-independent inheritances of many separate parts, while Eye-colour appears to be much less various in its origin. If that notwithstanding this two-fold difference between the qualities of Stature and Eye-colour, the shares of hereditary contribution from the various ancestors are alike in the two cases, as I shall show that they are, we may with some confidence expect that the law by which those hereditary contributions appear to be governed, may be widely, and perhaps universally applicable.

Data.—My data for hereditary Eye-colour are drawn from the same collection of "Records of Family Faculties" ("R.F.F.") as those upon which the inquiries into hereditary Stature were principally based. I have analysed the general value of these data in respect to

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Stature, and shown that they were fairly trustworthy. I think they are somewhat more accurate in respect to Eye-colour, upon which family portraits have often furnished direct information, while indirect information has been in other cases obtained from locks of hair that were preserved in the family as mementos.

Persistence of Eye-colour in the Population.—The first subject of our inquiry must be into the existence of any slow change in the statistics of Eye-colour in the English population, or rather in that particular part of it to which my returns apply, that ought to be taken into account before drawing hereditary conclusions. For this purpose I sorted the data, not according to the year of birth, but according to generations, as that method best accorded with the particular form in which all my R.F.F. data are compiled. Those persons who ranked in the Family Records as the "children" of the pedigree, were counted as generation I.; their parents, uncles and aunts, as generation II.; their grandparents, great uncles, and great aunts, as generation III.; their great grandparents, and so forth, as generation IV. No account was taken of the year of birth of the "children," except to learn their age; consequently there is much overlapping of dates in successive generations. We may however safely say, that the persons in generation I. belong to quite a different period to those in generation III., and the persons in II. to those in IV. I had intended to exclude all children under the age of eight years, but in this particular branch of the inquiry, I fear that some cases of young children have been accidentally included. I would willingly have taken a later limit than eight years, but could not spare the data that would in that case have been lost to me.

A great variety of terms are used by the various compilers of the "Family Records" to express Eye-colours. I began by classifying them under the following eight heads;—1, light blue; 2, blue, dark blue; 3, grey, blue-green; 4, dark grey, hazel; 5, light brown; 6, brown; 7, dark brown; 8, black. Then I constructed Table 15.

The accompanying diagram will best convey the significance of the figures in Table 15. Considering that the groups into which the observations are divided are eight in number, the observations are far from being sufficiently numerous to justify us in expecting clean results; nevertheless the curves come out surprisingly well, and in accordance with one another. There can be little doubt that the change, if any, during four successive generations is very small, and much smaller than mere memory is competent to take note of. I therefore disregard a current popular belief in the existence of a gradual darkening of the British population, and shall treat the eye-colours of those classes of our race who have contributed the records, as having been statistically persistent during the period under discussion.

The concurrence of the four curves for the four several generations, affords internal evidence of the trustworthiness of the data. For supposing we had

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curves that exactly represented the true Eye-colours for the four generations, they would either be concurrent or they would not. If these curves were concurrent, the errors in the R.F.F. data must have been so curiously distributed as to preserve the concurrence. If these curves were not concurrent, then the errors in the R.F.F. data must have been so curiously distributed as to neutralise the non-concurrence. Both of these suppositions are improbable, and we must conclude that the curves really agree, and that the R.F.F. errors are not large enough to spoil the agreement. The close similarity of the two curves, derived respectively from the whole of the male and the whole of the female data, and the more perfect form of the curve derived from the aggregate of all the cases, are additional evidences in favour of the goodness of the data on the whole.

Fundamental Eye-colours.—It is agreed among most writers on the subject (*cf.* A. de Candolle) that the one important division of eye-colours is into the light and the dark. The medium tints are not numerous, but may be derived from any one of four distinct origins. They may be hereditary with no notable variation, they may be varieties of light parentage, they may be varieties of dark parentage, or they may be blends. Medium tints are classed in my list under the heading "4. Dark grey, hazel;" these form only 12·7 per cent. of all the observed cases. In medium tints, the outer portion of the iris is often of a dark grey colour, Percentages of the Various Eye-colours in Four Successive Generations.

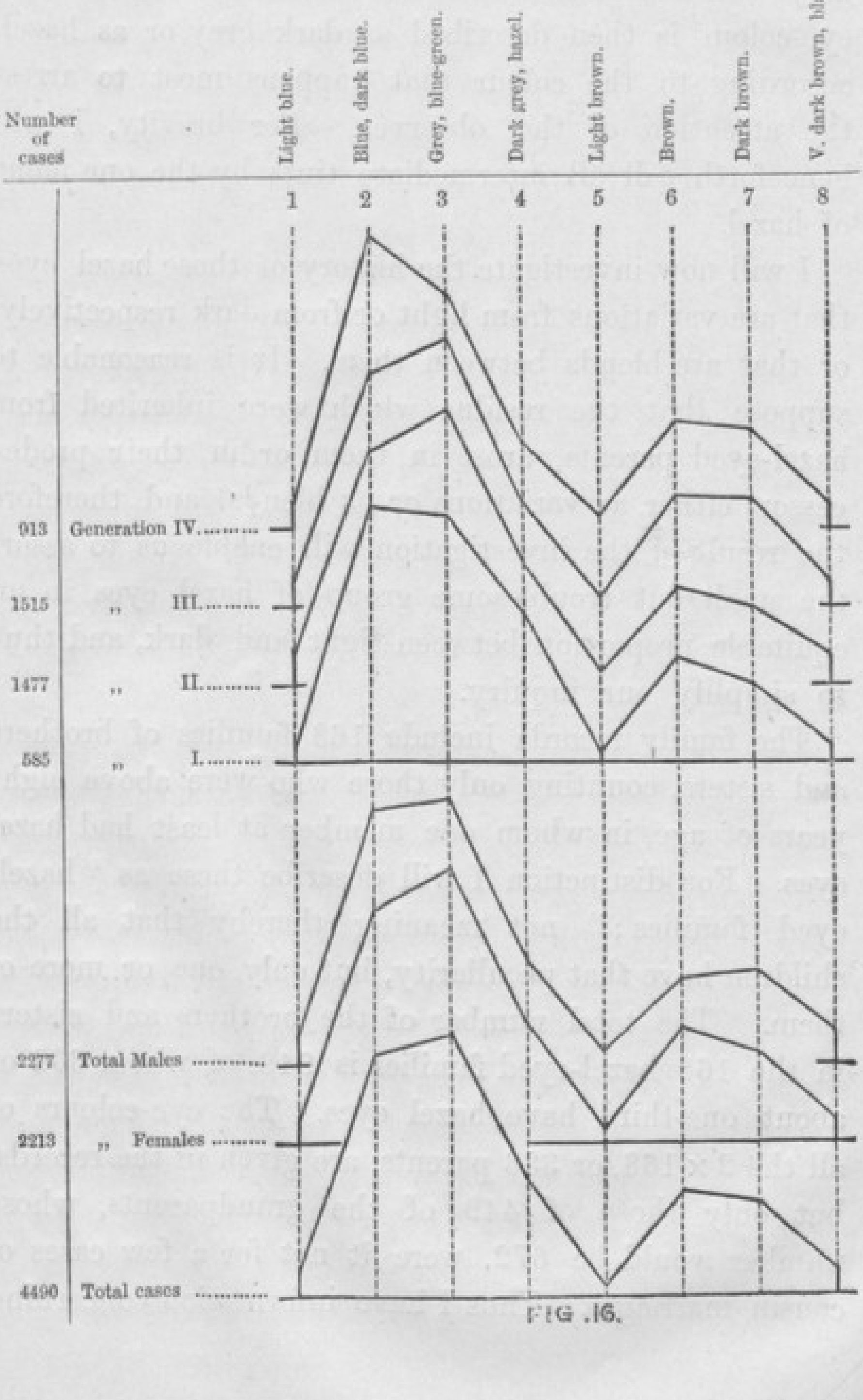

FIG. 16.

and the inner of a hazel. The proportion between the grey and the hazel varies in different cases, and the eye-colour is then described as dark grey or as hazel, according to the colour that happens most to arrest the attention of the observer. For brevity, I will henceforth call all intermediate tints by the one name of hazel.

I will now investigate the history of those hazel eyes that are variations from light or from dark respectively, or that are blends between them. It is reasonable to suppose that the residue which were inherited from hazel-eyed parents, arose in them or in their predecessors either as variations or as blends, and therefore the result of the investigation will enable us to assort the small but troublesome group of hazel eyes in an equitable proportion between light and dark, and thus to simplify our inquiry.

The family records include 168 families of brothers and sisters, counting only those who were above eight years of age, in whom one member at least had hazel eyes. For distinction I will describe these as "hazel-eyed families;" not meaning thereby that all the children have that peculiarity, but only one or more of them. The total number of the brothers and sisters in the 168 hazel-eyed families is 948, of whom 302 or about one-third have hazel eyes. The eye-colours of all the 2 × 168, or 336 parents, are given in the records, but only those of 449 of the grandparents, whose number would be 672, were it not for a few cases of cousin marriages. Thus I have information concerning about only two-thirds of the grandparents, but this will suffice for our purpose. The results are given in Table 16.

It will be observed that the distribution of eye-colour among the grandparents of the hazel-eyed families is nearly identical with that among the population at large. But among the parents there is a notable difference; they have a decidedly larger percentage of light eye-colour and a slightly smaller proportion of dark, while the hazel element is nearly doubled. A similar change is superadded in the children. The total result in passing from generations III. to I., is that the percentage of the light eyes is diminished from 60 or 61 to 45, therefore by one quarter of its original amount, and that the percentage of the dark eyes is diminished from 26 or 27 to 23, that is to about one-eighth of its original amount, the hazel element in either case absorbing the difference. It follows that the chance of a light-eyed parent having hazel off-spring, is about twice as great as that of a dark-eyed parent. Consequently, since hazel is twice as likely to be met with in any given light-eyed family as in a given dark-eyed one, we may look upon two-thirds of the hazel eyes as being fundamentally light, and one-third of them as fundamentally dark. I shall allot them rateably in that proportion between light and dark, as nearly as may be without using fractions, and so get rid of them. M. Alphonse de Candolle[1] has

[1] Hérédité de la couleur des yeux dans l'espèce humaine," par M. Alphonse de Condolle. "Arch. Sc. Phys. et Nat. Genèva," Aug. 1884, 3rd period. vol. xii. p. 97.

also shown from his data, that *yeux gris* (which I take to be the equivalent of my *hazel*) are referable to a light ancestry rather than to a dark one, but his data are numerically insufficient to warrant a precise estimate of the relative frequency of their derivation from each of these two sources.

In the following discussion I shall deal only with those fraternities in which the Eye-colours are known of the two Parents and of the four Grand-Parents. There are altogether 211 of such groups, containing an aggregate of 1023 children. They do not, however, belong to 211 different family stocks, because each stock which is complete up to the great grand-parents inclusive (and I have fourteen of these) is capable of yielding three such groups. Thus, group 1 contains *a*, the "children;" *b*, the parents; *c*, the grand-parents. Group 2 contains *a*, the father of the "children" and his brothers and his sisters; *b*, the parents of the father; *c*, the grand-parents of the father. Group 3 contains the corresponding selections on the mother's side. Other family stocks furnish two groups. Out of these and other data, Tables 19 and 20 have been made. In Table 19 I have classified the families together whose two parents and four grand-parents present the same combination of Eye-colour, no class, however, being accepted that contains less than twenty children. The data in this table enable us to test the *average* correctness of the law I desire to verify, because many persons and many families appear in the same class, and individual peculiarities

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tend to neutralise each other. In Table 20 I have separately classified on the same system all the families, 78 in number, that consist of six or more children. These data enable us to test the trustworthiness of the law as applied to *individual* families. It will be seen from my way of discussing them, that smaller fraternities than these could not be advantageously dealt with.

It will be noticed that I have not printed the number of dark-eyed children in either of these tables. They are implicitly given, and are instantly to be found by subtracting the number of light-eyed children from the total number of children. Nothing would have been gained by their insertion, while compactness would have been sacrificed.

The entries in the tables are classified, as I said, according to the various combinations of light, hazel, and dark Eye-colours in the Parents and Grand-Parents. There are six different possible combinations among the two Parents, and 15 among the four Grand-Parents, making 6 × 15, or 90 possible combinations altogether. The number of observations are of course by no means evenly distributed among the classes. I have no returns at all under more than half of them, while the entries of two light-eyed Parents and four light-eyed Grand-Parents are proportionately very numerous.

~~I shall not here discuss~~ The question of marriage selection in respect to Eye-colour, ~~which~~ is a less simple statistical question than at a first sight it may appear to be.

(has already been briefly discussed in p. 86. It)

Principles of Calculation.—I have next to show how the expectation of Eye-colour among the children of a given family is to be calculated on the basis of the same law that held in respect to stature, so that calculations of the probable distribution of Eye-colours may be made. They are those that fill the three last columns of Tables 19 and 20, which are headed I., II., and III., and are placed in juxtaposition with the observed facts entered in the column headed "Observed." These three columns contain calculations based on data limited in three different ways, in order the more thoroughly to test the applicability of the law that it is desired to verify. Column I. contains calculations based on a knowledge of the Eye-colours of the Parents only; II. contains those based on a knowledge of those of the Grand-Parents only; III. contains those based on a knowledge of those both of the Parents and of the Grand-Parents, and of them only.

I. Eye-colours given of the two Parents—

Let the letter S be used as a symbol to signify the subject (or person) for whom the expected heritage is to be calculated. Let F stand for the words "a parent of S;" G_1 for "a grandparent of S;" G_2 for "a great-grandparent of S," and so on.

We must begin by stating the problem as it would stand if Stature was under consideration, and then modify it so as to apply to Eye-colour. Suppose then, that the amount of the peculiarity of Stature possessed by F is equal to D, and that nothing whatever

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is known with certainty of any of the ancestors of S except F. We have seen that though nothing may actually be known, yet that something definite is implied about the ancestors of F, namely, that each of his two parents (who will stand in the order of relationship of G_1 to S) will on the average possess $\frac{1}{3}$D. Similarly that each of the four grandparents of F (who will stand in the order of G_2 to S) will on the average possess $\frac{1}{9}$D, and so on. Again we have seen that F, on the average, transmits to S only $\frac{1}{4}$ of his peculiarity; that G_1 transmits only $\frac{1}{16}$; G_2 only $\frac{1}{64}$, and so on. Hence the aggregate of the heritages that may be expected to converge through F upon S, is contained in the following series:—

$$D\left\{\frac{1}{4}+2\left(\frac{1}{3}\times\frac{1}{2^4}\right)+4\left(\frac{1}{9}+\frac{1}{2^6}\right)+\text{ \&c. }\right\}$$
$$=D\left\{\frac{1}{2^2}+\frac{1}{2^3.3}+\frac{1}{2^4.3^2}+\text{ \&c. }\right\}=D\times 0{\cdot}30.$$

That is to say, each parent must in this case be considered as contributing 0·30 to the heritage of the child, or the two parents together as contributing 0·60, leaving an indeterminate residue of 0·40 due to the influence of ancestry about whom nothing is either known or implied, except that they may be taken as members of the same race as S.

In applying this problem to Eye-colour, we must bear in mind that the fractional chance that each member of a family will inherit either a light or a dark Eye-colour, must be taken to mean that that same fraction

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of the total number of children in the family will probably possess it. Also, as a consequence of this view of the meaning of a fractional chance, it follows that the residue of 0·40 must be rateably assigned between light and dark Eye-colour, in the proportion in which those Eye-colours are found in the race generally, and this was seen to be (see Table 16) as 61·2 : 26·1 ; so I allot 0·28 out of the above residue of 0·40 to the heritage of light, and 0·12 to the heritage of dark. When the parent is hazel-eyed I allot $\frac{2}{3}$ of his total contribution of 0·30, *i.e.*, 0·20 to light, and $\frac{1}{3}$, *i.e.* 0·10 to dark. These chances are entered in the first pair of columns headed I. in Table V.

The pair of columns headed I. in Table 18 shows the way of summing the chances that are given in the columns that have a similar heading in Table 17. By the method there shown, I calculated all the entries that appear in the columns with the heading I. in Tables 19 and 20.

II. Eye-colours given of the four Grand Parents—

Suppose D to be possessed by G_1 and that nothing whatever is known with certainty of any other ancestor of S. Then it has been shown that the child of G_1 (that is F) will possess $\frac{1}{3}$D ; that each of the two parents of G_1 (who stand in the relation of G_2 to S) will also possess $\frac{1}{3}$D ; that each of the four grandparents of G_1 (who stand in the relation of G_3 to S) will possess $\frac{1}{9}$D, and so on. Also it has been shown that the shares of their several peculiarities that will on the average be transmitted by F, G_1, G_2, &c., are $\frac{1}{4}$, $\frac{1}{16}$, $\frac{1}{64}$, &c.,

respectively. Hence the aggregate of the probable heritages from G_1 are expressed by the following series :—

$$D\left\{\frac{1}{3}\times\frac{1}{2^2}+1\times\frac{1}{2^4}+\frac{1}{3}\times 2\times\frac{1}{2^6}+\frac{1}{9}\times 4\times\frac{1}{2^8}+\&c.\right\}$$

$$=D\left\{\frac{1}{12}+\left(\frac{1}{2^4}+\frac{1}{3\times 2^5}+\frac{1}{3^2\times 2^6}+\&c.\right)\right\}=D\times\left(\frac{1}{12}+\frac{3}{40}\right)=D\times 0{\cdot}16.$$

So that each grandparent contributes on the average 0·16 (more exactly 0·1583) of his peculiarity to the heritage of S, and the four grandparents contribute between them 0·64, leaving 36 indeterminate, which when rateably assigned gives 0·25 to light and 0·11 to dark. A hazel-eyed grandparent contributes, according to the ratio described in the last paragraph, 0·10 to light and 0·06 to dark. All this is clearly expressed and employed in the columns II. of Tables 17 and 18.

III. Eye-colours given of the two Parents and four Grand-Parents—

Suppose F to possess D, then F taken alone, and not in connection with what his possession of D might imply concerning the contributions of the previous ancestry, will contribute an average of 0·25 to the heritage of S. Suppose G_1 also to possess D, then his contribution together with what his possession of D may imply concerning the previous ancestry, was calculated in the last paragraph as $D\times\frac{3}{40}=D\times 0{\cdot}075$. For the convenience of using round numbers I take this as $D\times 0{\cdot}08$. So the two parents contribute between

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them 0·50 of the peculiarity of S, the four grand-parents together with what they imply of the previous ancestry contribute 0·32, being an aggregate of 0·82, leaving a residue of 0·18 to be rateably assigned as 0·12 to light, and 0·6 to dark. A hazel-eyed Parent is here reckoned as contributing 0·16 to light and 0·9 to dark; a hazel-eyed Grand-Parent as contributing 0·5 to light and 0·3 to dark. All this is tabulated in Table 17, and its working explained by an example in the columns headed III. of Table 18.

Results.—A mere glance at Tables 19 and 20 will show how surprisingly accurate the predictions are, and therefore how true the basis of the calculations must be. Their *average* correctness is shown best by the totals in Table 19, which give an aggregate of calculated numbers of light-eyed children under Groups I., II., and III. as 623, 601, and 614 respectively, when the observed numbers were 629; that is to say, they are correct in the ratios of 99, 96, and 98 to 100.

Their trustworthiness when applied to *individual* families is shown as strongly in Table 20 whose results are conveniently summarised in Table 18. I have there classified the amounts of error in the several calculations: thus if the estimate in any one family was 3 light-eyed children, and the observed number was 4, I should count the error as 1·0. I have worked to one place of decimals in this table, in order to bring out the different shades of trustworthiness in the three sets of calculations, which thus become very apparent. It will be seen that the calculations in Class III. are by far the most precise. In more than one-half of those calculations the error does not exceed 0·5, whereas in I. and II. more than three-quarters of them are wrong to at least that amount. Only one-quarter of Class III., but somewhere about the half of Classes I. and II., are more than 1·1 in error. In comparing I. with II., we find I. to be slightly but I think distinctly the superior estimate. The relative accuracy of III. as compared with I. and II., is what we should have expected, supposing the basis of the calculations to be true, because the additional knowledge utilised in III., over what is turned to account in I. and II., must be an advantage.

My returns are insufficiently numerous and too subject to uncertainty of observation, to make it worth while to submit them to a more rigorous analysis, but the broad conclusion to which the present results irresistibly lead, is that the same peculiar hereditary relation that was shown to subsist between a man and each of his ancestors in respect to the quality of Stature, also subsists in respect to that of Eye-colour.

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CHAPTER IX.

THE ARTISTIC FACULTY.

Data.—Sexual Distribution.—Marriage Selection.—Regression.—Effect of Bias in Marriage.

Data.—It is many years since I described the family history of the great Painters and Musicians in *Hereditary Genius.* The inheritance of much less exceptional gifts of Artistic Faculty will be discussed in this chapter, and from an entirely different class of data. They are the answers in my R.F.F collection, to the question of "Favourite pursuits and interests? Artistic aptitudes?"

The list of persons who were signalised as being especially fond of music and drawing, no doubt includes many who are artistic in a very moderate degree. Still they form a fairly well defined class, and one that is easy to discuss because their family history is complete. In this respect, they are much more suitable subjects for statistical inquiry than the great Painters and Musicians, whose biographers usually say little or nothing of their non-artistic relatives.

The object of the present chapter is not to give a reply to the simple question, whether or no the Artistic faculty tends to be inherited. A man must be very crotchety or very ignorant, who nowadays seriously doubts the inheritance either of this or of any other faculty. The question is whether or no its inheritance follows a similar law to that which has been shown to govern Stature and Eye-colour, and which has been worked out with some completeness in the foregoing chapters. Before answering this question, it will be convenient to compare the distribution of the Artistic faculty in the two sexes, and to learn the influence it may excercise on marriage selection.

I began by dividing my data into four classes of aptitudes; the first was for music alone; the second for drawing alone; the third for both music and drawing; and the fourth includes all those about whose artistic capacities a discreet silence was observed. After prefatory trials, I found it so difficult to separate aptitude for music from aptitude for drawing, that I determined to throw the three first classes into the single group of Artistic. This and the group of the Non-Artistic are the only two divisions now to be considered.

A difficulty presented itself at the outset in respect to the families that included boys, girls, and young children, whose artistic tastes and capacities can seldom be fairly judged, while they are liable to be appraised too favourably by the compiler of the Family records, especially if he or she was one of their parents. As the practice of picking and choosing is very hazardous in statistical inquiries, however fair our intentions may be, and as it in justice always excites suspicion, I decided, though with much regret at their loss, to omit the whole of those who were not adult.

Sexual Distribution.—Men and women, as classes, may differ little in their natural artistic capacity, but such difference as there is in adult life is somewhat in favour of the women. Table 9. *b.* contains 894 cases, 447 of men and 447 of women, divided into three groups according to the rank they hold in the pedigrees. These groups agree fairly well among themselves, and therefore their aggregate results may be freely accepted as trustworthy. They show that 28 per cent. of the males are Artistic and 72 are Not Artistic, and that there are 33 per cent. Artistic females to 67 who are Not Artistic. Part of this female superiority is doubtless to be ascribed to the large share that music and drawing occupy in the education of women, and to the greater leisure that most girls have, or take, for amusing themselves. If the artistic gifts of men and women are naturally the same, as the experience of schools where music and drawing are taught, apparently shows it to be, the small difference observed in favour of women in adult life would be a measure of the smallness of the effect of education compared to that of natural talent. Disregarding the distinction of sex, the figures in Table 9. *b.* show that the number of Artistic to Non-Artistic persons in the general population is in the proportion of $30\frac{1}{2}$ to $69\frac{1}{2}$. The data used in Table 22 refer to a considerably larger number of persons, and do not include more than two-thirds of those employed in Table 9. *b.*, and they make the proportion to be 31 to 69. So we shall be quite correct enough if we reckon that out of ten persons in the families of my R.F.F. correspondents, three on the average are artistic and seven are not.

Marriage Selection.—Table 9. *b.* enables us to ascertain whether there is any tendency, or any disinclination among the Artistic and the Not Artistic, to marry within their respective castes. It shows the observed frequency of their marriages in each of the three possible combinations; namely, both husband and wife artistic; one artistic and one not; and both not artistic. The Table also gives the calculated frequency of the three classes, supposing the pairings to be regulated by the laws of chance. There is I think trustworthy evidence of the existence of some slight disinclination to marry within the same caste, for signs of it appear in each of the three sets of families with which the Table deals. The total result is that there are only 36 per cent. of such marriages observed, whereas if there had been no disinclination but perfect indifference, the number would have been raised to 42. The difference is small and the figures are few, but for the above reasons it is not likely to be fallacious. I believe the facts to be, that highly artistic people keep pretty much to themselves, but that the very much larger body of moderately artistic people do not. A man of highly artistic temperament must look on those who are deficient in it, as barbarians; he would continually crave for a sympathy and response that such persons are incapable of giving. On the other hand, every quiet unmusical man must shrink a little from the idea of wedding himself to a grand piano in constant action, with its vocal and peculiar social accompaniments; but he might anticipate great pleasure in having a wife of a moderately artistic temperament, who would give colour and variety to his prosaic life. On the other hand, a sensitive and imaginative wife would be conscious of needing the aid of a husband who had enough plain common-sense to restrain her too enthusiastic and frequently foolish projects. If wife is read for husband, and husband for wife, the same argument still holds true.

Regression.—Having disposed of these preliminaries, we will now examine into the conditions of the inheritance of the Artistic Faculty. The data that bear upon it are summarised in Table 22, where I have not cared to separate the sexes, as my data are not numerous enough to allow of more subdivision than can be helped. Also, because from such calculations as I have made, the hereditary influences of the two sexes in respect to art appear to be pretty equal: as they are in respect to nearly every other characteristic, exclusive of diseases, that I have examined.

It is perfectly conceivable that the Artistic Faculty

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in any person might be somehow measured, and its amount determined, just as we may measure Strength, the power of Discrimination of Tints, or the tenacity of Memory. Let us then suppose the measurement of Artistic Faculty to be feasible and to have been often performed, and that the measures of a large number of persons were thrown into a Scheme.

It is reasonable to expect that the Scheme of Artistic Faculty would be approximately Normal in its proportions, like those of the various Qualities and Faculties whose measures were given in Tables 2 and 3.

It is also reasonable to expect that the same law of inheritance might hold good in the Artistic Faculty that was found to hold good both in Stature and in Eye colour; in other words, that value of Filial Regression would in this case also be $\frac{2}{3}$.

We have now to discover whether these assumptions are true without any help from direct measurement. The problem to be solved is a pretty one, and will illustrate the method by which many problems of a similar class have to be worked.

Let the graduations of the scale by which the Artistic Faculty is supposed to be measured, be such that the unit of the scale shall be equal to the Q of the Art-Scheme of the general population. Call the unknown M of the Art-Scheme of the population, P. Then, as explained in page 52, the measure of any individual will be of the form P + (± D), where D is the deviation from P. The first fact we have to deal with is, that only 30 per cent. of the population are Artistic. Therefore no person whose Grade in the Art-Scheme does not exceed 70° can be reckoned as Artistic. Referring to Table 8 we see that the value of D for the Grade of 70° is 0·78; consequently the art-measure of an Artistic person, when reckoned in units of the accepted scale, must exceed P + 0·78.

The average art-measure of all persons whose Grade is higher than 70°, may be obtained with sufficient approximation by taking the average of all the values given in Table 8, for every Grade or more simply, for every odd Grade from 71° to 99° inclusive. It will be found to be 1·71. Therefore an artistic person has, on the average, an art-measure of P + 1·71. We will consider persons of this measure to be representatives of the whole of the artistic portion of the Population. It is not strictly correct to do so, but for approximative purposes this rough and ready method will suffice, instead of the tedious process of making a separate calculation for each Grade.

The M of the Co-Fraternity born of a group of Mid-Parents whose measure is P + 1·71 will be P + ($\frac{2}{3}$ × 1·71) or (P + 1·4). We will call this value C. The Q of this or any other Co-Fraternity may be expected to bear approximately the same ratio to the Q of the general population, that it did in the case of Stature, namely, that of 1·5 to 1·7. Therefore the Q of the Co-Fraternity who are born of Mid-Parents whose Art-measure is C, will be 0·88.

The artistic members of this Co-Fraternity will be those whose measures exceed {P + 0·78}. We may write this value in the form of $\{(P + 1{\cdot}4) - 0{\cdot}36\}$, or $\{C - 0{\cdot}36\}$. Table 8 shows that the Deviation of $-0{\cdot}36$ is found at the Grade of 40°. Consequently 40 per cent. of this Co-Fraternity will be Non-Artistic and 60 per cent. will be Artistic. Observation Table 23 shows the numbers to be 36 and 64, which is a very happy agreement.

Next as regards the Non-Artistic Parents. The Non-Artistic portion of the Population occupy the 70 first Grades in the Art-Scheme, and may be divided into two groups; one consisting of 40 Grades, and standing between the Grades of 70° and 30°, or between the Grade of 50° and 20 Grades on either side of it, the average Art-measure of whom is P; the other group standing below 30°, whose average measure may be taken to be $P - 1{\cdot}71$, for the same reason that the group above 70° was taken as $P + 1{\cdot}71$. Consequently the average measure of the entire Non-Artistic class is

$$\tfrac{1}{70}\{(40 \times P) + 30\,(P - 1{\cdot}71)\}$$
$$= P - \tfrac{30}{70} \times 1{\cdot}71 = P - 0{\cdot}73.$$

Supposing Mid-Parents of this measure, to represent the entire Non-Artistic group, their offspring will be a Co-Fraternity having for their **M** the value of $P - \{\tfrac{2}{3} \times 0{\cdot}73\}$ or $P - 0{\cdot}49$, which we will call C′, and for their **Q** the value of 0·88 as before.

Such among them as exceed $\{P - 0{\cdot}78\}$, which we may write in the form of $\{(P - 0{\cdot}49) + (1{\cdot}27)\}$, or $\{C' + 1{\cdot}27\}$, are Artistic, and they are those who, according to Table 8, rank higher than the Grade 83°. In other words, 83 per cent. of the children of Non-

M

Artistic parents will be Non-Artistic, and the remainder of 17 per cent. will be Artistic. Observation gives the values of 79 and 21, which is a very fair coincidence.

When one parent is Artistic and the other Not, their joint hereditary influence would be the average of the above two cases; that is to say, $\frac{1}{2}$ (40 + 83), or $61\frac{1}{2}$ per cent. of their children would be Non-Artistic, and $\frac{1}{2}$ (60 + 17), or $38\frac{1}{2}$, would be Artistic. The observed numbers are 61 and 39, which agree excellently well.

We may therefore conclude that the same law of Regression, and all that depends upon it, which governs the inheritance both of Stature and Eye-colour, applies equally to the Artistic Faculty.

Effect of Bias in Marriage.—The slight apparent disinclination of the Artistic and the Not-Artistic to marry in their own caste, is hardly worth regarding, but it is right to clearly understand the extreme effect that might be occasioned by Bias in Marriage. Suppose the attraction of like to like to become paramount, so that each individual in a Scheme married his or her nearest available neighbour, then the Scheme of Mid-Parents would be practically identical with the Scheme drawn from the individual members of the population. In the case of Stature their Q would be 1·7 inch, instead of 1·7 divided by $\sqrt{2}$. The regression and subsequent dispersion remaining unchanged, the Q of the offspring would consequently be increased.

On the other hand, suppose the attraction of contrast to become suddenly paramount, so that Grade 99° paired in an instant with Grade 1°; next 98° with 2°; and so on in order, until the languid desires of 49° and 51° were satisfied last of all. Then every one of the Mid-Parents would be of precisely the same stature P. Consequently their Q would be zero; and that of the system of the Mid-Co-Fraternities would be zero also; hence the Q of the next generation would contract to the Q of a Co-Fraternity, that is to 1·5 inch.

Whatever might be the character or strength of the bias in marriage selection, so long as it remains constant the Q of the population would tend to become constant also, and the statistical resemblance between successive generations of the future Population would be ensured. The stability of the balance between the opposed tendencies of Regression and of Co-Fraternal expansion is due to the Regression increasing with the Deviation. Its effect is like that of a spring acting against a weight; the spring stretches until its gradually increasing resilient force balances the steady pull of the weight, then the two forces of spring and weight are in stable equilibrium. For, If the weight be lifted by the hand, it will obviously fall down again as soon as the hand is withdrawn; or again, if it be depressed by the hand, the resilience of the spring will become increased, and the weight will rise up again when ~~let go~~ it is left free to do so.

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CHAPTER X.

DISEASE.

Preliminary Problem.—Data.—Trustworthiness of R.F.F. Data.—Mixture of Inheritances.—CONSUMPTION: General Remarks; Parent to Child; Distribution of Fraternities; Severely Tainted Fraternities, Consumptivity.—Data for Hereditary Diseases.

The vital statistics of a population are those of ~~A POPULATION distributed according to age, is like~~ a vast army marching rank behind rank, across the treacherous table-land of life. Some of its members drop out of sight at every step, and a new rank is ever rising up to take the place vacated by the rank that preceded it, and which has already moved on. The population retains its peculiarities although the elements of which it is composed are never stationary, neither are the same individuals present at any two successive epochs. In these respects, a population may be compared to a cloud that seems to repose in calm upon a mountain plateau, while ~~though~~ a gale of wind is ~~may be~~ blowing over it. The outline of the cloud remains ~~is~~ unchanged, although its elements are in violent movement and in a condition of perpetual destruction and renewal. The well understood cause of such clouds is the deflection of a wind laden with invisible vapour, ~~upwards by~~ by means of the sloping flanks of the mountain up to a level at which the atmosphere is much colder and rarer than below. Part of the invisible vapour with which the wind was charged, becomes thereby condensed into the minute particles of water of which clouds are formed. After a while the process is reversed. The particles of cloud having been carried by the wind across the plateau, are swept down the other side of it again to a lower level, and during their descent they return into invisible vapour. Both in the cloud and in the population, there is on the one hand a continual supply and inrush of new individuals from the unseen; they remain awhile as visible objects, and then disappear. The cloud and the population are composed of elements that resemble each other in the brevity of their existence, while the general features of the cloud and of the population are alike in that they abide.

Preliminary Problem.—The proportion of the population that dies at each age, is well known, and the diseases of which they die are also well known, but the statistics of hereditary disease are as yet for the most part contradictory and untrustworthy.

It is most desirable as a preliminary to more minute inquiries, that the causes of death of a large number of persons should be traced during two successive generations in somewhat the same broad way that Stature and several other peculiarities were traced in the pre-

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ceding chapters. There are a certain number of recognized groups of disease, which we may call A, B, C, &c., and the proportion of persons who die of these diseases in each of the two generations is the same. The preliminary question to be determined is whether and to what extent those who die of A in the second generation, are more or less often descended from those who died of A in the first generation, than would have been the case if disease were neither hereditarily transmitted nor clung to the same families for any other reason. Similarly as regards B, C, D, and the rest.

This inquiry would be more difficult than those hitherto attempted, because longevity and fertility are both affected by the state of health, and the circumstances of home life and occupation have a great effect in causing and in checking disease. Also because the father and mother are found in some notable cases to contribute disease in very different degrees to their male and female descendants.

I had hoped even to the last moment, that my collection of Family Records would have contributed in some small degree towards answering this question, but after many attempts I find them too fragmentary for the purpose. It ~~is~~ was a necessary ~~for~~ condition of success to have the completed life-history, ~~of two generations; that is~~ of many Fraternities who were born some seventy or more years ago, that is, during the earlier part of this century, ~~and that of all~~ as well as those of their parents, and all their uncles and aunts. My Records contain excellent material of a later date, that will be valuable in future years; but they must bide their time; they are insufficient for the period in question. By attempting to work with incompleted life histories the risk of serious error is incurred.

Data.—The Schedule in Appendix G, which is illustrated in more detail by Tables A and B that follow it, shows the amount of information that I had hoped to obtain from those who were in a position to furnish complete returns. It relates to the "Subject" of the pedigree and to each of his 14 direct ancestors, up to the great-grandparents inclusive, making in all 15 persons. Also, to the Fraternities of which each of their 15 persons was a member. Reckoning the total average number of persons in each fraternity at 5, which is under the mark for my R.F.F collection, questions were thus asked concerning an average of 75 different persons in each family. The total number of the Records that I am able to use, is about 160; so the aggregate of the returns of disease ought to have been about twelve thousand, and should have included the causes of death of perhaps 6,000 of them. As a matter of fact, I have only about one-third of the latter number.

Trustworthiness of R.F.F. data.—The first object was to ascertain the trustworthiness of the medical information sent to me. There is usually much disinclination among families to allude to the serious diseases that they fear to inherit, and it was necessary to learn whether this tendency towards suppression notably vitiated the returns. The test applied was both simple and just.

If consumption, cancer, drink and suicide, appear among the recorded cases of death less frequently than they do in ordinary tables of mortality, then a bias towards suppression could be proved and measured, and would have to be reckoned with; otherwise the returns might be accepted as being on the whole honest and out-spoken. I find the latter to be the case. Sixteen per cent. of the causes of death (or 1 in $6\frac{1}{2}$) are ascribed to consumption, 5 per cent. to cancer, and nearly 2 per cent. to drink and to suicide respectively. Insanity was not specially asked about, as I did not think it wise to put too many disagreeable questions, however it is often mentioned. I dare say that it, or at least eccentricity, is not unfrequently passed over. Careful accuracy in framing the replies appears to have been the rule rather than the exception. In the preface to the blank forms of the *Records of Family Faculties* and elsewhere, I had explained my objects so fully and they were so reasonable in themselves, that my correspondents appear to have entered with interest into what was asked for, and to have shown themselves willing to trust me freely with their family histories. They seem generally to have given all that was known to them, after making much search and many inquiries, and after due references to registers of deaths. The insufficiency of their returns proceeds I feel sure, much less from a desire to suppress unpleasant truths than from pure ignorance, and the latter is in no small part due to the scientific ineptitude of the mass of the members of the medical profession two and more generations ago, when even the stethoscope was unknown. They were then incompetent to name diseases correctly.

Mixture of Inheritances.—The first thing that struck me after methodically classifying the diseases of each family, in the form shown in the Schedule, was their great intermixture. The Tables A and B in Appendix G are offered as ordinary specimens of what is everywhere to be found. They are actual cases, except that I have given fancy names and initials, and for further concealment, have partially transposed the sexes. Imagine an intermarriage between any two in the lower division of these tables, and then consider the variety of inheritable disease to which their children would be liable! The problem is rendered yet more complicated by the metamorphoses of disease. The disease A in the parent does not necessarily appear, even when inherited, as A in the children. We know very little indeed about the effect of a mixture of inheritable diseases, how far they are mutually exclusive and how far they blend; or how far when they blend, they change into a third form. Owing to the habit of free inter-marriage no person can be exempt from the inheritance of a vast variety of diseases or of special tendencies to them. Deaths by mere old age and the accompanying failure of vital powers without any well defined malady, are very common in my collection, but I do not find that the children of persons who die of old age have any marked immunity from specific diseases.

There is a curious double appearance in the Records,

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the one of an obvious hereditary tendency to disease and the other of the reverse. There are far too many striking instances of coincidence between the diseases of the parents and of the children to admit of reasonable doubt of their being often inherited. On the other hand, when I hide with my hand the lower part of a page such as those in Tables A and B, and endeavour to make a forecast of what I shall find under my hand after studying the upper portion, I am ~~often~~ sometimes greatly mistaken. Very unpromising marriages have often led to fairly good results, especially where the parental disease is one that usually breaks out late in life, as in the case of cancer. The children may then enjoy a fair length of days and die in the end of some other disease; although if that disease had been staved off it is quite possible that the cancer would ultimately have appeared. I have two remarkable instances of this. In one of them, three grandparents out of four died of cancer. In each of the fraternities of which the father and mother were members, one and one person only, died of it. As to the children, although four of them have lived to past seventy years, not one has shown any sign of cancer. The other case differs in details, but is equally remarkable. However diseased the parents may be, it is of course possible that the children may inherit the healthier constitutions of their remoter ancestry. Promising looking marriages are occasionally found to lead to a sickly progeny, but my materials are too scanty to permit of a thorough investigation of these cases.

The general conclusion thus far is, that owing to the hereditary tendencies in each person to disease being usually very various, it is rare that useful forecasts can be made concerning the future issue of any newly-married couple.

Consumption.

General Remarks.—The frequency of consumption in England being so great that one in at least every six or seven persons dies of it, and the fact that it usually appears early in life, and is therefore the less likely to be forestalled by any other disease, render it an appropriate subject for statistics. The fact that it may be acquired, although there has been no decided hereditary tendency towards it, introduces no serious difficulty, being more or less balanced by the opposite fact that it may be withstood by sanitary precautions although a strong tendency exists. Neither does it seem worth while to be hypercritical and to dwell overmuch on the different opinions held by experts as to what constitutes consumption. The ordinary symptoms are patent enough, and are generally recognized; so we may be content at first with lax definitions. At the same time, no one can be more strongly impressed than myself with the view that in proportion as we desire to improve our statistical work, so we must be increasingly careful to divide our material into truly homogeneous groups, in order that all the cases contained in the same group shall be alike in every important particular, differing only in petty details. This is far more important than adding to the number of cases. My material admits of no such delicacy of division; nevertheless it leads to some results worth mentioning.

In sorting my cases, I included under the head of Consumption all the causes of death described by one or the other following epithets, attention being also paid to the context, and to the phraseology used elsewhere by the same writer:—Consumption; Phthisis; Tubercular disease; Tuberculosis; Decline; Pulmonary, or lung disease; Lost lung; Abscess on lung; Hæmorrhage of lungs (fatal); Lungs affected (here especially the context was considered). All of these were reckoned as actual Consumption.

In addition to these there were numerous phrases of doubtful import that excited more or less reasonable suspicion. It may be that the disease had not sufficiently declared itself to justify more definite language, or else that the phrase employed was a euphemism to veil a harsh truth. Paying still more attention to the context than before, I classed these doubtful cases under three heads:—(1) Highly suspicious; (2) Suspicious; (3) Somewhat suspicious. They were so rated that four cases of the first should be reckoned equivalent to three cases of actual consumption, four cases of the second to two cases, and four of the third to one case.

The following is a list of some of the phrases so dealt with. The occasional appearance of the same phrase under different headings is due to differences in the context:—

1. Highly suspicious:—Consumptive tendency, Con-

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sumption feared, and died of bad chill. Chest colds with pleurisy and congestion of lungs. Died of an attack on the chest. Always delicate. Delicate lungs. Hæmorrhage of lungs. Loss of part of lung. Severe pulmonary attacks and chest affections.

2. Suspicious :—Chest complaints. Delicate chest. Colds, cough and bronchitis. Delicate, and died of asthma. Scrofulous tendency.

3. Somewhat suspicious :—Asthma when young. Pulmonary congestion. Not strong; anæmic. Delicate. Colds, coughs. Debility; general weakness. [The context was especially considered in this group.]

(Parent to Child.—)

I have only four cases in which both parents were consumptive; these will be omitted in the following remarks; but whether included or not, the results would be unaltered, for they run parallel to the rest.

There are 66 marriages in which one parent was consumptive; they produced between them 413 children, of whom 70 were actually consumptive; others who being suspiciously so in various degrees, and who when reckoned according to the above method of computation, amounted to 37 cases in addition, forming a total of 107. In other words, 26 per cent. of the children were consumptive. Where neither parent was consumptive, the proportion was as high as 18 or 19 per cent. in the ~~23~~ small batch of cases that I tried, but this is clearly too much, because that of the general population is about 16 per cent. Again, by taking each fraternity separately and dividing the quantity of consumption in it by the number of its members, I obtained the average consumptive taint of each fraternity. For instance, if in a fraternity of 10 members there was one actually consumptive member and four "somewhat suspiciously" so, it would count as a fraternity of ten members, of whom two were actually consumptive, and the average taint of the fraternity would be reckoned at one-fifth part of the whole or as 20 per cent.

Treating each fraternity separately in this way, and then averaging the whole of them, the mean taint of the children of one consumptive parent was made out to be 28 per cent.

Distribution of Fraternities.—Next I arranged the fraternities in such way as would show whether, if we reckoned each fraternity as a unit, their respective amounts of consumptive taint were distributed "normally" or not. The results are contained in line A of the following table :—

PERCENTAGE OF CASES HAVING VARIOUS PERCENTAGES OF TAINT.

	Percentages of Taint.					
	0 and under 9	10 and under 19	20 and under 29	30 and under 39	40 and above	Total.
A. 66 cases, one parent consumptive.	27	20	9	15	29	100
B. 84 cases, one brother consumptive.	49	14	10	13	14	100

They struck me as so remarkable, in the way shortly to be explained, that I proceeded to verify them by as different a set of data as my Records could afford. I took every fraternity in which at least one member was consumptive, and treated them in a way that would answer the following question. "One member of a fraternity, whose number is unknown, is consumptive; what is the chance that a named but otherwise unknown brother of that man will be consumptive also?" The fraternity that was taken above as an example, would be now reckoned as one of nine members, of whom one was actually consumptive. There were 84 fraternities available for the present purpose, and the results are given in the line B of the table. The data in A and B somewhat overlap, but for the most part they differ.

They concur in telling the same tale, namely, that it is totally impossible to torture the figures so as to make them yield the single-humped "Curve of Frequency" (Fig. 3 p.38). They make a distinctly double-humped curve, whose outline is no more like the normal curve than the back of a Bactrian camel is to that of an Arabian camel. Consumptive taints reckoned in this way are certainly not "normally" distributed. They depend mainly on one or other of two groups of causes, one of which tends to cause complete immunity and the other to cause severe disease, and these two groups do not blend freely together. Consumption tends to be transmitted strongly or not at all, and in this respect it resembles the baleful influence ascribed to cousin

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marriages, which appears to be very small when statistically discussed, but of whose occasional severity most persons have observed examples.

I interpret these results as showing that consumption is largely acquired, and that the hereditary influence of an acquired attack is small when there is no accompanying "malformation". This last phrase is intended to cover not only a narrow chest and the like, but whatever other abnormal features may supply the physical basis upon which consumptive tendencies depend, and which I presume to be as hereditary as any other malformations.

Severely-tainted Fraternities.—Pursuing the matter further, I selected those fraternities in which consumption was especially frequent, and in which the causes of the deaths both of the Father and of the Mother were given. They were 14 in number, and contained between them a total of 102 children, of whom rather more than half died before the age of 40. Though records of infant deaths were asked for, I doubt if they have been fully supplied. As 102 differs little from 100, the following figures will serve as percentages: 42 died of actual consumption and 11 others of lung disease variously described. Only one case was described as death from heart disease, but weakness of the heart during life was spoken of in a few cases. The remaining causes of death were mostly undescribed, and those that were named present no peculiarity worth notice. I then took out the causes of death of the

Fathers and Mothers and their ages at death, and severally classified them as in the Table below. It must be understood that there is nothing in the Table to show how the persons were paired. The Fathers are treated as a group by themselves, and the Mothers as a separate group, also by themselves.

CAUSES OF DEATH OF THE PARENTS OF THOSE FRATERNITIES IN WHICH CONSUMPTION GREATLY PREVAILED.

Father.	Age at death.	Mother.	Age at death.	Order of ages at death. F.	Order of ages at death. M.
Asthma	70	Consumption	40	51	40
Bronchitis	89	Consumption	43	52	42
Inf. kidneys and bronchitis	73	Consumption	47	59	43
Abscess of liver through lung	(alive)	Consumption	55	62	44
Heart	68	Consumption	65	68	47
Heart	74	Consumption	66	70	50
Apoplexy	62	Water on chest	60	73	58
Apoplexy	75	Weak chest	(alive)	74	60
Apoplexy	78	(1 br. and 2 ss. *d.* of cons.)		74	65
Decay	74	Hæmorrhage of lungs	44	75	66
Cancer	52	Ossification of heart	50	76	73
Senile gangrene	76	Nose bleeding	83	78	74
(2 bros. *d.* of cancer).		Cancer	42	89	83
Mortification of toe	59	Atrophy	73		
Accident	51	Age	74		
(3 bros. and 2 ss. *d.* of cons.)					

Very little account is given of the fraternities to which the fathers and mothers belong, and nothing of interest beyond what is included in the above.

The contrast is here most striking between the tendencies of the Father and Mother to transmit a serious consumptive taint to their children. The cases were selected without the slightest bias in favour of showing this result; in fact, such is the incapacity to see statistical facts clearly until they are pointed out, that I had no idea of the extraordinary tendency on the part of the mother to transmit consumption, as shown in this Table, until I had selected the cases and nearly finished sorting them. Out of the fourteen families, the mother was described as actually dying of consumption in six cases, of lung complaints in two others, and of having highly consumptive tendencies in another, making a total of nine cases out of the fourteen. On the other hand the Fathers show hardly any consumptive taints. One was described as of a very consumptive fraternity, though he himself died of an accident; and another who was still alive had suffered from an abscess of the liver that broke through the lungs. Beyond these there is nothing to indicate consumption on the Fathers' side.

Another way of looking at the matter is to compare the ages at death of the Mothers and of the Fathers respectively, as has been done at the side of the Table, when we see a notable difference between them, the Mid-age of the Mothers being 58, as against 73 of the Fathers.

The only other group of diseases that affords a fair number of instances (in my collection) in which fraternities are greatly affected ~~by it~~, are those of the Heart. The instances are only nine in number, but I give an analysis of them, not for any value of their own, but in order to bring the peculiarities of the consumptive fraternities more strongly into relief by means of comparison. In one of these there was no actual death from heart-disease ~~through it~~, though three had weak hearts and two others had rheumatic gout and fever. These nine families contained between them sixty-nine children, being at the rate of 7·7 to a family. The number of deaths from heart disease was 24; from ruptured blood vessels, 2; from consumption and lung disease, 8; from dropsy in various forms, 3; from apoplexy, paralysis, and epilepsy, 5; from suicide, 2; from cancer, 1. There is no obvious difference between the diseases of their Fathers and Mothers as shown in the Table, other than the smallness of the number of cases would account for. Their mid-ages at death were closely the same, 70 and 72, and the ages in the two groups run alike.

Causes of Death of the Parents of those Fraternities in which Heart Disease Prevailed.

Causes of death.	Ages at death.		Order of ages at death.	
	Father.	Mother.	F.	M.
Heart	59, 70	61, 63, 74	53	61
Apoplexy and paralysis	74, 78	62, 70, 72	55	62
Consumption	53	...	59	63
Asthma	70	...	70	70
Gout	55	...	70	72
Senile Gangrene	...	81	74	74
Tumour in liver	...	77	75	77
Cancer	75	...	78	81
Living	old.	...	old.	85
Unknown		85 2 bros. and 1 sis. *d.* of heart disease and 1 of paralysis *æt.* 40.		

I must leave it to medical men to verify the amount of truth that may be contained in what I have deduced from these results concerning the distinctly superior power of the mother over that of the father to produce a highly consumptive family. Any physician in large practice among consumptive cases could test the question easily by reference to his note-books. A "highly consumptive" fraternity may conveniently be defined as one in which at least half of its members have actually died of consumption, or else are so stricken that their ultimate deaths from that disease may be reckoned upon. Also to avoid statistical accidents, the fraternities selected for the inquiry should be large, consisting say of ~~five~~ six children and upwards. Of course the numerical proportions given by the above 14 fraternities are very rude indications indeed of the results to which a thorough inquiry might be expected to lead.

Accepting the general truth of the observation that consumptive mothers produce highly consumptive families much more commonly than consumptive fathers, it is easy to offer what seems to be an adequate explanation. Consumption is partly acquired by some form of contagion or infection, and is partly an hereditary malformation. So far as it is due to the latter in the wide sense already given to the word "malformation," it may perhaps be transmissible equally by either parent. But so far as it is contagious or infectious, we must recollect that the child is peculiarly exposed during all the time of its existence before birth, to contagion from its mother. During infancy, it lies perhaps for hours daily in its mother's arms, and afterwards lives much by her side, closely caressed, and breathing the tainted air of her sheltered rooms. The explanation of the fact that we have been discussing appears therefore to be summed up in the single word—Infection.

Consumptivity.—Before abandoning the topic of hereditary consumption, it may be well to discuss it from the same point of view that was taken when discussing the artistic temperament. Consumption being so common in this country that fully one person out of every six or seven die of it, and all forms of hereditary disease being intermixed through marriage, it follows that the whole population must be more or less tainted with consumption. That a condition which we may call "consumptivity," for want of a better word, may exist without showing any outward sign, is proved by the fact that as sanitary conditions worsen by ever so little, more persons are affected by the disease. It seems a fair view to take, that when the amount of consumptivity reaches a certain level, the symptoms of consumption declare themselves; that when it approaches but falls a little short of that level, there are threatening symptoms; that when it falls far below the level, there is a fallacious appearance of perfect freedom from consumptivity. We may reasonably proceed on the hypothesis that consumptivity might somehow be measured, and that if its measurement was made in each of any large group of persons, the measures would be distributed "normally."

So far we are on fairly safe ground, but now uncertainties begin upon which my data fail to throw sufficient light. Longevity, marriage, and fertility must all be affected by the amount of consumptivity, whereas in the case of the faculties hitherto discussed they are not affected to any sensible extent. It however happens that these influences tend to neutralize one another. It is true that consumptive persons die early, and many of them before a marriageable age. On the other hand, they certainly marry earlier as a rule than others, one cause of which lies in their frequent great attractiveness; and again, when they marry, they produce children more quickly than others. Consequently those who die even long before middle age, often contrive to leave large families. The greater rapidity with which the generations follow each other, is also a consideration of some importance. There is therefore a fair doubt whether a group of young persons destined to die of consumption, contribute considerably less to the future population than an equally large group who are destined to die of other diseases. I will at all events assume that consumptivity does not affect the numbers of the adult children, simply as a working hypothesis, and will afterwards compare its results with observed facts.

I should add that the question whether the sexes transmit consumption equally, lies outside the present work, at least for practical purposes; for whether they transmit it equally or not would not affect the results materially. Our list of data is therefore limited to these:—that 16 per cent. of the population die of consumption, that consumptivity is normally distributed, and that the law of hereditary regression from a deviation of three units on the part of either parent to an average of one unit in the child, may be supposed to apply here, just as it did to Stature and to the other subjects of the preceding chapters.

Let the scale by which consumptivity is measured be such that the Q of the general population = 1. Let its M = N, when measured on the same scale; the value of N is and will remain unknown. Let N + C be the number of units of consumptivity that just amount to actual consumption. Our data tell us that 16 per cent. of the population have an amount of consumptivity that exceeds N + C. On referring to Table 8, we find the value of C that corresponds to the Grade of (100°—16°), or of 84°, to be 1·47. Therefore whenever the consumptivity of a person exceeds N + 1·47, he has actual consumption.

Adding together the tabular values in Table 8 at all the odd grades above 84°, we shall find their average value to be 2·23. We may therefore assume (see p. 160) that a group of persons each of whom has a consumptivity of N + 2·23 will approximately represent all the grades above 84°. The Co-Fraternity descended from such a group will have an M whose value according to the law of Regression ought to be [N + $\frac{1}{3}$ (2·23)] or [N + 0·74] units.

Those members of the Co-Fraternity are consumptive whose consumptivity exceeds N + 1·47; these are the same as those whose deviation from [N + 0·74] which is the M of the Co-Fraternity, exceeds + 0·73 unit.

Let the Q of the Co-Fraternity be called n. The Grade at which this amount of deviation occurs should be found in Table 8 opposite to the value of 0·73 divided by n.

Next as regards the value to be assigned to n, we may be assured that the Q of a Co-Fraternity cannot exceed that of the general population. Therefore n cannot exceed 1. In the case of Stature the relation between the Q of the Co-Fraternity and that of the Population was found to be as 15 to 17. If the same proportion held good here, its value would be 0·9. This is I think too high an estimate for the following reasons. The variability of the Co-Fraternity depends on two groups of causes. First, on fraternal variability; which itself is due in part to mixed ancestry, and in part to variety of nurture in the same Fraternity, both before as well as after birth. Secondly, it depends upon the variety of ancestry and nurture in different Fraternities. As to the first of the two groups of causes, ~~nothing that is special to consumptivity occurs to me~~, but ~~as~~ to the second group, ~~there is a considerable difference~~. The household arrangements of vigorous, of moderately vigorous, and of invalided parents are not alike. I have already spoken of infection. There is also a tradition in families that are not vigorous, of the necessity of avoiding risks and of never entering professions that involve physical hardship. There is no such tradition in families who are vigorous. Thus there must be much greater variability in the environments of a group of persons taken from the population at large, than there is in a group of consumptive families. It would be quite fair to estimate the value of n at least as low as 0·8.

We have thus three values of n to try; viz. 1, 0·9, and 0·8, of which the first is scarcely possible and the last is much the most suitable of the other two. The corresponding values of 0·73 divided by n, are + 0.73, + 0·81, and + 0·91. Referring to Table 8 we find the Grades corresponding to those deviations to be 69, 71, and 73. We should therefore expect 69, 71, or 73 per cent. of the Co-Fraternity to be non-consumptive, according to the value of n we please to adopt, and the complement to those percentages, viz. 31, 29, or 27, to be consumptive. Observation p. 173, gave the value of 26 by one method of calculation, and 28 by another.

Too much stress must not be laid on this coincidence, because many important points had to be slurred over, as already explained. Still, the *prima facie* result is successful, and enables us to say that so far as this evidence goes, the statistical method we have employed in treating consumptivity seems correct, and that the law of heredity found to govern all the different faculties as yet examined, appears to govern that of consumptivity also, although the constants of the formula differ slightly.

Data for Hereditary Diseases.—The knowledge of the officers of Insurance Companies as to the average value of unsound lives is by the confession of many of

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them far from being as exact as is desirable. [See, for example, the discussion on a memoir by G. Humphreys, Actuary to the Eagle Insurance Company, read before the Institute of Actuaries, March, 1874.]

Considering the enormous money value concerned, it would seem well worth the while of the higher class of those offices to combine in order to obtain a collection of completed cases for at least two generations, or better still, for three; such as those in ~~Tables a~~ Examples A and B, Appendix G, but much fuller in detail. Being completed and anonymous, there could be little objection on the score of invaded privacy. They would have no perceptible effect on the future insurances of descendants of the families, even if they were identified, and they would lay the basis of a very much better knowledge of hereditary disease than we now possess, serving as a step for fresh departures. A main point is that the cases should not be picked and chosen to support any theory, but taken as they come to hand. There must be a vast amount of good material in existence at the command of the medical officers of Insurance Companies. If it were combined and made freely accessible, it would give material for many years' work to competent statisticians, and would be certain, judging from all experience of a like kind, to lead to unexpected results.

CHAPTER XI.

LATENT ELEMENTS.

Latent Elements not very numerous.—Pure Breed.—Simplification of Hereditary Inquiry.

Latent Elements not very numerous.—

It is not possible that more than one half of the varieties and number of each of the parental elements, latent or personal, can on the average subsist in the offspring. For if every variety contributed its representative, each child would on the average contain actually or potentially twice the variety and twice the number of the elements (whatever they may be) that were possessed at the same stage of its life by either of its parents, four times that of any one of its grandparents, 1024 times as many as any one of its ancestors in the 10th degree, and so on, which is absurd. Therefore as regards any variety of the entire inheritance, whether it be dormant or personal, the chance of its dropping out must on the whole be equal to that of its being retained, and only one half of the varieties can on the average be passed on by inheritance. Now we have seen that the *personal* heritage from either Parent is one quarter, therefore as the *total* heritage is one half, it follows that the Latent Elements must follow the same law of inheritance as the Personal ones. In other words, either Parent must contribute on the average only one quarter of the Latent Elements, the remainder of them dropping out and their breed becoming absolutely extinguished.

There seems to be much confusion in current ideas about the extent to which ancestral qualities are transmitted, supposing that what occurs occasionally must occur invariably. If a maternal grandparent be found to contribute some particular quality in one case, and a paternal grandparent in another, it seems to be argued that both contribute elements in every case. This is not a fair inference, as will be seen by the following illustration. A pack of playing cards consists, as we know, of 13 cards of each sort—hearts, diamonds, spades, and clubs. Let these be shuffled together and a batch of 13 cards dealt out from them, forming the deal, No. 1. There is not a single card in the entire pack that may not appear in these 13, but assuredly they do not all appear. Again, let the 13 cards derived from the above pack, which we will suppose to have green backs, be shuffled with another 13 similarly obtained from a pack with blue backs, and that a deal, No. 2, of 13 cards be made from the combined batches. The result will be of the same kind as before. Any card of either of the two original packs may be found in the deal, No. 2, but certainly not all of them. So I conceive it to be with hereditary transmission. No given pair can possibly transmit the whole of their ancestral qualities; on the other hand, there is probably no description of ancestor whose qualities have not been in some cases transmitted to a descendant.

The fact that certain ancestral forms persist in breaking out, such as the zebra-looking stripes on the donkey, is no argument against this view. The reversion may fairly be ascribed to precisely the same cause that makes it almost impossible to wholly destroy the breed of certain weeds in a garden, inasmuch as they are prolific and very hardy, and wage successful battle with their vegetable competitors whenever they are not heavily outmatched in numbers.

If the Personal and Latent Elements are transmitted on the average in equal numbers, it is difficult to suppose that there can be much difference in their variety.

Pure Breed.—In a perfectly pure breed, maintained during an indefinitely long period by careful selection, the tendency to regress towards the M of the general population, would disappear, so far as that tendency may be due to the inheritance of mediocre ancestral qualities, and not to causes connected with the relative stability of different types. The Q of Fraternal Deviations from their respective true Mid-Fraternities which we called b, would also diminish, because it is partly dependent on the children in the same family taking variously after different and unlike progenitors. But

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the difference between b in a mixed breed such as we have been considering, and the value which we may call β, which it would have in a pure breed, would be very small. Suppose the Prob: Error of the implied Stature of each separate Grand-Parent to be even as great as the Q of the general Population, which is 1·7 inch (it would be less, but we need not stop to discuss its precise value), then the Prob : Error of the implied Mid-Grand-Parental stature would be $\sqrt{\frac{1}{4}} \times 1{\cdot}7$ inch, or say 0·8 inch. The share 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 even than this small value, and we may disregard it. It results that b^2 is greater than $\beta^2 + 0{\cdot}04$. But we have found that $b = 1{\cdot}0$; therefore β is not greater than 0·98.

Simplification of Hereditary Inquiry. — These considerations make it probable that inquiries into human heredity may be much simplified. They assure us that the possibilities of inheritance are not likely to differ much more than those that are actually observed among the members of a large Fraternity. If then we have full life-histories of the Parents and of numerous Uncles and Aunts on both sides, we ought to have a very fair basis for hereditary inquiry. Information of this limited kind is incomparably more easy to obtain than that which I have hitherto striven for, namely, family histories during four successive generations. When the "children" in the pedigree are from 30 to

45 years of age, their own life-histories are sufficiently advanced to be useful, though they are incomplete, and it is still easy for them to compile good histories of their Parents, Uncles, and Aunts. Friends who knew them all would still be alive, and numerous documents such as near relations or personal friends preserve, but which are mostly destroyed at their decease, would still exist. If I were undertaking a fresh inquiry in order to verify and to extend my previous work, it would be on this basis. I should not care to deal with any family that did not number at least six adult children, and the same number of uncles and aunts on both the paternal and maternal sides. Whatever could be learnt about the grandparents and their brothers and sisters, would of course be acceptable, as throwing further light. I should however expect that the peculiarities distributed among any large Fraternity of Uncles and Aunts would fairly indicate the variety of the Latent Elements in the Parent. The complete heritage of the child, on the average of many cases, might then be assigned as follows: One quarter to the personal characteristics of the Father; one quarter to the average of the personal characteristics of the Fraternity taken as a whole, of whom the Father was one of the members; and similarly as regards the Mother's side.

CHAPTER XII.

SUMMARY.

The investigation now concluded is based on the fact that the characteristics of a population that is in harmony with its environment, may remain statistically identical during successive generations. This is true for every characteristic whether it be affected to a great degree by a natural selection, or only so slightly as to be practically independent of it. It was easy to see in a vague way, that an equation admits of being based on this fact; that the equation might serve to suggest a theory of descent, and that no theory of descent that failed to satisfy it could possibly be true.

A large part of the book is occupied with preparations for putting this equation into a working form. Obstacles in the way of doing so, which I need not recapitulate, appeared on every side; they had to be confronted in turns, and then to be either evaded or overcome. The final result was that the higher methods of statistics, which consist in applications of the law of Frequency of Error, were found eminently suitable for expressing the processes of heredity. By their aid, the desired equation was thrown into an exceedingly simple form of approximative accuracy, and it became easy to compare both it and its consequences with the varied results of observation, and thence to deduce numerical results.

A brief account of the chief hereditary processes occupies the first part of the book. It was inserted principally in order to show that a reasonable *à priori* probability existed, of the law of Frequency of Error being found to apply to them. It was not necessary for that purpose to embarrass ourselves with any details of theories of heredity beyond the fact, that descent either was particulate or acted as if it were so. I need hardly say that the idea, though not the phrase of particulate inheritance, is borrowed from Darwin's provisional theory of Pangenesis, but there is no need in the present inquiry to borrow more from it. Neither is it requisite to take Weissmann's views into account, unless I am mistaken as to their scope. It is freely conceded that particulate inheritance is not the only factor to be reckoned with in a complete theory of heredity, but that the stability of the organism has also to be taken into account. This is probably a factor of great importance in regulating the issue of highly bred animals, but it was not found to exercise any sensible influence on the results of the inquiries with which this book is chiefly concerned. Its existence has therefore been only noted, and not otherwise taken into account.

The data on which the results mainly depend had to be collected specially, as no suitable material for the purpose was, so far as I know, in existence. This was done by means of an offer of prizes some years since, that placed in my hands a collection of about 160 useful Family Records. These furnished an adequate though only just an adequate supply of the required data. In order to show the degree of dependence that might be placed on them they were subjected to various analyses, and the result proved to be even more satisfactory than might have been fairly hoped for. Moreover the errors in the Records probably affect different generations in the same way, and would thus be eliminated from the comparative results.

As soon as the character of the problem of Filial descent had become well understood, it was seen that a general equation of the same form as that by which it was expressed, also expressed the connection between Kinsmen in every degree. The unexpected law of universal Regression became a theoretical necessity, and on appealing to fact its existence was found to be conspicuous. If the word "peculiarity" be used to signify the difference between the amount of any faculty possessed by a man, and the average of that possessed by the population at large, then the law of Regression may be described as follows. Each peculiarity in a man is shared by his kinsmen, but *on the average* in a less degree. It is reduced to a definite fraction of its amount, quite independently of what its amount might be. The fraction differs in different orders of kinship, becoming smaller as they are more remote. When the kinship is so distant that its effects are not worth taking into account, the peculiarity of the man, however remarkable it may have been, is reduced to zero in his kinsmen. This apparent paradox is fundamentally due to the greater frequency of mediocre deviations than of extreme ones, between deviations separated by equal amounts.

Two causes affect family resemblance; the one is Heredity, the other is Circumstance. That which is transmitted is only a sample taken partly through the operation of "accidents," out of a store of otherwise unused material, and circumstance must always play a large part in the selection of the sample. Circumstance comprises all the additional accidents, and all the peculiarities of nurture both before and after birth, and every influence that may conduce to make the characteristics of one brother differ from those of another. Circumstances are more varied in Co-Fraternities than in Fraternities, and the Grandparents and previous ancestry of members of Co-Fraternities differ; consequently Co-Fraternals differ among themselves more widely than Fraternals.

The average contributions of each ancestor separately to the heritage of the child were determined apparently within narrow limits, for a couple of generations at least. The results proved to be very simple; they assign an average of one quarter from each parent, and one sixteenth from each grandparent. According to this geometrical scale continued indefinitely backwards, the total heritage of the child would be

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accounted for, but the factor of stability of type has to be reckoned with, and this has not yet been adequately discussed.

The ratio of filial Regression is found to be so bound up with co-fraternal variability, that when either is given the other can be calculated. There are no means of deducing the measure of fraternal variability solely from that of co-fraternal. They differ by an element of which the value is thus far unknown. Consequently the measure of fraternal variability has to be calculated separately, and this cannot be done directly, owing to the small size of human families. Four different and indirect methods of attacking the problem suggested themselves, but the calculations were of too delicate a kind to justify reliance on the R.F.F. data. Separate and more accurate measures, suitable for the purpose, had therefore to be collected. The four problems were then solved by their means, and although different groups of these measures had to be used with the different problems, the results were found to agree together.

The problem of expressing the relative nearness of different degrees of kinship, down to the point where kinship is so distant as not to be worth taking into account, was easily solved. It is merely a question of the amount of the Regression that is appropriate to the different degrees of kinship. This admits of being directly observed when a sufficiency of data are accessible, or else of being calculated from the values found in this inquiry. A table of these Regressions was given.

Finally, considerations were offered to show that latent elements probably follow the same law as personal ones, and that though a child may inherit qualities from any one of his ancestors (in one case from this one, and in another case from another), it does not follow that the store of hidden property so to speak, that exists in any parent, is made up of contributions from all or even very many of his ancestry.

Two other topics may be mentioned. Reason was given in p. 16 why experimenters upon the transmission of Acquired Faculty should not be discouraged on meeting with no affirmative evidence of its existence in the first generation, because it is among the grandchildren rather than among the children that it should be looked for. Again, it is hardly to be expected that an acquired faculty, if transmissible at all, would be transmitted without dilution. It could at the best be no more than a variation liable to Regression, which would probably so much diminish its original amount on passing to the grand children as to render it barely recognizable. The difficulty of devising experiments on the transmission of acquired faculties is much increased by these considerations.

The other subject to be alluded to is the fundamental distinction that may exist between two couples whose personal faculties are naturally alike. If one of the couples consist of two gifted members of a poor stock, and the other of two ordinary members of a gifted stock, the difference between them will betray itself in their offspring. The children of the former will tend to regress; those of the latter will not. The value of a good stock to the well-being of future generations is therefore obvious, and it is well to recall attention to an early sign by which we may be assured that a new and gifted variety possesses the necessary stability to easily originate a new stock. It is its refusal to blend freely with other forms. Some among the members of the same fraternity might possess the characteristics in question with much completeness, and the remainder hardly or not at all. If this alternative tendency was also witnessed among cousins, there could be little doubt that the new variety was a stable form, and therefore capable of being easily developed by interbreeding into a pure and durable variety.

TABLES.

Table 1.

Strength of Pull.
519 Males aged 23–26.
From measures made at the International Health Exhibition in 1884.

Strength of Pull.	No. of cases observed.	Percentages.	
		No. of cases observed.	Sums from beginning.
Under 50 lbs.	10	2	2
,, 60 ,,	42	8	10
,, 70 ,,	140	27	37
,, 80 ,,	168	33	70
,, 90 ,,	113	21	91
,, 100 ,,	22	4	95
Above 100 ,,	24	5	100
Total	519	100	

TABLE 2.

DATA FOR SCHEMES OF DISTRIBUTION of various qualities and faculties among the persons measured at the Anthropometric Laboratory in the International Exhibition of 1884.

Subject of measurement.	Age.	Unit of measurement.	Sex.	No. of persons in the group.	Values at the undermentioned Grades, from 0° to 100°. 5°	10°	20°	30°	40°	50°	60°	70°	80°	90°	95°
Height, standing, without shoes	23–51	Inches	M.	811	63·2	64·5	65·8	66·5	67·3	67·9	68·5	69·2	70·0	71·3	72·4
			F.	770	58·9	59·9	61·3	62·1	62·7	63·3	63·9	64·6	65·3	66·4	67·3
Height, sitting, from seat of chair	23–51	Inches	M.	1013	33·6	34·2	34·9	35·3	35·4	36·0	36·3	36·7	37 1	37·7	38·2
			F.	775	31·8	32·3	32·9	33·3	33·6	33·9	34·2	34·6	34·9	35·6	36·0
Span of arms	23–51	Inches	M.	811	65·0	66·1	67·2	68·2	69·0	69·9	70·6	71·4	72·3	73·6	74·8
			F.	770	58·6	59·5	60·7	61·7	62·4	63·0	63·7	64·5	65·4	66·7	68·0
Weight in ordinary indoor clothes	23–26	Pounds	M.	520	121	125	131	135	139	143	147	150	156	165	172
			F.	276	102	105	110	114	118	122	129	132	136	142	149
Breathing capacity.	23–26	Cubic Inches	M.	212	161	177	187	199	211	219	226	236	248	277	290
			F.	277	92	102	115	124	131	138	144	151	164	177	186
Strength of pull as archer with bow.	23–26	Pounds	M.	519	56	60	64	68	71	74	77	80	82	89	96
			F.	276	30	32	34	36	38	40	42	44	47	51	54
Strength of squeeze with strongest hand	23–26	Pounds	M.	519	67	71	76	79	82	85	88	91	95	100	104
			F.	276	36	39	43	47	49	52	55	58	62	67	72
Swiftness of blow	23–26	Ft. per second	M.	516	13·2	14·1	15·2	16·2	17·3	18·1	19·1	20·0	20·9	22·3	23·6
			F.	271	9·2	10·1	11·3	12·1	12·8	13·4	14·0	14·5	15·1	16·3	16·9
Sight, keenness of — by distance of reading diamond test-type	23–26	Inches	M.	398	13	17	20	22	23	25	26	28	30	32	34
			F.	433	10	12	16	19	22	24	26	27	29	31	32

Table 3.

Deviations from M in each of the series in Table 2, after reduction to a Scale in which Q′ = 1, where Q′ is the *Mean* of the observed Deviations at the Grades 20°, 30°, 70°, and 80°.

Subject of measurement.	Values of Q′	Unit of measurement in Table 2.	Sex.	No. of persons	Deviations reckoned in units of Q′. 5°	10°	20°	30°	40°	50°	60°	70°	80°	90°	95°
Height, standing, without shoes	1·72	Inches	M.	811	2·73	1·98	1·22	0·81	0·35	0	0·35	·76	1·22	1·98	2·61
	1·62		F.	770	2 71	2·10	1·23	·74	·37	0	·37	·80	1·23	1·91	2·46
Height, sitting, from seat of chair	0·95	Inches	M.	1013	2·52	1·89	1·15	·73	·63	0	·31	·73	1·15	1·79	2·31
	0·82		F.	775	2·55	1·95	1·22	·73	·36	0	·36	·85	1·22	2·07	2·55
Span of arms	2·07	Inches	M.	811	2·36	1·83	1·30	·82	·43	0	·33	·72	1·16	1·79	2 36
	1·87		F.	770	2·35	1·87	1·23	·69	·32	0	·37	·80	1·28	1·98	2·67
Weight in ordinary indoor clothes	10·00	Pounds	M.	520	2 20	1·80	1·20	·80	·40	0	·40	·70	1·30	2·20	2·90
	11·00		F.	276	1·80	1·60	1·10	·70	·40	0	·60	·90	1·30	1·80	2·40
Breathing capacity	24·50	Cubic Inches	M.	212	2·32	1·68	1·28	·80	·32	0	·28	.68	1·16	2·32	2·84
	19·00		F.	277	2·39	1·87	1·20	·73	·36	0	·31	·67	1·35	2·03	2·49
Strength of pull as archer with bow	7·50	Pounds	M.	519	2·39	1·86	1·33	·80	·40	0	·40	·80	1·06	1·99	2·92
	5·22		F.	276	1·92	1·06	·80	·53	·27	0	·27	·53	·93	1·46	1·86
Strength of squeeze with strongest hand	7·75	Pounds	M.	519	2·32	1·81	1·16	·77	·39	0	·39	·77	1·29	1·93	2·45
	7·50		F.	276	2 12	1·73	1·20	·66	·40	0	·40	·80	1·33	1·99	2·66
Swiftness of blow	2·37	Ft. per second	M.	516	2·06	1·68	1·22	·80	·34	0	·42	·80	1·18	1·77	2·31
	1·55		F.	271	2·71	2·13	1·35	·84	·38	0	·38	·71	1·10	1·87	2·26
Sight, keenness of— by distance of reading diamond test-type	4·00	Inches	M.	398	3·00	2·00	1·25	·75	·50	0	·25	·75	1·25	1·75	2·25
	5·22		F.	433	2·66	2·28	1·52	·95	·38	0	·38	·57	·95	1·33	1 52
Sums					43·11	33·12	21·96	13·65	7·00	0	6·57	13·34	21·46	33·96	43·82
Means					2·40	1·84	1·22	0·76	0·39	0	0·37	0·74	1·19	1·89	2·43
Means multiplied by 1·015, to change unit to Q = 1					2·44	1·87	1·24	0·77	0·40	0	0·38	0·75	1·21	1·92	2·47
Normal Values, when Q = 1					2·44	1·90	1·25	0·78	0·38	0	0·38	0·78	1·25	1·90	2·44

Tables 4 to 8 inclusive give data for drawing Normal Curves of Frequency and Distribution. They also show the way in which the latter is derived from the values of the Probability Integral.

The equation for the Probablity Curve[1] is $y = k\,e^{-h^2x^2}$ in which h is "the Measure of Precision." By taking k and h each as unity, the values in Table 4 are computed.

Table 4.

Data for a Normal Curve of Frequency.

$$y = e^{-x^2}$$

x	y	x	y	x	y	x	y
0	1·00	± 1·0	0·37	± 2·0	0·0183	± 3·0	0·0001
± 0·2	0·96	± 1·2	0·23	± 2·2	0·0079		
± 0·4	0·85	± 1·4	0·14	± 2·4	0·0032	± infinity	0·0000
± 0·6	0·70	± 1·6	0·78	± 2·6	0·0012		
± 0·8	0·53	± 1·8	0·40	± 2·8	0·0004		

Table 5.

Values of the Probability Integral, $\frac{2}{\sqrt{\pi}}\int_0^t e^{-t^2}\,dt$, for Argument t.

$t\ (=hx)$	·0	·1	·2	·3	·4	·5	·6	·7	·8	·9
0	0·00	0·11	0·22	0·33	0·43	0·52	0·60	0·68	0·74	0·80
1·0	0·843	0·880	0·910	0·934	0·952	0·966	0·976	0·984	0·989	0·923
2·0	·9953	·9970	·9981	·9989	·9993	·9996	·9998	·9999	·9999	·9999
infinite	1·0000									

When $t =$ ·4769 the corresponding tabular is ·50 ; therefore, ·4769 is the value of the "Probable Error."

[1] See Merriman *On the Method of Least Squares* (Macmillan, 1885), pp. 26, 186, where fuller Tables than 4, 5, and 6 will be found.

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TABLE 6.

Values of the Probability Integral for Argument $\frac{t}{0.4769}$; that is, when the unit of measurement = the Probable error.

Multiples of the Probable Error.	·0	·1	·2	·3	·4	·5	·6	·7	·8	·9
0	0·00	0·05	0·11	0·16	0·21	0·26	0·31	0·36	0·41	0·46
1·0	·50	·54	·58	·62	·66	·69	·72	·75	·78	·80
2·0	·82	·84	·86	.88	·89	·91	·92	·93	·94	·95
3·0	·957	.964	·969	·974	·978	·982	·985	·987	·990	·992
4·0	·9930	.9943	·9954	·9963	·9970	·9976	·9981	·9985	·9988	·9990
5·0	·9993	·9994	·9996	·9997	·9997	·9998	·9998	·9999	·9999	·9999
infinite	1·000									

Tables 5 and 6 show the proportion of cases in any Normal system, in which the amount of Error lies within various extreme values, the total number of cases being reckoned as 1·0. Here no regard is paid to the sign of the Error, whether it be *plus* or *minus*, but its amount is alone considered. The unit of the scale by which the Errors are measured, differs in the two Tables. In Table 5 it is the "Modulus," and the result is that the Errors in one half of the cases, that is in 0·50 of them lie within the extreme value (found by interpolation) of 0·4769, while the other half exceed that value. In Table 6 the unit of the scale is 0·4769. It is derived from Table 5 by dividing all the tabular entries by that amount. Consequently one half of the cases have Errors that do not exceed 1·0 in terms of the new unit, and that unit is the Probable Error of the System. It will be seen in Table 6 that the entry of ·50 stands opposite to the argument of 1·0.

If it be desired to transform Tables 5 and 6 into others that shall show the proportion of cases in which the *plus* Errors and the *minus* Errors respectively lie within various extreme limits, their entries would have to be halved.

Let us suppose this to have been done to Table 6, and that a new Table, which it is not necessary to print, has been thereby produced and which we will call 6*a*. Next multiply all the entries in the new Table by 100 in order to make them refer to a total number of 100 cases, and call this second Table 6*b*. Lastly make a converse Table to 6*b*; one in which the arguments of 6*b* become the entries, and the entries of 6*b* become the arguments. From this Table 7 is made. For example, in Table 6, opposite to the argument 1·0 the entry of ·50 is found; that entry becomes ·25 in 6*a*, and 25 in 6*b*. In Table 7 25 is the argument, and the corresponding entry is 25. The meaning of this is, that in 25 per cent. of the cases the greatest of the Errors just attains to ± 1·0. Similarly the Table shows that in 30 per cent. of the cases, the greatest of the Errors just attains to ± 1·25; in 40 per cent. to 1·90, and so on. These various percentages correspond to the centesimal Grades in a Curve of Distribution, when the Grade 0° is placed at the middle of the axis, which is the point where it is cut by the Curve, and where the other Grades are reckoned outwards on either hand, up to + 50° on the one side, and to — 50° on the other.

To recapitulate:—In order to obtain Table 7 from the primary Table 5, we first halve each of the entries in the body of Table 5, then we multiply each of the arguments by 100, and divide it by ·4769. Then we expand the Table by interpolations, so as to include among its entries every whole number from 1 to 99 inclusive. Selecting these and disregarding the rest, we turn them into the arguments of Table 7, and their corresponding arguments into the entries in Table 7.

Table 7.

Ordinates to Normal Curve of Distribution

on a scale whose unit = the Probable Error; and in which the 100 Grades run from 0° to +50° on the one side, and to −50° on the other.

Grades.	0	1	2	3	4	5	6	7	8	9
0	0·00	0·04	0·07	0·11	0·15	0.19	0·22	0·26	0·30	0·34
10	0·38	0·41	0·45	0·49	0·53	0·57	0·61	0·65	0·69	0·74
20	0·78	0·82	0·86	0·97	0·95	1·00	1·05	1·10	1·15	1·20
30	1·25	1·30	1·36	1·42	1·47	1·54	1·60	1·67	1·74	1·82
40	1·90	1·99	2·08	2·19	2·31	2·44	2·60	2·79	3·05	3·45

But in the Schemes, the 100 Grades do not run from—50° through 0° to + 50°, but from 0° to 100°. It is therefore convenient to modify Table 7 in a manner that will admit of its being used directly for drawing Schemes without troublesome additions or subtractions. This is done in Table 8, where the values from 50° onwards, and those from 50° backwards are identical with those from 0° to ± 50° in Table 7, but the first of half those in Table 8 are positive and the latter half are negative.

TABLE 8.

ORDINATES TO NORMAL CURVE OF DISTRIBUTION on a scale whose unit = the Probable Error, and in which the 100 Grades run from 0° to 100°.

Grades	0	1	2	3	4	5	6	7	8	9
0	−∞	−3·45	−3·05	−2·79	−2·60	−2·44	−2·31	−2·19	−2·08	−1·99
10	−1·90	−1·82	−1·74	−1·67	−1·60	−1·54	−1·47	−1·42	−1·36	−1·30
20	−1·25	−1·20	−1·15	−1·10	−1·05	−1·00	−0·95	−0·91	−0·86	−0·82
30	−0·78	−0·74	−0.69	−0·65	−0·61	−0·57	−0·53	−0·49	−0·45	−0·41
40	−0·38	−0·34	−0·30	−0·26	−0·22	−0·19	−0·15	−0·11	−0·07	−0·04
50	0·00	+0·04	+0·07	+0·11	+0·15	+0·19	+0·22	+0·26	−0·30	+0·34
60	+0·38	+0·41	+0·45	+0·49	+0·53	+0·57	+0·61	+0·65	+0·69	+0·74
70	+0·78	+0·82	+0·86	+0·91	+0·95	+1·00	+1·05	+1·10	+1·15	+1·20
80	+1·25	+1·30	+1·36	+1·42	+1·47	+1·54	+1·60	+1·67	+1·74	+1·82
90	+1·90	+1·99	+2·08	+2·19	+2·31	+2·44	+2·60	+2·79	+3·05	+3·45

Examples of the way in which Table 8 is to be read :—

The ordinate at 0° is − infinity ; at 10° it is −1·90 ; at 11° it is −1·82 ; at 25° it is −1·00 ; at 75° it is +1·00. The Table does not go beyond Grade 99°, where the ordinate is + 3·45. At the Grade 100°, the ordinate would be + infinity.

Table 9.

Marriage Selection in respect to Stature.

The 205 male parents and the 205 female parents are each divided into three groups—T, M, and S, and *t*, *m*, and *s*, respectively—that is, Tall, Medium, and Short (medium male measurements being taken as 67 inches, and upwards to 70 inches). The number of marriages in each possible combination between them ~~(see Table 9)~~ were then counted, with the result that men and women of contrasted heights, Short and Tall, or Tall and Short, married about as frequently as men and women of similar heights, both Tall or both Short; there were 32 cases of the one to 27 of the other.

S., t. 12 cases.	M., t. 20 cases.	T., t. 18 cases.
S., m. 25 cases.	M., m. 51 cases.	T., m. 28 cases.
S., s. 9 cases.	M., s. 28 cases.	T., s. 14 cases.

Short and tall, 12 + 14 = 32 cases.
Short and short, 9 }
Tall and tall, 18 } = 27 cases.

We may therefore regard the married folk as couples picked out of the general population at haphazard when applying the law of probabilities to heredity of stature.

Table 9a.

Marriage Selection in respect to Eye-Colour in 78 Parental Couples.

Eye Colour of		No. of cases observed.	Per Cents.				Eye Colour of Husband and Wife.
Husband	Wife.		Obs.	Chance.	Observed.	Chance.	
Light	Light	29	37	37	48	46	Alike
Hazel	Hazel	2	3	2			
Dark	Dark	6	8	7			
Light / Hazel	Hazel / Light	18	23	15	28	22	Half-contrasted
Hazel / Dark	Dark / Hazel	4	5	7			
Light / Dark	Dark / Light	19	24	32	24	32	Contrasted

The chance combinations in pairs are calculated for a population containing 61·2 per cent. of Light Eye-colour, 12·7 of Hazel, and 26·1 of Dark.

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TABLE 9B.

MARRIAGES OF THE ARTISTIC AND THE NOT ARTISTIC.

Rank in Pedigrees.	No. of persons.	Percentages.									
		Males.		Females.		Pairs of artistic and not artistic persons.					
						Marriages observed.			Chance combinations.		
		art.	not.	art.	not.	both art.	1 art. 1 not.	both not.	both art.	1 art. 1 not.	both not.
Parents	326	32	68	39	61	14	31	50	12	46	42
Paternal grandparents	280	27	73	30	70	12	31	57	8	41	51
Maternal grandparents	288	24	76	28	72	9	41	50	7	39	54
Totals and means	894	28	72	33	67	12	36	52	9	42	49
Tastes of Husband and Wife—alike						12 + 52 = 64			9 + 49 = 58		
,, ,, ,, contrasted						36			42		

TABLE 10.

EFFECT UPON ADULT CHILDREN OF DIFFERENCES IN HEIGHT OF THEIR PARENTS.

Difference in inches between the Heights of the Parents.	Proportion per 50 of cases in which the Heights[1] of the Children deviated to various amounts from the Mid-filial Stature of their respective families.					Number of Children whose Heights were observed. (Total 525.)
	Less than 1 inch.	Less than 2 inches.	Less than 3 inches.	Less than 4 inches.	Less than 5 inches.	
Under 1 inch	21	35	43	46	48	105
1 and under 2	23	37	46	49	50	122
2 ,, 3	16	34	41	45	49	112
3 ,, 5	24	35	41	47	49	108
5 and above	18	30	40	47	49	78

[1] Every female height has been transmuted to its male equivalent by multiplying it by 1·08, and only those families have been included in which the number of adult children amounted to six, at least.

NOTE.—When these figures are protracted into curves, it will be seen—(1) that they run much alike; (2) that their peculiarities are not in sequence; and (3) that the curve corresponding to the first line occupies a medium position. It is therefore certain that differences in the heights of the Parents have on the whole an inconsiderable effect on the heights of their Offspring.

Table 11 (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 or Values of M.
	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 72·5...	...	...	...	...	...	...	...	...	...	...	...	1	3	...	4[1]	5[1]	
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.

[1] I have reprinted this Table without alteration from that published in the *Proc. Roy. Soc.*, notwithstanding a small blunder since discovered in sorting the entries between the first and second lines. It is obvious that 4 children cannot have 5 Mid-Parents. The first line is not considered at all, on account of the paucity of the numbers it contains. The bottom line, which looks suspicious, is correct.

W.B.

TABLE 12 (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 73·2		
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	68·9
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	

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TABLE 13 (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 14 (Special Data).

DEVIATIONS OF INDIVIDUAL BROTHERS FROM THEIR MID-FRATERNAL STATURES.

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	...

TABLE 15.
FREQUENCY OF DIFFERENT EYE-COLOURS IN FOUR SUCCESSIVE GENERATIONS.

Sex and the No. of the (ascending) generation.	No. of cases of eye-colour observed.									Percentages.								
	1. Light blue.	2. Blue. Dark blue.	3. Grey. Blue-green.	4. Dark grey. Hazel.	5. Light brown.	6. Brown.	7. Dark Brown.	8. Very dark brown. Black.	Totals.	1. Light blue.	2. Blue. Dark blue.	3. Grey. Blue-green.	4. Dark Grey. Hazel.	5. Light brown.	6. Brown.	7. Dark brown.	8. Very dark brown. Black.	Totals.
Males IV.	13	177	136	40	2	39	44	12	463	2·8	38·2	29·4	8·6	0·4	8·4	9·5	2·6	99·9
Males III.	19	234	233	84	3	79	97	24	773	2·4	30·3	30·1	10·9	0·4	10·1	12·6	3·1	99·9
Males II.	30	167	236	108	8	83	74	36	742	4·0	22·5	31·8	14·6	1·1	11·2	10·0	4·8	100·0
Males I.	3	89	82	47	1	37	31	9	299	1·0	28·9	27·4	15·7	0·3	12·4	10·4	3·0	100·0
General	65	687	687	279	14	238	246	81	2277	2·9	29·3	30·2	12·3	0·6	10·4	10·8	3·6	100·0
Females IV.	7	132	114	48	2	70	58	19	450	1·5	29·3	25·3	10·7	0·4	15·6	12·9	4·2	99·9
Females III.	22	173	241	89	7	100	9g	17	742	2·9	23·3	32·5	12·1	0·9	13·5	12·5	2·3	100·0
Females II.	21	210	241	98	3	78	60	24	735	2·9	28·6	32·8	13·3	0·4	10·6	8·2	3·3	100·1
Females I.	6	78	82	55	5	33	22	5	286	2·1	27·3	28·7	19·2	1·7	11·5	7·7	1·7	99·0
General	56	593	678	290	17	281	233	65	2213	2·5	26·8	30·6	13·1	0·8	12·7	10·5	2·9	99·9
Males and Females IV.	20	309	240	88	4	109	102	31	913	2	34	27	10	1	12	11	3	100
Males and Females III.	41	407	474	173	10	179	190	41	1515	3	27	31	11	1	12	12	3	100
Males and Females II.	51	377	477	206	11	161	134	60	1477	3	26	32	14	1	11	9	4	100
Males and Females I.	9	167	164	102	6	70	53	14	585	1	29	28	18	1	12	9	2	100
General	121	1260	1365	569	31	519	479	146	4490	2·7	28·1	30·4	12·7	0·7	11·6	10·7	3·3	100·2

TABLE 16.

THE DESCENT OF HAZEL-EYED FAMILIES.

	Total cases.	Observed.			Percentages.		
		Light.	Hazel.	Dark.	Light.	Hazel.	Dark.
General population	4490	2746	569	1175	61·2	12·7	26·1
III. Grandparents...	449	267	61	121	60	13	27
II. Parents	336	165	85	86	49	25	26
I. Children...............	948	430	302	216	45	32	23

TABLE 17.

CALCULATED CONTRIBUTIONS OF EYE-COLOUR.

Contribution to the heritage from each.	Data limited to the eye-colours of the					
	2 parents.		4 grandparents.		2 parents and 4 grandparents.	
	I.		II.		III.	
	Light.	Dark.	Light.	Dark.	Light.	Dark.
Light-eyed parent.........	0·30	...	...	...	0·25	...
Hazel-eyed parent.........	0·20	0·10	...	...	0·16	0·09
Dark-eyed parent	...	0·30	...	...	...	0·25
Light-eyed grandparent..	...	...	0·16	...	0·08	...
Hazel-eyed grandparent .	...	...	0·10	0·06	0·05	0·03
Dark-eyed grandparent...	...	...	...	0·16	...	0·08
Residue, rateably assigned	0·28	0·12	0·25	0·11	0·12	0·06

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TABLE 18.

EXAMPLE OF ONE CALCULATION IN EACH OF THE THREE CASES.

Ancestry and their eye-colours.	I. No. about whom data exist.	I. Contribute to Light.	I. Contribute to Dark.	II. No. about whom data exist.	II. Contribute to Light.	II. Contribute to Dark.	III. No. about whom data exist.	III. Contribute to Light.	III. Contribute to Dark.
Light-eyed parents.	2	0·60	...	...	...	...	...	...	...
Hazel-eyed parents.	...	...	...	...	...	...	1	0·16	0·09
Dark-eyed parents.	...	...	...	...	...	...	1	...	0·25
Light-eyed grand-parents	...	...	...	1	0·16	...	1	0·08	...
Hazel-eyed grand-parents	...	...	...	2	0·20	0·12	2	0·10	0·06
Dark-eyed grand-parents	...	...	...	1	...	0·16	1	...	0·08
Residue, rateably as signed	...	0·28	0·12		0·25	0·11		0·12	0·06
Total contributions	...	0·88	0·12		0·61	0·39		0·46	0·54
		1·00			1·00			1·00	

TABLE 19.

OBSERVED AND CALCULATED EYE-COLOURS IN 16 GROUPS OF FAMILIES.

Those families are grouped together in whom the distribution of Light, Hazel, and Dark Eye-colour among the Parents and Grandparents is alike. Each group contains at least Twenty Brothers or Sisters.

Eye-colours of the						Total children.	Number of the light eye-coloured children.			
Parents.			Grandparents.				Observed.	Calculated.		
Light.	Hazel.	Dark.	Light.	Hazel.	Dark.			I.	II.	III.
2	...	...	4	...	...	183	174	161	163	172
2	...	...	3	1	...	53	46	47	44	48
2	...	...	3	...	1	92	88	81	67	79
2	...	...	2	1	1	27	26	24	18	22
...	...	2	2	...	2	22	11	6	12	6
1	1	...	3	1	...	62	52	48	51	51
1	1	...	3	...	1	42	30	33	31	32
1	1	...	2	2	...	31	28	24	24	20
1	1	...	2	...	2	49	35	38	28	34
1	1	...	2	1	1	31	25	24	21	23
1	...	1	3	...	1	76	45	44	55	46
1	...	1	2	...	2	66	30	38	38	35
1	...	1	2	...	1	27	15	16	18	16
1	...	1	1	...	3	20	9	12	8	9
1	...	1	1	1	2	22	8	13	11	11
...	1	1	1	1	2	24	9	14	12	10
							629	623	601	614

TABLE 20.

OBSERVED AND CALCULATED EYE-COLOURS IN 78 SEPARATE FAMILIES, EACH OF NOT LESS THAN SIX BROTHERS OR SISTERS.

Eye-colours of the						Total children.	Number of the light eye-coloured children.			
Parents.			Grandparents.				Observed.	Calculated.		
Light.	Hazel.	Dark.	Light.	Hazel.	Dark.			I.	II.	III.
2	...	...	4	...	...	6	6	5·3	5·3	5·6
2	...	...	4	...	...	6	6	5·3	5·3	5·6
2	...	...	4	...	...	6	6	5·3	5·3	5·6
2	...	...	4	...	...	6	5	5·3	5·3	5·6
2	...	...	4	...	...	7	7	6·2	6·2	6·6
2	...	...	4	...	...	7	7	6·2	6·2	6·6
2	...	...	4	...	...	7	7	6·2	6·2	6·6
2	...	...	4	...	...	7	7	6·2	6·2	6·6
2	...	...	4	...	...	7	7	6·2	6·2	6·6
2	...	...	4	...	...	8	8	7·0	7·1	7·5
2	...	...	4	...	...	8	8	7·0	7·1	7·5
2	...	...	4	...	...	8	8	7·0	7·1	7·5
2	...	...	4	...	...	8	8	7·0	7·1	7·5
2	...	...	4	...	...	8	7	7·0	7·1	7·5
2	...	...	4	...	...	8	7	7·0	7·1	7·5
2	...	...	4	...	...	12	12	10·6	10·7	11·3
2	...	...	3	1	...	7	7	6·2	5·8	6·4
2	...	...	3	1	...	10	4	8·8	8·3	9·1
2	...	...	3	1	...	12	12	10·6	10·0	10·9
2	...	...	3	...	1	7	6	6·2	5·1	6·0
2	...	...	3	...	1	8	8	7·0	5·8	6·9
2	...	...	3	...	1	9	9	7·9	6·6	7·7
2	...	...	3	...	1	9	9	7·9	6·6	7·7
2	...	...	3	...	1	9	7	7·9	6·6	7·7
2	...	...	3	...	1	10	10	8·8	7·3	8·6
2	...	...	2	2	...	7	7	6·2	5·4	6·2
2	...	...	2	2	...	10	9	8·8	7·7	8·8
2	...	...	2	1	1	6	6	5·3	4·0	5·0
2	...	...	2	1	1	10	10	8·8	6·7	8·3
...	2	...	2	1	1	7	4	6·2	4·7	4·6
...	2	...	2	...	2	8	5	5·4	4·6	4·8
...	...	2	3	...	1	6	2	1·7	4·4	2·2
...	...	2	2	...	2	9	1	2·5	5·1	2·5
...	...	2	1	...	3	6	1	2·7	2·5	1·2
...	...	2	1	...	3	11	3	3·1	4·5	2·2
...	...	2	1	1	2	6	...	1·7	3·0	1·5
...	...	2	1	1	2	7	4	2·0	3·6	1·8
1	1	...	3	1	...	6	6	4·7	5·0	4·9
1	1	...	3	1	...	7	6	5·5	5·7	5·7
1	1	...	3	1	...	8	6	6·2	6·6	6·6
1	1	...	3	1	...	9	7	7·0	7·5	7·4
1	1	...	3	1	...	11	10	8·6	9·1	9·2

TABLE 20—*continued.*

Eye-colours of the						Total children.	Number of the light eye-coloured children.			
Parents.			Grandparents.				Observed.	Children.		
Light.	Hazel.	Dark.	Light.	Hazel.	Dark.			I.	II.	III.
1	1	...	3	...	1	9	6	7·0	6·6	6·9
1	1	...	3	...	1	11	7	8·6	8·0	8·5
1	1	...	2	2	...	7	6	5·5	5·4	4·4
1	1	...	2	2	...	9	9	7·0	6·9	5·7
1	1	...	2	2	...	11	1	8·6	8·5	6·9
1	1	...	2	...	2	6	6	4·7	3·4	4·1
1	1	...	2	...	2	6	4	4·7	3·4	4·1
1	1	...	2	...	2	8	5	6·2	4·6	5·5
1	1	...	2	...	2	9	7	7·0	5·1	6·2
1	1	...	2	1	1	6	6	4·7	4·0	4·4
1	1	...	2	1	1	10	9	7·8	6·7	7·4
1	1	...	1	3	...	9	4	7·0	5·5	6·8
1	1	...	1	1	2	8	5	6·2	4·1	5·3
1	...	1	4	...	...	7	3	4·1	6·2	4·8
1	...	1	3	...	1	6	4	3·5	4·4	3·7
1	...	1	3	...	1	7	3	4·1	5·1	4·3
1	...	1	3	...	1	8	6	4·6	5·8	4·9
1	...	1	3	...	1	8	5	4·6	5·8	4·9
1	...	1	3	...	1	8	4	4·6	5·8	4·9
1	...	1	3	...	1	9	6	5·2	6·6	5·5
1	...	1	3	...	1	9	5	5·2	6·6	5·5
1	...	1	2	...	2	6	5	3·5	3·4	3·2
1	...	1	2	...	2	6	3	3·5	3·4	3·2
1	...	1	2	...	2	8	4	4·6	4·6	4·2
1	...	1	2	...	2	10	2	5·8	5·7	5·3
1	...	1	2	...	2	14	9	8·1	8·0	7·4
1	...	1	2	1	1	7	5	4·1	4·7	4·1
1	...	1	1	2	1	7	3	4·1	4·3	3·9
1	...	1	1	1	2	7	4	4·1	3·6	3·5
1	...	1	1	...	3	8	4	4·6	3·3	3·6
1	...	1	1	...	3	8	3	4·6	3·3	3·6
1	...	1	...	1	3	6	3	3·5	2·1	2·6
...	1	1	2	...	2	6	3	4·8	3·4	2·6
...	1	1	2	1	1	9	4	7·0	6·0	4·4
...	1	1	1	...	3	13	8	10·1	5·3	4·7
...	1	1	...	4	...	7	2	5·5	4·6	3·4

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TABLE 21.

ERROR IN CALCULATIONS.

Numbers of Errors of various Amounts in the 3 Calculations, Table 20, of the Number of Light Eye-coloured Children in the 78 Families.

Data employed referring to	Amount of Errors.					Total Cases.
	0·0 to 0·5.	0·6 to 1·1	1·2 to 1·7	1·8 to 2·3	2·4 and above.	
I. The 2 parents only	19	30	18	5	6	78
II. The 4 grandparents only.....	16	28	10	10	14	78
III. The 2 parents and 4 grandparents	41	17	8	4	8	78

TABLE 22.

INHERITANCE OF THE ARTISTIC FACULTY.

Parents.	Children.						
	Observed.			Per cents.			
				Observed.		Calculated.	
	Number of Fraternities.	Total children.	Of whom are artistic.	art.	not art.	art.	not art.
Both artistic	30	148	95	64	36	60	40
One artistic; one not..	101	520	201	39	61	39	61
Neither artistic.........	150	839	173	21	79	17	83
Totals...............	281	1507	469	100	100	100	100

The "parents" and the "children" in this Table usually rank respectively as Grandparents and Parents in the R.F.F. pedigrees.

APPENDIX.

A.

The following memoirs by the author, bearing on Heredity, have been variously utilised in this volume:

Experiments in Pangenesis. *Proc. Royal Soc.*, No. 127, 1871, p. 393.

Blood Relationship. *Proc. Royal Soc.*, No. 136, 1872, p. 394.

A Theory of Heredity. *Journ. Anthropol. Inst.*, 1875, p. 329.

Statistics by Intercomparison. *Phil. Mag.*, Jan. 1875.

*On the Probability of the Extinction of Families. *Journ. Anthropol. Inst.*, 1875.

Typical laws of Heredity. *Journ. Royal Inst.*, Feb. 1877.

*Geometric Mean in Vital and Social Statistics. *Proc. Royal Soc.*, No. 198, 1879. See subsequent memoir by Dr. Macalister.

Address to Anthrop. Section British Association at Aberdeen. *Journ. Brit. Assoc.*, 1885.

Regression towards Mediocrity in Hereditary Stature. *Journ. Anthropol. Inst.*, 1885.

Presidential Addresses to Anthropol. Inst., 1885, 6 and 7.

Family Likeness in Stature. *Proc. Royal Soc.*, No. 242, 1886.

Family Likeness in Eye-colour. *Proc. Royal Soc.*, No. 245, 1886.

Good and Bad Temper in English Families. *Fortnightly Review*, July, 1887.

Pedigree Moth Breeding. *Trans. Entomolog. Soc.*, 1887. See also subsequent memoir by Mr. Merrifield, and another read by him, Dec. 1887.

Those marked with an asterisk (*) are [reprinted with slight revision in the Appendices F, D, and E.

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WORKS on Heredity BY THE AUTHOR.

(Published by Messrs. Macmillan & Co.)

Hereditary Genius. 1869. [Out of print.]
English Men of Science. 1874.
Inquiries into Human Faculty. 1883.
Record of Family Faculties.[1] 1884. 2*s.* 6*d.*
Life History Album [2] (edited by F. Galton). 1884. 3*s.* 6*d.* and 4*s.* 6*d.*

[1] The Record of Family Faculties consists of Tabular Forms and Directions for entering Data, with an Explanatory Preface. It is a large thin quarto book of seventy pages, bound in limp cloth. The first part of it contains a preface, with explanation of the object of the work and of the way in which it is to be used. The rest consists of blank forms, with printed questions and blank spaces to be filled with writing. The Record is designed to facilitate the orderly collection of such data as are important to a family from an hereditary point of view. It allots equal space to every direct ancestor in the nearer degrees, and is supposed to be filled up in most cases by a parent, say the father of a growing family. If he takes pains to make inquiries of elderly relatives and friends, and to seek in registers, he will be able to ascertain most of the required particulars concerning not only his own parents, but also concerning his four grandparents; and he can ascertain like particulars concerning those of his wife. Therefore his children will be provided with a large store of information about their two parents, four grandparents, and eight great-grandparents, which form the whole of their fourteen nearest ancestors. A separate schedule is allotted to each of them. Space is afterwards provided for the more important data concerning many at least, of the brothers and sisters of each direct ancestor. The schedules are followed by Summary Tables, in which the distribution of any characteristic throughout the family at large may be compendiously exhibited.

[2] The Life History Album was prepared by a Sub-Committee of the Collective Investigation Committee of the British Medical Association. It is designed to serve as a continuous register of the principal biological facts in the life of its owner. The book begins with a few pages of explanatory remarks, followed by tables and charts. The first table is to contain a brief medical history of each member of the near ancestry of the owner. This is followed by printed forms on which the main facts of the owner's growth and development from birth onwards may be registered, and by charts on which measurements may be laid down at appropriate intervals and compared with the curves of normal growth. Most of the required data are such as any intelligent person is capable of recording; those that refer to illnesses should be brief and technical, and ought to be filled up by the medical attendant. Explanations are given of the most convenient tests of muscular force, of keenness of eyesight and hearing, and of the colour sense. The 4*s.* 6*d.* edition contains a card of variously coloured wools to test the sense of colour.

*** These two works pursue similar objects of personal and scientific utility, along different paths. The Album is designed to lay the foundation of a practice

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of maintaining trustworthy life-histories that shall be of medical service in after-life to the person who keeps them. The Record shows how the life histories of members of the same family may be collated and used to forecast the development in mind and body of the younger generation of that family. Both works are intended to promote the registration of a large amount of information that has hitherto been allowed to run to waste in oblivion, instead of accumulating and forming stores of recorded experience for future personal use, and from which future inquirers into heredity may hope to draw copious supplies.

B.

PROBLEMS BY J. D. HAMILTON DICKSON, FELLOW AND TUTOR OF ST. PETER'S COLLEGE, CAMBRIDGE.

(*Reprinted from Proc. Royal Soc., No.* 242, 1886, *p.* 63.)

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,

$$\frac{y^2}{(1{\cdot}22)^2}+\frac{(3x-2y)^2}{9(1{\cdot}50)^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

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,

$$\frac{y}{(1{\cdot}22)^2}+\frac{(3x-2y)^2}{9(1{\cdot}50)^2}=\text{C, a constant} \quad . \quad . \quad . \quad (2)$$

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,

$$\left.\begin{aligned} &\frac{y}{(1\cdot22)^2} - 2\frac{3x-2y}{9(1\cdot50)^2} = 0, \\ \text{that is} \quad &\frac{y}{x} = \frac{\dfrac{6}{9(1\cdot50)^2}}{\dfrac{1}{(1\cdot22)^2} + \dfrac{4}{9(1\cdot50)^2}} = \frac{6}{17\cdot6} \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (3)$$

or, approximately, on the line $y = \frac{1}{3}x$. Let this be the line OM. (See Fig. 11, p. 101.)

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 z OP.

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)

$$\frac{y_1}{(1\cdot22)^2} - 2\frac{3x_1 - 2y_1}{9(1\cdot50)^2} = 0$$

the exponential takes the form

$$\left\{\frac{1}{(1\cdot22)^2} + \frac{4}{9(1\cdot50)^2}\right\}y'^2 + \left\{\frac{y_1^2}{(1\cdot22)^2} + \frac{(3x_1-2y_1)^2}{9(1\cdot50)^2}\right\} \quad . \quad . \quad (4)$$

whence, if e be the probable error of this section,

$$\left.\begin{aligned} &\frac{1}{e^2} = \frac{1}{(1\cdot22)^2} + \frac{4}{9(1\cdot50)^2} \\ \text{or [on referring to (3)]} \quad &e = 1\cdot50\sqrt{\frac{9}{17\cdot6}} \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (5)$$

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.

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It is important to notice that all sections parallel to the same co-ordinate 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 $\frac{x'^2}{7} + \frac{y'^2}{2} = \text{const.}$, 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 = θ, XOM = ϕ, and replacing the probable errors 1·22 and 1·50 by a and b respectively; then the ellipses (2) are,

$$\frac{y^2}{a^2} + \frac{(x - y \tan \theta)^2}{b^2} = \text{C}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

equation (3) becomes

$$\left.\begin{aligned} &\frac{y^2}{a^2} + \tan \theta \frac{x - y \tan \theta}{b^2} = 0 \\ \text{or} \quad &\frac{y}{x} = \tan \phi = \frac{a^2 \tan \theta}{b^2 + a^2 \tan^2 \theta} \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (8)$$

and (5) becomes

$$\frac{1}{e^2} = \frac{1}{a^2} + \frac{\tan^2 \theta}{b^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

whence

$$\frac{\tan \phi}{\tan \theta} = \frac{e^2}{b^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

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

$$\frac{\tan \phi}{\tan \theta} = \frac{a^2}{b^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

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

$$\frac{x^2}{c^2} + \frac{y^2}{b^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

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

$$\frac{1}{v^2} = \frac{1}{c^2} + \frac{1}{b^2}, \text{ or } v = \frac{bc}{\sqrt{b^2 + c^2}} \quad . \quad . \quad . \quad (14)$$

which it is important to notice, is the same for all values of a.

C.

EXPERIMENTS ON SWEET PEAS BEARING ON THE LAW OF REGRESSION.

The reason why Sweet Peas were chosen, and the methods of selecting and planting them are described in Chapter VI., p. 79. The following Table justifies their selection by the convenient and accurate method of weighing, as equivalent to that of measuring them. It will be seen that within the limits of observed variation a difference of 0·172 grain in weight corresponds closely to an average difference of 0·01 inch in diameter.

TABLE 1.

COMPARISON OF WEIGHTS OF SWEET PEAS WITH THEIR DIAMETERS.

Distinguishing letter of seed.	Weight of one seed in grains. Common difference = 0·172 grain.	Length of row of 100 seeds in inches.	Diameter of one seed in hundredths of inch. Common difference = 0·01 inch.
K	1·750	21·0	21
L	1·578	20·2	20
M	1·406	19·2	19
N	1·234	17·9	18
O	1·062	17·0	17
P	·890	16·1	16
Q	·718	15·2	15

The results of the experiment are given in Table 2; its first and last columns are those that especially interest us; the remaining columns showing how these two were obtained.

It will be seen that for each increase of one unit on the part of the parent seed, there is a mean increase of only one-third of a unit in the filial seed; and again that the mean filial seed resembles the parental when the latter is about 15·5 hundredths of an inch in diameter. Taking 15·5 as the point towards which Filial Regression points, whatever may be the parental deviation from that point, the mean Filial Deviation will be in the same direction, but only one-third as much.

TABLE 2.

PARENT SEEDS AND THEIR PRODUCE.

The proportionate number of sweet peas of different sizes, produced by parent seeds also of different sizes, are given below. The measurements are those of their mean diameters, in hundredths of an inch.

Diameter of Parent Seed.	Diameters of Filial Seeds.								Total.	Mean Diameter of Filial Seeds.	
	Under 15.	15–	16–	17–	18–	19–	20–	Above 21–		Observed	Smoothed
21	22	8	10	18	21	13	6	2	100	17·5	17·3
20	23	10	12	17	20	13	3	2	100	17·3	17·0
19	35	16	12	13	11	10	2	1	100	16·0	16·6
18	34	12	13	17	16	6	2	0	100	16·3	16·3
17	37	16	13	16	13	4	1	0	100	15·6	16·0
16	34	15	18	16	13	3	1	0	100	16·0	15·7
15	46	14	9	11	14	4	2	0	100	15·3	15·4

This point is so low in the scale, that I possess less evidence than I desired to prove the bettering of the produce of very small seeds. The seeds smaller than Q were such a miserable set that I could hardly deal with them. Moreover, they were very infertile. It did, however, happen that in a few of the sets some of the Q seeds turned out very well.

If I desired to lay much stress on these experiments, I could make my case considerably stronger by going minutely into other details, including confirmatory measurements of the foliage and length of pod, but I do not care to do so.

D.

GOOD AND BAD TEMPER IN ENGLISH FAMILIES.[1]

[1] Reprinted after slight revision from *Fortnightly Review*, July, 1887.

ONE of the questions put to the compilers of the Family Records spoken of in page 72, referred to the "Character and Temperament" of the persons described. These were distributed through three and sometimes four generations, and consisted of those who lay in the main line of descent, together with their brothers and sisters.

Among the replies, I find that much information has been incidentally included concerning what is familiarly called the "temper" of no less than 1,981 persons. As this is an adequate number to allow for many inductions, and as temper is a strongly marked characteristic in all animals; and again, as it is of social interest from the large part it plays in influencing domestic happiness for good or ill, it seemed a proper subject for investigation.

The best explanation of what I myself mean by the word "temper" will be inferred from a list of the various epithets used by the compilers of the Records, which I have interpreted as expressing one or other of its qualities or degrees. The epithets are as follows, arranged alphabetically in the two main divisions of good and bad temper:—

Good temper.—Amiable, buoyant, calm, cool, equable, forbearing, gentle, good, mild, placid, self-controlled, submissive, sunny, timid, yielding. (15 epithets in all.)

Bad temper.—Acrimonious, aggressive, arbitrary, bickering, capricious, captious, choleric, contentious, crotchety, decisive, despotic, domineering, easily offended, fiery, fits of anger, gloomy, grumpy, harsh, hasty, headstrong, huffy, impatient, imperative, impetuous, insane temper, irritable, morose, nagging, obstinate, odd-tempered, passionate, peevish, peppery, proud, pugnacious, quarrelsome, quick-tempered, scolding, short, sharp, sulky, sullen, surly, uncertain, vicious, vindictive. (46 epithets in all.)

I also grouped the epithets as well as I could, into the following five classes: 1, mild; 2, docile; 3, fretful; 4, violent; 5, masterful.

Though the number of epithets denoting the various kinds of bad temper is three times as large as that used for the good, yet the number of persons described under the one general head is about the same as that described under the other. The first set of data that I tried, gave the proportion of the good to the bad-tempered as 48 to 52; the second set as 47 to 53. There is little difference between the two sexes in the frequency of good and bad temper, but that little is in favour of the women, since about 45 men are recorded as good-tempered for every 55 who are bad, and conversely 55 women as good-tempered for 45 who are bad.

I will not dwell on the immense amount of unhappiness, ranging from family discomfort down to absolute misery, or on the breaches of friendship that must have been occasioned by the cross-grained, sour, and savage dispositions of those who are justly labelled by some of the severer epithets; or on the comfort, peace, and good-will diffused through domestic circles by those who are rightly described by many of the epithets in the first group. We can hardly, too, help speculating uneasily upon the terms that our own relatives would select as most appropriate to our particular selves. But these considerations, interesting as they are in themselves, lie altogether outside the special purpose of this inquiry.

In order to ascertain the facts of which the above statistics are a brief summary, I began by selecting the larger families out of my lists, namely, those that consisted of not less than four brothers or sisters, and by noting the persons they included who were described as good or bad-tempered; also the remainder about whose temper nothing was said either one way or the other, and whom perforce I must call neutral. I am at the same time well aware that, in some few cases a tacit refusal to describe the temper should be interpreted as reticence in respect to what it was thought undesirable even to touch upon.

I found that out of a total of 1,361 children, 321 were described as good-tempered, 705 were not described at all, and 342 were described as bad-tempered. These numbers are nearly in the proportion of 1, 2, and 1, that is to say, the good are equal in number to the bad-tempered, and the neutral are just as numerous as the good and bad-tempered combined.

The equality in the total records of good and bad tempers is an emphatic testimony to the correct judgments of the compilers in the choice of their epithets, for whenever a group has to be divided into three classes, of which the second is called neutral, or medium, or any other equivalent term, its nomenclature demands that it should occupy a strictly middlemost position, an equal number of contrasted cases flanking it on either hand. If more cases were recorded of good temper than of bad, the compilers would have laid down the boundaries of the neutral zone unsymmetrically, too far from the good end of the scale of temper, and too near the bad end. If the number of cases of bad temper exceeded that of the good, the error would have been in the opposite direction. But it appears, on the whole, that the compilers of the records have erred neither to the right hand nor to the left. So far, therefore, their judgments are shown to be correct.

Next as regards the proportion between the number of those who rank as neutrals to that of the good or of the bad. It was recorded as 2 to 1; is that the proper poportion? Whenever the nomenclature is obliged to be somewhat arbitrary, a doubtful term should be interpreted in the sense that may have the widest suitability. Now a large class of cases exist in which the interpretation of the word neutral is fixed. It is that in which the three grades of magnitude are conceived to result from the various possible combinations of two elements, one of which is positive and the other negative, such as good and bad, and which are supposed to occur on each occasion at haphazard, but in the long run with equal frequency. The number of possible combinations of the two elements is only four, and each of these must also in the long run occur with equal frequency. They are: 1, both positive; 2, the first positive, the second negative; 3, the first negative, the second positive; 4, both negative. In the second and third of these combinations the negative counterbalances the positive, and the result is neutral. Therefore the proportions in which the several events of good, neutral, and bad would occur is as 1, 2, and 1. These proportions further commend themselves on the ground that the whole body of cases is thereby divided into two main groups, equal in number, one of which includes all neutral or medium cases, and the other all that are exceptional. Probably it was this latter view that was taken, it may be half unconsciously, by the compilers of the Records. Anyhow, their entries conform excellently to the proportions specified, and I give them credit for their practical appreciation of what seems theoretically to be the fittest standard. I speak, of course, of the Records taken as a whole; in small groups of cases the proportion of the neutral to the rest is not so regular.

The results shown in Table I. are obtained from all my returns. It is instructive in many ways, and not least in showing to a statistical eye how much and how little value may reasonably be attached to my materials. It was primarily intended to discover whether any strong bias existed among the compilers to spare the characters of their nearest relatives. In not a few cases they have written to me, saying that their records had been drawn up with perfect frankness, and earnestly reminding me of the importance of not allowing their remarks to come to the knowledge of the persons described. It is almost needless to repeat what I have published more than once already, that I treat the Records quite confidentially. I have left written instructions that in case of my death they should all be destroyed unread, except where I have left a note to say that the compiler wished them returned. In some instances I know that the Records were compiled by a sort of family council, one of its members acting as secretary; but I doubt much whether it often happened that the Records were known to many of the members of the family in their complete form. Bearing these and other considerations in mind, I thought the best test for bias would be to divide the entries into two contrasted groups, one including those who figured in the pedigrees as either father, mother, son, or daughter—that is to say, the compiler and those who were very nearly related to him—and the other including the uncles and aunts on both sides.

TABLE 1.

DISTRIBUTION OF TEMPER IN FAMILIES (per cents.)

Relationships.	1. Mild.	2. Docile.	3. Fretful.	4. Violent.	5. Masterful.	Total.	No. of cases observed.
a. Fathers and Sons	35	12	32	12	9	100	188
b. Mothers and Daughters	39	18	31	8	4	100	179
c. Uncles....................	32	13	25	18	12	100	272
d. Aunts	39	14	29	9	9	100	238
$a + b$. Direct line..........	74	30	63	20	13	200	367
$c + d$. Collaterals..........	71	27	54	27	21	200	510
	Good.		Bad Temper.				
$a + b$. Direct line..........	104		96			200	367
$c + d$. Collaterals..........	98		102			200	510

On comparing the entries, especially the summaries in the lower lines of the Table, it does not seem that the characters of near relatives are treated much more tenderly than those of the more remote. There is little indication of the compilers having been biassed by affection, respect, or fear. More cases of a record being left blank when a bad temper ought to have been recorded, would probably occur in the direct line, but I do not see how this could be tested. An omission may be due to pure ignorance; indeed I find it not uncommon for compilers to know very little of some of their uncles or aunts. The Records seem to be serious and careful compositions, hardly ever used as vehicles for personal animosity, but written in much the same fair frame of mind that most people force themselves into when they write their wills.

TABLE 2.

COMBINATIONS OF TEMPER IN MARRIAGE (per cents.).

Tempers of Husbands.	A.—Observed Pairs.					B.—Haphazard Pairs.				
	Tempers of Wives.					Tempers of Wives.				
	Good.		Bad Tempers.			Good.		Bad Tempers.		
	1	2	3	4	5	1	2	3	4	5
Good 1	6	10	9	6	2	13	5	10	3	2
,, ... 2	4	2	5	2	—	5	2	4	1	1
Bad 3	14	4	9	3	2	11	5	8	2	2
,, ... 4	7	3	3	2	1	6	2	5	1	1
,, ... 5	3	—	2	—	1	4	2	3	1	1
Good..........	22		24			25		21		
Bad...........	31		23			30		24		

The sexes are separated in the Table, to show the distribution of the five classes of temper among them severally. There is a large proportion of the violent and masterful among the men, of the fretful, the mild, and the docile among the women. On adding the entries it will be found that the proportion of those who fall within the several classes are 36 per cent. of mild-tempered, 15 per cent. of docile, 29 per cent. of fretful, 12 per cent of violent, 8 per cent. of masterful.

The importance assigned in marriage-selection to good and bad temper is an interesting question, not only from its bearing on domestic happiness, but also from the influence it may have in promoting or retarding the natural good temper of our race, assuming, as we may do for the moment, that temper is hereditary. I cannot deal with the question directly, but will give some curious facts in Table II. that throw indirect light upon it. There a comparison is made of (A) the actual frequency of marriage between persons each of the various classes of temper, with (B) the calculated frequency according to the laws of chance, on the supposition that there had been no marriage-selection at all, but that the pairings, so far as temper is concerned, had been purely at haphazard. There are only 111 marriages in my lists in which the tempers of both parents are recorded. On the other hand, the number of possible combinations in couples of persons who belong to the five classes of temper is very large, so I make the two groups comparable by reducing both to percentages.

It will be seen that with two apparent exceptions in the upper left-hand corners of either Table (of 6 against 13, and of 10 against 5), there are no indications of predilection for, or avoidance of marriage between persons of any of the five classes, but that the figures taken from observation run as closely with those derived through calculation, as could be expected from the small number of observations. The apparent exceptions are that the percentage of mild-tempered men who marry mild-tempered women is only 6, as against 13 calculated by the laws of chance, and that those who marry docile wives are 10, as against a calculated 5. There is little difference between mildness and docility, so we may throw these entries together without much error, and then we have 6 and 10, or 16, as against 13 and 5, or 18, which is a close approximation. We may compare the frequency of marriages between persons of like temper in each of the five classes by reading the Table diagonally. They are as (6), 2, 9, 2, 1, in the observed cases, against (13), 2, 8, 1, 1, in the calculated ones; here the irregularity of the 6 and 13, which are put in brackets for distinction sake, is conspicuous. Elsewhere there is not the slightest indication of a dislike in persons of similar tempers, whether mild, docile, fretful, violent, or masterful, to marry one another. The large initial figures 6 and 13 catch the eye, and at a first glance impress themselves unduly on the imagination, and might lead to erroneous speculations about mild-tempered persons, perhaps that they find one another rather insipid; but the reasons I have given, show conclusively that the recorded rarity of the marriages between mild-tempered persons is only apparent. Lastly, if we disregard the five smaller classes and attend only to the main divisions of good and bad temper, there does not appear to be much bias for, or against, the marriage of good or bad-tempered persons in their own or into the opposite division.

The admixture of different tempers among the brothers and sisters of the same family is a notable fact, due to various causes which act in different directions. It is best to consider them before we proceed to collect evidence and attempt its interpretation. It becomes clear enough, and may be now taken for granted, that the tempers of progenitors do not readily blend in the offspring, but that some of the children take mainly after one of them, some after another, but with a few threads, as it were, of various ancestral tempers woven in, which occasionally manifest themselves. If no other influences intervened, the tempers of the children in the same family would on this account be almost as varied as those of their ancestors; and these, as we have just seen, married at haphazard, so far as their tempers were concerned; therefore the numbers of good and bad children in families would be regulated by the same laws of chance that apply to a gambling-table. But there are other influences to be considered. There is a well-known tendency to family likeness among brothers and sisters, which is due, not to the blending of ancestral peculiarities, but to the prepotence of one of the progenitors, who stamped more than his or her fair share of qualities upon the descendants. It may be due also to a familiar occurrence that deserves but has not yet received a distinctive name, namely, where all the children are alike and yet their common likeness cannot be traced to their progenitors. A new variety has come into existence through a process that affects the whole Frater-

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nity and may result in an unusually stable variety (see Chapter III.). The most strongly marked family type that I have personally met with, first arose simultaneously in the three brothers of a family who transmitted their peculiarities with unusual tenacity to numerous descendants through at least four generations. Other influences act in antagonism to the foregoing; they are the events of domestic life, which instead of assimilating tempers tend to accentuate slight differences in them. Thus if some members of a family are a little submissive by nature, others who are naturally domineering are tempted to become more so. Then the acquired habit of dictation in these reacts upon the others and makes them still more submissive. In the collection I made of the histories of twins who were closely alike, it was most commonly said that one of the twins was guided by the other. I suppose that after their many childish struggles for supremacy, each finally discovered his own relative strength of character, and thenceforth the stronger developed into the leader, while the weaker contentedly subsided into the position of being led. Again, it is sometimes observed that one member of an otherwise easygoing family, discovers that he or she may exercise considerable power by adopting the habit of being persistently disagreeable whenever he or she does not get the first and best of everything. Some wives contrive to tyrannise over husbands who are mild and sensitive, who hate family scenes and dread the disgrace attending them, by holding themselves in readiness to fly into a passion whenever their wishes are withstood. They thus acquire a habit of "breaking out," to use a term familiar to the warders of female prisons and lunatic asylums; and though their relatives and connections would describe their tempers by severe epithets, yet if they had married masterful husbands their characters might have developed more favourably.

To recapitulate briefly, one set of influences tends to mix good and bad tempers in a family at haphazard; another set tends to assimilate them, so that they shall all be good or all be bad; a third set tends to divide each family into contrasted portions. We have now to ascertain the facts and learn the results of these opposing influences.

In dealing with the distribution of temper in Fraternities,[1] we

[1] A Fraternity consists of the brothers of a family, and of the sisters after the

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can only make use of those in which at least two cases of temper are recorded; they are 146 in number. I have removed all the cases of neutral temper, treating them as if they were non-existent, and dealing only with the remainder that are good or bad. We have next to eliminate the haphazard element. Beginning with Fraternities of two persons only, either of whom is just as likely to be good as bad tempered, there are, as we have already seen, four possible combinations, resulting in the proportions of 1 case of both good, 2 cases one good and one bad, and one case of both bad. I have 42 such Fraternities, and the observed facts are that in 10 of them both are good tempered, in 20 one is good and one bad, and in 12 both are bad tempered. Here only a trifling and untrustworthy difference is found between the observed and the haphazard distribution, the other conditions having neutralised each other. But when we proceed to larger Fraternities the test becomes shrewder, and the trifling difference already observed becomes more marked, and is at length unmistakable. Thus the successive lines of Table III. show a continually increasing diverg-

TABLE 3.

Distribution of Temper in Fraternities.

		A.—Observed.			B.—Haphazard.		
Number in each Fraternity.	Number of Fraternities.	All good-tempers.	Intermediate cases.	All bad-tempers.	All good-tempers.	Intermediate cases.	All bad-tempers.
2	42	10	20	12	10	21	11
3	55	11	15 21	8	7	20 21	7
4	29	5	6 9 8	1	2	8 12 8	2
5	6	1	0 2 1 0	2	0	1 2 2 1	0
6	14	1	0 1 3 3 2	4	0	2 4 5 4 2	0
4 to 6	49	7		7	2		2

qualities of the latter have been transmuted to their Male Equivalents; but as no change in the Female values seems really needed, so none has been made in respect to Temper.

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ence between the observed and the haphazard distribution of temper, as the Fraternities increase in size. A compendious comparison is made in the bottom line of the Table by adding together the instances in which the Fraternities are from 4 to 6 in number, and in taking only those in which all the members of the Fraternity were alike in temper, whether good or bad. There are 7 + 7, or 14, observed cases of this against 2 + 2, or 4, haphazard cases, found in a total of 49 Fraternities. Hence it follows that the domestic influences that tend to differentiate temper wholly fail to overcome the influences, hereditary and other, that tend to make it uniform in the same Fraternity.

As regards direct evidences of heredity of temper, we must frame our inquiries under a just sense of the sort of materials we have to depend upon. They are but coarse portraits scored with white or black, and sorted into two heaps, irrespective of the gradations of tint in the originals. The processes I have used in discussing the heredity of stature, eye-colour, and artistic faculty, cannot be employed in dealing with the heredity of temper. I must now renounce those refined operations and set to work with ruder tools on my rough material.

The first inquiry will be, Do good-tempered parents have, on the whole, good-tempered children, and do bad-tempered parents have bad-tempered ones? I have 43 cases where both parents are recorded as good-tempered, and 25 where they were both bad-tempered. Out of the children of the former, 30 per cent. were good-tempered and 10 per cent. bad; out of the latter, 4 per cent. were good and 52 per cent. bad-tempered. This is emphatic testimony to the heredity of temper. I have worked out the other less contrasted combinations of parental temper, but the results are hardly worth giving. There is also much variability in the proportions of the neutral cases.

I then attempted, with still more success, to answer the converse question, Do good-tempered Fraternities have, on the whole, good-tempered ancestors, and bad-tempered Fraternities bad-tempered ones? After some consideration of the materials, I defined—rightly or wrongly—a good-tempered Fraternity as one in which at least two members were good-tempered and none were bad, and a bad-tempered Fraternity as one in which at least two members were bad-tempered, whether or no any cases of good temper were said to be associated with them. Then, as regards the ancestors, I thought by far the most trustworthy group was that which consisted of the two parents and of the uncles and aunts on both sides. I have thus 46 good-tempered Fraternities with an aggregate of 333 parents, uncles, and aunts; and 71 bad-tempered, with 633 parents, uncles, and aunts. In the former group, 26 per cent. were good tempered and 18 bad; in the latter group, 18 were good-tempered and 29 were bad, the remainder being neutral. These results are almost the exact counterparts of one another, so I seem to have made good hits in framing the definitions. More briefly, we may say that when the Fraternity is good-tempered as above defined, the number of good-tempered parents, uncles, and aunts, exceeds that of the bad-tempered in the proportion of 3 to 2; and that when the Fraternity is bad-tempered, the proportions are exactly reversed.

I have attempted in other ways to work out the statistics of hereditary tempers, but none proved to be of sufficient value for publication. I can trace no prepotency of one sex over the other in transmitting their tempers to their children. I find clear indications of strains of bad temper clinging to families for three generations, but I cannot succeed in putting them into a numerical form.

It must not be thought that I have wished to deal with temper as if it were an unchangeable characteristic, or to assign more trustworthiness to my material than it deserves. Both these views have been discussed; they are again alluded to to show that they are not dismissed from my mind, and partly to give the opportunity of adding a very few further remarks.

Persons highly respected for social and public qualities may be well-known to their relatives as having sharp tempers under strong but insecure control, so that they "flare up" now and then. I have heard the remark that those who are over-suave in ordinary demeanour have often vile tempers. If this be the case—and I have some evidence of its truth—I suppose they are painfully conscious of their infirmity, and through habitual endeavours to subdue it, have insensibly acquired an exaggerated suavity at the times when their temper is unprovoked. Illness, too, has much influence in affecting the temper. Thus I sometimes come across entries to the effect of, "not naturally ill-tempered, but peevish through illness. Overwork and worry will make even mild-tempered men exceedingly touchy and cross.

The accurate discernment and designation of character is almost beyond the reach of any one, but, on the other hand, a rough knowledge and description of its prominent features is easily obtainable; and it seems to me that the testimony of a member of a family who has seen and observed a person in his unguarded moments and under very varied circumstances for many years, is a verdict deserving of much confidence. I shall have fulfilled my object in writing this paper if it leaves a clear impression of the great range and variety of temper among persons of both sexes in the upper and middle classes of English society; of its disregard in Marriage Selection; of the great admixture of its good and bad varieties in the same family; and of its being, nevertheless, as hereditary as any other quality. Also, that although it exerts an immense influence for good or ill on domestic happiness, it seems that good temper has not been especially looked for, nor ill temper especially shunned, as it ought to be in marriage-selection.

E.

[1] THE GEOMETRIC MEAN, IN VITAL AND SOCIAL STATISTICS.

My purpose is to show that an assumption which lies at the basis of the well-known law of "Frequency of Error" is incorrect when applied to many groups of vital and social phenomena, although that law has been applied to them by statisticians with partial success. Next, I will point out the correct hypothesis upon which a Law of Error suitable to these cases ought to be calculated; and subsequently I will communicate a memoir by Mr. (now Dr.) Donald MacAlister, who, at my suggestion, has mathematically investigated the subject.

The assumption to which I refer is, that errors in excess or in

[1] Reprinted, with slight revision, from the *Proceedings of the Royal Society*, No. 198, 1879.

deficiency of the truth are equally probable; or conversely, that if two fallible measurements have been made of the same object, their arithmetical mean is more likely to be the true measurement than any other quantity that can be named.

This assumption cannot be justified in vital phenomena. For example, suppose we endeavour to match a tint; Weber's law, in its approximative and simplest form of Sensation varying as the logarithm of the Stimulus, tells us that a series of tints, in which the quantities of white scattered on a black ground are as 1, 2, 4, 8, 16, 32, &c., will appear to the eye to be separated by equal intervals of tint. Therefore, in matching a grey that contains 8 portions of white, we are just as likely to err by selecting one that has 16 portions as one that has 4 portions. In the first case there would be an error in excess, of 8 units of absolute tint; in the second there would be an error in deficiency, of 4. Therefore, an error of the same magnitude in excess or in deficiency is not equally probable in the judgment of tints by the eye. Conversely, if two persons, who are equally good judges, describe their impressions of a certain tint, and one says that it contains 4 portions of white and the other that it contains 16 portions, the most reasonable conclusion is that it really contains 8 portions. The arithmetic mean of the two estimates is 10, which is *not* the most probable value; it is the geometric mean 8 (4 : 8 : : 8 : 16) which is the most probable.

Precisely the same condition characterises every determination by each of the senses; for example, in judging of the weight of bodies or of their temperatures, of the loudness and of the pitches of tones, and of estimates of lengths and distances *as wholes*. Thus, three rods of the lengths a, b, c, when taken successively in the hand, appear to differ by equal intervals when $a : b :: b : c$, and not when $a - b = b - c$. In all physiological phenomena, where there is on the one hand a stimulus and on the other a response to that stimulus, Weber's or some other geometric law may be assumed to prevail; in other words, the true mean is geometric rather than arithmetic.

The geometric mean appears to be equally applicable to the majority of the influences, which, combined with those of purely vital phenomena, give rise to the events with which sociology deals. It is difficult to find terms sufficiently general to apply to the varied topics of sociology, but there are two categories which are of common oc-

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currence in which the geometric mean is certainly appropriate. The one is increase, as exemplified by the growth of population, where an already large nation tends to receive larger accessions than a small one under similar circumstances, or when a capital employed in a business increases in proportion to its size. The other category is the influences of circumstances or of "milieux" as they are often called, such as a period of plenty in which a larger field or a larger business yields a greater excess over its mean yield than a smaller one. Most of the causes of those differences with which sociology are concerned, and which are not purely vital phenomena, such as those previously discussed, may be classified under one or other of these two categories, or under such as are in principle almost the same. In short, sociological phenomena, like vital phenomena are, as a general rule, subject to the condition of the geometric mean.

The ordinary law of Frequency of Error, based on the arithmetic mean, corresponds, no doubt, sufficiently well with the observed facts of vital and social phenomena, to be very serviceable to statisticians, but it is far from satisfying their wants, and it may lead to absurdity when applied to wide deviations. It asserts that deviations in excess must be balanced by deviations of equal magnitude in deficiency; therefore, if the former be greater than the mean itself, the latter must be less than zero, that is, must be negative. This is an impossibility in many cases, to which the law is nevertheless applied by statisticians with no small success, so long as they are content to confine its application within a narrow range of deviation. Thus, in respect of Stature, the law is very correct in respect to ordinary measurements, although it asserts that the existence of giants, whose height is more than double the mean height of their race, implies the possibility of the existence of dwarfs, whose stature is less than nothing at all.

It is therefore an object not only of theoretical interest but of practical use, to thoroughly investigate a Law of Error, based on the geometric mean, even though some of the expected results may perhaps be apparent at first sight. With this view I placed the foregoing remarks in Mr. Donald McAllister's hands, who contributed a memoir that will be found in the *Proc. Royal Soc.*, No. 198, 1879, following my own. It should be referred to by such mathematicians as may read this book

F.

PROBABLE EXTINCTION OF FAMILIES.[1]

THE decay of the families of men who occupied conspicuous positions in past times has been a subject of frequent remark, and has given rise to various conjectures. It is not only the families of men of genius or those of the aristocracy who tend to perish, but it is those of all with whom history deals, in any way, even such men as the burgesses of towns, concerning whom Mr. Doubleday has inquired and written. The instances are very numerous in which surnames that were once common have since become scarce or have wholly disappeared. The tendency is universal, and, in explanation of it, the conclusion has been hastily drawn that a rise in physical comfort and intellectual capacity is necessarily accompanied by diminution in "fertility"—using that phrase in its widest sense and reckoning abstinence from marriage as one cause of sterility. If that conclusion be true, our population is chiefly maintained through the "proletariat," and thus a large element of degradation is inseparably connected with those other elements which tend to ameliorate the race. On the other hand, M. Alphonse de Candolle has directed attention to the fact that, by the ordinary law of chances, a large proportion of families are continually dying out, and it evidently follows that, until we know what that proportion is, we cannot estimate whether any observed diminution of surnames among the families whose history we can trace, is or is not a sign of their diminished "fertility." I give extracts from M. De Candolle's work in a foot-note,[2] and may add that, although I have not hitherto published anything on the matter, I took considerable pains some years ago to obtain numerical results in respect to this

[1] Reprinted, with slight revision, from the *Journ. Anthropol. Inst.* 18 .

[2] "Au milieu des renseignements précis et des opinions très-sensées de MM. Benoiston de Châteauneuf, Galton, et autres statisticiens, je n'ai pas rencontré la réflexion bien importante qu'ils auraient dû faire de l'extinction *inévitable* des noms de famille. Évidemment tous les noms doivent s'éteindre . . . . Un mathématicien pourrait calculer comment la réduction des noms ou titres aurait lieu, d'après la probabilité des naissances toutes féminines ou toutes masculines ou mélangées et la probabilité d'absence de naissances dans un couple quelconque," &c.—ALPHONSE DE CANDOLLE, *Histoire des Sciences et des Savants*, 1873.

very problem. I made certain very simple, ~~but~~ and very inaccurate, suppositions, concerning average fertility, and I worked to the nearest integer, starting with 10,000 persons, but the computation became intolerably tedious after a few steps, and I had to abandon it. The Rev. H. W. Watson kindly, at my request, took the problem in hand, and his results form the subject of the following paper. They do not give what can properly be called a general solution, but they do give certain general results. They show (1) how to compute, though with great labour, any special case; (2) a remarkably easy way of computing those special cases in which the law of fertility approximates to a certain specified form; and (3) how all surnames tend to disappear.

The form in which I originally stated the problem is as follows. I purposely limited it in the hope that its solution might be more practicable if unnecessary generalities were excluded :—

A large nation, of whom we will only concern ourselves with the adult males, N in number, and who each bear separate surnames, colonise a district. Their law of population is such that, in each generation, a_0 per cent. of the adult males have no male children who reach adult life; a_1 have one such male child; a_2 have two; and so on up to a_5 who have five. Find (1) what proportion of the surnames will have become extinct after r generations; and (2) how many instances there will be of the same surname being held by m persons.

Discussion of the problem by the Rev. H. W. Watson, D.Sc. F.R.S. *formerly Fellow of Trinity College, Cambridge.*

Suppose that at any instant all the adult males of a large nation have different surnames, it is required to find how many of these surnames will have disappeared in a given number of generations upon any hypothesis, to be determined by statistical investigations, of the law of male population.

Let, therefore, a_0 be the percentage of males in any generation who have no sons reaching adult life, let a_1 be the percentage that have one such son, a_2 the percentage that have two, and so on up to a_q, the percentage that have q such sons, q being so large that it is not worth while to consider the chance of any man having more than q adult sons—our first hypothesis will be that the numbers

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a_0, a_1, a_2, etc., remain the same in each succeeding generation. We shall also, in what follows, neglect the overlapping of generations—that is to say, we shall treat the problem as if all the sons born to any man in any generation came into being at one birth, and as if every man's sons were born and died at the same time. Of course it cannot be asserted that these assumptions are correct. Very probably accurate statistics would discover variations in the values of a_0, a_1, etc., as the nation progressed or retrograded; but it is not at all likely that this variation is so rapid as seriously to vitiate any general conclusions arrived at on the assumption of the values remaining the same through many successive generations. It is obvious also that the generations must overlap, and the neglect to take account of this fact is equivalent to saying, that at any given time we leave out of consideration those male descendants, of any original ancestor who are more than a certain average number of generations removed from him, and compensate for this by giving credit for such male descendants, not yet come into being, as are not more than that same average number of generations removed from the original ancestors.

Let then $\frac{a_0}{100}$, $\frac{a_1}{100}$, $\frac{a_2}{100}$, etc., up to $\frac{a_q}{100}$ be denoted by the symbols t_0, t_1, t_2, etc., up to t_q, in other words, let t_0, t_1, etc., be the chances in the first and each succeeding generation of any individual man, in any generation, having no son, one son, two sons, and so on, who reach adult life. Let N be the original number of distinct surnames, and let ${}_rm_s$ be the fraction of N which indicates the number of such surnames with s representatives in the rth generation.

Now, if any surname have p representatives in any generation, it follows from the ordinary theory of chances that the chance of that same surname having s representatives in the next succeeding generation is the coefficient of x^s in the expansion of the multinomial

$$(t_0 + t_1x + t_2x^2 +, \text{ etc. } + t_qx^q)^p$$

Let then the expression $t_0 + t_1x + t_2x^2 +$ etc. $+ t_qx^q$ be represented by the symbol T.

Then since, by the assumption already made, the number of surnames with no representative in the r-1th generation is ${}_{r-1}m_0$ N, the number with one representative ${}_{r-1}m_1.N$, the number with two ${}_{r-1}m_2$. N and so on, it follows, from what we last stated, that the number of surnames with s representatives in the rth generation must be the coefficient of x^s in the expression

$$\left\{ {}_{r-1}m_0 + {}_{r-1}m_1 T + {}_{r-1}m_2 T^2 + \text{ etc. } + {}_{r-1}m_{q^{r-1}} T^{q^{r-1}} \right\} N$$

If, therefore, the coefficient of N in this expression be denoted by $f_r\,(x)$ it follows that ${}_{r-1}m_1$, ${}_{r-1}m_2$ and so on, are the coefficients of x, x^2 and so on, in the expression $f_{r-1}\,(x)$.

If, therefore, a series of functions be found such that

$$f_1(x) = t_0 + t_1 x + \text{ etc. } + t_q x^q \text{ and } f_r\,(x) = f_{r-1}\,(t_0 + t_1 x \text{ etc. } + t^q x^q)$$

then the proportional number of groups of surnames with s representatives in the rth generation will be the coefficient of x^s in $f_r\,(x)$ and the actual number of such surnames will be found by multiplying this coefficient by N. The number of surnames unrepresented or become extinct in the rth generation will be found by multiplying the term independent of x in $f_r\,(x)$ by the number N.

The determination, therefore, of the rapidity of extinction of surnames, when the statistical data, t_0, t_1, etc., are given, is reduced to the mechanical, but generally laborious process of successive substitution of $t_0 + t_1 x + t_2 x^2 +$ etc., for x in successively determined values of $f_r\,(x)$, and no further progress can be made with the problem until these statistical data are fixed; the following illustrations of the application of our formula are, however, not without interest.

(1) The very simplest case by which the formula can be illustrated is when $q = 2$ and t_0, t_1, t_2 are each equal to $\frac{1}{3}$.

Here $f_1\,(x) = \dfrac{1 + x + x^2}{3}$ $f_2(x) = \dfrac{1}{3}\left\{ 1 + \dfrac{1}{3}\,(1 + x + x^2) + \dfrac{1}{9}\,1 + x + x^2)^2 \right\}^2$

and so on.

Making the successive substitutions, we obtain

$$f_2\,(x) = \frac{1}{3}\left\{ \frac{13}{9} + \frac{5x}{9} + \frac{6x^2}{9} + \frac{2x}{9} + \frac{x}{9} \right\}$$

$$f_3(x) = \frac{1249}{2187} + \frac{265x}{2187} + \frac{343x^2}{2187} + \frac{166x^3}{2187} + \frac{109x^4}{2187} + \frac{34x^5}{2187} + \frac{16x^6}{2187} + \frac{4x^7}{2187} + \frac{x^8}{2187}$$

$$\begin{aligned} f_4(x) = {} & {\cdot}63183 + {\cdot}08306x + {\cdot}10635x^2 + {\cdot}07804x^3 + {\cdot}06489x^4 + {\cdot}05443x^5 + {\cdot}01437x^6 \\ & + {\cdot}01692x^7 + {\cdot}01144x^8 + {\cdot}00367x^9 + {\cdot}00104x^{10} + {\cdot}00015x^{11} + {\cdot}00005x^{12} \\ & + {\cdot}00001x^{13} + {\cdot}00000x^{14} + {\cdot}00000x^{15} + {\cdot}00000x^{16} \end{aligned}$$

and the constant term in $f_5(x)$ or ${}_5m_0$ is therefore

$$\cdot 63183 + \frac{\cdot 08306}{3} + \frac{\cdot 10635}{9} + \frac{\cdot 07804}{27} + \frac{\cdot 06489}{81} + \frac{\cdot 05443}{243} + \frac{\cdot 01437}{729} + \frac{\cdot 01692}{2187} + \frac{\cdot 01144}{6561} + \frac{\cdot 00367}{19683} + \frac{\cdot 00104}{59049} + \frac{\cdot 00015}{177147} +$$

The value of which to five places of decimals is ·67528.

The constant terms, therefore, in f_1, f_2 up to f_5 when reduced to decimals, are in this case ·33333, ·48148, ·57110, ·64113, and ·65628 respectively. That is to say, out of a million surnames at starting, there have disappeared in the course of one, two, etc., up to five generations, 333333, 481480, 571100, 641130, and 675280 respectively.

The disappearances are much more rapid in the earlier than in the later generations. Three hundred thousand disappear in the first generation, one hundred and fifty thousand more in the second, and so on, while in passing from the fourth to the fifth, not more than thirty thousand surnames disappear.

All this time the male population remains constant. For it is evident that the male population of any generation is to be found by multiplying that of the preceding generation, by $t_1 + 2t_2$, and this quantity is in the present case equal to one.

If axes Ox and Oy be drawn, and equal distances along Ox represent generations from starting, while two distances are marked along every ordinate, the one representing the total male population in any generation, and the other the number of remaining surnames in that generation, of the two curves passing through the extremities of these ordinates, the *population* curve will, in this case, be a straight line parallel to Ox, while the *surname* curve will intersect the population curve on the axis of y, will proceed always convex to the axis of x, and will have the positive part of that axis for an asymptote.

The case just discussed illustrates the use to be made of the general formula, as well as the labour of successive substitutions, when the expressions $f_1(x)$ does not follow some assigned law. The calculation may be infinitely simplified when such a law can be found; especially if that law be the expansion of a binomial, and only the extinctions are required.

For example, suppose that the terms of the expression $t_0 + t_1 x +$ etc. $+ t_q x_q$ are proportional to the terms of the expanded binomial

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$(a+bx)^q$ *i.e.* suppose that $t_0 = \frac{a^q}{(a+b)^q}$, $t_1 = q\frac{a^{q-1}b}{(a+b)^q}$ and so on.

$$\text{Here } f_1(x) = \frac{(a+bx)^q}{(a+b)^q} \text{ and } {}_1m_0 = \frac{a^q}{(a+b)^q}$$

$$f_2(x) = \frac{1}{(a+b)^q}\left\{ a + b\frac{(a+bx)^q}{(a+b)^q} \right\}^q$$

$${}_2m_0 = \frac{1}{(a+b)^q}\left\{ a + b\,{}_1m_0 \right\}^q$$

$$\text{Generally } {}_rm_0 = \frac{1}{(a+b)^q}\left\{ a + b\,{}_{r-1}m_0 \right\}^q = \frac{b_q}{(a+b)_q}\left\{ \frac{a}{b} + {}_{r-1}m_0 \right\}^q$$

If, therefore, we wish to find the number of extinctions in any generation, we have only to take the number in the preceding generation, add it to the constant fraction $\frac{a}{b}$, raise the sum to the power of q, and multiply by $\frac{b^q}{(a+b)^q}$

With the aid of a table of logarithms, all this may be effected for a great number of generations in a very few minutes. It is by no means unlikely that when the true statistical data t_0, t_1, etc., t_q are ascertained, values of a, b, and q may be found, which shall render the terms of the expansion $(a+bx)^q$ approximately proportionate to the terms of $f_1(x)$. If this can be done, we may *approximate* to the determination of the rapidity of extinction with very great ease, for any number of generations, however great.

For example, it does not seem very unlikely that the value of q might be 5, while $t_0, t_1 \ldots t_q$ might be ·237, ·396, ·264, ·088, ·014, ·001, or nearly, $\frac{1}{4}, \frac{1}{3}, \frac{7}{24}, \frac{1}{23}, \frac{1}{138}$, and $\frac{1}{1000}$.

Should that be the case, we have, $f_1(x) = \frac{(3+x)^5}{4^5}$ ${}_1m_0 = \frac{3^5}{4^5}$

and generally ${}_rm_0 = \frac{1}{4^5}\left\{ 3 + {}_{r-1}m_0 \right\}^5$

Thus we easily get for the number of extinctions in the first ten generations respectively.

·237, ·346, ·410, ·450, ·477, ·496, ·510, ·520, ·527, ·533.

We observe the same law noticed above in the case of $\frac{1+x+x^2}{3}$ viz., that while 237 names out of a thousand disappear in the first

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step, and an additional 109 names in the second step, there are only 27 disappearances in the fifth step, and only six disappearances in the tenth step.

If the curves of surnames and of population were drawn from this case, the former would resemble the corresponding curve in the case last mentioned, while the latter would be a curve whose distance from the axis of x increased indefinitely, inasmuch as the expression

$$t_1+2t_2+3t_3+4t_4+5t_5$$

is greater than one.

Whenever $f_1\ (x)$ can be represented by a binomial, as above suggested, we get the equation

$$_rm_0=\frac{1}{(a+b)^q}\left\{\ a+b_{r-1}m\ \right\}^q$$

whence it follows that as r increases indefinitely the value of $_rm_0$ approaches indefinitely to the value y where

$$y=\frac{1}{(a+b)}\left\{a+by\right\}$$

that is where $y=1$.

All the surnames, therefore, tend to extinction in an indefinite time, and this result might have been anticipated generally, for a surname once lost can never be recovered, and there is an additional chance of loss in every successive generation. This result must not be confounded with that of the extinction of the male population; for in every binomial case where q is greater than 2, we have t_1+2t_2+ etc. $+qt_q>1$, and, therefore an indefinite increase of male population.

The true interpretation is that each of the quantities, $_rm_1$, $_rm_2$, etc., tends to become zero, as r is indefinitely increased, but that it does not follow that the product of each by the infinitely large number N is also zero.

As, therefore, time proceeds indefinitely, the number of surnames extinguished becomes a number of the *same order of magnitude* as the total number at first starting in N, while the number of surnames represented by one, two, three, etc., representatives is some infinitely smaller but finite number. When the finite numbers are multiplied by the corresponding number of representatives, sometimes infinite in number, and the products added together, the sum will generally exceed the original number N. In point of fact, just as in the cases calculated above to five generations, we had a continual, and indeed at first, a rapid extinction of surnames, combined in the one case with a stationary, and in the other case an increasing population, so is it when the number of generations is increased indefinitely. We have a continual extinction of surnames going on, combined with constancy, or increase of population, as the case may be, until at length the number of surnames remaining is absolutely insensible, as compared with the number at starting; but the total number of representatives of those remaining surnames is infinitely greater than the original number.

We are not in a position to assert from *actual calculation* that a corresponding result is true for every form of $f_1(x)$, but the reasonable inference is that such is the case, seeing that it holds whenever $f_1(x)$ may be compared with $\frac{(a+bx)^q}{(a+b)^q}$ whatever a, b, or q may be.

G.

ORDERLY ARRANGEMENT OF HEREDITARY DATA.

THERE are many methods both of drawing pedigrees and of describing kinship, but for my own purposes I still prefer those that I designed myself. The chief requirements that have to be fulfilled are compactness, an orderly and natural arrangement, and clearly intelligible symbols.

Nomenclature.—A symbol ought to be suggestive, consequently the initial letter of a word is commonly used for the purpose. But this practice would lead to singular complications in symbolizing the various ranks of kinship, and it must be applied sparingly. The letter F is equally likely to suggest any one of the three very different words of Father, Female, and Fraternal. The letter M suggests both Mother and Male; S would do equally for Son and for Sister. Whether they are English, French, or German words, much the same complexity prevails. The system employed in *Hereditary Genius* had the merit of brevity, but was apt to cause mistake; it was awkward in manuscript and difficult to the printer, and I have now abandoned it in favour of the method employed in the *Records* *of Family Faculties.* This will now be briefly described again. Each kinsman can be described in two ways, either by letters or by a number. In ordinary cases both the letter and number are intended to be used simultaneously, thus FF.8. means the Father's Father of the person described, though either FF or 8, standing by themselves, would have the same meaning. The double nomenclature has great practical advantages. It is a check against mistake and makes reference and orderly arrangement easy.

As regards the letters, F stands for Father and M for Mother, whenever no letter succeeds them; otherwise they stand for Father's and for Mother's respectively. Thus F is Father; FM is Father's Mother; FMF is Father's Mother's Father.

As regards the principle upon which the numbers are assigned, arithmeticians will understand me when I say that it is in accordance with the binary system of notation, which runs parallel to the binary distribution of the successive ranks of ancestry, as two parents, four grandparents, eight great-grandparents, and so on. The "subject" of the pedigree is of generation O; that of his parents, of generation 1; that of his grandparents, of generation 2, &c. This is clearly shown in the following table:—

Kinship.	Order of Generation.	Numerical Values in Binary Notation.				Numerical Values in Decimal Notation.			
Child.........	0	1				1			
Parents	1	10		11		2		3	
Gr. Par.........	2	100	101	110	111	4	5	6	7
G. Gr. Par.	3	1000	1010	1100	1110	8	10	12	14
		1001	1011	1100	1111	9	11	13	15

All the male ancestry are thus described by even numbers and the female ancestry by odd ones. They run as follows:—

F, 2.		M, 3.	
FF, 4.	FM, 5.	MF, 6.	MM; 7.
FFF, 8.	FMF, 10.	MFF, 12.	MMF, 14.
FFM, 9.	FMM, 11.	MFM, 13.	MMM, 15.

It will be observed that the double of the number of any ancestor is that of his or her Father; and that the double of the number *plus* 1 is that of his or her Mother; thus FM 5 has for her father FMF 10, and for her mother FMM 11.

When the word Brother or Sister has to be abbreviated it is safer not to be too stingy in assigning letters, but to write *br*, *sr*, and in the plural *brs*, *srs*, and for the long phrase of "brothers and sisters," to write *brss*.

All these symbols are brief enough to save a great deal of space, and they are perfectly explicit. When such a phrase has to be expressed as "the Fraternity of whom FF is one" I write in my own notes simply FF′, but there has been no occasion to adopt this symbol in the present book.

I have not satisfied myself as to any system for expressing descendants. Theoretically, the above binary system admits of extension by the use of negative indices, but the practical application of the idea seems cumbrous.

We and the French sadly want a word that the Germans possess to stand for Brothers and Sisters. Fraternity refers properly to the brothers only, but its use has been legitimately extended here to mean the brothers and the sisters, after the qualities of the latter have been reduced to their male equivalents. The Greek word *adelphic* would do for an adjective.

Pedigrees.—The method employed in the *Record of Family Faculties* for entering all the facts concerning each kinsman in a methodical manner was fully described in that book, and could not easily be epitomised here; but a description of the method in which the manuscript extracts from the records have been made for my own use will be of service to others when epitomising their own family characteristics. It will be sufficient to describe the quarto books that contain the medical extracts. Each page is ten and a half inches high and eight and a half wide, and the two opposite pages that are

SCHEDULE.

<table>
<tr><td colspan="6">JAMES LIPHOOK.</td><td colspan="6">JAMES LIPHOOK.</td></tr>
<tr><td colspan="3">Father's Father's Father and his fraternity.</td><td colspan="3">Father's Mother's Father and his fraternity.</td><td colspan="3">Mother's Father's Father and his fraternity.</td><td colspan="3">Mother's Mother's Father and his fraternity.</td></tr>
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<tr><td>Spare space.</td><td colspan="4">Father and his fraternity.</td><td>Spare space.</td><td>Spare space.</td><td colspan="4">Mother and her fraternity.</td><td>Spare space.</td></tr>
<tr><td colspan="6">Spare space.</td><td colspan="6">Children.</td></tr>
</table>

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EXAMPLE A.

Father's name . . . . JAMES GLADDING.
Mother's maiden name . . . . MARY CLAREMONT.

Initials.	Kin.	Principal illnesses and cause of death.	Age at death.
J. G.	Father	Bad rheum. fever; agonising headaches; diarrhœa; bronchitis; pleurisy . . *Heart disease*	54
R. G.	bro.	Rheum. gout . . . . *Apoplexy*	56
W. G.	bro.	Good health except gout; paralysis later *Apoplexy*	83
F. L.	sis.	Rheum. fever and rheum. gout . . . *Apoplexy*	73
C. G.	sis.	Delicate . . . . (inoculated) *Small pox*	
M. G.	Mother	Tendency to lung disease; biliousness; frequent heart attacks . *Heart disease and dropsy*	63
A. C.	bro.	Good health . . . . *Accident*	46
W. C.	bro.	Led a wild life . . . . . . *Premature old age*	62
E. C.	bro.	Always delicate . . . . *Consumption*	19
F. R.	sis.	Small-pox three times . . . . *General failure*	85
R. N.	sis.	Bilious; weak health . . . . *Cancer*	50
L. C.	sis.	. . . . *Fever*	21
M. G.	bro.	Inflam. lungs; rheum. fever . . *Heart disease*	17
K. G.	bro.	Debility; heart disease; colds . *Consumption*	40
G. L.	sis.	Bad headaches; coughs; weak spine; hysteria; apoplexy . . . . *Paralysis*	50
F. S.	sis.	Bad colds; inflam. lungs; hysteria	living
R. F.	sis.	Infantile paralysis; colds; nervous depression .	living
L. G.	sis.	Inflam. brain, also lungs; neuralgia; nervous fever	living

(Space left for remarks.)

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EXAMPLE B.

Father's name . . . . JULIUS FITZROY.
Mother's maiden name . . . AMELIA MERRYWEATHER.

Initials.	Kin.	Principal illnesses and cause of death.	Age at death.
R. F.	Father	Gouty Habit . . *Weak Heart and Congest. Liver*	73
L. F.	bro.	. . . . *Gout and Decay*	88
A. G. F.	bro.	. . . . *Accident*	4
W. F.	bro.	. . . . *Typhoid*	16
	Mother	Gall stones . . . . *Internal Malady (?) Cancer*	55
P. M.	bro.	. . . . *Paralysis*	86
A. M.	bro.	. . . . *Paralysis*	85
N. M.	bro.	Still living.	
R. B.	sis.	. . . . *Consumption*	33
C. M.	sis.	. . . . *Rheum. in Head*	88
F. L.	sis.	. . . . *Softening of Brain*	76
		1 died an infant.	
G. F.	bro.	Gout: tendency to mesenteric disease; eruptive disorders . . . *Blood poisoning after a cut*	46
H. F.	bro.	Liver deranged; bad headaches; once supposed consumptive . . . . *Gradual Paralysis*	45
S. T. F.	bro.	Eruptive disorder; mesentery disease; inflammation of liver . . *Inflammation of Lungs*	42
H. G.	sis.	Eruptive disorder; liver . . . *Inflam. of Lungs*	47
H. B. R.	sis.	Delicate; tend. to mesent. disease . *Consumption*	29
N. F.	sis.	Colds; liver disorder . . . . . . *Consumption*	30
E. L. F.	sis.	Mesenteric disease; grandular swellings . *Atrophy*	16
		2 died infants.	

(Space left for remarks.)

found wherever the book is opened, relate to the same family. The open book is ruled so as to resemble the accompanying schedule, which is drawn on a reduced scale in page 251. The printing within the compartments of the schedule does not appear in the MS. books, it is inserted here merely to show to whom each compartment refers. It will be seen that the paternal ancestry are described in the left page, the maternal in the right. The method of arrangement is quite orderly, but not altogether uniform. To avoid an unpleasing arrangement like a tree with branches, and which is very wasteful of space, each grandparent and his own two parents are arranged in a set of three compartments one above the other. There are, of course, four grandparents and therefore four such sets in the schedule. Reference to the examples A and B pages 252-3 will show how these compartments are filled up. The rest of the Schedule explains itself. The children of the pedigree are written below the compartment assigned to the mother and her brothers and sisters; the vacant spaces are of much occasional service, to receive the overflow from some of the already filled compartments as well as for notes. It is astonishing how much can be got into such a schedule by writing on ruled paper with the lines one-sixth of an inch apart, which is not too close for use. Of course the writing must be small, but it may be bold, and the figures should be written very distinctly.

For a less ambitious attempt, including the grandparents and their fraternity, but not going further back, the left-hand page would suffice, placing 'Children' where 'Father' now stands, 'Father's Father' for 'Father,' and so on throughout.

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Correct this
Anthropometric Committee of Brit. Assoc., 95; anthro: laboratory data, 43, 46, 79

of Brit. Assoc.

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Bias, statistical effect of, in marriage selection 162;

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THE END.

RICHARD CLAY AND SONS, LIMITED, LONDON AND BUNGAY.

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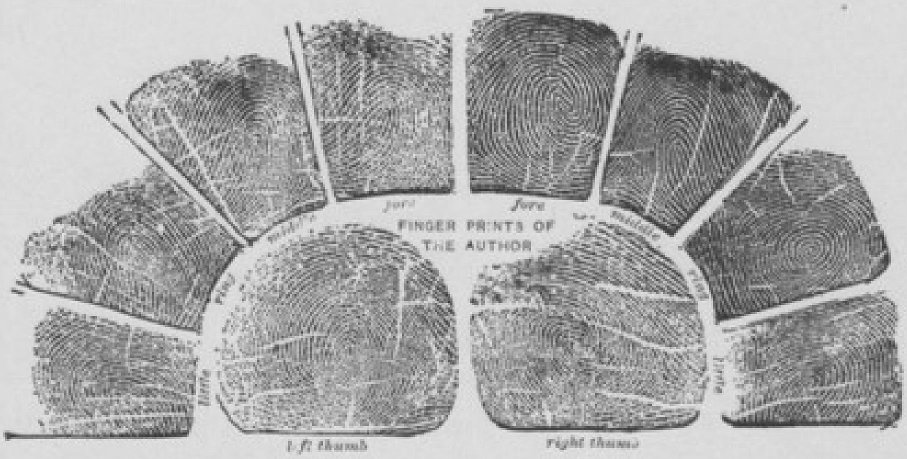
FINGER PRINTS

FINGER PRINTS



BY

FRANCIS GALTON, F.R.S., ETC.

London
MACMILLAN AND CO.
AND NEW YORK
1892

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DESCRIPTION OF THE TABLES

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DESCRIPTION OF THE PLATES

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CHAPTER I

INTRODUCTION

The palms of the hands and the soles of the feet are covered with two totally distinct classes of marks. The most conspicuous are the creases or folds of the skin which interest the followers of palmistry, but which are no more significant to others than the creases in old clothes; they show the lines of most frequent flexure, and nothing more. The least conspicuous marks, but the most numerous by far, are the so-called papillary ridges; these are the subject of the present book. If they had been only twice as large as they are they would attract general attention and have been commented on from the earliest times. Had Dean Swift known and thought of them, when writing about the Brobdignags, whom he constructs on a scale twelve times as great as our own, he would certainly have made Gulliver express horror at the ribbed fingers of the giants who handled him. The ridges on their palms would have been as broad as the thongs of our coach whips.

Let no one despise the ridges on account of their smallness, for they are in some respects the most important of all anthropological data. We shall see that they form patterns, considerable in size and of a curious variety of shape, whose boundaries can be firmly outlined, and which are little worlds in themselves. They have the unique merit of retaining all their peculiarities unchanged throughout life, and afford in consequence an incomparably surer criterion of identity than any other bodily feature. They may be made to throw welcome light on some of the most interesting biological questions of the day, such as heredity, symmetry, correlation, and the nature of genera and species. A representation of their lineations is easily secured in a self-recorded form, by inking the fingers in the way that will be explained, and pressing them on paper. There is no prejudice to be overcome in procuring these most trustworthy sign manuals, no vanity to be pacified, no untruths to be guarded against.

My attention was first drawn to the ridges in 1888 when preparing a lecture on Personal Identification for the Royal Institution, which had for its principal object an account of the anthropometric method of Bertillon, then newly introduced into the prison administration of France. Wishing to treat the subject generally, and having a vague knowledge of the value sometimes assigned to finger marks, I made inquiries, and was surprised to find, both how much had been done, and how much there remained to do, before establishing their theoretical value and practical utility.

Quite enough was seen to show that the subject was of real importance, and I resolved to investigate it; all the more so, as the modern processes of photographic printing would enable the evidence of such results as might be arrived at, to be presented to the reader on an enlarged and easily legible form, and in a trustworthy shape. Those that are put forward in the following pages, admit of considerable extension and improvement, and it is only the fact that an account of them seems useful, which causes me to delay no further before submitting what has thus far been attained, to the constructive criticism of others.

I have already published the following memoirs upon this subject:

1. "Personal Identification." *Journal Royal Inst.* 25th May 1888, and *Nature*, 28th June 1888.

2. "Patterns in Thumb and Finger Marks." *Phil. Trans. Royal Society*, vol. clxxxii. (1891) b. pp. 1-23. [This almost wholly referred to thumb marks.]

3. "Method of Indexing Finger Marks." *Proc. Royal Society*, vol. xlix. (1891).

4. "Identification by Finger Tips." *Nineteenth Century*, August 1891.

This first and introductory chapter contains, for the convenience of the reader, a brief and orderly summary of the contents of those that follow.

The second chapter treats of the previous employment of finger prints among various nations, which has been almost wholly confined to making daubs, without paying any regard to the delicate lineations with which this book is alone concerned. Their object was partly superstitious and partly ceremonial; superstitious, so far as a personal contact between the finger and the document was supposed to be of mysterious efficacy: ceremonial, as a formal act whose due performance in the presence of others could be attested. A few scattered instances are mentioned of persons who had made finger prints with enough care to show their lineations, and who had studied them; some few of these had used them as signatures. Attention is especially drawn to Sir William Herschel, who brought the method of finger prints into regular official employment when he was "Collector" or chief administrator of the Hooghly district in Bengal, and my large indebtedness to him is expressed in this chapter and in other places.

In the third chapter various methods of making good prints from the fingers are described at length, and more especially that which I have now adopted on a somewhat large scale, at my anthropometric laboratory, which, through the kindness of the authorities of South Kensington, is at present lodged in the galleries of their Science Collections. There the ten digits of both hands of all the persons who come to be measured, are impressed with clearness and rapidity, and a very large collection of prints is steadily accumulating, each set being, as we shall see, a sign manual that differentiates the person who made them, throughout the whole of his life, from all the rest of mankind.

Descriptions are also given of various methods of enlarging a finger print to a convenient size, when it is desired to examine it closely. Photography is the readiest of all; on the other hand the prism (as in a camera lucida) has merits of its own, and so has an enlarging pantagraph, when it is furnished with a small microscope and cross wires to serve as a pointer.

In the fourth chapter the character and purpose of the ridges, whose lineations appear in the finger print, are discussed. They have been the topic of a considerable amount of careful physiological study in late years, by writers who have investigated their development in early periods of unborn life, as well as their evolutionary history. They are perfectly defined in the monkeys, but appear in a much less advanced stage in other mammalia. Their courses run somewhat independently of the lines of flexure. They are studded with pores, which are the open mouths of ducts proceeding from the somewhat deeply-seated glands which secrete perspiration, so one of their functions is to facilitate the riddance of that excretion. The ridges increase in height as the skin is thickened by hard usage, until callosities begin to be formed, which may altogether hide them. But the way in which they assist the touch and may tend to neutralise the dulling effect of a thick protective skin, is still somewhat obscure. They certainly seem to help in the discrimination of the character of surfaces that are variously rubbed between the fingers.

These preliminary topics having been disposed of, we are free in the fifth chapter to enter upon the direct course of our inquiry, beginning with a discussion of the various patterns formed by the lineations. It will be shown how systems of parallel ridges sweep in bold curves across the palmar surface of the hand, and how whenever the boundaries of two systems diverge, the interspace is filled up by a compact little system of its own, variously curved or whorled, having a fictitious resemblance to an eddy between two currents. An interspace of this kind is found in the bulb of each finger. The ridges run in parallel lines across the finger, up to its last joint, beyond which the insertion of the finger nail causes a compression of the ridges on either side; their intermediate courses are in consequence so much broadened out that they commonly separate, and form two systems with an interspace between them. The independent patterns that appear in this interspace upon the bulbs of the fingers, are those with which this book is chiefly concerned.

At first sight, the maze formed by the minute lineations is bewildering, but it is shown that every interspace can be surely outlined, and when this is done, the character of the pattern it encloses, starts conspicuously into view. Examples are given to show how the outlining is performed, and others in which the outlines alone are taken into consideration. The cores of the patterns are also characteristic, and are described separately. It is they alone, that have attracted the notice of previous inquirers. The outlines fall for the most part into nine distinct genera, defined by the relative directions of the divergent ridges that enclose them. The upper pair (those on the side of the finger tip) may unite, or one or other of them may surmount the other, thus making three possibilities. There are three similar possibilities in respect to the lower pair; so, as any one of the first group may be combined with any one of the second, there are 3 × 3, or nine possibilities in all. The practice of somewhat rolling the finger when printing from it, is necessary in order to impress enough of its surface to ensure that the points at which the boundaries of the pattern begin to diverge, shall be always included.

A plate is given of the principal varieties of patterns, having regard only to their more fundamental differences, and names are attached for the convenience of description; specimens are also given of the outlines of the patterns in all the ten digits of eight different persons, taken at hazard, to afford a first idea of the character of the material to be dealt with. Another and less minute system of classification under three heads is then described, which is very useful for rough preliminary purposes, and of which frequent use is made further on. It is into Arches, Loops, and Whorls. In the Arches, there is no pattern strictly speaking, for there is no interspace; the need for it being avoided by a successive and regular broadening out of the ridges as they cross the bulb of the finger. In Loops, the interspace is filled with a system of ridges that bends back upon itself, and in which no one ridge turns through a complete circle. Whorls contain all cases in which at least one ridge turns through a complete circle, and they include certain double patterns which have a whorled appearance. The transitional cases are few; they are fully described, pictured, and assorted. One great advantage of the rude A.L.W. system is that it can be applied with little risk of error, to impressions that are smudged or imperfect; it is therefore very useful so far as it goes. Thus it can be easily applied to my own finger prints on the title page, made as they are from digits that are creased and roughened by seventy years of life, and which have moreover been closely clipped in order to fit them into a limited space.

A third method of classification is determined by the origin of the ridges which supply the interspace, whether it be from the thumb side or the little-finger side; in other words, from the Inner or the Outer side.

Lastly a translation from the Latin, is given of the famous Thesis or *Commentatio* of Purkenje, delivered at the University of Breslau in 1823, together with his illustratioins. It is a very rare pamphlet, and has the great merit of having first drawn attention to the patterns and attempted to classify them.

In the sixth chapter we reach the question of Persistence, whether or no the patterns are so durable as to afford a sure basis for identification. The answer was different to what had been expected. So far as the proportions of the patterns go, they are *not* absolutely fixed, even in the adult, inasmuch as they change with the shape of the finger. If the finger is plumped out or emaciated, or if it is deformed by usage, gout, or age, the proportions of the pattern will vary also. Two prints of the same finger, one taken before and the other after an interval of many years, cannot be expected to be as closely alike as two prints similarly made from the same woodcut. They are far from satisfying the shrewd test of the stereoscope, which shows if there has been an alteration even of a letter, in two otherwise duplicate pages of print. The measurements vary at different periods, even in the adult, just as much if not more than his height, span, and the lengths of his several limbs. On the other hand, the numerous bifurcations, origins, islands, and enclosures in the ridges that compose the pattern, are proved to be *almost beyond change*. A comparison is made between the pattern on a finger, and one on a piece of lace; the latter may be stretched or shrunken as a whole, but the threads of which it is made, retain their respective peculiarities. The evidence on which these conclusions are founded is considerable, and almost wholly derived from the collections made by Sir W. Herschel, who most kindly placed them at my disposal. They refer to one or more fingers, and in a few instances to the whole hand, of fifteen different persons. The intervals before and after which the prints were taken, amount in some cases to thirty years. Some of them reach from babyhood to boyhood, some from childhood to youth, some from youth to advanced middle age, one from middle life to incipient old age. These four stages nearly include the whole of the ordinary life of man. I have compared altogether some 700 points of reference in these couplets of impressions, and only found a single instance of discordance, in which a ridge that was cleft in a child became united in later years. Photographic enlargements are given in illustration, which include between them a total of 157 pairs of points of reference, all bearing distinctive numerals to facilitate comparison and to prove their unchangeableness. Reference is made to another illustrated publication of mine, which raises the total number of points compared, to 389, all of which were successful, with the single exception above mentioned. The fact of an almost complete persistence in the peculiarities of the ridges from birth to death, may now be considered as determined. They existed before birth, and they persist after death, until effaced by decomposition.

In the seventh chapter an attempt is made to appraise the evidential value of finger prints by the common laws of Probability, paying great heed not to treat variations that are really correlated as if they were independent. An artifice is used by which the number of portions is determined, into which a print may be divided, in each of which the purely local conditions introduce so much uncertainty, that a guess derived from a knowledge of the outside conditions, is as likely as not to be wrong. A square of six ridge-intervals in the side, was shown by three different sets of experiments to be larger than required; one of four ridge-intervals in the side, was too small, but one of five ridge-intervals appeared to be closely correct. A six-ridge interval square was, however, at first adopted, in order to gain assurance that the error should be on the safe side. As

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an ordinary finger print contains about twenty-four of these squares, the uncertainty in respect to the entire contents of the pattern *due to this cause alone*, is expressed by a fraction of which the numerator is 1, and the denominator is 2 multiplied into itself twenty-four times, which amounts to a number so large that it requires eight figures to express it.

A further attempt was made to roughly appraise the neglected uncertainties relating to the outside conditions, but large as they are, they seem much inferior in their joint effect to the magnitude of that just discussed.

Next it was found possible, by the use of another artifice, to obtain some idea of the evidential value of identity when two prints agree in all but one, two, three, or any other number of particulars. This was done by using the five ridge-interval squares, of which thirty-five may be considered to go into a single finger print, being about the same as the number of the bifurcations, origins, and other points of comparison. The accidental similarity in their numbers, enables us to treat them roughly as equivalent. On this basis the well-known method of binomial calculation is easily applied, with the general result that, notwithstanding a failure in ~~the evidence of identity with respect to a few points, in the prints of each of, say,~~ three fingers, amply enough evidence would be supplied by the remainder to prevent any doubt that the two sets of prints were made by the same ~~fingers~~ person. When a close correspondence exists in respect to all the ten digits, the thoroughness

of evidence in a few points, as to the identity of two sets of prints, each say, of of the differentiation of each man from all the rest of the human species, is multiplied to an extent far beyond the capacity of human imagination. There can be no doubt that the evidential value of ideality afforded by prints of two or three of the fingers, is so great as to render it superfluous to seek confirmation from other sources.

The eighth chapter deals with the frequency with which the several kinds of pattern appear on the different digits of the same person, severally and in connection. The subject is a curious one, and the inquiry establishes unexpected relationships and distinctions between different fingers and between the two hands, to whose origin there is at present no clue. Among the relationships, the following is strongly marked;—calling any two of the digits on either hand, by the letters A and B, and that digit on the other hand which corresponds to B, by the symbol B 1, then the relationships between A and B, and that between A and B 1, are identical in a statistical sense.

The chief novelty in this chapter is an attempt to classify nearness of relationship upon a centesimal scale, in which the number of correspondences due to mere chance counts as 0°, and complete identity as 100°. It seems reasonable to adopt the scale with only slight reservation, when the average numbers of the Arches, Loops, and Whorls are respectively the same in the two kinds of digit which are compared together; but when they differ greatly, there are no means free from objection, of determining the 100°

division of the scale; so the results, if noted at all, are subject to grave doubt.

Applying this scale, it appears that digits on opposite hands, which bear the same name, are more nearly related together than digits bearing different names, in about the proportion of three to two. It seems also, that of all the digits, none are so nearly related as the middle finger to the two adjacent ones.

In the ninth chapter, various methods of indexing are discussed and proposed, by which a set of finger prints may be so described by a few ~~figures~~ letters, that it can be easily searched for and found in any large collection, just as the name of a person is found in a directory. The procedure adopted, is to apply the Arch-Loop-Whorl classification to all ten digits, describing each digit in the order in which it is taken, by the letter A, L, or W, as the case may be, and arranging the results in alphabetical sequence. The downward direction of the slopes of loops on the fore-fingers, is also taken into account, whether it be towards the Inner or the Outer side, thus replacing L on the fore-finger by either I or O.

Many alternative methods are examined, including both the recognition and the non-recognition of all sloped patterns. Also the gain in differentiation, when all the ten digits are catalogued, instead of only a few of them. There is so much correlation between the different fingers, and so much peculiarity in each, that theoretical notions of the value of different methods of classification, are of little worth; it is only by actual trial that the best can be determined.

Whatever plan of index be adopted, many patterns must fall under some few headings and few or no patterns under others, the former class resembling in that respect the Smiths, Browns, and other common names that occur in directories. The general value of the index, much depends on the facility with which these frequent forms can be broken up by sub-classification, the rarer forms being easily dealt with. This branch of the subject has, however, been but lightly touched, under the belief that experience with larger collections than my own, was necessary before it could be treated thoroughly; means are however indicated for breaking up the large battalions, which have answered well thus far, and seem to admit of considerable extension. Thus the number of ridges in a loop (which is by far the commonest pattern) on any particular finger, at the part of the impression where the ridges are cut by the axis of the loop, is a fairly definite and effective datum as well as a simple one; so also is the character of its inmost lineation, or core.

In the tenth chapter we come to a practical result of the inquiry, namely, its possible use as a means of differentiating a man from his fellows. In civil as well as in criminal cases, the need of some such system is shown to be greatly felt in many of our dependencies, where the features of natives are distinguished with difficulty, where there is but little variety of surnames, where there are strong motives for prevarication, especially connected with land-tenure and pensions, and a proverbial prevalence of unveracity.

It is also shown that the value to honest men of sure means of identifying themselves is not so small among civilised nations even in peace time, as to be disregarded, certainly not in times of war and of strict passports. But the value to honest men is always great of being able to identify offenders, whether they be merely deserters or formerly convicted criminals, and the method of finger prints is shown to be applicable to that purpose. ~~Its most suitable rank for the present,~~ in a criminal intelligence bureau, is probably a secondary one; the primary being some form of the already established Bertillon anthropometric method. Whatever power the latter gives of successfully searching registers, that power would be multiplied many hundredfold by the inclusion of finger prints, because their peculiarities are entirely unconnected with other personal characteristics, as we shall see further on. A brief account is given in this chapter of the Bertillon system, and an attempt is made on a small scale to verify its performance, by analysing five hundred sets of measures made at my own laboratory. These, combined with the quoted experiences in attempting to identify deserters in the United States, allow a high value to this method, ~~but~~ not so high as has been claimed for it, and ~~they~~ show the importance of supplementary means. Whenever two suspected duplicates of measurements, bodily marks, photographs and finger prints have to be compared, the lineations of the finger prints would give an incomparably more trustworthy answer to the question, whether or no the suspicion of their referring to the same person was justified, than all

f16v

the rest put together. Besides this, while measurements and photographs are serviceable only for adults, and even then under restrictions, the finger prints are available throughout life. It seems difficult to believe, now that their variety and persistence have been proved, the means of classifying them worked out, and the method of rapidly obtaining clear finger prints largely practised at my laboratory and elsewhere, that our criminal administration can long neglect the use of such a powerful auxiliary. It requires no higher skill and judgment to make, register, and hunt out finger prints, than is to be found in abundance among ordinary clerks. Of course some practice is required before facility can be gained in reading and recognising them, but not a few persons of whom I have knowledge, have interested themselves in doing so, and found no difficulty.

The eleventh chapter treats of Heredity, and affirmatively answers the question whether patterns are transmissible by descent. The inquiry proved more troublesome than was expected, on account of the great variety in patterns and the consequent rarity with which the same pattern, other than the common Loop, can be expected to appear in relatives. The available data having been attacked both by the Arch-Loop-Whorl method, and by a much more elaborate system of classification; described and figured as the C system, the resemblances between children of either sex, of the same parents (or more briefly "fraternal" resemblances, as they are here called, for want of a better term), have been tabulated and discussed. A batch of twins have also been analysed. Then cases have been treated in which both parents had the same pattern on corresponding fingers; this pattern was compared with the pattern on the corresponding finger of the child. In these and other ways, results were obtained, all testifying to the conspicuous effect of heredity, and giving results that can be measured on the centesimal scale already described. But though the qualitative results are clear, the quantitative are as yet not well defined, and that part of the inquiry must lie over until a future time, when I shall have more data and when certain foreseen improvements in the method of work may perhaps be carried out. There is a decided appearance, first observed by Mr. F. H. Collins, of whom I shall again have to speak, of the influence of the mother being stronger than that of the father, in transmitting these patterns.

In the twelfth chapter we come to a branch of the subject of which I had great expectations, that have been falsified, namely, their use in indicating Race and Temperament. I thought that any hereditary peculiarities would almost of necessity vary in different races, and that so fundamental and enduring a feature as the finger markings must in some way be correlated with temperament.

The races I have chiefly examined are English, most of whom were of the upper and middle classes; the others chiefly from London board schools; Welsh, from the purest Welsh-speaking districts of South

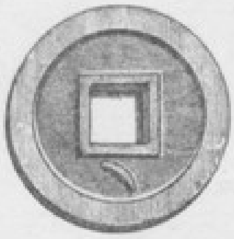
Wales; Jews from the large London schools, and Negroes from the territories of the Royal Niger Company. I have also a collection of Basque prints taken at Cambo, some twenty miles inland from Biarritz, which although small, is large enough, to warrant a provisional conclusion. As a first and only an approximatively correct description, the English, Welsh, Jews, Negroes, and Basques, may all be spoken of as identical in the character of their finger prints; the same familiar patterns appearing in all of them with much the same degrees of frequency, the differences between groups of different races being not larger than those that occasionally occur between groups of the same race. The Jews have, however, a decidedly larger proportion of Whorled patterns than other races, and I should have been tempted to make an assertion about a peculiarity in the Negroes, had not one of their groups differed greatly from the rest. The task of examination has been laborious thus far, but it would be much more so to arrive with correctness at a second and closer approximation to the truth. It is doubtful at present, whether it is worth while to pursue the subject, except in the case of the hill tribes of India and a few other peculiarly diverse races, for the chance of discovering some characteristic and perhaps a more monkey-like pattern.

Considerable collections of prints of persons belonging to different classes have been analysed, such as students in science, and students in arts; farm labourers; men of much culture; and the lowest idiots in the London district (who are all sent to Darenth Asylum), but I do not, still as a first approximation, find any decided difference between their finger prints. The ridges of artists are certainly not more delicate and close than those of men of quite another stamp.

In chapter thirteen, the question is discussed and answered affirmatively, of the right of the nine fundamentally differing patterns to be considered as different genera; also of their more characteristic varieties to rank as different genera, or species, as the case may be. The chief test applied, respected the frequency with which the various Loops that occurred on the thumbs, were found to differ, in successive degrees of difference, from the central form of all of them; it was found to accord with the requirements of the well-known law of Frequency of Error, proving the existence of a central type, from which the departures were, in common phraselogy, accidental. Now, all the evidence in the last chapter, concurs in showing that no sensible amount of correlation exists between any of the patterns on the one hand, and any of the bodily faculties or characteristics on the other. It would be absurd therefore to assert that in the struggle for existence, a person with, say, a loop on his right middle finger has a better chance of survival, or a better chance of early marriage, than one with an arch. Consequently genera and species are here seen to be formed without the slightest aid from either Natural or Sexual Selection, and these finger patterns are apparently the only peculiarity in which Panmixia, or the effect of promiscuous marriages, admits of being studied on a large scale. The result of Panmixia in finger markings, corroborates the arguments I have used in *Natural Inheritance* and elsewhere, to show that "organic stability" is the primary factor by which the distinctions between genera are maintained; consequently that the progress of evolution is not a smooth and uniform progression, but one that proceeds by jerks, through successive "sports" (as they are called), some of them implying considerable organic changes, and each in its turn being favoured by Natural Selection.

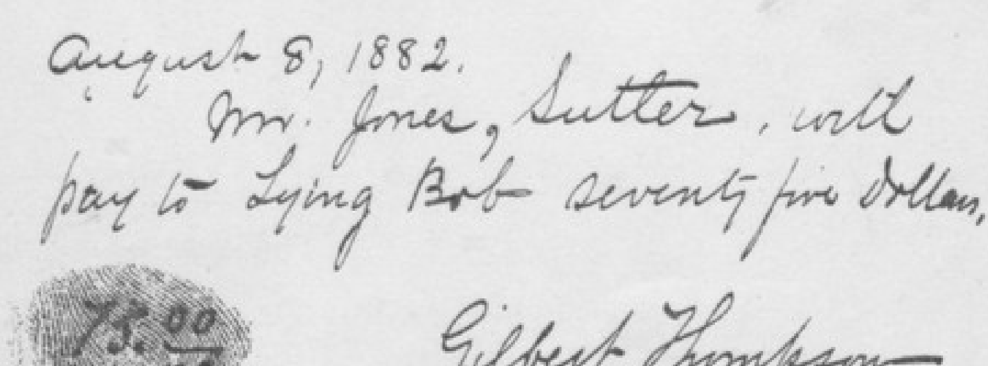
The same word "variation" has been indiscriminately applied to two very different conceptions, which ought to be clearly distinguished; the one is that of the "sports" just alluded to, which are changes in the position of organic stability and may, through the aid of Natural Selection, become fresh steps in the onward course of evolution; the other is that of the Variations proper, which are merely strained conditions of a stable form of organisation, and not in any way an overthrow of them. Sports do not blend freely together; variations proper do so. Natural Selection acts upon variations proper, just as it does upon sports, by preserving the best to become parents, and eliminating the worst, but its action upon mere variations can, as I conceive, be of no permanent value to evolution, because there is a constant tendency in the offspring to "regress" towards the parental type. The amount and results

FIG. 1.



Chinese Coin, Tang Dynasty, about 618 A.D., with nail mark of the Empress Wen-teh, figured in relief.

FIG. 2.



August 8, 1882.
Mr. Jones, Sutler, will
pay to Lying Bob seventy five dollars.

75.00/100

Gilbert Thompson
U.S.G.S.

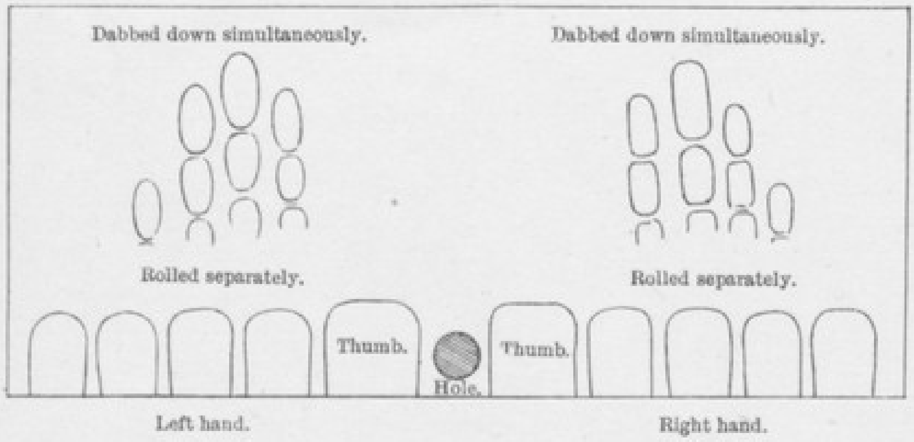
Order on a Camp Sutler, by the officer of a surveying party in New Mexico. 1882.

f.18bv

1

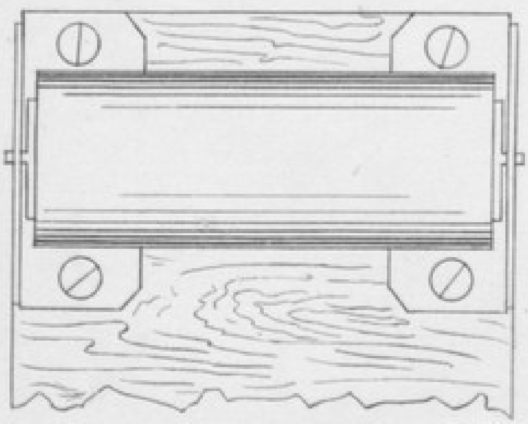
black

FIG. 3.

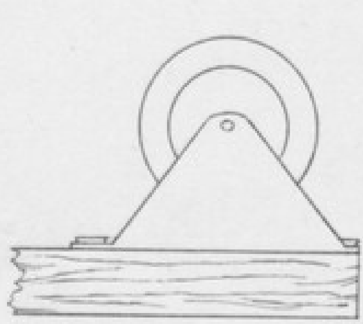


Form of card used for impressions of the ten digits. $11\frac{1}{2} \times 5$ inches.

FIG. 4.

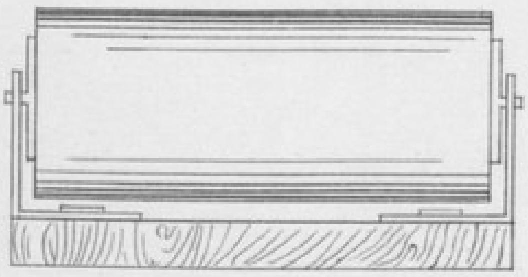


Roller seen from above.



Side view.

The outer ring is to be taken as representing either a thin india-rubber tube, or a thick layer of the composition used in printers' rollers.



End view.

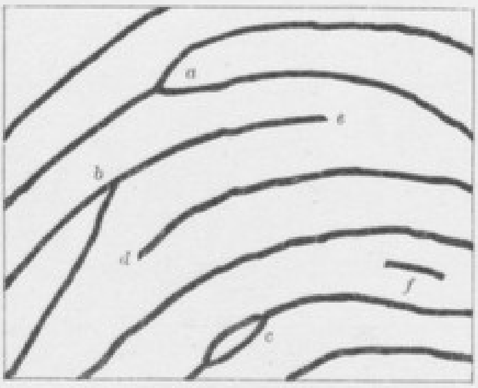
Roller and its bearings, of a pocket printing apparatus.

f.186v

2
Black

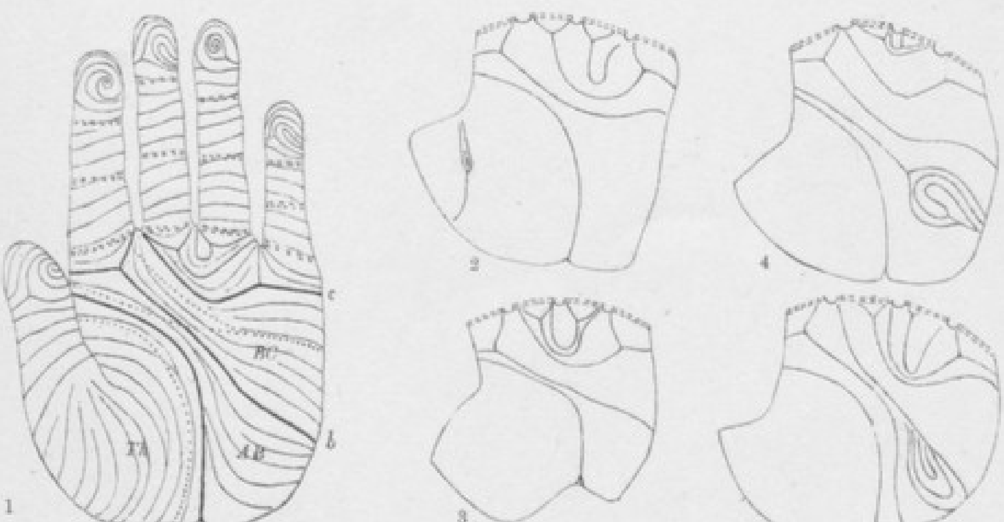
f.18dr

FIG. 5.



Characteristic peculiarities in Ridges
(about 8 times the natural size).

FIG. 6.



Systems of Ridges, and the Creases in the Palm.

f.18dr

3
black

Fig. 7.

SCARS AND CUTS, AND THEIR EFFECTS ON THE RIDGES.

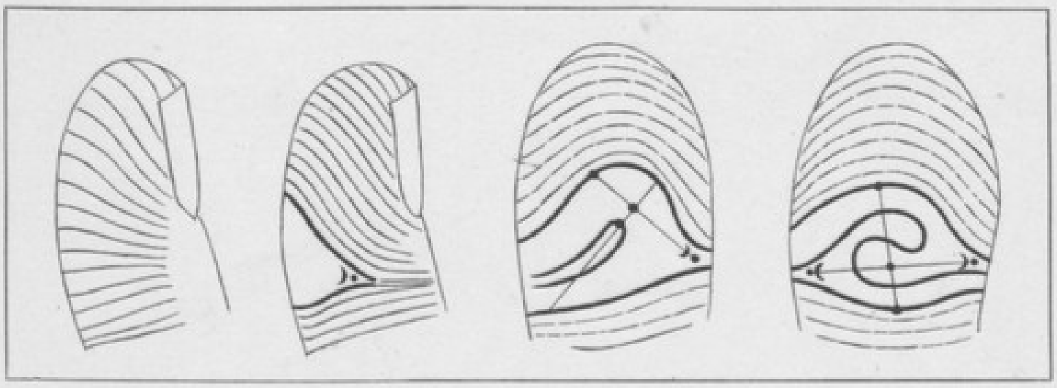
a
Effect of an Ulcer.

b
Finger of a Tailor.

c
Effect of a Cut.

Fig. 8.

FORMATION OF INTERSPACE AND EXAMPLES OF THE ENCLOSED PATTERNS.



1 2 3 4

P.18ev

(Black

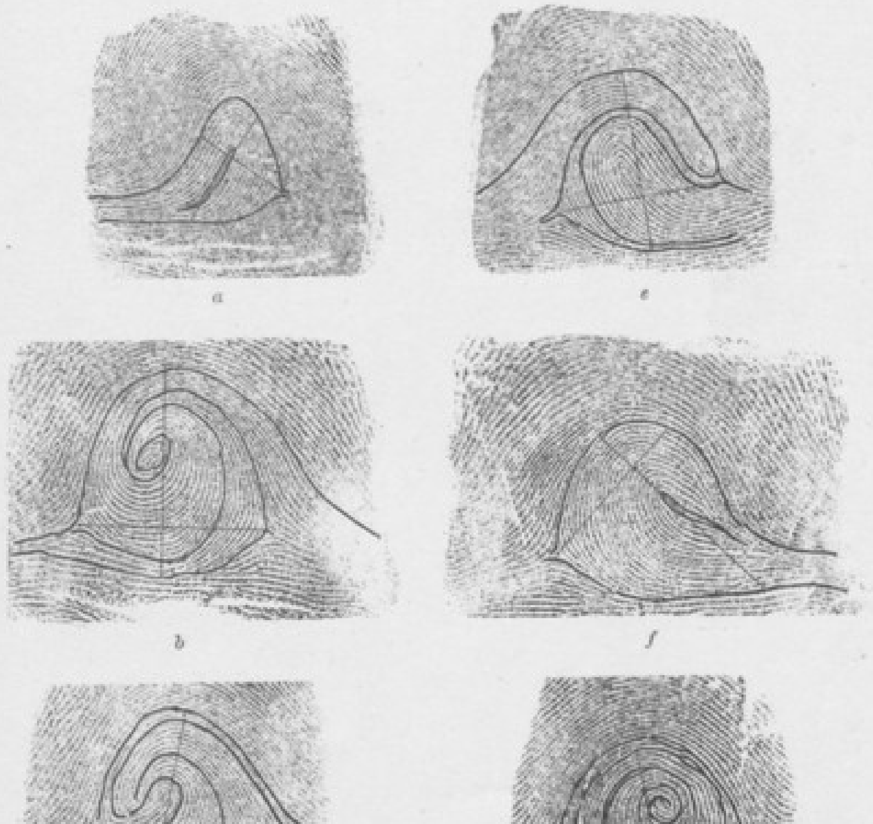
Plates 4 and 15

Figs 7 & 8 — and 22, 23

FIG. 9.

EXAMPLES OF OUTLINED PATTERNS

(The Specimens are rolled impressions of natural size).



f. 18fr

5

blank

(Block)

f.1895 6 double

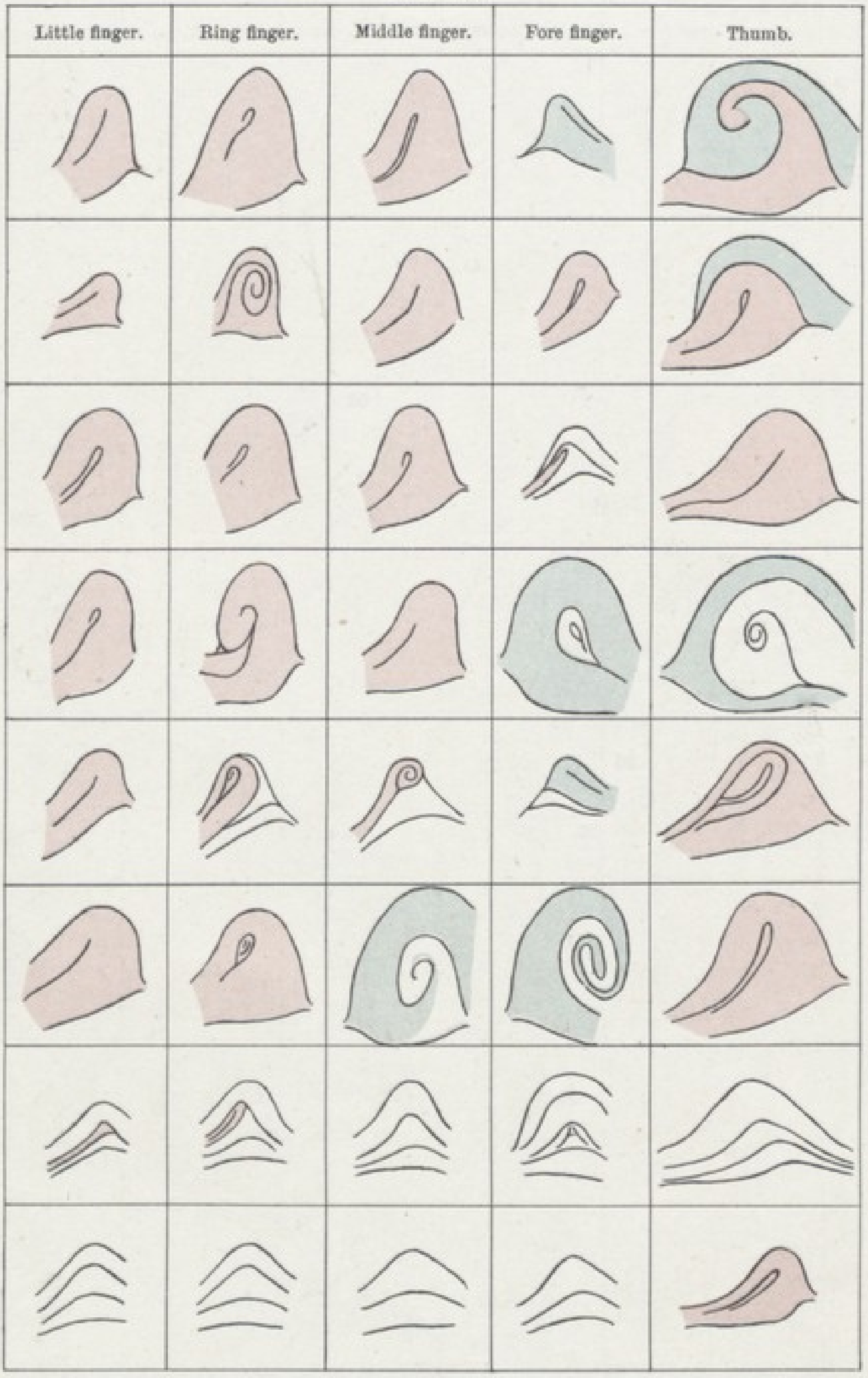
Plate 6 (double plate)
fig 10

f.189v

F

OUTLINES OF THE PATTERNS OF THE D

LEFT HAND.



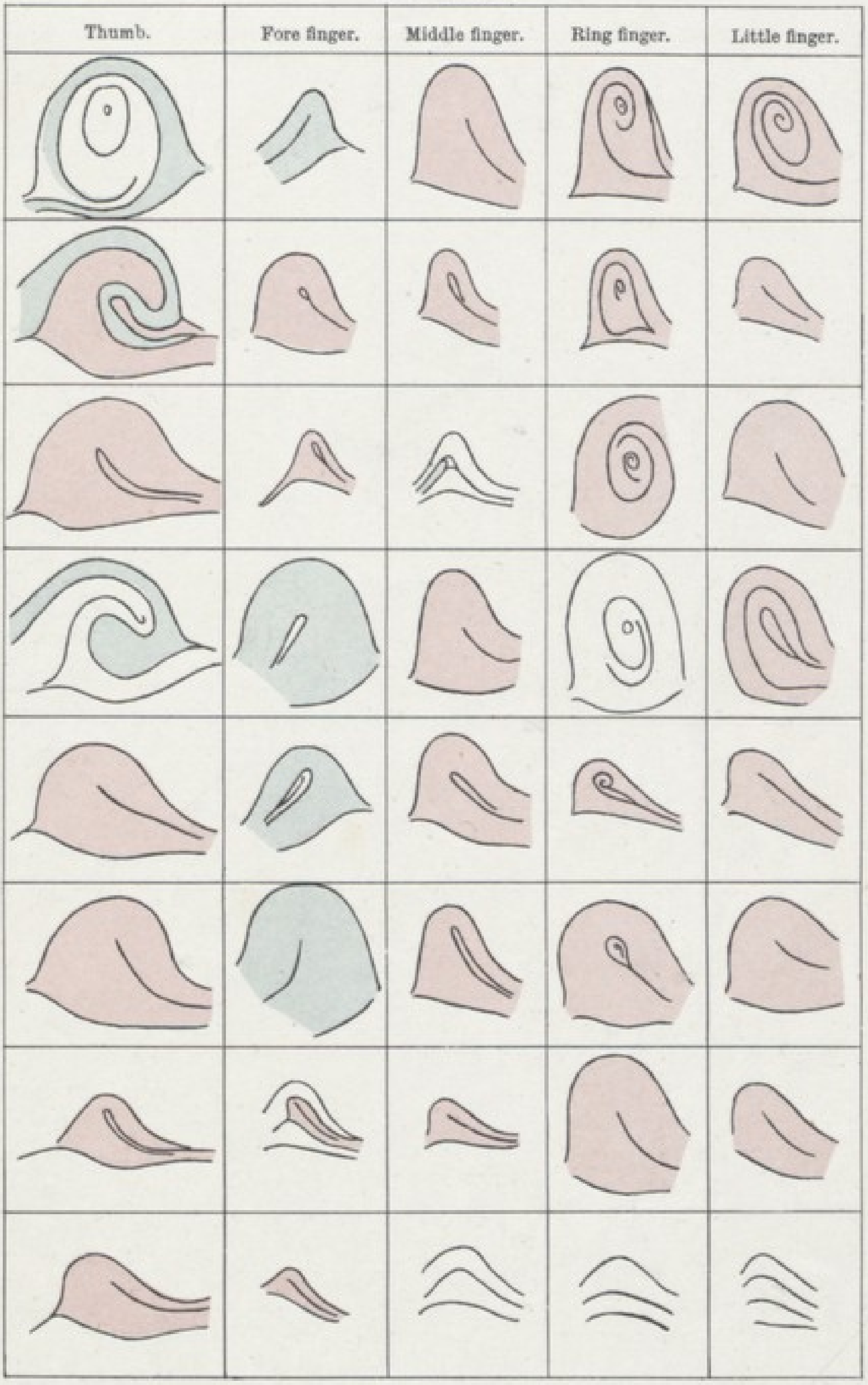
f.18h

PLATE 6.

ig. 10.

igits of Eight Persons, taken at random.

Right Hand.



f.181r

7. 8
black

FIG. 11. ARCHES.

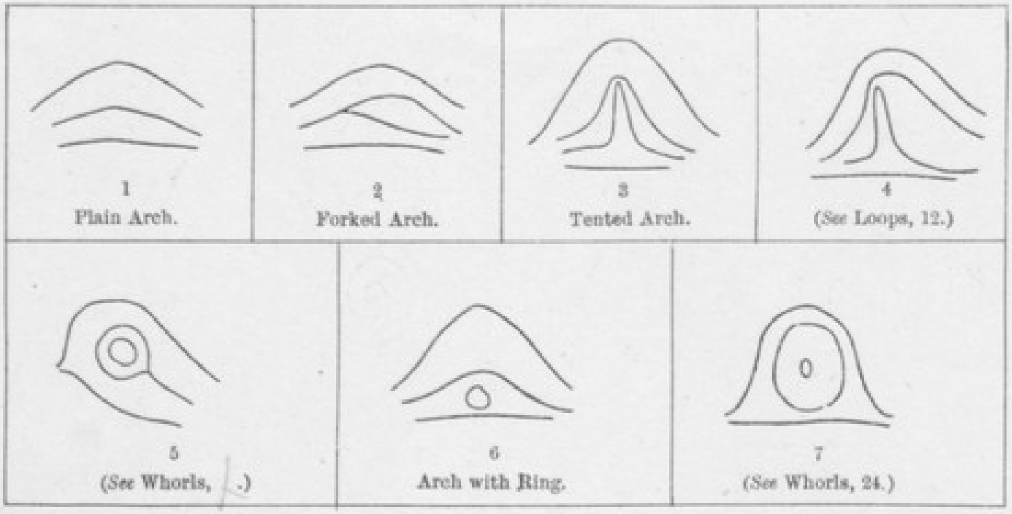


FIG. 12. LOOPS.

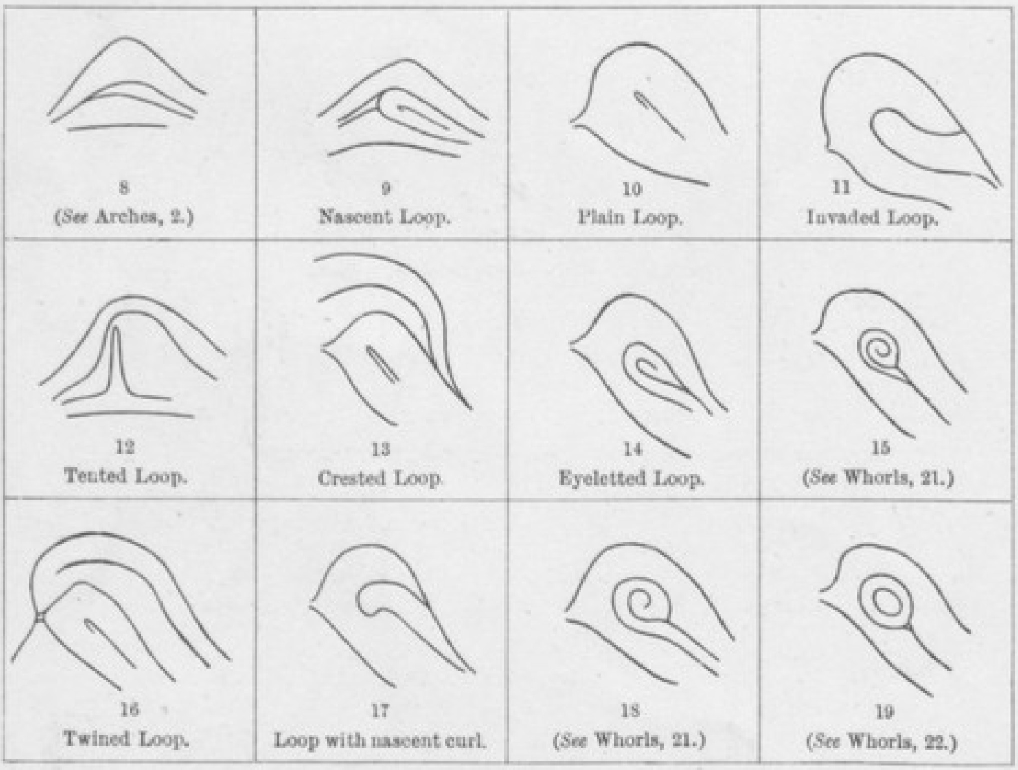
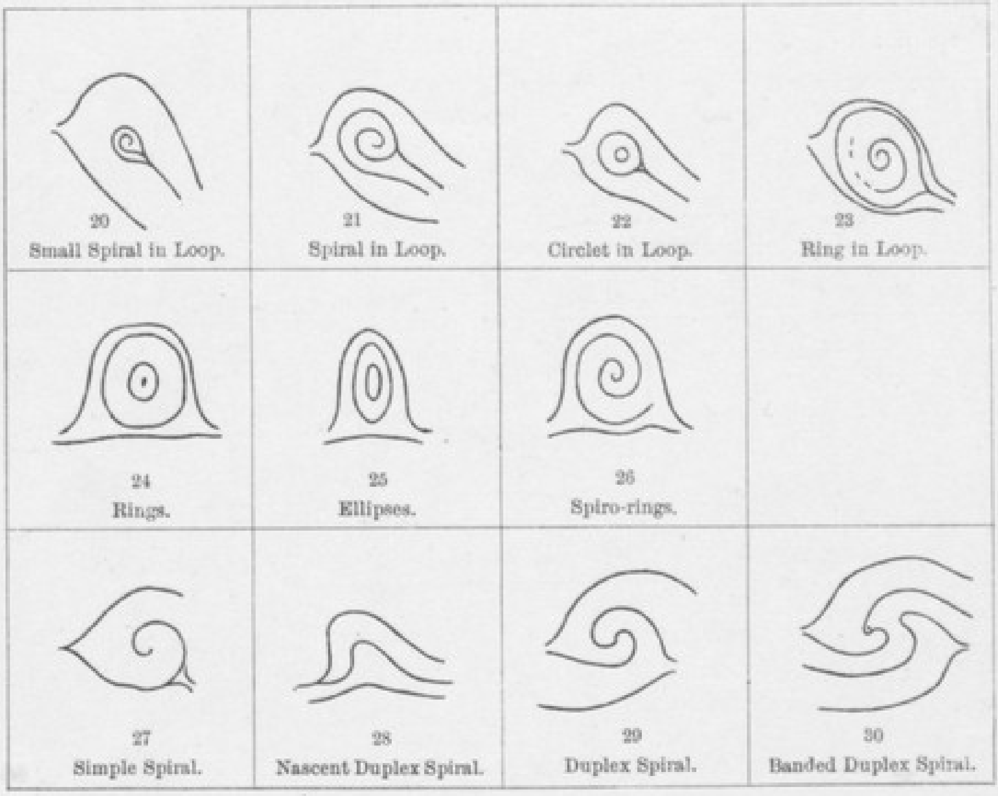


Fig. 13. WHORLS.



CORES TO LOOPS.

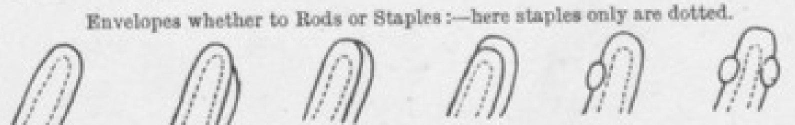
Fig. 14. Rods :—their envelopes are indicated by dots.

31 Single. 32 Eyed. 33 Double. 34 Multiple. 35 Monkey.

Staples :—their envelopes are indicated by dots.

36 Plain. 37 ¼ parted. 38 ½ parted. 39 ¾ parted. 40 Tuning fork. 41 Single eyed. 42 Double eyed.

Envelopes whether to Rods or Staples :—here staples only are dotted.



43 Plain. 44 ¼ parted. 45 ½ parted. 46 ¾ parted. 47 Single eyed. 48 Double eyed.

Fig. 15. CORES TO WHORLS.

49 Circles. 50 Ellipses. 51 Spiral. 52 Twist 53 Plait. 54 Deep Spiral.

Black

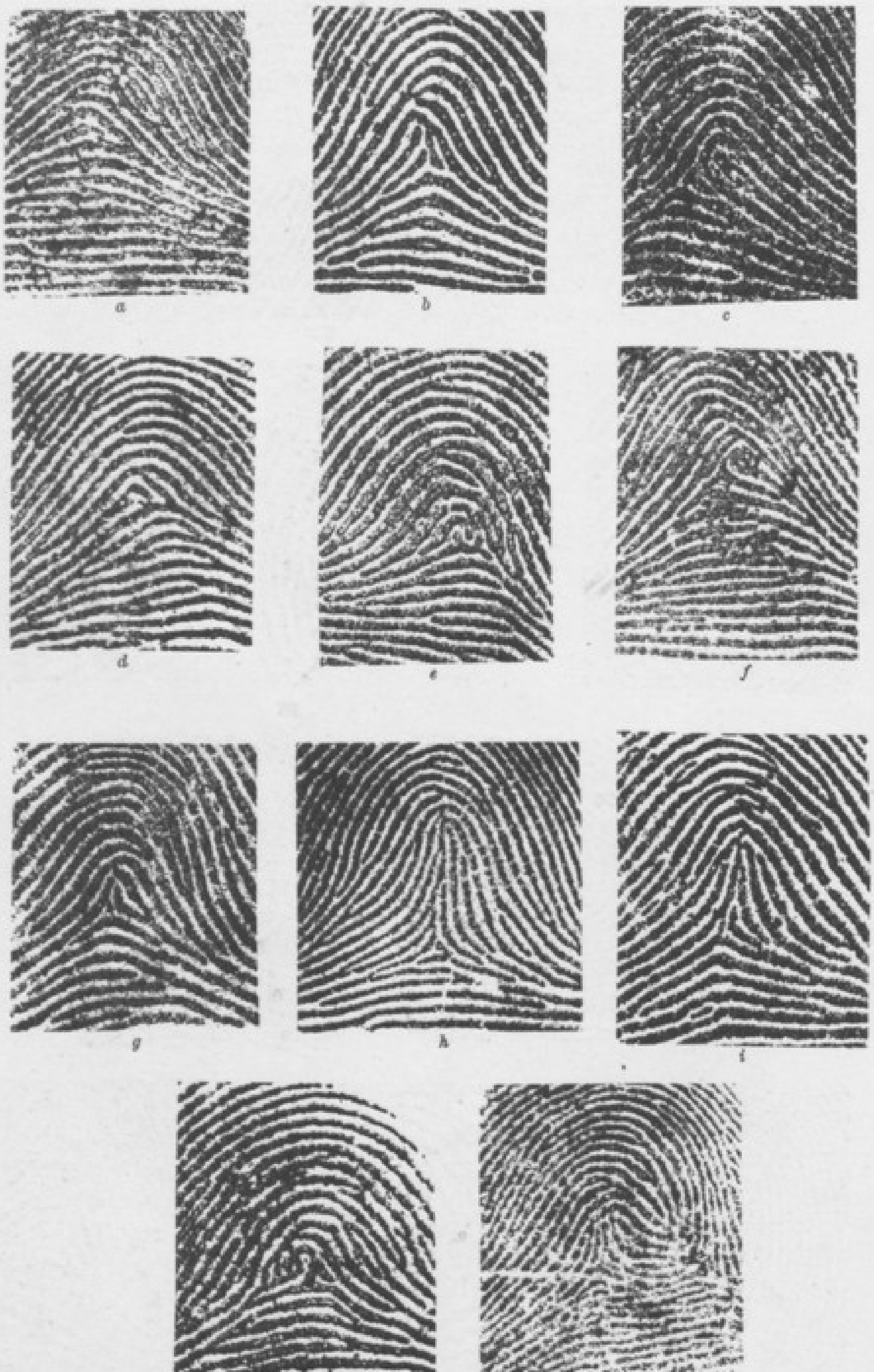
f.18Kr 9 x 10 double

Plates 9 & 10

figs 15 & 16

Fig. 15.

TRANSITIONAL PATTERNS—Arches and Loops (enlarged three times).



f.18l.

Fig. 16.

TRANSITIONAL PATTERNS—Loops and Whorls (enlarged three times).

f18mr

Fig. 17.

ORIGIN OF SUPPLY OF RIDGES TO PATTERNS OF PRINTS OF RIGHT HAND.

Of the two letters in the left upper corner of each compartment, the first refers to the source of upper boundary of the pattern, the second to the lower boundary.
For patterns on the prints of left hands, *Ii* and *Oo* must be interchanged.

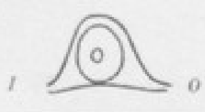
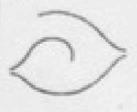
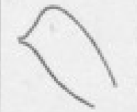
Arches		Rings		Duplex Spirals	
from both sides		from neither side		from both sides	
I and *O* both absent.		*I* and *O* both present		upper supply from	
				I side	*O* side
jj 	*jj* 	*jj* 		*oi* 	*io* 
Spirals		Loops		Spirals	
from *I* side		from *I* side	from *O* side	from *O* side	
above	below	*I* absent	*O* absent	above	below
oj 	*jo* 	*oo* 	*ii* 	*ij* 	*ji* 

Fig. 18.

Ambiguities in prints of the Minutiæ.

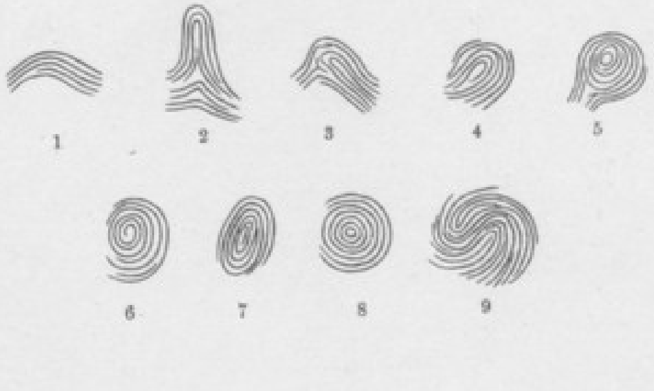
f18mv

11
black

FIG. 19.

THE STANDARD PATTERNS OF PURKENJE.

THE CORES OF THE ABOVE PATTERNS.



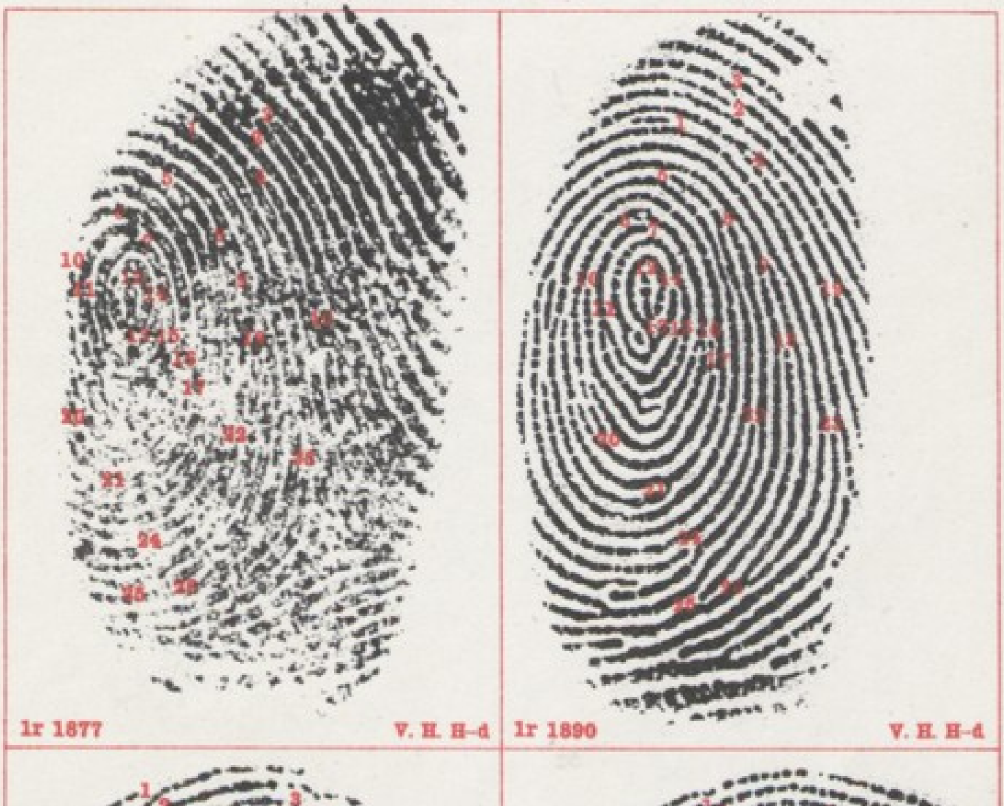
Black

f 18vv

Plate 12
Fig 19

FIG. 20.

V. H. H-D æt. $2\frac{1}{2}$ in 1877, and again as a boy in Nov. 1890.



(Black & Red)

f. 180v

Plates 13 14

fig. 20 21

f.18pr

Fig. 21.


1881 1 AEHH
1r 1890
1862 5 FKH
1r 1888
1881 2 AEHH
3r 1890
1859 6 RFH
2r 1890
1862 3 NHT
1890
7 WJH
thumb
1862 4 NHT
2r 1890
1859 8 WJH
3r 1890

Black & red

f.18pv

Plate 14
fig. 21

Fig. 22.

RIGHT FOREFINGER of Sir W. J. H. in 1860 and in 1888.

in 1860. *in 1888.*

Fig. 23.

DISTRIBUTION of the PERIODS of LIFE, to which the evidence of persistency refers.

Persons.	Age at first print.	Interval in years.	Age at second print.
H. H—d	2	13	15
A. H—l	4	12	16
J. H—l	8	13	21
E. H—l	10	13	23
W. J. H—l	26	30	56
R. F. H—n	26	31	57
N. H. T—n	27	28	55
F. H. H—t	27	26	53
W. G—e	62	17	79

f.189v

Black

Plate 15

Figs 22, 23

f.185r

FIG. 24.

The "C" set of standard patterns, for prints of the Right Hand.

wrongly numbered throughout

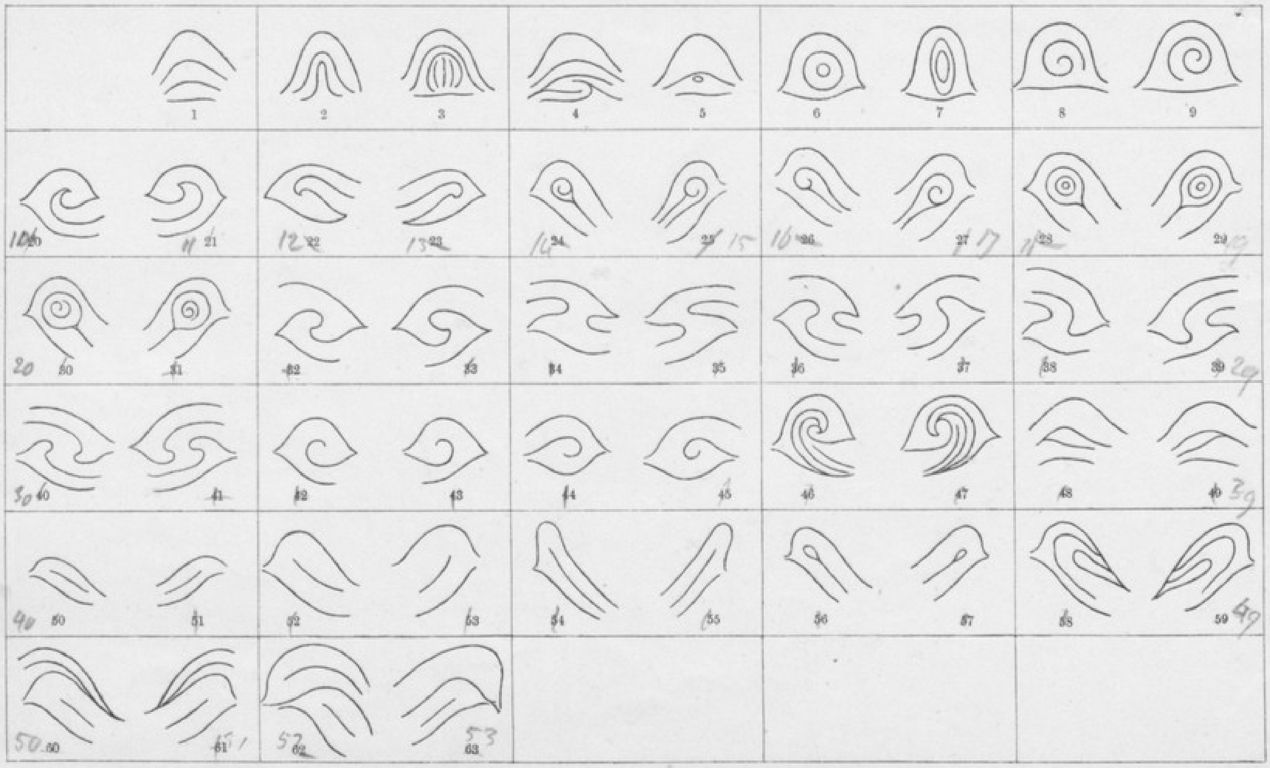


PLATE 16.

f.185v

16
black

f.19r

of this tendency have been fully established in *Natural Inheritance.* It is there shown, that after a certain departure from the central typical form has been reached in any race, a further departure becomes impossible without the aid of these sports. In the successive generations of such a population, the average tendency of filial regression towards the racial centre, must at length counterbalance the effects of filial dispersion; consequently the best of the produce cannot advance beyond the level already attained by the parents, the rest falling short of it in various degrees.

In concluding these introductory remarks, I have to perform the grateful duty of acknowledging my indebtedness to Mr. F. Howard Collins, who materially helped me during the past year. He undertook the numerous and tedious tabulations upon which the chapters on Heredity, and on Races and Classes, are founded, and he thoroughly revised nearly the whole of my MS., to the great advantage of the reader of this book.

CHAPTER II

Previous use of Finger Prints

THE employment of impressions of the hand or fingers, to serve as sign manuals, will probably be found in every nation of importance, but the significance attached to them differs. It ranges from a mere superstition that personal contact is important, up to the conviction of which this book will furnish assurance, that when they are properly made, they are incomparably the most sure and unchanging of all forms of signature. The existence of the superstitious basis is easily noted in children and the uneducated; it occupies a prominent place in the witchcrafts of barbarians. The modern witness who swears on the Bible, is made to hold it and afterwards to kiss it; he who signs a document, touches a seal or wafer, and declares that "this is my act and deed." Students of the primitive customs of mankind, find abundant instances of the belief, that personal contact communicates some mysterious essence from the thing touched to the person who touches it, and *vice versa*; but it is unnecessary here to enter further into these elementary human reasonings, which are fully described and discussed by various well-known writers.

The next grade of significance attached to an impression, resembles that which commends itself to the mind of a hunter who is practised in tracking. He notices whether a footprint he happens to light upon, is larger or smaller, broader or narrower, or otherwise differs from the average, in any special peculiarity; he thence draws his inferences as to the individual who made it. So, when a chief presses his hand smeared with blood or grime, upon a clean surface, a mark is left in some degree characteristic of him. It may be that of a broad stumpy hand, or of a long thin one; it may be large or small; it may even show lines corresponding to the principal creases of the palm. Such hand prints have been made and repeated in many semi-civilised nations, and have even been impressed in vermilion on their state documents, as formerly by the sovereign of Japan. Though mere smudges, they serve in a slight degree to individualise the signer, while they are more or less clothed with the superstitious attributes of personal contact. No higher form of finger printing than this has ever existed, so far as I can learn, in regular and well-understood use, in any barbarous or semi-civilised nation. The ridges dealt with in this book, could not be seen at all in such rude prints, much less could they be utilised as strictly distinctive features. It is possible that when impressions of the fingers have been made in wax, and used as seals to documents, they may sometimes have been subjected to minute scrutiny; but no account has yet reached me of trials in any of their courts of law, about disputed signatures, in which the identity of the party who was said to have signed with his finger print, had been established or disproved by comparing it with a print made by him then and there. The reader need be troubled with only a few examples, taken out of a considerable collection of extracts from books and letters, in which prints, or rather daubs of the above kind, are mentioned.

A good instance of their small real value may be seen in the *Trans. China Branch of the Royal Asiatic Society*, part 1, 1847, published at Hong-Kong, which contains a paper on "Land Tenure in China," by T. Meadows Taylor, of a deed concerning a sale of land, in fac-simile, with its translation: it ends, "The mother and the son, the sellers, have in the presence of all the parties, received the price of the land in full, amounting to sixty-four taels and five mace, in perfect dollars weighed in scales. *Impression of the finger of the mother, of the maiden name of Chin.*" The impression, as it appears in the woodcut, is roundish in outline, and was therefore made by the tip and not the bulb of the finger. Its surface is somewhat mottled, but there is no trace of any ridges.

The native clerks of Bengal give the name of *tipsahi* to the mark impressed by illiterate persons who, refusing to make either a X or their caste-mark, dip their finger into the ink-pot and touch the document. The tipsahi is not supposed to individualise the signer, it is merely a personal ceremony performed in the presence of witnesses.

Many impressions of fingers are found on ancient pottery, as on Roman tiles; indeed the Latin word *palmatus* is said to mean an impression in soft clay, or else a stain upon a wall, made by a blow with the palm. Nail marks are used ornamentally by potters of various nations. They exist on Assyrian bricks as signatures; for instance, in the Assyrian room of the British Museum, on the west side of the case C 43, one of these bricks contains a notice of sale and is prefaced by words that were translated for me thus: "nail mark of Nabu-sum-usur, the seller of the field, (used) like his seal." A somewhat amusing incident affected the design of the Chinese money during the great Tang dynasty, about 618 A.D. A new and important issue of coinage was to be introduced, and the Secretary of the Censors himself moulded the design in wax, and humbly submitted it to the Empress Wen-teh for approval. She, through maladroitness, dug the end of her enormously long finger nail into its face, marking it deeply as with a carpenter's gouge. The poor Secretary of the Censors, Ngeu-yang-siun, who deserves honour from professional courtiers, suppressing such sentiments as he must have felt when his work was mauled, accepted the nail mark of the Empress as an interesting supplement to the design; he changed it into a crescent in relief, and the new coins were stamped accordingly. (See *Coins and Medals*, edited by Stanley Lane Poole, 1885, p.

221.) A drawing of one of these is given in Plate 1, Fig. 1.

The European practitioners of palmistry and cheiromancy do not seem to have paid particular attention to the ridges with which we are concerned. A correspondent of the American Journal *Science*, viii. 166, states, however, that the Chinese class the striæ at the ends of the fingers into "pots" when arranged in a coil, and into "hooks." They are also regarded by the cheiromantists in Japan. A curious account has reached me of negroes in the United States who laying great stress on the possession of finger prints in wax or dough, for witchcraft purposes, are also said to examine their striæ.

Leaving Purkenje to be spoken of in a later chapter, because he deals chiefly with classification, the first well-known person who appears to have studied the lineations of the ridges as a means of identification, was Bewick, who made an impression of his own thumb on a block of wood and engraved it, as well as an impression of a finger. They were used as fanciful designs for his illustrated books. Occasional instances of careful study may also be noted, such as that of Mr. Fauld (*Nature*, xxii. p. 605, Oct. 28, 1880), who seems to have taken much pains, and that of Mr. Tabor, the eminent photographer of San Francisco, who, noticing the lineations of a print that he had accidentally made with his own inked finger upon a blotting paper, experimented further, and finally proposed the method of finger prints for the registration of Chinese, whose identification has always been a difficulty, and was giving a great deal of trouble at that particular time; but his proposal dropped through. Again Mr. Gilbert Thompson, an American geologist, when on government duty in 1882 in the wild parts of New Mexico, paid the members of his party by order of the camp suttler. To guard against forgery he signed his name across the impression made by his finger upon the order, after first pressing it on his office pad. He was good enough to send me the duplicate of one of these cheques made out in favour of a man who bore the ominous name of "Lying Bob" (Plate 1, Fig. 2). The impression took the place of the scroll work on a ordinary cheque; it was in violent aniline ink, and looked decidedly pretty. From time to time sporadic instances like these are met with, but none are comparable in importance to the regular and official employment made of finger prints by Sir William Herschel, during more than a quarter of a century in Bengal. I was exceedingly obliged to him for much valuable information when first commencing this study, and have been almost wholly indebted to his kindness for the materials used in this book for proving the persistence of the lineations throughout life.

Sir William Herschel has presented me with one of the two original "Contracts" in Bengali, dated 1858, which suggested to his mind the idea of using this method of identification. It was so difficult to obtain credence to the signatures of the natives, that he thought he would use the signature of the hand

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itself, chiefly with the intention of frightening the man who made it from afterwards denying his formal act; however the impression proved so good that Sir W. Herschel became convinced that the same method might be further utilised. He finally introduced the use of finger prints in several departments at Hooghly in 1877, after seventeen years' experience of the value of the evidence they afforded. A too brief account of his work was given by him in *Nature*, xxiii. p. 23 (Nov. 25, 1880). He mentions there that he had been taking finger marks as sign-manuals for more than twenty years, and had introduced them for practical purposes in several ways in India with marked benefit. They rendered attempts to repudiate signatures quite hopeless. Finger prints were taken of Pensioners to prevent their personation by others after their death; they were used in the office for Registration of Deeds, and at a Gaol where each prisoner had to sign with his finger. By comparing the prints of persons then living, with their prints taken twenty years previously, he considered he had proved that the lapse of at least that period, made no change sufficient to affect the utility of the plan. He informs me that he submitted, in 1877, a report in semi-official form, to the Inspector-General of Gaols asking to be allowed to extend the process; but no result followed. In 1881, at the request of the Governor of the gaol at Greenwich (Sydney), he sent a description of the method, but no further steps appear to have been taken there.

If the use of finger prints ever becomes of general importance, Sir William Herschel must be regarded as the first who devised a feasible method for regular use, and afterwards officially adopted it. His method of printing for those purposes will be found in the next chapter.

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CHAPTER III

METHODS OF PRINTING

It will be the aim of this chapter to show how to make really good and permanent impressions of the fingers. It is easy to do so when the principles of the art are understood and practised, but difficult otherwise.

One example of the ease of making good, but not permanent impressions, is found, and should be tried, by pressing the bulb of a finger against well-polished glass, or against the highly-polished blade of a razor. The finger must be *very slightly* oiled, as by passing it through the hair; if it be moist, dry it with a handkerchief before the oiling. Then press the bulb of the finger on the glass or razor, as the case may be, and a beautiful impression will be left. The hardness of the glass or steel prevents its surface from rising into the furrows under the pressure of the ridges, while the layer of oil which covers the bottom of the furrows is too thin to reach down to the glass or steel; consequently the ridges alone are printed. There is no capillary or other action to spread the oil, so the impression

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remains distinct. A merely moist and not oily finger leaves a similar mark, but it soon evaporates.

This simple method is often convenient for quickly noting the character of a finger pattern. The impression may be made on a window pane, a watch glass, or even an eye glass, if nothing better is at hand. The impression is not seen to its fullest advantage except by means of a single small source of bright light. The glass or steel has to be so inclined as just *not* to reflect the light into the eye. That part of the light which falls on the oily impression is not so cleanly reflected from it as from the surface of the glass or steel. Consequently some stray beams of the light which is scattered from the oil, reaches the eye, while all of the light reflected from the highly-polished glass or steel passes in another direction and is unseen. The result is a brilliantly luminous impression on a dark background. The impression ceases to be visible when the glass or steel is not well polished, and itself scatters the light, like the oil.

There are two diametrically opposed methods of printing, each being the complement of the other. The method used in ordinary printing, is to ink the projecting surfaces only, leaving the depressed parts clean. The other method, used in printing from engraved plates, is to ink the whole surface, and then to clean the ink from the projecting parts, leaving the depressions still filled with it. Either of these two courses can be adopted in taking finger prints, but not the two together, for when they are combined in equal degrees the result is a plain black blot.

The following explanations will be almost entirely confined to the first method, namely that of ordinary printing, as the second method has so far not given equally good results.

The ink used, may be either printer's ink or water colour, but for producing the best work, rapidly and on a large scale, the method of printer's ink seems in every respect preferable, however as water colour suffices for some purposes, and as there is so much convenience in a pad, drenched with dye, such as is commonly used for hand stamps, and which is always ready for use, that many may prefer it. The processes with printer's ink will be described first:

The relief formed by the ridges is low. In the fingers of very young children, and of some ladies whose hands are rarely submitted to rough usage, the ridges are exceptionally faint; their crests hardly rise above the furrows, yet it is the crests only that are to be inked. Consequently the layer of ink on the slab or pad on which the finger is pressed for the purpose of blackening it, must be *very thin*. Its thickness must be less than half the elevation of the ridges, for when the finger is pressed down, the crests displace the ink immediately below them, and drives it upwards into the furrows which would otherwise be choked with it.

It is no violent misuse of metaphor to compare the ridges to the crests of mountain ranges, and the depth of the blackening that they ought to receive, to that of the newly-fallen snow upon the mountain tops in the early autumn, when it powders them from above downwards to a sharply defined level. The most desirable blackening of the fingers corresponds to a snow-fall which covers all the higher passes, but descends no lower.

With a finger so inked it is scarcely possible to fail in making a good imprint; the heaviest pressure cannot spoil it. The first desideratum is, then, to cover the slab by means of which the finger is to be blackened, with an extremely thin layer of ink.

This cannot be accomplished with printer's ink unless the slab is very clean, the ink somewhat fluid, and the roller that is used to spread it, in good condition. When a plate of glass is used for the slab, it is easy, by holding the inked slab between the eye and the light, to judge of the correct amount of inking. It should appear by no means black, but of a somewhat light brown.

The thickness of ink transferred by the finger to the paper, is much less than that which lay upon the slab. The ink adheres to the slab as well as to the finger; when they are separated, only a portion of the ink is removed by the finger. Again, when the inked finger is pressed on the paper, only a portion of the ink that was on the finger is transferred to the paper. Owing to this double reduction, it seldom happens that a clear impression is at the same time black. An ideally perfect material for blackening would lie loosely on the slab like dust, it would cling very lightly to the finger, but adhere firmly to the paper.

The last preliminary to be noticed, is the slowness with which the printer's ink hardens on the slab, and the rapidity with which it dries on paper. While serviceable for hours in the former case, in the latter it will be dry in a very few seconds. The drying or hardening of this oily ink has nothing whatever to do with the loss of moisture in the ordinary sense of the word, that is to say, of the loss of the contained water: it is wholly due to oxidisation of the oil. An extremely thin oxidised film soon forms on the surface of the layer on the slab, and this shields the lower-lying portions of the layer from the air, and retards further oxidisation. But paper is very unlike a polished slab; it is a fine felt, full of minute interstices. When a printed period (.) is placed under the microscope it looks like a drop of tar in the middle of a clean bird's-nest. The ink is minutely divided among the interstices of the paper, and a large surface being thereby exposed to the air, it oxidises at once, while a print from the finger upon glass will not dry for two or three days. One effect of oxidisation is to give a granulated appearance to the ink on rollers which have been kept in a dirty state. This granulation leaves clots on the slab which are fatal to good work: whenever they are seen, the roller must be cleaned at once.

The best ink for finger printing is not the best for ordinary printing. It is important to a commercial printer that his ink should dry rapidly on the paper, and he does not want a particularly thin layer of it; consequently, he prefers ink that contains various

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drying materials, such as litharge, which easily part with their oxygen. In finger prints this rapid drying is unnecessary, and the drying materials do harm by making the ink too stiff. The most serviceable ink for our purpose, is made of any pure "drying" oil (or oil that oxidises rapidly), mixed with lampblack and very little else. I get mine in small collapsible tubes, each holding about a quarter of an ounce, from Messrs. Reeve & Sons, 113 Cheapside, London, W.C. Some thousands of fingers may be printed from the contents of one of these little tubes.

Let us now pass on to descriptions of printing apparatus. First, of that in regular use at my anthropometric laboratory at South Kensington, which has acted perfectly for three years; then of a similar but small apparatus convenient to carry about or send abroad, and of temporary arrangements in case any part of it may fail. Then lithographic printing will be noticed. In all the foregoing cases printer's ink has to be used. Next, smoke prints will be described, which at times are very serviceable; after this the methods of water colours and aniline dies; then casts of various kinds; last of all, enlargements.

Laboratory apparatus.—Mine consists of: 1, slab; 2, roller; 3, bottle of benzole (paraffin, turpentine, or solution of washing soda); 4, a funnel, with blotting paper to act as a filter; 5, printer's ink; 6, rags and duster; 7, a small glass dish; 8, cards to print on.

The *Slab* is a sheet of polished copper, $10\frac{1}{2}$ inches by 7, and about $\frac{1}{16}$ inch thick, mounted on a solid board $\frac{3}{4}$ inch thick, with projecting ears for ease of handling. The whole weighs $2\frac{1}{2}$ lbs. Each day it is cleaned with the benzole and left bright. [A slab of more than double the length and less than half the width might, as my assistant thinks, answer better.]

The *Roller* is an ordinary small-sized printer's roller, to be obtained from Messrs. Harrild, 25 Farringdon Street, 6 inches long and 3 in diameter. Mine remained in good condition for quite a year and a half. When it is worn the maker exchanges it for a new one at a trifling cost. A good roller is of the highest importance; it affords the only means of spreading ink evenly and thinly, and with quickness and precision, over a large surface. The ingenuity of printers during more than four centuries in all civilised nations, has been directed to invent the most suitable composition for rollers, with the result that particular mixtures of glue, treacle, etc., are now in general use, the proportions between the ingredients differing according to the temperature at which the roller is intended to be used. The roller, like the slab, is cleansed with benzole every day (a very rapid process) and then put out of the reach of dust. Its clean surface is smooth and shining.

The *Benzole* is kept in a pint bottle. Sometimes paraffin or turpentine have been used instead; washing soda does not smell, but it dissolves the ink more slowly. They are otherwise nearly equally effective in cleansing the rollers and fingers. When dirty, the benzole can be rudely filtered and used again.

The *Funnel* holds blotting paper for filtering the benzole. Where much printing is going on, and consequent washing of hands, it is worth while to use a filter, as it saves a little daily expense, though benzole is very cheap, and a few drops of it will clean a large surface.

The *Ink* has already been spoken of. The more fluid it is the better, so long as it does not "run." A thick ink cannot be so thinned by adding turpentine, etc., as to make it equal to ink that was originally fluid. The variety of oils used in making ink, and of the added materials, is endless. For our purpose, any oil that dries and does not spread, such as boiled or burnt linseed oil, mixed with lampblack, is almost all that is wanted. The burnt oil is the thicker of the two, and dries the faster. Unfortunately the two terms, burnt and boiled linseed oil, have no definite meaning in the trade, boiling or burning not being the simple processes these words express, but including an admixture of drying materials, which differ with each manufacturer; moreover, there are two, if not three, fundamentally distinct qualities of linseed, in respect to the oil extracted from it. The ink used in the laboratory and described above, answers all requirements. Many other inks have suited less well; less even than that which can be made, in a very homely way, with a little soot off a plate that had been smoked over a candle, mixed with such boiled linseed oil as can be bought at unpretentious oil and colour shops, its only fault being a tendency to run.

Rags, and a comparatively clean duster, are wanted for cleaning the slab and roller, without scratching them.

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The small *Glass Dish* holds the benzole, into which the inked fingers are dipped before wiping them with the duster. Soap and water complete the preliminary cleansing.

Cards, lying flat, and being more easily manipulated than paper, are now used at the laboratory for receiving the impressions. They are of rather large size, $11\frac{1}{2} \times 5$ inches, to enable the prints of the ten digits to be taken on the same card in two rather different ways (see Plate 2, Fig. 3), and to afford space for writing notes. The cards must have a smooth and yet slightly absorbent surface. If too highly glazed they cease to absorb, and more ink will remain on the fingers and less be transferred from them to the paper. A little trial soon determines the best specimen from among a few likely alternatives. "Correspondence cards" are suitable for taking prints of not more than three fingers, and are occasionally employed in the laboratory. Paper books and pads were tried, but their surfaces are inferior to cards in flatness, and their use is now abandoned.

The cards should be *very* white, as if a photographic enlargement should at any time be desired, a slight tint on the card will be an impediment to making a photograph that shall be as sharp in its lines as an engraving, it being recollected that the cleanest prints are brown, and therefore not many shades darker than the tints of ordinary cards.

The method of printing at the laboratory is to squeeze a drop or so of ink on to the slab, and to work it thoroughly with the roller until a thin and even layer is spread, just as is done by printers, from one of whom a beginner might well purchase a lesson. The thickness of the layer of ink is tested from time to time by taking a print of a finger, and comparing its clearness and blackness with that of a standard print, hung up for the purpose close at hand. If too much ink has been put on the slab, some of it must be cleaned off, and the slab rolled afresh from what remains on it and on the roller. But this fault should seldom be committed; little ink should be put on at first, and more added little by little, until the required result is attained.

The right hand of the subject, which should be quite passive, is taken by the operator, and the bulbs of his four fingers laid flat on the inked slab and pressed gently but firmly on it by the flattened hand of the operator. Then the inked fingers are laid flat upon the upper part of the right-hand side of the card (Plate 2, Fig. 3), and pressed down gently and firmly, just as before, by the flattened hand of the operator. This completes the process for one set of prints of the four fingers of the right hand. Then the bulb of the thumb is slightly *rolled* on the inked slab, and again on the lower part of the card, which gives a more extended but not quite so sharp an impression. Each of the four fingers of the same hand, in succession, is similarly rolled and impressed. This completes the process for the second set of prints of the digits of the right hand. Then the left hand is treated in the same way.

The result is indicated by the diagram, which shows on what parts of the card the impressions fall. Thus each of the four fingers is impressed twice, once above with a simple dab, and once below with a rolled impression, but each thumb is only impressed once; the thumbs being more troublesome to print from than fingers. Besides, the cards would have to be made even larger than they are, if two impressions of each thumb had to be included. It takes from two and a half to three minutes to obtain the eighteen impressions that are made on each card.

The *pocket apparatus* is similar to one originally made and used by Sir William J. Herschel (see Plate 3, Fig. 4, in which the roller and its bearings are drawn of the same size as those I use). A small cylinder of hard wood, or of brass tube, say $1\frac{3}{4}$ inch long, and $\frac{1}{2}$ or $\frac{3}{4}$ inch in diameter, has a pin firmly driven into each end to serve as an axle. A piece of tightly fitting india-rubber tubing is drawn over the cylinder. The cylinder, thus coated with a soft smooth compressible material, turns on its axle in two brackets, each secured by screws, as shown in Plate 2, Fig. 4, to a board (say $6 \times 2\frac{1}{2} \times \frac{1}{4}$ inch) that serves as handle. This makes a very fair and durable roller; it can be used in the heat and damp of the tropics, and is none the worse for a wetting, but it is by no means so good for delicate work as a cylinder covered with roller composition. These are not at all difficult to make; I have cast them for myself. The mould is a piece of brass tube, polished inside. A thick disc, with a central hole for the lower pin of the cylinder, fits smoothly into the lower end of the mould, and a ring with a thin bar across it, fits over the other end, the upper pin of the cylinder entering a hole in the middle of the bar; thus the cylinder is firmly held in the right position. After slightly oiling the inside of the mould, warming it, inserting the disc and cylinder, and fitting on the ring, the melted composition is poured in on either side of the bar. As it contracts on cooling, rather more must be poured in than at first appears necessary. Finally the roller is pushed out of the mould by a wooden ramrod, applied to the bottom of the disc. The composition must be melted like glue, in a vessel surrounded by hot water, which should never be allowed to boil; otherwise it will be spoilt. Harrild's best composition is more than twice the cost of that ordinarily used, and is expensive for large rollers, but for these miniature ones the cost is unimportant. The mould with which my first roller was made, was an old pewter squirt with the nozzle cut off; its piston served the double purpose of disc and ramrod.

The *Slab* is a piece of thick plate glass, of the same length and width as the handle to the roller, so they pack up easily together; its edges are ground to save the fingers and roller alike from being cut. (Porcelain takes the ink better than glass, but is not to be commonly found in the shops, of a convenient shape and size; a glazed tile makes a capital slab.) A collapsible tube of printer's ink, a few rags, and a phial of washing soda, complete the equipment (benzole may spoil india-rubber). When using the apparatus, spread a newspaper on the table to prevent accident, have other pieces of newspaper ready to clean the roller, and to remove any surplus of ink from it by the simple process of rolling it on the paper. Take care that the washing soda is in such a position that it cannot be upset and ruin the polish of the table. With these precautions, the apparatus may be used with cleanliness even in a drawing-room. The roller is of course laid on its back when not in use.

My assistant has taken good prints of the three first fingers of the right hands of more than 300 school children, say 1000 fingers, in a few hours during the same day, by this apparatus. Hawksley, 357 Oxford Street, sells a neatly fitted-up box with all the necessary apparatus.

Rougher arrangements.—A small ball made by tying chamois leather round soft rags, may be used in the absence of a roller. The fingers are inked from the ball, over which the ink has been evenly distributed, by dabbing it many times against a slab or plate. This method gives good results, but is slow; it would be intolerably tedious to employ it on a large scale, on all ten digits of many persons.

It is often desirable to obtain finger prints from persons at a distance, who could not be expected to trouble themselves to acquire the art of printing for the purpose of making a single finger print. On these occasions I send folding cases to them, each consisting of two pieces of thin copper sheeting, fastened side by side to a slip of pasteboard, by bending the edges of the copper over it. The pasteboard is half cut through at the back, along the space between the copper sheets, so that it can be folded like a reply post-card, the copper sheets being thus brought face to face, but prevented from touching by the margin of an interposed card, out of which the middle has been cut away. The two pieces of copper being inked and folded up, may then be sent by post. On arrival the ink is fresh, and the folders can be used as ordinary inked slabs. (See also smoke prints, page .)

The fluidity of even a very thin layer of ink seems to be retained for an indefinite time if the air is excluded to prevent oxidisation. I made experiments, and found that if pieces of glass (photographic quarter plates) be inked, and placed face to face, separated only by narrow paper margins, and then wrapped up without other precaution, they will remain good for a year and a half.

A slight film of oxidisation on the surface of the ink is a merit, not a harm; it is cleaner to work with and gives a blacker print, because the ink clings less tenaciously to the finger, consequently more of it is transferred to the paper.

If a blackened plate becomes dry, and is re-inked without first being cleaned, the new ink will rob the old of some of its oxygen and it will become dry in a day or even less.

Lithography.—Prints may be made on "transfer paper," and thence transferred to stone. It is better not to impress the fingers directly upon the stone, as the print from the stone would be reversed as compared with the original impression, and mistakes are likely to arise in consequence. The print is re-reversed, or put right, by impressing the fingers on transfer paper. It might sometimes be desirable to obtain rapidly a large number of impressions of the finger prints of a suspected person. In this case lithography would be easier, quicker, and cheaper than photography.

Water Colours and Dyes.—The pads most commonly used with office stamps are made of variously prepared gelatine, covered with fine silk to protect the surface, and saturated with an aniline dye. If the surface be touched, the finger is inked, and if the circumstances are all favourable, a good print may be made, but there is much liability to blot. The pad remains ready for use during many days without any attention, fresh ink being added at long intervals. The advantage of a dye over an ordinary water colour is, that it percolates the silk without any of its colour being kept back. A solution of lampblack or Indian ink consists of particles of soot suspended in water; when carefully filtered the black particles will remain behind, and only clear water passes through.

A serviceable pad may be made out of a few thicknesses of cloth or felt with fine silk or cambric stretched over it. The ink should be of a slowly drying sort, made, it might be, of ordinary ink, with the admixture of sugar, honey, glycerine or the like, to bring it to a proper consistence.

Mr. Gilbert Thompson's results by this process have already been mentioned. A similar process was employed for the Bengal finger prints by Sir W.

Herschel, who sent me the following account: "As to the printing of the fingers themselves, no doubt practice makes perfect. But I took no pains with my native officials, some dozen or so of whom learnt to do it quite well enough for all practical purposes from Bengali written instructions, and using nothing but a kind of lampblack ink made by the native orderly for use with the office seal." A batch of these impressions, which he was so good as to send me, are all clear, and in most cases very good indeed. It would be easier to employ this method in a very damp climate than in England, where a very thin layer of lampblack is apt to dry too quickly on the fingers.

Printing as from Engraved Plates.—Professor Ray Lankester kindly sent me his method of taking prints with water colours. "You take a watery brush-full or two of the paint and rub it over the hands, rubbing one hand against the other until they feel sticky. A *thin* paper (tissue is best) placed on an oval cushion the shape of the hand, should be ready, and the hand pressed not too firmly on to it. I enclose a rough sample, done without a cushion. You require a cushion for the hollow of the hand, and the paint must be rubbed by the two hands until they feel sticky, not watery." This is the process of printing from engravings, the ink being removed from the ridges, and lying in the furrows. Blood can be used in the same way.

The following is extracted from an article by Dr. Louis Robinson in the *Nineteenth Century*, May 1892, p. 303:—

"I found that direct prints of the infant's feet on paper would answer much better (than photography). After trying various methods I found that the best results could be got by covering the foot by means of a soft stencil brush with a composition of lampblack, soap, syrup, and blue-black ink; wiping it gently from heel to toe with a smoothly folded silk handkerchief to remove the superfluous pigment, and then applying a moderately flexible paper, supported on a soft pad, direct to the foot."

A curious method with paper and ordinary writing ink, lately contrived by Dr. Forgeot, is analogous to lithography. He has described in one of the many interesting pamphlets published by the "Laboratoire d'Anthropologie Criminelle" of Lyon (*Stenheil*, 2 Rue Casimir-Delavigne, Paris), his new process of rendering visible the previously invisible details of such faint finger prints as thieves may have left on anything they have handled, the object being to show how evidence may sometimes be obtained for their identification. It is well known that pressure of the hand on the polished surface of glass or metal leaves a latent image very difficult to destroy, and which may be rendered visible by suitable applications, but few probably have suspected that this may be the case, to a considerable degree, with ordinary paper. Dr. Forgeot has shown that if a slightly greasy hand, such for example as a hand that has just been passed through the hair, be pressed on clean paper, and if common ink be afterwards brushed lightly over the paper, it will refuse to lie thickly on the greasy parts, and that the result will be a very fair picture of the minute markings on the fingers. He has even used these productions as negatives, and printed good photographs from them. He has also sent me a photographic print made from a piece of glass which had been exposed to the vapour of hydrofluoric acid, after having been touched by a greasy hand. I have made many trials of his method with considerable success. It affords a way of obtaining serviceable impressions in the absence of better means. Dr. Forgeot's pamphlet describes other methods of a generally similar kind, which he has found to be less good than the above.

Smoke Printing.—When other apparatus is not at hand, a method of obtaining very clear impressions is to smoke a plate over a lighted candle, to press the finger on the blackened surface, and then on an adhesive one. The following details must, however, be borne in mind: the plate must not be smoked too much, for the same reason that a slab must not be inked too much; and the adhesive surface must be only slightly damped, not wetted, or the impression will be blurred. A crockery plate is better than glass or metal, as the soot does not adhere to it so tightly, and it is less liable to crack. Professor Bowditch finds mica (which is sold at photographic stores in small sheets) to be the best material. Certainly the smoke comes wholly off the mica on to the parts of the finger that touch it, and a beautiful negative is left behind, which can be utilised in the camera better than glass that has been similarly treated; but it does not serve so well for a plate that is intended to be kept ready for use in a pocket-book; its softness rendering it too liable to be scratched. I prefer to keep a slip of very thin copper sheeting in my pocket-book, with which, and with the gummed back of a postage stamp, or even the gummed fringe to a sheet of stamps, impressions can easily be taken. The thin copper quickly cools, and a wax match supplies enough smoke. The folders spoken of (p.) may be smoked instead of being inked, and are in some cases preferable to carry in the pocket or to send by post, being so easy to smoke afresh. Luggage labels that are thickly gummed at the back furnish a good adhesive surface. The fault of gummed paper lies in the difficulty of damping it without its curling up. The gummed paper sold by stationers is usually thinner than luggage labels, and still more difficult to keep flat. Paste rubbed in a very thin layer over a card makes a surface that holds soot firmly, and one that will not stick to other surfaces if accidentally moistened. Glue, isinglass, size, and mucilage, are all suitable. It was my fortune as a boy to receive rudimentary lessons in drawing from a humble and rather grotesque master. He confided to me the discovery, which he claimed as his own, that pencil drawings could be fixed by licking them; and as I write these words, the image of his broad swab-like tongue performing the operation, and of his proud eyes gleaming over the drawing he was operating on, come vividly to remembrance. This reminiscence led me to try whether licking a piece of paper would give it a sufficiently adhesive surface. It did so. Nay, it led me a step further, for I took two pieces of paper and licked both.

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The dry side of the one was held over the candle as an equivalent to a plate for collecting soot, being saved by the moisture at the back from igniting (it had to be licked two or three times during the process), and the impression was made on the other bit of paper. An ingenious person determined to succeed in obtaining the record of a finger impression, can hardly fail altogether under any ordinary circumstances.

Physiologists who are familiar with the revolving cylinder covered with highly glazed paper, which is smoked, and then used for the purpose of recording the delicate movements of a tracer, will have noticed the beauty of the impression sometimes left by a finger that had accidentally touched it. They are also well versed in the art of varnishing such impressions to preserve them in a durable form.

A cake of blacklead (plumbago), such as is sold for blackening grates, when rubbed on paper leaves a powdery surface that readily blackens the fingers, and shows the ridges distinctly. A small part of the black comes off when the fingers are pressed on sticky paper, but I find it difficult to ensure good prints. The cakes are convenient to carry and cleanly to handle. Whiting, and still more, whiting mixed with size, may be used in the same way, but it gathers in the furrows, not on the ridges.

Casts give undoubtedly the most exact representation of the ridges, but they are difficult and unsatisfactory to examine, puzzling the eye by showing too conspicuously the variation of their heights, whereas we only want to know their courses. Again, as casts must be of a uniform colour, the finer lines are indistinctly seen except in a particular light. Lastly, they are both cumbrous to preserve and easily broken.

A sealing-wax impression is the simplest and best kind of cast, and the finger need not be burnt in making it. The plan is to make a considerable pool of flaming sealing-wax, stirring it well with the still unmelted piece of the stick, while it is burning. Then blow out the flame and wait a little, until the upper layer has cooled. Sealing-wax that has been well aflame, takes a long time to harden thoroughly after it has parted with nearly all its heat. By selecting the proper moment after blowing out the flame, the wax will be cool enough for the finger to press it without discomfort, and it will still be sufficiently soft to take a sharp impression. Dentist's wax, which is far less brittle, is easily worked, and takes impressions that are nearly as sharp as those of sealing-wax; it has to be well heated and kneaded, then plunged for a moment in cold water to chill the surface, and immediately impressed. Gutta-percha can also be used. The most delicate of all impressions is that left upon a thick clot of varnish, which has been exposed to the air long enough for a thin film to have formed over it. The impression is transient, but lingers sufficiently to be easily photographed. It happened, oddly enough, that a few days after I had noticed this effect, and had been experimenting upon it, I heard an interesting memoir "On the Minute Structure of Striped Muscle, with special allusion to a new method of investigation by means of 'Impressions' stamped in Collodion," submitted to the Royal Society by Dr. John Berry Haycraft, in which an analogous method was used to obtain impressions of delicate microscopic structures.

Enlargements for the purposes of special study are best made by photography. These are unquestionably accurate, and the labour being mechanical may be delegated. If the print be one of printer's ink, on white paper, the process is straightforward, first of obtaining a negative and afterwards photo-prints from it. The importance of the paper or card used to receive the finger print, being quite white, has already been pointed out. An imprint on white crockery-ware is beautifully clear. Some of the positive prints may be advantageously printed by the ferro-prussiate process. The paper used for it does not curl when dry, its texture is good for writing on, and the blue colour of the print makes handwriting clearly legible, whether it be in ink or in pencil.

Prints on glass have great merits for use as lanthorn slides, but it must be recollected that they may take some days to dry, and that when dry the ink can be only too easily detached from them by water, which insinuates itself between the dry ink and the glass. Of course they could be varnished, if the trouble and cost were no objection, and so preserved. The negative print left on an inked slab, after the finger has touched it, is sometimes very clear; that on smoked glass better, and on smoked mica the clearest of all.

Photographic copies are valuable in themselves, and most useful for subsequent enlargement. They (see also p. 90) These have merely to be placed in the enlarging camera, where the negative image thrown on argento-bromide paper will yield a positive print.

I have made, by hand, many enlargements with a prism (camera lucida), but it is difficult to enlarge more than five times by means of it. So much shade is cast by the head that the prism can hardly be used at a less distance than 3 inches from the print, or one quarter the distance (12 inches) at which a book is usually read, while the paper on which the drawing is made cannot well be more than 15 inches below the prism; so it makes an enlargement of $\frac{4 \times 15}{12}$, or 5-fold. This is a very convenient method of analysing a pattern, since the lines follow only the axes of the ridges, as in Plate 3, Fig. 5. The prism and attached apparatus may be kept permanently mounted, ready for use at any time, without the trouble of any adjustment.

I have also made frequent use of an enlarging pantagraph, in which the cross-wires of a low-power microscope took the place of the pointer. It has many merits, but its action is not equally free in all directions; the enlarged traces are consequently jagged, and require subsequent smoothing.

All hand-made enlargements are tedious to produce, as the total length of lineations to be followed is considerable. In a single finger print made by dabbing down the finger, their actual length amounts to about 18 inches; therefore in a five-fold enlargement of the entire print the pencil has to be carefully directed over five times that distance, or more than 6 feet.

Large copies of tracings made on transparent paper, either by the Camera Lucida or by the Pantagraph, are easily printed by the ferro-prussiate photographic process mentioned above, just as plans are copied by engineers.

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CHAPTER IV

THE RIDGES AND THEIR USES

THE palmar surface of the hands and the soles of the feet, both in men and monkeys, are covered with minute ridges that bear a superficial resemblance to those made on sand by wind or flowing water. They form systems which run in bold sweeps, though the courses of the individual ridges are less regular. Each ridge (Plate 3, Fig. 5) is characterised by numerous minute peculiarities, called *minutiæ* in this book, here dividing into two, and there uniting with another (*a*, *b*), or it may divide and almost immediately reunite, enclosing a small circular or elliptical space (*c*); at other times its beginning or end is markedly independent (*d*, *e*); lastly, the ridge may be so short as to form a small island (*f*).

Whenever an interspace is left between the boundaries of different systems of ridges, it is filled by a small system of its own. This will have some characteristic shape, called a "pattern" in this book.

There are three particularly well-marked systems of ridges in the palm of the hand marked in Plate 3, Fig 6, I, as Th, AB, and BC. The system Th is that which runs over the ball of the thumb and adjacent parts of the palm. It is bounded by the line *a* which starts from the middle of the palm close to the wrist, sweeping thence round the ball of the thumb to the thumb-side of the palm, which it reaches about half an inch, sometimes more sometimes less, below the base of the fore-finger. The system AB is bounded towards the thumb by the above line *a*, and towards the little finger by the line *b*; the latter starts from about the middle of the outer or little finger-side of the palm, and emerges on the opposite side just below the fore-finger. Consequently, every ridge that wholly crosses the palm is found in AB. The system BC is bounded thumbwards by the line *b*, until that line arrives at a point immediately below the axis of the fore-finger; there the boundary of BC leaves the line *b*, and skirts the base of the fore-finger until it reaches the interval which separates the fore and middle fingers. The upper boundary of BC is the line *c*, which leaves the little finger-side of the palm at a small distance below the base of the little finger, and terminates between the fore and middle fingers. Other systems ~~that lie~~ between *c* and the middle, ring and little fingers; they are somewhat more variable than those just described, as will be seen by comparing the five different palms shown in Fig. 6.

An interesting example of the interpolation of a small and independent system occurs frequently in the middle of one or other of the systems AB or BC, at the place where the space covered by the systems of ridges begins to broaden out very rapidly. There are two ways in which the necessary supply of ridges makes its appearance, the one is by a series of successive embranchments (Fig. 6, 1), the other is by the insertion of an independent system, as shown in 4, 5. Another example of an interpolated system, but of rarer occurrence, is found in the system Th, on the ball of the thumb, as seen in 2.

Far more definite in position, and complex in lineation, are the small independent systems which appear on the bulbs of the thumb and fingers. They are more instructive to study, more easy to classify, and will alone be discussed in this book.

In the diagram of the hand, Fig. 6, 1, the three chief cheiromantic creases are indicated by dots, but are not numbered. They are made (1) by the flexure of the thumb, (2) of the four fingers simultaneously, and (3) of the middle, ring, and little fingers simultaneously, while the fore-finger remains extended. There is no exact accordance between the courses of the creases and those of the adjacent ridges, less still do the former agree with the boundaries of the systems. The accordance is closest between the crease (1) and the ridges in Th, but for all that the crease does not agree with the line *a*; it usually lies considerably within it. The crease (2) cuts the ridges on either side, at an angle of about 30 degrees. The crease (3) is usually parallel to the ridges between which it runs, but is often far from accordant with the line *c*. The creases at the various joints of the

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thumb and fingers cut the ridges at small angles, say, very roughly, of 15 degrees.

The supposition is therefore untenable that the courses of the ridges are wholly determined by the flexures. It appears, on the other hand, that the courses of the ridges and the lines of flexure must be in part, but in part only, due to the action of the same causes.

The fact of the creases of the hand being strongly marked in the newly-born child, has been considered by some to testify to the archaic and therefore important character of their origin. For my part, I should have thought that the crumpled condition of the hand of the infant, during some months before its birth, was quite enough to account for it.

I possess a few specimens of hand prints of persons taken when children, and again, after an interval of several years: they show a general accordance in respect to the creases, but not sufficiently close for identification.

The ridges on the feet and toes are less complex than those on the hands and digits, and are less serviceable for present purposes, though equally interesting to physiologists. Having given but little attention to them myself, they will not be again referred to.

The ridges are studded with minute pores which are the open mouths of the ducts of the somewhat deeply-seated glands, whose office is to secrete perspiration; see Plate 10, *n*, for a good example of them. The distance between adjacent pores on the same ridge is, roughly speaking, about half that which separates the ridges. The lines of a pattern are such as an artist would draw, if dots had been made on a sheet of paper in positions corresponding to the several pores, and he endeavoured to connect them by evenly flowing curves; it would be difficult to draw a pattern under these conditions, within definite boundaries, that cannot be matched in a living hand.

The embryological development of the ridges has been studied by many, but more especially by Dr. A. Kollmann,[1] whose careful investigations and bibliography should be consulted by physiologists interested in the subject. He conceives the ridges to be formed through lateral pressures between nascent structures.

The ridges are said to be first discernible in the fourth month of fœtal life, and fully formed by the sixth. In babies and children the delicacy of the ridges is proportionate to the smallness of their stature. They grow simultaneously with the general growth of the body, and continue to be sharply defined until old age has set in, when an incipient disintegration of the texture of the skin spoils, and may largely obliterate them (see the finger prints on the title-page). They develop most in hands that do a moderate amount of work, and they are strongly developed in the foot, which has the hard work of

[1] *Der Tastapparat der Hand der menschlichen Rassen und der Affen.* Dr .Arthur Kollmann. Leopold Voss, Leipzig, 1883. He has also published a more recent memoir.

supporting the weight of the body. They are, as already mentioned, but faintly developed in the hands of ladies, rendered delicate by the continual use of gloves and lack of manual labour, and in idiots of the lowest type who are incapable of labouring at all. When the skin becomes thin, the ridges simultaneously subside in height. They are obliterated by the callosities formed on the hands of labourers and artisans in many trades, by the constant pressure of their peculiar tools. The ridges on the side of the left fore-finger of tailors and seamstresses are often temporarily destroyed by the needle; an instance of this is given in Plate 4, Fig. 7, *b*. Injuries, when they are sufficiently severe to leave permanent scars, destroy the ridges to that extent. If a piece of flesh is sliced off, or if an ulcer has eaten so deeply as to obliterate the perspiratory glands, a white cicatrix, without pores or ridges, is the result (Fig. 7, *a*). Lesser injuries are not permanent. My assistant happened to burn his finger rather sharply; the daily prints he took of it, illustrated the progress of healing in an interesting manner; finally the ridges were wholly restored. A deep clean cut leaves a permanent thin white line across the ridges (Fig. 7, *c*), sometimes without any accompanying puckering; but there is often a displacement of the ridges on both sides of it, exactly like a "fault" in stratified rocks. A cut or other injury that is not a clean incision leaves a scar with puckerings on all sides, as in Fig. 7, *a*, making the ridges at that part undecipherable, even if it does not wholly obliterate them.

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The latest and best investigations on the evolution of the ridges has been made by Dr. H. Klaatsch.[1] He shows that the earliest appearance in the Mammalia of structures analogous to ridges is one in which small eminences occur on the ball of the foot, through which the sweat glands issue in no particular order. The arrangement of the papillæ into rows, and the accompanying orderly arrangement of the sweat glands, is a subsequent stage in evolution. The prehensile tail of the Howling Monkey serves as a fifth hand, and the naked concave part of the tail, with which it grasps and holds on to boughs, is furnished with ridges arranged transversely in beautiful order. The numerous drawings of the hands of monkeys by Allix[2] may be referred to with advantage.

The uses of the ridges are primarily, as I suppose, to raise the mouths of the ducts, so that the excretions which they pour out may the more easily be got rid of; and secondarily, in some obscure way, to assist the sense of touch. They are said to be moulded upon the subcutaneous papillæ in such a manner that the ultimate organs of touch, namely, the Pacinian bodies, etc.—into the variety of which it is unnecessary here to enter—are more closely congregated under the bases of the ridges than under the furrows, and it is easy, on those grounds, to make reasonable guesses how the ridges may assist the sense of touch. They must concentrate pres-

[1] "Morphologie der Tastballen der Saugethiere," *Jahrbuch* xiv. p. 407. Leipzig, 1888.

[2] *Ann. Sc. Nat.*, 5th serie, vol. ix. 1868.

sures, that would otherwise be spread over the surface generally, upon the parts which are most richly supplied with the terminations of nerves. By their means it would become possible to neutralise the otherwise dulling effect of a thick protective epidermis. Their existence in transverse ridges on the inner surface of the prehensile tails of monkeys admits of easy justification from this point of view. The ridges so disposed cannot prevent the tail from curling, and they must add materially to its sensitiveness. They seem to produce the latter effect on the hands of man, for as the epidermis thickens under use within moderate limits, so the prominence of the ridges increases.

Supposing the ultimate organs of the sense of touch to be really congregated more thickly under the ridges than under the furrows—on which there has been some question—the power of tactile discrimination would depend very much on the closeness of the ridges. The well-known experiment with the two points of a pair of compasses, is exactly suited to test the truth of this. It consists in determining the smallest distance apart, of the two points, at which their simultaneous pressure conveys the sensation of a double prick. Those persons in whom the ridge-interval was short might be expected to perceive the double sensation, while others whose ridge-interval was wide would only perceive a single one, the distance apart of the compass points, and the parts touched by them, being the same in both cases. I was very glad to avail myself of the kind offer of Mr.

E. B. Titchener to make an adequate course of experiments at Professor Wundt's psycho-physical laboratory at Leipzig, to decide this question. He had the advantage there of being able to operate on fellow-students who were themselves skilled in such lines of investigation, so while his own experience was a considerable safeguard against errors of method, that safety was reinforced by the fact that his experiments were conducted under the watchful eyes of competent and critical friends. The result of the enquiry was decisive. It was proved to demonstration that the fineness or coarseness of the ridges in different persons had no effect whatever on the delicacy of their tactile discrimination. Moreover, it made no difference in the results, whether one or both points of the compasses rested on the ridges or in the furrows.

The width of the ridge-interval is certainly no test of the relative power of discrimination of the different parts of the same hand, because while the ridge-interval is nearly uniform over the whole of the palmar surface, the least distance between the compass points that gives the sensation of doubleness, is more than four times as great when they are applied to some parts of the palm as when they are applied to the bulbs of the fingers.

The ridges may subserve another purpose in the act of touch, namely, that of enabling the character of surfaces to be perceived by the act of rubbing them with the fingers. We all of us perform this, as it were, intuitively. It is interesting to ask a person who is ignorant of the real intention, to shut his eyes and to ascertain as well as he can by the sense of touch alone, the material of which any object is made that is afterwards put into his hands. He will be observed to explore it very carefully by rubbing its surface in many directions, and with many degrees of pressure. The ridges engage themselves with the roughness of the surface, and greatly help in calling forth the required sensation, which is that of a thrill; usually faint, but always to be perceived when the sensation is analysed, and which becomes very distinct when the indentations are at equal distances apart, as in a file or in velvet. A thrill is analogous to a musical note, and the characteristics to the sense of touch, of different surfaces when they are rubbed by the fingers, may be compared to different qualities of noise. There are, however, no pure overtones in the case of touch, as there are in nearly all sounds.

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CHAPTER V

PATTERNS: THEIR OUTLINES AND CORES

THE patterns on the thumb and fingers were first discussed at length by Purkenje in 1822, in a University Thesis or *Commentatio*, of which I have translated the part that chiefly concerns us, and appended it to this chapter together with his corresponding illustrations. Subsequent writers have adopted his standard types, diminishing or adding to their number as the case may be, and guided as he had been, by the superficial appearance of the lineations.

In my earlier trials some three years ago, an attempt at classification was made upon that same principle, when the experience gained was instructive. It had seemed best to limit them to the prints of a single digit, and the thumb was selected. I collected enough specimens to fill fourteen sheets, containing in the aggregate 504 prints of right thumbs, arranged in six lines and six columns (6 × 6 × 14 = 504), and another set of fourteen sheets containing the corresponding left thumbs. Then for the greater convenience of study these sheets were photographed, and enlargements upon paper to about two and a half times the natural size were made from the negatives. The enlargements of the right thumb prints were reversed, in order to make them comparable on equal terms with those of the left. The sheets were then cut up into rectangles about the size of small playing cards, each of which contained a single print, and the register number in my catalogue was entered on its back, together with the letters L. for left, or R.R. for reversed right, as the case might be.

On trying to sort them according to Purkenje's standards, I failed completely, and many analogous plans were attempted without success. Next I endeavoured to sort the patterns into groups so that the central pattern of each group should differ by a unit of "equally discernible difference" from the central patterns of the adjacent groups, proposing to adopt those central patterns as standards of reference. After tedious re-sortings, some sixty standards were provisionally selected, and the whole laid by for a few days. On returning to the work with a fresh mind, it was painful to find how greatly my judgment had changed in the interim, and how faulty a classification that seemed tolerably good a week before, looked then. Moreover, I suffered the shame and humiliation of discovering that I had failed to observe the identity of certain duplicates, and that one print had been mistaken for another. Repeated trials of the same kind made it certain that finality would never be reached by the path hitherto pursued.

On considering the causes of these doubts and blunders, different influences were found to produce them, any one of which was sufficient by itself to give rise to serious uncertainty. A complex pattern is capable of suggesting various readings, as the figuring on a wall paper may suggest a variety of forms and faces to those who have such fancies. The number of illusive renderings of prints taken from the same finger, is greatly increased by such trifles as the relative breadths of their respective lineations and the differences in their depths of tint. The ridges themselves are soft in substance, and of various heights, so that a small difference in the pressure applied, or in the quantity of ink used, may considerably affect the width of the lines and the darkness of portions of the print. Certain ridges may thereby catch the attention at one time, though not at others, and give a bias to some false conception of the pattern. Again, it seldom happens that different impressions of the same digit are printed from exactly the same part of it, consequently the portion of the pattern that supplies the dominant character will be often quite different in the two prints. Hence the eye is apt to be deceived when it is guided merely by the general appearance. A third cause of error is still more serious; it is that patterns, especially those of a spiral form, may be apparently similar, yet fundamentally unlike, the unaided eye being frequently unable to analyse them and to discern real differences. Besides all this, the judgment is distracted by the mere size of the pattern, which catches the attention at once, and by other secondary matters such as the number of turns in the whorled patterns, and the relative dimensions of their different parts. The first need to be satisfied, before it could become possible to base the classification upon a more sure foundation than that of general appearance, was to establish a well-defined point or points of reference in the patterns. This was done by utilising the centres of the one or two triangular plots (see Plate 4, Fig 8, 2, 3, 4), which are found in the great majority of patterns, and whose existence was pointed out by Purkenje, but not their more remote cause, which is as follows:

The ridges, as was shown in the diagram (Plate 3) of the palm of the hand, run athwart the fingers in rudely parallel lines up to the last joint, and if it were not for the finger nail, would apparently continue parallel up to the extreme finger tip. But the presence of the nail disturbs their parallelism and squeezes them downwards on both sides of the finger. (See Fig. 8, 2.) Consequently the ridges that run close to the tip are greatly arched, those that successively follow are gradually less arched until, in some cases, all signs of the arch disappear at about the level of the first joint (Fig. 8, 1). Usually, however, this gradual transition from an arch to a straight line, fails to be carried out, so there is a break in the orderly sequence, and a consequent interspace (Fig. 8, 2). The topmost boundary of the interspace is formed by the lowermost arch, and its lowermost boundary by the topmost straight ridge. But an equally large number of ducts exist within the interspace, as are to be found in adjacent areas of equal size, whose mouths require to be supported and connected. This is effected by the interpolation of an independent system of ridges arranged in loops (Fig. 8, 3; also Fig. 9, *a*, *f*), or in scrolls (Fig. 8, 4; also Plate 5, Fig. 9, *g*, *h*), and this interpolated system forms the "pattern." Now the existence of an interspace implies the divergence of two previously adjacent ridges (Fig. 8, 2), in order to embrace it. Just in front of the place where the divergence begins, and before the sweep of the pattern is reached, there are usually one or more very short cross-ridges. Their effect is to complete the enclosure of the minute triangular plot in question. Where there is a plot on both sides of the finger, the line that connects them (Fig. 8, 4) serves as a base line whereby the pattern may be oriented, and the position of any point roughly charted. Where there is a plot on only one side of the finger (Fig. 8, 3), the pattern has almost necessarily an axis, which serves for orientation, and the pattern can still be charted, though on a different principle, by dropping a perpendicular from the plot on to the axis, in the way there shown.

These plots form corner stones to my system of outlining and subsequent classification; it is therefore extremely important that a sufficient area of the finger should be printed to include them. This can always be done by slightly *rolling* the finger (p.), the result being, in the language of map-makers, a cylindrical projection of the finger (see Plate 5, Fig.

9, *a-h*). Large as these impressions look, they are of the natural size, taken from ordinary thumbs.

The outlines.—The next step is to give a clear and definite shape to the pattern by drawing its outline (Fig. 9). Take a fine pen, pencil, or paint brush, and follow in succession each of the two diverging ridges that bound either plot. The course of the ridge must be followed with scrupulous conscientiousness, marking it with a clean line as far as it can be traced. If the ridge bifurcates, always follow the branch that trends towards the middle of the pattern. If it stops short, let the outline stop short also, and recommence on a fresh ridge, choosing that which to the best of the judgment, prolongs the course of the one that stopped. These outlines have an extraordinary effect in making finger markings intelligible to an untrained eye. What seemed before to be a vague and bewildering maze of lineations over which the glance wandered distractedly, seeking in vain for a point on which to fix itself, now suddenly assumes the shape of a sharply-defined figure. Whatever difficulties may arise in classifying these figures, they are as nothing compared to those experienced in attempts to classify unoutlined patterns, the outlines giving a precision to their general features which was wanting before.

After a pattern has been treated in this way, there is no further occasion to pore minutely into the finger print, in order to classify it correctly, for the bold firm curves of the outline, are even more distinct than the largest capital letters in the title-page of a book.

A fair idea of the way in which the patterns are distributed, is given by Plate 6, Fig. 10. Eight persons were taken in the order in which they happened to present themselves, and Fig. 10 shows the result. For greater clearness, colour has been employed to distinguish between the ridges that are supplied from the inner and outer sides of the hand respectively. The words right and left *must be avoided* in speaking of patterns, for the two hands are symmetrically disposed, only in a reversed sense. The right hand does not look like a left hand, but like the reflection of a left hand in a looking-glass, and *vice versa.* The phrases we shall employ will be the *Inner* and the *Outer*; or thumb-side and little-finger-side. (Terms which were unfortunately misplaced in my memoir in the *Phil. Trans.*)

There need be no difficulty in remembering the meaning of these terms, if we bear in mind that the great toes are undoubtedly innermost; that if we walked on all fours as children do, and as our remote ancestors probably did, the thumbs also would be innermost, as is the case when the two hands are impressed side by side on paper. Inner and outer are better than thumb-side and little-finger-side, because the latter cannot be applied to the thumbs and little fingers themselves. The anatomical words radial and ulnar referring to the two bones of the fore-arm, are not in popular use, and they may be similarly inappropriate, for it would sound oddly to speak of the radial side of the radius.

The two plots just described will therefore be henceforth designated as the Inner and the Outer plots respectively, and symbolised by the letters I and O.

The system of ridges in Fig. 10 that comes from the inner side "I," are coloured blue; those from the outer "O," are coloured red. The employment of colour instead of variously stippled surfaces, is of conspicuous advantage to the great majority of persons, though unhappily nearly useless to about one man in every twenty-five, who is constitutionally colour blind.

It may be convenient when marking finger prints with letters for reference, to use those that look alike, both in a direct and in a reversed aspect, as they may require to be read either way. The print is a reversed picture of the pattern upon the digit that made it. The pattern on one hand is, as already said, a reversed picture of a similar pattern as it shows on the other. In the various processes by which prints are multiplied, the patterns may be reversed and re-reversed. Thus, if a finger is impressed on a lithographic stone, the impressions from that stone are reversals of the impression made by the same finger upon paper. If made on transfer paper and thence transferred to stone, there is a re-reversal. There are even more varied possibilities when photography is employed. It is worth recollecting that there are eleven capital letters in the English alphabet, which, if printed in block type, are unaffected by being reversed. They are **A.H.I.M.O.T.U.V.W.X.Y.Z.** Some symbols do the same,

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such as, * + — = : . These and the letters **H.O.I.X.** have the further peculiarity of appearing unaltered when upside down.

Lenses.—As a rule, only a small magnifying power is needed for drawing outlines, sufficient to allow the eye to be brought within six inches of the paper, for it is only at that short distance that the *minutiæ* of a full-sized finger print begin to be clearly discerned. Persons with normal sight, and during their childhood and early boyhood, are able to read as closely as this without using a lens, the range in adjustment of the focus of the eye being then large. But as age advances the range contracts, and an elderly person with otherwise normal eyesight, requires glasses to read a book even at twelve inches from his eye. I now require much optical aid; when reading a book, I have to use spectacles of 12-inch focus; when studying a finger print I put on my 12-inch eye-glasses as well, the double power enabling me to see clearly at a distance of only six inches. Perhaps the most convenient focus for a lens in ordinary use is 3 inches. It should be mounted at the end of a long arm that can easily be pushed in any direction, sideways, backwards, forwards, and up or down. It is undesirable to use a higher power than this unless it is necessary, because the field of view becomes narrowed to an inconvenient degree, and the nearer the head is to the paper, the darker is the shadow that it casts; also there is insufficient room for the use of a pencil.

Every now and then a closer inspection is wanted;

for which purpose a doublet of ½-inch focus, standing on three slim legs answers well.

For studying the markings on the fingers themselves, a small folding lens, sold at opticians' shops under the name of a "linen tester," is very convenient. It is so called because it was originally constructed for the purpose of counting the number of threads in a given space, in a sample of linen. It is equally well adapted for counting the number of ridges in a given space.

Whoever desires to occupy himself with finger prints, ought to give much time and practice to drawing outlines of different impressions of the same digits. His own ten fingers, and those of a few friends, will furnish the necessary variety of material on which to work. He should not rest satisfied until he has gained an assurance that all patterns possess definite figures, which may be latent but are potentially present, and that the ridges form something more than a nondescript congeries of ramifications and twists. He should continue to practise until he finds that the same ridges have been so nearly followed in duplicate impressions, that even in difficult cases his work will rarely vary more than a single ridge-interval.

When the triangular plot happens not to be visible, owing to the print failing to include it, as is often the case when the finger is not rolled (see instances of this, in the prints of my own ten digits on the title-page), the trend of the ridges so far as

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they are seen, usually enables a practised eye to roughly estimate its true position. By means of this guidance an approximate, but fairly correct, outline can be drawn. When the habit of judging patterns by their outlines has become familiar, the eye will trace them for itself without caring to draw them, and will prefer an unoutlined pattern to work upon, but even then it is essential now and then to follow the outline with a fine point, say that of a penknife or a dry pen.

In selecting standard forms of patterns for the convenience of description, we must be content to disregard a great many of the more obvious characteristics. For instance, the size of generally similar patterns in Fig. 10 will be found to vary greatly, but the words, large, medium, or small may be applied to any pattern, so there is no necessity to draw a standard outline for each size. Similarly as regards the inwards or outwards slope of patterns, it is needless to print here a separate standard outline for either slope, and equally unnecessary to print outlines in duplicate, with reversed titles, for the right and left hands respectively. The phrase "a simple spiral" conveys a well-defined general idea, but there are four concrete forms of it (see bottom row of Plate 11, Fig. 17, *oj*, *jo*, *ij*, *ji*), which admit of being verbally distinguished. Again the internal proportions of any pattern, say those of simple spirals, may vary greatly without affecting the fact of their being simple spirals. They may be wide or narrow at their mouths, they may be twisted up into a point

(Plate 8, Fig. 14, 52), or they may run in broad curls of uniform width (Fig. 14, 51, 54). Perhaps the best general rule in selecting standard outlines, is to limit them to such as cannot be turned into any other by viewing them in an altered aspect, as upside down or from the back, or by magnifying or deforming them, whether it be through stretching, shrinking, or puckering any part of them. Subject to this general rule and to further and more particular descriptions, the sets (Plates 7 and 8, Figs. 11, 12, 13) will be found to give considerable help to the nomenclature of the usual patterns.

It will be observed that they are grouped under the three principal heads of Arches, Loops, and Whorls, and that under each of these heads, some analogous patterns as 4, 5, 7, 8, etc., are introduced and underlined with the word "see" so and so, and thus noted as really belonging to one of the other heads. This is done to indicate the character of the transitional cases that unite respectively the Arches with the Loops, the Arches with the Whorls, and the Loops with the Whorls. More will follow in respect to these. The "tented arch" (3) is extremely rare on the thumb; I doubt if I have ever seen it there, consequently it did not appear in the plate of patterns in the *Phil. Trans.*, which referred to thumbs. On the other hand the "banded duplex spiral" (30) is common in the thumb, but rare elsewhere. There are some compound patterns, especially the "spiral in loop" (21) and the "circlet in loop" (22), which are as much loops as whorls; but are reckoned as whorls.

The "twinned loop" (16) is of more frequent occurrence than would be supposed from the examination of dabbed impressions, as the only part of the outer loop then in view, resembles outside arches; it is due to a double separation of the ridges (Plate 4, Fig. 8), and a consequent double interspace. The "crested loop" (13) may sometimes be regarded as an incipient form of a "duplex spiral" (29).

The reader may also refer to Plate 16, which contains what is there called the C set of standard patterns. They were arranged and used for a special purpose, as described in Chapter XI. They refer to impressions of the right hand.

As a variety of Cores, differing in shape and size may be found within each of the outlines, it is advisable to describe them separately. Plate 8, Fig. 14 shows a series of the cores of loops, in which the innermost lineations may be either straight, or curved back; in the one case they are here called rods (31 to 35); in the other (36 to 42), staples. The first of the lineations that envelopes the core, whether it be a rod, many rods, or a staple, is also shown and named (43 to 48). None of the descriptions are intended to apply to more than the very end of the core, say, from the tip downwards to a distance equal to two average ridge-intervals in length. If more of the core be taken into account, the many varieties in their lower parts begin to make description confusing. In respect to the "parted" staples and envelopes, and those that are single-eyed, the description may further mention the side on which the parting or the eye occurs, whether it be the Inner or the Outer.

At the bottom of Fig. 14, 49-54, is given a series of rings, spirals, and plaits, in which nearly all the clearly distinguishable varieties are included, no regard being paid to the direction of the twist or to the number of turns. 49 is a set of concentric circles, 50 of ellipses: they are rarely so in a strict sense throughout the pattern, usually breaking away into a more or less spiriform arrangement as in 51. A curious optical effect is connected with the circular forms, which becomes almost annoying when many specimens are examined in succession. They seem to be cones standing bodily out from the paper. This singular appearance becomes still more marked when they are viewed with only one eye; no sterescopic guidance then correcting the illusion of their being contour lines.

Another curious effect is seen in 53, which has the appearance of a plait or overlap; two systems of ridges that roll together, end bluntly, the end of the one system running right into a hollow curve of the other, and there stopping short; it seems, at the first glance, to run beneath it, as if it were a plait. This mode of ending forms a singular contrast to that shown in 51 and 52, where the ridges twist themselves into a point. 54 is a deep spiral, sometimes having a large core filled with upright and nearly parallel lines; occasionally they are bulbous, and resemble the commoner "monkey" type, see 35.

When the direction of twist is described, the language must be unambiguous: the following are the rules I adopt. The course of the ridge is always followed *towards* the *centre* of the pattern, and not away from it. Again, the direction of its course when so followed is specified at the place where it attains its *highest* point, or that nearest to the finger tip; its course at that point must needs be horizontal, and therefore directed either towards the inner or the outer side.

The amount of twist has a strong tendency to coincide with either one, two, three, four, or more half-turns, and not to stop short in intermediate positions. Here are indications of some unknown fundamental law, analogous apparently to that which causes Loops to be by far the commonest pattern.

The classification into Arches, Loops, and Whorls is based on the degree of curvature of the ridges, and enables us to sort almost any pattern under one or other of those three heads. There are a few ambiguous patterns, and others which are nondescript, but the former are uncommon and the latter rare; these exceptions giving little real inconvenience, the classification works easily and well.

Arches are formed when the ridges run from one side to the other of the bulb of the digit, without making any backward turn or twist. Loops, when there is a single backward turn, but no twist. Whorls, when there is a turn through at least one complete circle; they are also considered to include all duplex spirals.

The chief theoretical objection to this threefold system of classification lies in the existence of certain compound patterns, by far the most common of which are Whorls enclosed within Loops (Plates 7, 8, Fig. 12, 1, 15, 18, 19, and Fig. 13, 20-23). They are as much Loops as Whorls, and properly ought to be relegated to a fourth class. I have not done so, but called them Whorls, for a practical reason which is cogent. In an imperfect impression, such as is made by merely dabbing the inked finger upon paper, the enveloping loop is often too incompletely printed to enable its existence to be surely ascertained, especially when the enclosed whorl is so large (Fig. 13, 23) that there are only one or two enveloping ridges to represent the loop. On the other hand, the whorled character of the core can hardly fail to be recognised. The practical difficulties lie almost wholly in rightly classifying a few transitional forms, diagrammatically and roughly expressed in Fig. 11, 4, 5, and Fig. 12, 8, 18, 19, with the words "see" so and so, written below, and of which actual examples are given on an enlarged scale in Plates 9 and 10, Figs. 15 and 16. Here Fig. 15, *a* is an undoubted arch, and *c* an undoubted nascent loop; but *b* is transitional between them, though nearer to a loop than an arch. *d* may be thought transitional in the same way, but it has an incipient curl which becomes marked in *e*, while it has grown into a decided whorl in *f*; *d* should also be compared with *j*, which is in some sense a stage towards *k*. *g* is a nascent tented-arch, fully developed in *i*, where the pattern as a whole has a f 48r

slight slope, but is otherwise fairly symmetrical. In *h* there is incipient dis-symmetry, and a tendency to the formation of a loop on the right side (refer back to Plate 7, Fig. 11, 4, and Fig. 12, 12); it is a transitional case between a tented arch and a loop, with most resemblance to the latter. Plate 10, Fig. 16 illustrates eyed patterns; here *l* and *m* are parts of decided loops; *p*, *q*, and *r* are decided whorls, but *n* is transitional, inclining towards a loop, and *o* is transitional, inclining towards a whorl. *s* is a nascent form of an invaded loop, and is nearly related to *l*; *t* and *u* are decidedly invaded loops.

The Arch-Loop-Whorl, or, more briefly, the A.L.W. system of classification, while in some degree artificial, is very serviceable for preliminary statistics, such as are needed to obtain a broad view of the distribution of the various patterns. A minute subdivision under numerous heads would necessitate a proportional and somewhat overwhelming amount of statistical labour. Fifty-four different standard varieties are by no means an extravagant number, but to treat fifty-four as thoroughly as three would require eighteen times as much material and labour. Effort is economised by obtaining broad results from a discussion of the A.L.W. classes, afterwards verifying or extending them by special inquiries into a few of the further subdivisions.

The divergent ridges that bound any simple pattern admit of nine, and only nine, distinct variations in the first part of their course. The bounding ridge that has attained the summit of any such pattern must have arrived either from the Inner plot (I), the Outer plot (O), or from both. Similarly as regards the bounding ridge that lies at the lowest point of the pattern. Any one of the three former events may occur in connection with any of the three latter events, so they afford in all 3 × 3, or nine possible combinations. It is convenient to distinguish them by easily intelligible symbols. Thus, let *i* signify a bounding line which starts from the point I, whether it proceeds to the summit or to the base of the pattern; let *o* be a line that similarly proceeds from O, and let *u* be a line that unites either by summit or by base the two plots I and O. Again, let two symbols be used, of which the first shall always refer to the summit, and the second to the base of the pattern. Then the nine possible cases are—*uu*, *ui*, *uo*; *iu*, *ii*, *io*; *ou*, *oi*, *oo*. The case of the arches are peculiar, but they may be fairly classed under the symbol *uu*.

This easy method of classification has much power. For example, the four possible kinds of simple spirals (see the 1st, 2nd, and the 5th and 6th diagrams in the lowest row of Plate 11, Fig. 17) are wholly determined by the letters *oj*, *jo*, *ij*, *ji* respectively. The two forms of duplex spirals are similarly determined by *oi* and *io* (see 4th and 5th diagrams in the upper row of Fig. 17). The two slopes of loops by *oo* and *ii* (3rd and 4th in the lower row). It also shows very distinctly the sources whence the streams of ridges proceed that feed the pattern, which itself affords another basis for classification. The resource against uncertainty in respect to ambiguous or difficult patterns is to compile a dictionary of them, with the heads under which it is advisable that they should severally be classed. It would load these pages too heavily to give such a dictionary here. Moreover, it ought to be revised by many experienced eyes, and the time is hardly ripe for this; when it is, it would be no difficult task, out of the large number of prints of separate fingers which for instance I possess (some 15,000), to make an adequate selection, to enlarge them photographically, and finally to print the results in pairs, the one untouched, the other outlined and classified.

It may be asked why ridges are followed and not furrows, the furrow being the real boundary between two systems? The reply is, that the ridges are the easiest to trace; and, as the error through following the ridges cannot exceed one-half of a ridge-interval, I have been content to disregard it. I began by tracing furrows, but preferred the ridges after trial.

Measurements.—It has been already shown that when both plots are present (Plate 4, Fig. 8, 4), they form the termini of a base line, from which any part of the pattern may be triangulated, as surveyors would say. Also, that when only one plot exists (3), and the pattern has an axis (which it necessarily has in all ordinary *ii* and *oo* cases), a perpendicular can be let fall upon that axis, whose intersection with it will serve as a second point of reference. But our methods must not be too refined. The centres of the plots are not determinable with real exactness, and repeated prints from so soft a substance as flesh are often somewhat dissimilar, the one being more or less broadened out than the other, owing to unequal pressure. It is therefore well to use such other more convenient points of reference as the particular pattern may present. In loops the intersection of the axis with the summit of the innermost bend, whether it be a staple or the envelope to a rod (Fig. 14, second and third rows of diagrams), is a well-defined position. In spirals, the centre of the pattern is fairly well-defined; also a perpendicular erected from the middle of the base to the outline above and below (Fig. 8, 4) is precise and convenient.

In prints of adults, measurements may be made in absolute units of length, as in fractions of an inch, or else in millimetres. An average ridge-interval makes, however, a better unit, being independent of growth; it is strictly necessary to adopt it in prints made by children, if present measurements are hereafter to be compared with future ones. The simplest plan of determining and employing this unit is to count the number of ridges to the nearest half ridge, within the space of one-tenth of an inch, measured along the axis of the finger at and about the point where it cuts the *summit* of the outline; then, having already prepared scales suitable for the various likely numbers, to make the measurements with the appropriate scale. Thus, if five ridges were crossed by the axis at that part, in the space of one-tenth of an inch, each unit of the scale to be used would be one-fiftieth of an inch; if there were four ridges, each unit of the scale would be one-fortieth of an inch; if six ridges one-sixtieth, and so forth. There is no theoretical or practical difficulty, only rough indications being required.

It is unnecessary to describe in detail how the bearings of any point may be expressed after the fashion of compass bearings, the direction I-O taking the place of East-West, the uppermost direction that of North, and the lowermost of South. Little more is practically wanted than to be able to describe roughly the position of some remarkable feature in the print, as of an island or an enclosure. A ridge that is characterised by these or any other marked peculiarity is easily identified by the above means, and it thereupon serves as an exact basis for the description of other features.

Purkenje's "Commentatio."

Reference has already been made to Purkenje, who has the honour of being the person who first described the inner scrolls (as distinguished from the outlines of the patterns) formed by the ridges. He did so in a University Thesis delivered at Breslau in 1823, entitled *Commentatio de examine physiologico organi visus et systematis cutanei* (a physiological examination of the visual organ and of the cutaneous system). The thesis is an ill-printed small 8vo. pamphlet of fifty-eight pages, written in a form of Latin that is difficult to translate accurately into free English. It is however of great historical interest and reputation, having been referred to by nearly all

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subsequent writers, some of whom there is reason to suspect never saw it, but contented themselves with quoting a very small portion at second-hand. No copy of the pamphlet existed in any public medical library in England, nor in any private one so far as I could learn; neither could I get a sight of it at some important continental libraries. One copy was known of it in America. The very zealous Librarian of the Royal College of Surgeons was so good as to take much pains at my instance, to procure one: his zeal was happily and unexpectedly rewarded by success, and the copy is now securely lodged in the library of the College.

The Title

Commentatio de Examine physiologico organi visus et systematis cutanei quam pro loco in gratioso medicorum ordine rite obtinendo die Dec. 22, 1823. H.X.L.C. publice defendit Johannes Evangelista Purkenje, Med. doctor, Phys. et Path. Professor publicus ordinarius des. Assumto socio Guilielmo Kraus Medicinae studioso.

Translation, p. 42.

"Our attention is next engaged by the wonderful arrangement and curving of the minute furrows connected with the organ of touch[1] on the inner surfaces of the hand and foot,

[1] The Latin is obscure. "Mira vallecularum tangentium in interna parte manus pedisque . . . dispositio flexuraque attentionem . . . in se trahit." There are three ways of translating "tangentium," and none of them makes good sense. In the index of prints he uses the phrase "vallecularum tactui." It would seem that he looked upon the furrows, and not the ridges, as the special seat of touch.

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especially on the last phalanx of each finger. Some general account of them is always to be found in every manual of physiology and anatomy, but in an organ of such importance as the human hand, used as it is for very varied movements, and especially serviceable to the sense of touch, no research, however minute, can fail in yielding some gratifying addition to our knowledge of that organ. After numberless observations, I have thus far met with nine principal varieties of curvature according to which the tactile furrows are disposed upon the inner surface of the last phalanx of the fingers. I will describe them concisely, and refer to the diagrams for further explanation (see Plate 12, Fig. 19).

1. *Transverse flexures.*—The minute furrows starting from the bend of the joint, run from one side of the phalanx to the other; at first transversely in nearly straight lines, then by degrees they become more and more curved towards the middle, until at last they are bent into arches that are almost concentric with the circumference of the finger.

2. *Central Longitudinal Stria.*—This configuration is nearly the same as in 1, the only difference being that a perpendicular stria is enclosed within the transverse furrows, as if it were a nucleus.

3. *Oblique Stria.*—A solitary line runs from one or other of the two sides of the finger, passing obliquely between the transverse curves in 1, and ending near the middle.

4. *Oblique Sinus.*—If this oblique line recurves towards the side from which it started, and is accompanied by several others, all recurved in the same way, the result is an oblique sinus, more or less upright, or horizontal, as the case may be. A junction at its base, of minute lines proceeding from either of its sides, forms a triangle. This distribution of the furrows, in which an oblique sinus is found, is by far the most common, and it may be considered as a special characteristic of man; the furrows that are packed in longitudinal rows are, on the other hand, peculiar to monkeys. The vertex of the oblique sinus is generally inclined towards the radial side of the hand, but it must be observed that the contrary is more frequently the case in the fore-finger, the vertex there tending towards the ulnar

side. Scarcely any other configuration is to be found on the toes. The ring finger, too, is often marked with one of the more intricate kinds of pattern, while the remaining fingers have either the oblique sinus or one of the other simpler forms.

5. *Almond.*—Here the oblique sinus, as already described, encloses an almond-shaped figure, blunt above, pointed below, and formed of concentric furrows.

6. *Spiral.*—When the transverse flexures described in 1, do not pass gradually from straight lines into curves, but assume that form suddenly with a more rapid divergence, a semi-circular space is necessarily created, which stands upon the straight and horizontal lines below, as it were upon a base. This space is filled by a spiral either of a simple or composite form. The term 'simple' spiral is to be understood in the usual geometric sense. I call the spiral 'composite' when it is made up of several lines proceeding from the same centre, or of lines branching at intervals and twisted upon themselves. At either side, where the spiral is contiguous to the place at which the straight and curved lines begin to diverge, in order to enclose it, two triangles are formed, just like the single one that is formed at the side of the oblique sinus.

7. *Ellipse*, or *Elliptical Whorl.*—The semi-circular space described in 6 is here filled with concentric ellipses enclosing a short single line in their middle.

8. *Circle*, or *Circular Whorl.*—Here a single point takes the place of the short line mentioned in 7. It is surrounded by a number of concentric circles reaching to the ridges that bound the semi-circular space.

9. *Double Whorl.*—One portion of the transverse lines runs forward with a bend and recurves upon itself with a half turn, and is embraced by another portion which proceeds from the other side in the same way. This produces a doubly twisted figure which is rarely met with except on the thumb, fore, and ring fingers. The ends of the curved portions may be variously inclined; they may be nearly perpendicular, of various degrees of obliquity, or nearly horizontal.

In all of the forms, 6, 7, 8, and 9, triangles may be seen at the points where the divergence begins between the transverse and the arched lines, and at both sides. On the remaining phalanges, the transverse lines proceed diagonally, and are straight or only slightly curved."

(He then proceeds to speak of the palm of the hand in men and in monkeys.)

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CHAPTER VI

PERSISTENCE

The evidence that the minutiæ persist throughout life is derived from the scrutiny and comparison of various duplicate impressions, one of each pair having been made many years ago, the other recently. Those which I have studied more or less exhaustively are derived from the digits of fifteen different persons. In some cases repeated impressions of one finger only were available; in most cases of two fingers; in some of an entire hand. Altogether the whole or part of repeated impressions of between twenty and thirty different digits have been studied. I am indebted to Sir W. J. Herschel for almost all these valuable data, without which it would have been impossible to carry on the inquiry. The only other prints are those of Sir W. G., who, from curiosity, took impressions of his own fingers in sealing-wax in 1874, and fortunately happened to preserve them. He was good enough to make others for me last year, from which photographic prints were made. The following table gives an analysis of the above data. It would be well worth while to hunt up and take the present finger prints of such of the Hindoos as may now be alive, whose impressions were taken in India by Sir W. J. Herschel, and are still preserved. Many years must elapse before my own large collection of finger prints will be available for the purpose of testing persistence during long periods.

The pattern in every distinct finger print, even though it be only a dabbed impression, contains on a rough average thirty-five different points of reference, in addition to its general peculiarities of outline and core. They consist of forkings, beginnings or ends of ridges, islands, and enclosures. These minute details are by no means peculiar to the pattern itself, but are distributed with almost equal abundance throughout the whole palmar surface. In order to make an exhaustive comparison of two impressions they ought to be photographically enlarged to a size not smaller than those shown in Plate 15. Two negatives of impressions can thus be taken side by side on an ordinary quarter-plate, and any number of photographic prints made from them; but, for still more comfortable working, a further enlargement is desirable, say by the prism, p. . Some of the prints may be made on ferro-prussiate paper, as already mentioned p. ; they are more convenient by far than prints made by the silver or by the platinum process.

Having placed the enlarged prints side by side, two or three conspicuous and convenient points of reference, whether islands, enclosures, or particularly distinct bifurcations, should be identified and marked. By their help, the position of the prints should be readjusted, so that they shall be oriented exactly alike. From each point of reference, in succession, the spines of the ridges are then to be followed with a fine pencil, in the two prints alternately, neatly marking each new point of comparison with a numeral in coloured ink (Plate 13). When both of the prints are good and clear, this is rapidly done; wherever the impressions are faulty, there may be many ambiguities requiring patience to unravel. At first I was timid, and proceeded too hesitatingly when one of the impressions was indistinct, making short alternate traces. Afterwards on gaining confidence, I traced boldly, starting from any well-defined point of reference and not stopping until there were reasonable grounds for hesitation, and found it easy in this way to trace the unions between opposite and incompletely printed ends of ridges, and to disentangle many bad impressions.

An exact correspondence between the *details* of two minutiæ is of secondary importance. Thus, the commonest point of reference is a bifurcation; now the neck or point of divergence of a new ridge is apt to be a little low, and sometimes fails to take the ink; hence a new ridge may appear in one of the prints to have an independent origin, and in the other to be a branch. The *apparent* origin is therefore of little importance, the main fact to be attended to is that a new ridge comes into existence at a particular point; *how* it came into existence is a secondary matter. Similarly an apparently broken ridge may in reality be due to an imperfectly printed

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enclosure ; and an island in one print may appear as part of an enclosure in the other. Moreover, this variation in details may be the effect not only of imperfect inking or printing, but of disintegration due to old age, which renders the impressions of the ridges ragged and broken, as in my own finger prints on the title-page.

Plate 11, Fig. 18 explains the nature of the apparent discrepancies better than a verbal description. In *a* a new ridge appears to be suddenly intruded between two adjacent ones, which have separated to make room for it ; but a second print, taken from the same finger, may have the appearance of either *b* or *c*, showing that the new ridge is in reality a fork of one or other of them, the low connecting neck having failed to leave an impression. The second line of examples shows how an enclosure which is clearly defined in *d*, may give rise to the appearance of broken continuity shown in *e*, and how a distinct island *f* in one of the prints, may be the remnant of an enclosure which is shown in the other. These remarks are offered as a caution against attaching undue importance to disaccord in the details of the minutiæ that are found in the same place in different prints. Usually however the distinction between a fork and the beginning of a new ridge is clear enough ; the islands and enclosures are also mostly well-marked.

Plate 13 gives impressions taken from the fingers of a child of 2½ years in 1877, and again in 1890, when a boy of 13. They are enlarged photographically to the same size, and are therefore on different scales. The impressions from the baby-hand are not sharp, but sufficiently distinct for comparison. Every bifurcation, and beginning or ending of a ridge, common to the two impressions, is marked with a numeral in blue ink. There is only one island in the present instance, and that is in the upper pair of prints; it is clearly seen in the right hand print, lying to the left of the inscribed number 13, but the badness of the left hand print makes it hardly decipherable, so it is not numbered. There are a total of twenty-six good points of comparison common to the upper pair of prints; there are forty-three points in the lower pair, forty-two of which appear in both, leaving a single point of disagreement; it is marked A on the fifth ridge counting from the top. Here a bifurcated ridge in the baby is filled up in the boy. This one exception, small though it be, is in my experience unique. The total result of the two pairs of prints is to afford sixty-eight successes and one failure. The student will find it well worth his while to study these and the following prints step by step, to satisfy himself of the extraordinarily exact coincidences between the two members of either of the pairs. Of course the patterns generally must be the same, if the ridges composing them are exactly alike, and the most cursory glance shows them to be so.

Plate 14, Fig. 21 contains rather less than a quarter of each of eight pairs that were published in the *Phil. Trans.* memoir above alluded to. They were there enlarged photographically to twice their natural size, which was hardly enough, as it did not allow sufficient space for inserting the necessary reference numbers. Consequently they have been again considerably enlarged, so much so that it is impossible to put more than a portion of each on the page. However, what is given suffices. The omitted portions may be studied in the memoir. The cases of 1 and 2, are prints of different fingers of the same individual, first as a child $7\frac{1}{2}$ years old, and then as a boy of $16\frac{1}{2}$. They have been enlarged on the same scale but not to the same size; so the print of the child includes a larger proportion of the original impression than that of the boy. It is therefore only a part of the child's print which is comparable with that of the boy. The remaining six cases refer to four different men, belonging to three quite different families, although their surnames happen to have the same initial, H. They were adults when the first print was made, and from 28 to 30 years older on the second occasion. There is an exact agreement throughout between the two members of each of the eight several couplets.

In the pair 2. A.E.H.H., there is an interesting dot at the point 4 (being an island it deserved to have had two numbers, one for the beginning and one for the end). Small as it is, it persists; its growth in size corresponding to the growth of the child in stature.

For the sake of those who are deficient in the colour sense and therefore hardly able, if at all, to distinguish even the blue numerals in Figs. 20, 21, I give an eleventh example, Plate 15, Fig. 22, printed all in black. The numerals are here very legible, but space for their insertion had to be obtained by sacrificing some of the lineations. It is the right fore-finger of Sir W. Herschel and has been already published twice; first in the account of my lecture at the Royal Institution, and secondly in my paper in the *Nineteenth Century*, in its present conspicuous form. The number of years that elapsed between the two impressions is thirty-one, and the prints contain twenty-four points of comparison, all of which will be seen to agree. The final result of the prints in these pages, is that they give photographic enlargements of the whole or portions of eleven couplets belonging to six different persons, who are members of five unrelated families, and which contain between them 158 points of comparison, of which only one failed. Adding the portions of the prints that are omitted here, but which will be found in the *Phil. Trans.* the material that I have thus far published contains 389 points of comparison, of which one failed. The details are given in the annexed table :—

[TABLE

Order in the Figs.	Initials.	Digit of right hand.	Age at date of first print.	Dates of the two prints. 1st. 2nd.	Years elapsed between the two prints.	Total points of agreement in Figs. 20 & 21.	Total points of agreement in Figs. 20, 22, & in *Ph. Trans.*
FIG. 20							
1.	V. H. Hd.	Fore	$2\frac{1}{2}$	1877-90	13	26	26
2.	V. H. Hd.	Ring	$2\frac{1}{2}$	1877-90	13	42	42
FIG. 21							
1.	A. E. Hl.	Fore	9	1881-90	9	11	33
2.	A. E. Hl.	Ring	9	1881-90	9	5	36
3.	N. H. Tn.	Fore	28	1862-90	28	6	27
4.	N. H. Tn.	Middle	28	1862-90	28	10	36
5.	F. K. Ht.	Fore	28	1862-90	28	12	55
6.	R. F. Hn.	Middle	31	1859-90	31	6	27
7.	W. J. Hl.	Thumb	30	1860-90	30	9	50
8.	W. J. Hl.	Ring	31	1859-90	31	6	32
FIG. 22							
1.	W. J. Hl.	Fore	31	1859-90	31	24	24
				Total points of agreement		157	388
				Do. of disagreement		1	1

It is difficult to give a just estimate of the number of points of comparison that I have studied in many other couplets of prints, because I did not proceed as exhaustively as in these. There were no less than one hundred and eleven of them in the ball of the thumb of the child V. H. Hd., besides twenty-five in the imperfect prints of his middle and little fingers; these alone raise the total of 389 to 525. I must on the whole have looked for more than 700 points of comparison, and have found agreement in every single case that was examined, except the one already mentioned in Fig. 20, of a ridge that was split in the child, but had closed up some few years later.

The prints in the two plates cover the intervals from childhood to boyhood, from boyhood to early manhood, from manhood to about the age of 60, and another set that of Sir. W. G. covers the interval from 67 to 80. This is clearly expressed by the diagram (Plate 15, Fig. 23). As there is no sign, except in one case, of change during any one of these four intervals, which together almost wholly cover the ordinary life of man, we are justified in inferring that between birth and death there is absolutely no change in, say, 699 out of 700 of the numerous characteristics in the markings of the fingers of the same person, such as can be impressed by them whenever it is desirable to do so. Neither can there be any change after death, up to the time when the skin perishes through decomposition; for example, the marks on the fingers of many Egyptian mummies, and on the paws of stuffed monkeys, still remain legible. Very good evidence and careful inquiry is thus seen to justify the popular idea of the persistence of finger-markings, that has hitherto been too rashly jumped at, and which wrongly ascribed the persistence to the general appearance of the pattern, rather than to the minutiæ it contains. There appear to be no external bodily characteristics, other than deep scars and tattoo marks, comparable in their persistence to these markings, whether they be on the finger or on other parts of the palmar surface of the hand, or of the sole of the foot. At the same time they are out of all proportion more numerous than any other measurable features; about thirty-five of them are situated on the bulb of each of the ten digits, in addition to more than 100 on the ball of the thumb, which has not one-fifth of the superficies of the rest of the palmar surface. The total number of points suitable for comparison on the two hands must therefore be not less than one thousand and nearer to two; an estimate which I verified by a rough count on my own hand; similarly in respect to the feet. The dimensions of the limbs and body alter in the course of growth and decay; the colour, quantity, and quality of the hair, the tint and quality of the skin, the number and set of the teeth, the expression of the features, the gestures, the handwriting, even the eye-colour changes after many years. There seems no persistence in the visible parts of the body, except in these minute and hitherto too much disregarded ridges.

It must be emphasised that it is in the minutiæ, and *not* in the measured dimensions of any portion of the pattern, that this remarkable persistence is observed, not even if the measurements be made in units of a ridge-interval. The pattern grows simultaneously with the finger, and its proportions vary with its fatness, leanness, usage, gouty deformation, or age. But, though the pattern as a whole may become considerably altered in length or breadth, the number of ridges, 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 this way, or shrunk in that, and its outline altogether altered; nevertheless every one of the component threads, and every knot in every thread, can easily be traced and identified in both. Therefore, in speaking of the persistence of the marks on the finger, the phrase must be taken to apply principally to the minutiæ, and to the general character of the pattern; not to the measure of its length, breadth, or other diameter; these being no more constant than the stature, or any other of the ordinary anthropometric data.

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CHAPTER VII

EVIDENTIAL VALUE

THE object of this chapter is to give an approximate numerical idea of the value of finger prints as a means of Personal Identification. Though the estimates that will be made are professedly and obviously far below the truth, they are amply sufficient to prove that the evidence afforded by finger prints may be trusted in a most remarkable degree.

Our problem is this: given two finger prints, which are alike in their minutiæ, what is the chance that they were made by different persons?

The first attempt at comparing two finger prints would be directed to a rough general examination of their respective patterns. If they do not agree in being arches, loops, or whorls, there can be no doubt that the prints are those of different fingers, neither can there be doubt when they are distinct forms of the same general class. But to agree thus far goes only a short way towards establishing identity, for the number of patterns that are promptly distinguishable from one another is not large. My earlier inquiries showed this, when endeavouring to sort the prints of 1000 thumbs into groups that differed each from the rest by a "~~just~~-discernible" interval. While the attempt, as already mentioned, was not successful in its main object, it showed that nearly all the collection could be sorted into 100 groups, in each of which the prints had a fairly near resemblance. Moreover twelve or fifteen of the groups referred to different varieties of the loop, and as two-thirds of all the prints are loops, two-thirds of the 1000 specimens fell into twelve or fifteen groups. The chance that an unseen pattern is some particular variety of loop, is therefore compounded of 2 to 3 against its being a loop at all, and of 1 to 12 or 15, as the case may be, against its being the specified kind of loop. This makes an adverse chance of only 2 to 36, or to 45, say as 2 to 40, or as 1 to 20. This very rude calculation suffices to show that on the average, no great reliance can be placed on a general resemblance in the appearance of two finger prints, as a proof that they were made by the same finger, though the obvious disagreement of two prints is conclusive evidence that they were made by different fingers.

When we proceed to a much more careful comparison, and collate successively the numerous minutiæ, their coincidence throughout would be an evidence of identity, whose value we will now try to appraise.

Let us first consider the question how far may the minutiæ, or groups of them, be treated as *independent* variables?

Suppose that a tiny square of paper of only one average ridge-interval in the side, be cut out and dropped at random on a finger print; it will mask from view a minute portion of one, or possibly of two ridges. There can be little doubt that what was hidden could be correctly interpolated by simply joining the ends of the ridge or ridges that were interrupted. It is true, the paper might possibly have fallen exactly upon, and hidden, a minute island or enclosure, and that our reconstruction would have failed in consequence, but such an accident is improbable in a high degree, and may be almost ignored.

Repeating the process with a much larger square of paper, say of twelve ridge-intervals in the side, the improbability of correctly reconstructing the masked portion will have immensely increased. The number of ridges that enter the square on any one side will perhaps, as often as not, differ from the number which emerge from the opposite side; and when they are the same, it does not at all follow that they would be continuous each to each, for in so large a space forks and junctions are sure to occur between some, and it is impossible to know which of the ridges. Consequently there must exist a certain size of square with more than one and less than twelve ridge-intervals in the side, which will mask so much of the print, that it will be an even chance whether the hidden portion can, on the average, be rightly reconstructed or not. The size of that square must now be considered.

If the reader will refer to Plate 14, in which there are eight much enlarged photographs of portions of different finger prints, he will observe that the length of each of the portions exdceeds the breath in the proportion of 3 to 2. Consequently, by drawing one line down the middle and two lines across, each portion may be divided into six squares. Moreover, it will be noticed that the side of each of these squares has a length of about six ridge-intervals. I cut out squares of paper of this size, and throwing one of them at random on any one of the eight portions, succeeded almost as frequently as not in drawing lines on its back, which comparison afterwards showed to have followed the true course of the ridges. The provisional estimate that a length of six ridge-intervals approximated to but exceeded that of the side of the desired square, proved to be correct by the following more exact observations, and by three different methods.

I. The first set of trials consisted of thirty-two different attempts, made upon photographic enlargements of various thumb prints, to double their natural size. A six-ridge-interval square of paper was damped and laid at random on the print, the core of the pattern, which was too complex in many cases to serve as an average test, being alone avoided. The prints being on ordinary albuminised paper, which is slightly adherent when moistened, the patch stuck temporarily wherever it was placed and pressed down. Next a sheet of tracing paper, which we will call No. 1, was laid over all, and the margin of the square patch was traced upon it, together with the course of the surrounding ridges up to that margin. Then, I interpolated on the tracing paper, what seemed to be the most likely course of those ridges which were hidden by the square. No. 1 was then removed, and a second sheet, No. 2, was laid on, and the margin of the patch was outlined on it as before, together with the ridges leading up to it. Next, a corner only of No. 2 was raised, the square patch was whisked away from underneath, the corner was replaced, the sheet was flattened down, and the actual courses of the ridges within the already marked outline were traced in. Thus, there were two tracings of the margin of the square; No. 1 containing the ridges as I had interpolated them, No. 2 as they really were, and it was easy to compare the two. The results are given in the first column of the following table:

INTERPOLATION OF RIDGES IN A SIX-RIDGE-INTERVAL SQUARE.

Result.	Double Enlargements.	Six-fold scale with prism.	Twenty-fold scale with chequer-work.	Total.
Right .	12	8	7	27
Wrong .	20	12	16	48
Total .	32	20	23	75

II. In the second method the tracing papers were discarded, and the prism of a camera lucida used. It threw an image three times the size of the photo-enlargement, upon a card, and there it was traced. The same general principle was adopted as in the first method, but the results being on a larger scale, and drawn on stout paper, were more satisfactory and convenient. They are given in the second column of the table. In this and the foregoing methods, two different portions of the same print, were sometimes dealt with, for it was a trifle more convenient and seemed as good a way of obtaining average results as that of always using portions of different finger prints. The total number of fifty-two trials, by one or other of the two methods, were made from about forty different prints. (I am not sure of the exact number.)

The results in each of the two methods were sometimes quite right, sometimes quite wrong, sometimes neither one nor the other. They depended, in fact, on the individual judgment as to which class it belonged, and might be battled over with more or less show of reason by advocates on opposite sides. Equally dividing these intermediate cases between "right" and "wrong" the results were obtained as shown. In one, and only one, of the cases, the most reasonable interpretation had not been given, and the result had been wrong when it ought to have been right. The purely personal error was therefore disregarded, and the result entered as "right."

III. A third attempt was made by a different method, upon the lineations of a finger print drawn on about a twenty-fold scale. It had first been enlarged four times by photography, and from this enlargement the axes of the ridges had been drawn with a five-fold enlarging pantagraph. The aim now was to reconstruct the entire finger print by two successive and independent acts of interpolation. A sheet of transparent tracing paper was ruled into six-ridge-interval squares, and every one of its alternate squares was rendered opaque by pasting white paper upon it, giving it the appearance of a chess board. When this chequer-work was laid on the print, exactly one half of the six-ridge squares were masked by the opaque squares, while the ridges running up to them could be seen. They were not quite so visible as if each opaque square had been wholly detached from its neighbours, instead of touching them at the extreme corners, still the loss of information thereby occasioned was small, and not worth laying stress upon. It is easily understood that when the chequer-work was moved parallel to itself, through the space of one square, whether upwards or downwards, or to the right or left, the parts that were previously masked became visible, and those that were visible became masked. The object was to interpolate the ridges in every opaque square under one of these conditions; then to do the same for the remaining squares under the other condition, and finally, by combining the results, to obtain a complete scheme of the ridges wholly by interpolation. This was easily done by using two sheets of tracing paper, laid in succession over the chequer-work, whose position on the print had been shifted meanwhiles, and afterwards tracing the lineations that were drawn on one of the two sheets upon the vacant squares of the other. The results are given in the third column of the table.

The three methods give roughly similar results and we may therefore accept the ratios of their totals which is 27 to 75, or say 1 to 3, as representing the chance that the reconstruction of any six-ridge-interval square would be correct under the given conditions. On reckoning the chance as 1 to 2, which will be done at first, it is obvious that the error whatever it may be, is on the safe side. A closer equality in the chance that the ridges in a square might run in the observed way or in some other way, would result from taking a square of five ridge-intervals in the side. I believe this to be very closely the right size. A four-ridge-interval square is certainly too small.

When the reconstructed squares were wrong, they had none the less a natural appearance. This was especially seen, and on a large scale, in the result of the method by chequer-work, in which the lineations of an entire print were constructed by guess. I am so familiar with the run of these ridges in finger prints, that I can speak with confidence on this. My assumption is, that any one of these reconstructions represent lineations that might have occurred in Nature, in association with the conditions outside the square, just as well as the lineations of the actual finger print. The courses of the ridges in each square are subject to uncertainties, due to petty *local* incidents, to which the conditions outside of the square give no sure indication. They must be in great part determined by the particular disposition of each one or more of the half hundred or so sweat glands which the square contains. The ridges rarely run in evenly flowing lines, but may be compared to footways across a broken country, which, while they follow a general direction, are continually deflected by such trifles as a tuft of grass, a stone or a puddle. Even if the number of ridges emerging from a six-ridge-interval square equals the number of those which enter, it does not follow that they run across in parallel lines, for there is plenty of room for any one of the ridges to end, and another to bifurcate. It is impossible, therefore, to know beforehand in which, if in any of the ridges, these peculiarities will be found. When the number of entering and issuing ridges is unequal, the difficulty is increased. There may moreover be islands or enclosures in any particular part of the square. It therefore seems right to look upon the squares as independent variables, in the sense that when the surrounding conditions are alone taken into account, the ridges within their limits may either run in the observed way or in a different way, the chance of these two contrasted events being taken (for safety sake) as approximately equal.

In comparing finger prints which are alike in their general pattern, it may well happen that the proportions of the patterns differ; one may be that of a slender boy, the other that of a man, whose fingers have been broadened or deformed by ill usage. It is therefore requisite to imagine that only one of the prints is divided into exact squares, and to draw that a reticulation has been drawn over the other, in which each mesh included the corresponding parts of the former print. Frequent trials have shown that there is no practical difficulty in actually doing this, and it is the only way of making a fair comparison between the two.

These six-ridge-interval squares may thus be regarded as independent units, each of which is equally liable to fall into one or other of two alternative classes, when the surrounding conditions are alone known. The inevitable consequence from this datum is that the chance of an exact correspondence between two different finger prints, in each of the six-ridge-interval squares into which they may be divided, and which are about 24 in number, is at least as 1 to 2 multiplied into itself 24 times (usually written 2^{24}), that is as 1 to about ten thousand millions. But we must not forget that the six-ridge square was taken in order to ensure under-estimation, a five-ridge square would have been preferable, so the adverse chances would in reality be enormously greater still.

It is hateful to blunder in calculations of adverse chances, by overlooking correlations between variables, and falsely assuming them to be independent, with the result that inflated estimates are made which require to be vastly reduced. Here however there seems to be no room for such an error.

We must next combine the above enormously unfavourable chance, which we will call a, with the other chances of not guessing correctly beforehand, the surrounding conditions under which a was calculated. These latter are divisible into b and c; the chance b is that of not guessing correctly the general course of the ridges adjacent to each square, and c that of not guessing rightly the number of ridges that enter and issue from the square. The chance b has already been discussed, with the result that it might be taken as 1 to 20 for two-thirds of all the patterns. It would be higher for the remainder, and very high indeed for some few of them, but as it is advisable always to underestimate, it may be taken as 1 to 20; or, to obtain the convenience of dealing only with values of 2 multiplied into itself, the still lower ratio of 1 to 24, that is as 1 to 16, or 2^4. As to the remaining chance c with which a and b have to be compounded, namely, that of guessing aright the number of ridges that enter and leave each side of a particular square, I can offer no careful observations. The number of the ridges would for the most part vary between five and seven, and those in the different squares are certainly not quite independent of one another. We have already arrived at such large figures that it is surplusage to heap up more of them, therefore, let us say, as a mere nominal sum much underneath the real figure, that the chance against guessing each and every one of these data correctly is as 1 to 250, or say 2^8 ($=256$).

The result is, that the chance of lineations constructed by the imagination, according to strictly natural forms, which shall be found to resemble those of a single finger print in all their minutiæ, is less than 1 to $2^{24} \times 2^4 \times 2^8$, or 1 to 2^{36}; or 1 to about sixty-four thousand millions. The inference is, that as the number of the human race is reckoned at about sixteen thousand millions, it is a smaller chance than 1 to 4 that the print of a *single* finger of any given person would be exactly like that of the same finger of any other member of the human race.

When two fingers of each of the two persons are compared, and found to have the same minutiæ, the improbability of 1 to 2^{36} becomes squared, and reaches a figure altogether beyond the range of the imagination; when three fingers, it is cubed, and so on.

A single instance has shown that the minutiæ are *not* invariably permanent throughout life, but that one or more of them may possibly change. They may also be destroyed by wounds, and more or less disintegrated by hard work or age. Ambiguities will thus arise in their interpretation, one person asserting a resemblance in respect to a particular feature, while another asserts dissimilarity. It is therefore of interest to know how far a conceded resemblance in the great majority of the minutiæ combined with some doubt as to the remainder, will tell in favour of identity. It will now be convenient to change our datum from a six-ridge to a five-ridge square of which about thirty-five are contained in a single print, 35×5^2 or 35×25 being much the same as 24×6^2 or 24×36. The reason is that this number of thirty-five happens to be the same as that of the minutiæ. We shall therefore not be acting unfairly if, with cautious reservation, and for the sake of obtaining some result, however rough, we consider the thirty-five minutiæ themselves as so many independent variables, and accept the chance now as 1 to 2^{35}.

This has to be multiplied, as before, into the factor of $2^4 \times 2^8$ (which may still be considered appropriate, though it is too small), making a total of adverse chances of 1 to 2^{47}. Upon such a basis, the calculation is simple. There would on the average be 47 instances, out of the total 2^{47} combinations, of similarity in all but one particular; $\frac{47\times46}{1\times2}$ in all but two; $\frac{47\times46\times45}{1\times2\times3}$ in all but three, and so on according to the well-known binomial law. Taking for convenience the powers of 2 to which these values approximate, or rather with the view of not overestimating, let us take the power of 2 that falls short of each of them; these may be reckoned as respectively equal to 2^6, 2^{10}, 2^{14}, 2^{18}, etc. Hence the roughly approximate chances of resemblance in all particulars is as 2^{47} to 1; in all particulars but one, as 2^{47-6}, or 2^{41} to 1; in all but two, as 2^{37} to 1; in all but three, as 2^{33} to 1; in all but four, as 2^{29} to 1. Even 2^{29} is so large, as to require a row of nine figures to express it. Hence a few instances of dissimilarity in the two prints of a single finger, still leave untouched an enormously large residue of evidence in favour of identity, and when two, three or more fingers in the two persons, agree to that extent, the strength of the evidence rises by squares, cubes, etc., far away above the level of that amount of probability which begins to rank as certainty.

Whatever reductions a legitimate criticism may make in the numerical results arrived at in this chapter, bearing in mind the occasional ambiguities pictured in Fig. 18, the broad fact remains, that a complete or nearly complete accordance between two prints of a single finger, and vastly more so between the prints of two or more fingers, affords evidence requiring no corroboration, that the persons from whom they were made are the same. Let it also be remembered, that this evidence is applicable not only to adults, but can establish the identity of the same person at any stage of his life between babyhood and old age, and for some time after his death.

We read of the dead body of Jezebel being devoured by the dogs of Jezreel, so that no man might say, "this is Jezebel," and that the dogs left only her skull, the palms of her hands, and the soles of her feet; but the palms of the hands and the soles of the feet are the very remains by which a corpse might be most surely identified, if impressions of them, made during life, were available.

CHAPTER VIII

PECULIARITIES OF THE DIGITS

THE data used in this chapter are the prints of 5000 different digits, namely, the ten digits of 500 different persons; each digit can thus be treated both separately, and in combination, in 500 cases. Five hundred cannot be called a large number, but it suffices for approximate results; the percentages that it yields may, for instance, be expected to be trustworthy, more often than not, within two units.

When preparing the tables for this chapter, I gave a more liberal interpretation to the word "Arch" than subsequently. At first every pattern between a Forked-Arch and a Nascent-Loop (Plate 7) was rated as an Arch; afterwards they were rated as Loops.

The relative frequency of the three several classes in the 5000 digits, was as follows:—

Arches	6·5 per cent.
Loops	67·5 per cent.
Whorls . . .	26·0 per cent.
Total	100·0

From this it appears, that on the average out of every

15 or 16 digits, one has an arch; out of every 3 digits, two have loops; out of every 4 digits, one has a whorl.

This coarse statistical treatment leaves an inadequate impression, each digit and each hand having its own peculiarity, as we shall see in the following table:—

TABLE I.

Percentage frequency of Arches, Loops and Whorls on the different digits, as observed in the 5000 digits of 500 persons.

Digit.	Right Hand.				Left Hand.			
	Arch.	Loop.	Whorl.	Total.	Arch.	Loop.	Whorl.	Total.
Thumb	3	53	44	100	5	65	30	100
Fore-finger	17	53	30	100	17	55	28	100
Middle do.	7	78	15	100	8	76	16	100
Ring do.	2	53	45	100	3	66	31	100
Little do.	1	86	13	100	2	90	8	100
Total .	30	323	147	500	35	352	113	500

The percentage of arches on the various digits varies from 1 to 17; of loops, from 53 to 90; of whorls, from 13 to 45, consequently the statistics of the digits must be separated, and not massed indiscriminately.

Are the A. L. W. patterns distributed in the same way upon the corresponding digits of the two hands? The answer from the last table is distinct and curious, and will be best appreciated on re-arranging the entries as follows:—

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TABLE II.

Digit.	Arches.		Loops.		Whorls.	
	Right.	Left.	Right.	Left.	Right.	Left.
Fore-finger	17	17	53	53	30	28
Middle do.	7	8	78	76	15	16
Little do.	1	2	86	90	13	8
Thumb	3	5	53	65	44	30
Ring do.	2	3	53	66	45	31
Total 1000	30	35	323	352	147	113

The digits are seen to fall into two well-marked groups; the one including the fore, middle, and little fingers, the other including the thumb and ring-finger. As regards the first group the frequency with which any pattern occurs in any named digit, is statistically the same, whether that digit be on the right or on the left hand; as regards the second group, the frequency differs greatly in the two hands. But though in the first group, the two fore-fingers, the two middle, and the two little fingers of the right hand are severally circumstanced alike in this respect, the difference between the frequency of the several patterns on a fore, a middle, and a little finger is very great.

In the second group, though the thumbs on opposite hands do not resemble each other in the statistical frequency of the A. L. W. patterns, nor do the ring-fingers, there is a great resemblance

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between the respective frequencies in the thumbs and ring-fingers; for instance, the Whorls on either of these fingers on the right hand are only two-thirds as common as those in the left. The figures in each line and in each column are consistent throughout in expressing these curious differences, which must therefore be accepted as facts, and not as statistical accidents, whatever may be their explanation.

One of the most noticeable peculiarities in Table I. is the much greater frequency of Arches on the fore-fingers than on any other of the four digits. It amounts to 17 per cent on the fore-fingers, while on the thumbs and on the remaining fingers, the frequency diminishes (Table III.) in a ratio that roughly accords with the distance of each digit from the fore-finger.

TABLE III.

Percentage frequency of Arches.					
Hand.	Thumb.	Fore-finger.	Middle finger.	Ring-finger.	Little finger.
Right . .	3	*17*	7	2	1
Left . .	5	*17*	8	3	4
Mean . .	4	*17*	7·5	2·5	2·5

The frequency of Loops (Table IV.) has two maxima; the principal one is on the little finger, the secondary on the middle finger.

TABLE IV.

Percentage frequency of Loops.					
Hand.	Thumb.	Fore-finger.	Middle finger.	Ring-finger.	Little finger.
Right . .	53	53	78	66	86
Left . .	65	55	76	53	90
Mean .	59	54	77	59·5	88

Whorls (Table V.) are most common on the thumb and the ring-finger, most rare in the middle and little fingers.

TABLE V.

Percentage frequency of Whorls.					
Hand.	Thumb.	Fore-finger.	Middle finger.	Ring-finger.	Little finger.
Right . .	44	30	15	45	13
Left . .	30	28	16	31	8
Mean .	37	29	15·5	38	10·5

The fore-finger is peculiar in the frequency with which the direction of the slopes of its loops differs from that which is by far most common in all other digits. A loop *must* have a slope, being caused by the disposition of the ridges into the form of a pocket, opening downwards to one or other side of the finger. If it opens towards the inner or thumb-side of the hand, it will be called an inner slope;

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if towards the outer or little-finger-side, it will be called an outer slope. In all digits, except the fore-fingers, the inner slope is much the rarer of the two; but in the fore-fingers the inner slope appears two-thirds as frequently as the outer slope. Out of the percentage of 53 loops of the one or other kind on the right fore-finger, 21 of them have an inner and 32 an outer slope; out of the percentage of 55 loops on the left fore-finger, 21 have inner and 34 have outer slopes. These subdivisions 21-24, and 32-34, corroborate the strong statistical similarity that was observed to exist between the frequency of the several patterns on the right and left fore-fingers; a condition which was found to characterise the middle and little fingers as well.

(p 86, 4,)

It is strange that Purkenje considers the "inner" slope on the fore-finger to be more frequent than the "outer." My nomenclature differs from his but there is no doubt as to the disagreement in meaning. The facts to be adduced hereafter make it most improbable that the persons observed were racially unlike in this particular.

The tendencies of digits to resemble one another will now be considered in their various combinations. They will be taken two at a time, in order to learn the frequency with which both members of the various couplets are affected by the same A. L. W. class of pattern. Every combination will be discussed, except those into which the little finger enters. These are omitted, because the overwhelming frequency of loops in the little fingers would make the results of comparatively little interest, while their insertion would greatly increase the size of the table.

TABLE VI*a*.

Percentage of cases in which the same class of pattern occurs in the same digits *of the two hands.*

(From observation of 5000 digits of 500 persons.)

Couplets of Digits.	Arches.	Loops.	Whorls.	Total.
The two thumbs . .	2	48	24	71
„ fore-fingers .	9	38	20	67
„ middle fingers .	3	65	9	77
„ ring-fingers .	2	46	26	74
Mean of the Totals	.	.	.	72

TABLE VI*b*.

Percentage of cases in which the same class of pattern occurs in various couplets of different digits.

(From 500 persons as above.)

Couplets of digits.	Of Same Hands.				Of Opposite Hands.			
	Arch.	Loops.	Whorls.	Total.	Arch.	Loops.	Whorls.	Total.
Thumb and fore-finger .	2	35	16	53	2	33	15	50
Thumb and middle finger	1	48	9	58	1	47	8	56
Thumb and ring-finger .	1	40	20	61	1	38	18	57
Fore and middle finger	5	48	12	65	5	46	11	62
Fore and ring-finger . .	2	35	17	54	2	35	17	54
Middle and ring-finger .	2	50	13	65	2	50	12	64
Means of the Totals	.	.		59	.	.	.	57

A striking feature in this last table is the close similarity between corresponding entries relating to the same and to the opposite hands. There are eighteen sets to be compared; namely, six couplets of different names, in each of which the frequency of three different classes of patterns is discussed. The eighteen pairs of corresponding couplets are closely alike in every instance. It is worth while to rearrange the figures as below, for the greater convenience of observing their resemblances.

TABLE VII.

Couplet.	Arches in		Loops in		Whorls in	
	Same hand.	Opposite hand.	Same hand.	Opposite hand.	Same hand.	Opposite hand.
Thumb and fore-finger . .	2	2	35	33	16	15
Thumb and middle finger .	1	1	48	47	9	8
Thumb and ring-finger . .	1	1	40	38	20	18
Fore and middle finger . .	5	5	48	46	12	11
Fore and ring-finger . .	2	2	35	35	17	17
Middle and ring-finger . .	2	2	50	50	13	12

The agreement in the above entries is so curiously close as to have excited grave suspicion that it was due to some absurd blunder, by which the same figures were made inadvertently to do duty twice over, but subsequent checking disclosed no error.

Though the unanimity of the results is wonderful, they are fairly arrived at, and leave no doubt that the relationship of any one particular digit, whether thumb, fore, middle, ring or little finger, to any other particular digit, is the same, whether the two digits are on the same or on opposite hands. It would be a most interesting subject of statistical inquiry to ascertain whether the distribution of malformations, or of the various forms of skin disease among the digits, corroborates this unexpected and remarkable result. I am sorry to have no means of undertaking it, being assured on good authority that no adequate collection of the necessary data has yet been published.

It might be hastily inferred from the statistical identity of the connection between, say, the right thumb and each of the two fore-fingers, that the patterns on the two fore-fingers ought always to be alike, whether arch, loop, or whorl. If X, it may be said, is identical both with Y and with Z, then Y and Z must be identical with one another. But the statement of the problem is wrong; X is not identical with Y and Z, but only bears an identical amount of statistical resemblance to each of them; so this reasoning is inadmissible. The character of the pattern on any digit is determined by causes of whose precise nature we are ignorant; but we may rest assured that they are numerous and variable, and that their variations are in large part independent of one another. We can in imagination divide them into groups, calling those that are common to the

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thumb and the fore-finger of either hand, and to those couplets exclusively, the A causes; those that are common to the two thumbs and to these exclusively, the B causes; and similarly those common to the two fore-fingers exclusively, the C causes.

Then the sum of the variable causes determining the class of pattern in the four several digits now in question are these:—

Right thumb	A + B + an unclassed residue called	$X_{(1)}$
Left thumb	A + B + " " "	$X_{(2)}$
Right fore-finger	A + C + " " "	$Z_{(1)}$
Left fore-finger	A + C + " " "	$Z_{(2)}$

The nearness of relationship between the two thumbs is sufficiently indicated by a fraction that expresses the proportion between all the causes common to the two thumbs exclusively, and the totality of the causes by which the A. L. W. class of the patterns of the thumbs is determined, that is to say, by

$$\frac{A + B}{A + B + X_{(1)} + X_{(2)}} \quad . \quad . \quad . \ (1).$$

Similarly the nearness of the relationship between the two fore-fingers by

$$\frac{A + C}{A + C + Z_{(1)} + Z_{(2)}} \quad . \quad . \quad . \ (2).$$

And that between a thumb and a fore-finger by

$$\frac{A}{A + B + C + X_{(1)} \text{ (or } X_{(2)}) + Z_{(1)} \text{ (or } Z_{(2)})} \quad . \ (3).$$

The fractions (1) and (2) being both greater than (3), it follows that the relationships between the two thumbs, or between the two fore-fingers, are closer than that between the thumb and either fore-finger; at the same time it is clear that neither of the two former relationships is so close as to reach identity. Similarly as regards the other couplets of digits. The tabular entries fully confirm this deduction, for, without going now into further details, it will be seen from the "Mean of the Totals" at the bottom line of Table VI*b* that the average percentage of cases in which two different digits have the same class of patterns, whether they be on the same or on opposite hands, is 59 or 57 (say 58), while the average percentage of cases in which right and left digits bearing the same name have the same class of pattern (Table VI*a*) is 72. This is barely two-thirds of the 100 which would imply identity. At the same time, the 72 considerably exceeds the 58.

Let us now endeavour to measure the relationships between the various couplets of digits on a well-defined centesimal scale, first recalling the fundamental principles of the connection that subsists between relationships of all kinds, whether between digits, or between kinsmen, or between any of those numerous varieties of related events with which statisticians deal.

Relationships are all due to the joint action of two groups of variable causes, the one common to both of the related objects, the other special to each, as in the case just discussed. Using an analogous nomenclature to that already employed, the peculiarity of one of the two objects is due to an aggregate of variable causes that we may call C + X, and that of the other to C + Z, in which C are the causes common to both, and X and Z the special ones. In exact proportion as X and Z diminish, and C becomes of overpowering effect, so does the closeness of the relationship increase. When X and Z both disappear, the result is identity of character. On the other hand, when C disappears, all relationship ceases, and the variations of the two objects are strictly independent. The simplest case is that in which X and Z are equal, and *in this*, it becomes easy to devise a scale in which 0° shall stand for no relationship, and 100° for identity, and upon which the intermediate degrees of relationship may be marked at their proper value. I will, but with some misgiving, attempt to subject the digits to this form of measurement, and upon this assumption. It will save time first to work out an example, and then, after gaining in that way, a clearer understanding of what the process is, to discuss its defects. Let us select for our example the case that brings out these defects in the most conspicuous manner, as follows:—

Table V. tells us that the percentage of whorls in the right ring-finger is 31, and in the left ring-finger 45. Table VI*a* tells us that the percentage of the double event of a whorl occurring on both the ring-fingers of the same person is 26. It is required to express the relationship between the right and left ring-fingers on a centesimal scale, in which 0° shall stand for no relationship at all, and 100° for the closest possible relationship.

If no relationship should exist, there would nevertheless be a certain percentage of instances, due to pure chance, of the double event of whorls occurring in both ring-fingers, and it is easy to calculate their frequency from the above data. The number of possible combinations of 100 right ring-fingers with 100 left ones is 100×100, and of these 31×45 would be double events as above (call these for brevity "double whorls"). Consequently the chance of a double whorl in any single couplet is $\frac{31 \times 45}{100 \times 100}$, and their average frequency in 100 couplets,—in other words, their average percentage is $\frac{31 \times 45}{100} = 13{\cdot}95$, say 14. If, then, the observed percentage of double whorls should be only 14, it would be a proof that the A.L.W. classes of patterns on the right and left ring-fingers were quite independent; so their relationship, as expressed on the centesimal scale, would be 0°. There could never be less than 14 double whorls under the given conditions, except through some statistical irregularity.

Now consider the opposite extreme of the closest possible relationship, subject however, and this is the weak point, to the paramount condition that the average frequencies of the A.L.W. classes may be taken as *pre-established*. As there are 45 per cent of whorls on the left ring-finger, and only 31 on the right, the tendency to form double whorls, however stringent it may be, can only be satisfied in 31 cases. There remains a superfluity of 14 per cent cases in the left ring-finger which perforce must have for their partners either arches or loops. Hence the percentage of frequency that indicates the closest feasible relationship under the pre-established conditions, would be 31.

The range of all possible relationships in respect to whorls, would consequently lie between a percentage frequency of the minimum 14 and the maximum 31, while the observed frequency is of the intermediate value of 26. Subtracting the 14 from these three values, we have the series of 0, 12, 17. These terms can be converted into their equivalents in a centesimal scale that reaches from 0° to 100° instead of from 0° to 17°, by the ordinary rule of three, $12 : x :: 17 : 100$; $x = 70$ or 71, whence the value x of the observed relationship on the centesimal scale would be 70° or 71°, neglecting decimals.

This method of obtaining the value of 100° is open to grave objection in the present example. We have no right to consider that the 45 per cent of whorls on the left ring-finger, and the 31 on the right, can be due to pre-established conditions, which would exercise a paramount effect even though the whorls were due entirely to causes common to both fingers. There is some self-contradiction in such a supposition. Neither are we at liberty to assume that the respective effects of the special causes X and Z are equal in average amount; if they were, the percentage of whorls on the right and on the left finger would invariably be equal.

In this particular example the difficulty of determining correctly the scale value of 100° is exceptionally great; elsewhere, the percentages of frequency in the two members of each couplet are more alike. In the two fore-fingers, and again in the two middle fingers, they are closely alike. Therefore it is not unreasonable to pass over the objection in these latter cases, that X and Z have not been proved to be equal, but we must accept the results in all other cases with great caution.

When the digits are of different names,—as the thumb and the fore-finger,—whether the digits be on the same or on opposite hands, there are two cases to be worked out; namely, such as (1) right thumb and left fore-finger, and (2) left thumb and right fore-finger. Each accounts for 50 per cent of the observed cases; therefore the mean of the two percentages is the correct percentage. The relationships calculated in the following table do not include arches, except in two instances mentioned in a subsequent paragraph, as the arches are elsewhere too rare to furnish useful results.

It did not seem necessary to repeat the calculation for couplets of digits of different names, situated on opposite hands, as those that were calculated on closely the same data for similar couplets situated on the same hands, suffice for both. It is evident from the irregularity in the run of the figures that the units in the several entries cannot be more than vaguely approximate. They have however been retained, as being possibly better than nothing at all.

[TABLE VIII

TABLE VIII.

Approximate Measures of Relationship between the various Digits, on a Centesimal Scale.

(0° = no relationship; 100° = the utmost feasible likeness.)

Couplets.	Loops.	Whorls.	Means.
Digits of same name.			
Right and left thumbs .	57	64	61
" " fore-fingers .	37	59	48
" " middle fingers	34	52	43
" " ring-fingers .	61	70	65
Means .	47°	61°	53°
Digits of different names on the same or on opposite hands.			
Thumb and fore-finger . .	19	29	24
" middle finger . .	19	34	27
" ring-finger	33	44	39
Fore and middle finger . .	52	68	60
" ring-finger . .	13	34	23
Middle and ring-finger . .	31	74	52
Means . .	28°	47°	37°

The arches were sufficiently numerous in the fore-fingers (17 per cent), to fully justify the application of this method of calculation. The result was 43°, which agrees fairly with 48°, the mean of the loops and the whorls. In the middle finger the frequency of the arches was only half the above amount and barely suffices for calculation. It gave the result of 38°, which also agrees fairly with 43°. the mean of the loops and the whorls for that finger.

Some definite results may be gathered from this table notwithstanding the irregularity with which the figures run. Its upper and lower halves clearly belong to different statistical groups, the entries in the former being almost uniformly larger than those in the latter, in the proportion of 53° to 37°, say 3 to 2, which roughly represents in numerical terms the nearer relationship between digits of the same name, as compared to that between digits of different names. It seems also that of the 6 couplets of digits bearing different names, the relationship is closest between the middle finger and the two adjacent ones (60° and 52°, as against 24°, 27°, 39° and 23°). It is further seen in every pair of entries, that whorls are related together more closely than loops. I note this, but cannot explain it. So far as my statistical inquiries into heredity have hitherto gone, all peculiarities were found to follow the same law of transmission, none being more surely inherited than others. If there were a tendency in any one out of many alternative characters to be more heritable than the rest, that character would become universally prevalent, in the absence of restraining influences. But it does not follow that there are no peculiar restraining influences here, nor that what is true for heredity, should be true, in all its details, as regards the relationships between the different digits.

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CHAPTER IX

METHODS OF INDEXING

In this chapter the system of classification by Arches, Loops, and Whorls described in Chapter V. will be used for indexing two, three, six or ten digits, as the case may be.

An index to each set of finger marks made by the same person, is needful in almost every kind of inquiry, whether it be for descriptive purposes, for investigations into race and heredity, or into questions of symmetry and correlation. It is essential to possess an index to the finger marks of known criminals before the method of finger prints can be utilised as an organised means of detection.

The ideal index might be conceived to consist of a considerable number of compartments, or their equivalents, each bearing a different index-heading, into which the sets of finger prints of different persons may be severally sorted, so that all similar sets shall lie in the same compartment.

The principle of the proposed method of index-headings is, that they should depend upon a few conspicuous differences of pattern in many fingers,

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and not upon many minute differences in a few fingers. It is carried into effect by distinguishing the A.L.W. class of pattern on each digit in succession, by a letter,—*a* for Arch, *l* for Loop, *w* for Whorl; or else, as an alternative method, to subdivide *l* by using *i* for a loop with an Inner slope, and *o* for one with an Outer slope, as the case may be. In this way, the class of pattern in each set of ten digits is described by a sequence of ten letters, the various combinations of which are alphabetically arranged and form the different index-headings. Let us now discuss the best method of carrying out this principle, by collating the results of alternative methods of applying it. We have to consider the utility of the *i* and *o* as compared to the simple *l*, and the gain through taking all ten digits into account, instead of only some of them.

It will be instructive to print here an actual index to the finger prints of 100 different persons, who were not in any way selected, but taken as they came, and to use it as the basis of a considerable portion of the following remarks, to be checked where necessary, by results derived from an index to 500 cases, in which these hundred are included.

[Table IX.

This index is drawn up on the principle shortly to be explained, that will be entitled the "i and o in forefinger" method

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Table IX.—Index to 100 Sets of Finger Prints.

Order of Entry	A Right.	B Left.	C Rt.	D Lt.	Order of Entry.	A Right.	B Left.	C Rt.	D Lt.
	F.M.R.	F.M.R.	T.L.	T.L.		F.M.R.	F.M.R.	T.L.	T.L.
1	*a a a*	*a a a*	*a a*	*l a*	41	*i l l*	*i l l*	*w l*	*l l*
2	"	"	*a l*	*a l*	42	"	*i w w*	*w l*	*w l*
3	"	"	"	"	43	*i l w*	*i l l*	*l l*	*w l*
4	"	"	*w l*	*l l*	44	"	"	*w w*	*w l*
5	*a a l*	*a a l*	*a l*	*a l*	45	"	*i l w*	*w w*	*w l*
6	"	"	*l l*	*l l*	46	"	*i w l*	*l l*	*l l*
7	"	"	"	"	47	"	*w l w*	*w l*	*w l*
8	"	*a a w*	*l l*	*l l*	48	"	*w w l*	*l l*	*l l*
9	"	*a l l*	*l l*	*l l*	49	*i w w*	*a l l*	*w l*	*w l*
10	"	"	*l w*	*w l*	50	"	*w w w*	*w l*	*w l*
11	"	*o l l*	*l l*	*l l*	51	"	"	"	"
12	*a a w*	*a a l*	*l l*	*l l*	52	*o a w*	*o l l*	*l l*	*l l*
13	"	*a l l*	*l l*	*l l*	53	*o l l*	*a l l*	*w l*	*l l*
14	*a l a*	*a a a*	*l a*	*l a*	54	"	"	"	"
15	"	"	*l a*	*l w*	55	"	"	"	"
16	"	*o l l*	*w l*	*l l*	56	"	"	*w l*	*w l*
17	*a l l*	*a a l*	*l l*	*a l*	57	"	*i l l*	*l l*	*l l*
18	"	"	*l l*	*l l*	58	"	"	"	"
19	"	"	"	"	59	"	"	"	"
20	"	"	"	"	60	"	*o l l*	*l l*	*l l*
21	"	"	"	"	61	"	"	"	"
22	"	"	*w l*	*l l*	62	"	"	"	"
23	"	*a l w*	*l l*	*l l*	63	"	"	"	"
24	"	*i l l*	*l l*	*l l*	64	"	"	"	"
25	"	"	"	"	65	"	"	"	"
26	*a l l*	*i l l*	*w l*	*l l*	66	"	*w a l*	*l l*	*w l*
27	"	*o a l*	*w l*	*l l*	67	"	*w w w*	*l l*	*w l*
28	"	*o l l*	*w l*	*l l*	68	*o l w*	*a l l*	*l l*	*l l*
29	"	*w w w*	*w l*	*l l*	69	"	"	*w l*	*w l*
30	*a l w*	*i l w*	*l l*	*l l*	70	"	*i l l*	*w l*	*w l*
31	"	*o a l*	*l l*	*l l*	71	"	*o l l*	*l l*	*l l*
32	"	*o l l*	*l w*	*l l*	72	"	*o l w*	*l l*	*l l*
33	"	"	*w l*	*w l*	73	"	"	*w l*	*l l*
34	"	*o l w*	*a l*	*a l*	74	"	"	"	"
35	*i l l*	*a l l*	*w l*	*l l*	75	*w l l*	*i l l*	*l l*	*w l*
36	"	"	*w l*	*w l*	76	*w l l*	*w l l*	*l l*	*l l*
37	"	*i l l*	*l l*	*l l*	77	"	"	"	"
38	"	"	"	"	78	"	"	"	"
39	"	"	"	"	79	"	"	*w l*	*w l*
40	"	"	"	"	80	"	*w l w*	*l l*	*l l*

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TABLE IX.—INDEX TO 100 SETS OF FINGER PRINTS—*Continued.*

Order of Entry	A Right. F.M.R.	B Left. F.M.R.	C Rt. T.L.	D Lt. T.L.	Order of Entry	A Right. F.M.R.	B Left. F.M.R.	C Rt. T.L.	D Lt. T.L.
81	*w l w*	*o l w*	*l l*	*l l*	91	*w w w*	*o l w*	*w l*	*l l*
82	,,	,,	*l l*	*a l*	92	,,	*w l w*	*w l*	*w l*
83	,,	,,	*w l*	*l l*	93	,,	,,	,,	,,
84	,,	*w w w*	*w l*	*w l*	94	,,	*w w l*	*l l*	*l w*
85	,,	,,	*w w*	*l l*	95	,,	*w w w*	*l l*	*l l*
86	,,	,,	*w w*	*l w*	96	,,	,,	*w l*	*l l*
87	,,	,,	*w w*	*w w*	97	,,	,,	*w l*	*w l*
88	,,	,,	,,	,,	98	,,	,,	*w w*	*w l*
89	*w w l*	*i l l*	*l l*	*l l*	99	,,	,,	,,	,,
90	,,	*w l l*	*w l*	*l l*	100	,,	,,	*w w*	*w w*

The sequence in which the digits have been registered is not from the thumb outwards to the little finger, but, on account of various good reasons that will be appreciated as we proceed, in the following order.

The ten digits are registered in four groups, which are distinguished in the Index by the letters A, B, C, D:—

A. *First.* The fore, middle, and ring-fingers of the *right* hand taken in that order.

B. *Second.* The fore, middle, and ring-fingers of the *left* hand taken in that order.

C. *Third.* The thumb and little finger of the *right* hand.

D. *Fourth.* The thumb and little finger of the *left* hand.

Consequently an index-heading will be of the form—

First group.	Second group.	Third group.	Fourth group.
a a l	*a a w*	*l l*	*l l*

These index headings are catalogued in alphabetical order. The method used in the Index is that which takes note of no slopes, except those of loops in the fore-finger of either hand. Consequently the index-heading for my own digits, printed on the title-page, is *w l w* ‖ *o l l* ‖ *w l* ‖ *w l*. Those of the eight sets in Plate VI. are as follows:—

i l w	*i l l*	*w w*	*w l*
o l w	*o l w*	*w l*	*l l*
o l w	*o l l*	*l l*	*l l*
i l w	*i l w*	*w l*	*w l*
i l w	*i w l*	*l l*	*l l*
i l l	*w w l*	*l l*	*l l*
o l l	*a a l*	*l l*	*a l*
o a a	*a a a*	*l a*	*l a*

For convenience of description and reference the successive entries in the specimen index have been numbered from 1 to 100, but that is no part of the system: those figures would be replaced in a real index by names and addresses.

A preliminary way of obtaining an idea of the differentiating power of an index is to count the number of the different headings that are required to classify a specified number of cases. A table is appended which shows the numbers of the headings in the three alternative methods of (1) noting slopes of all kinds in all digits, (2) of noting slopes of Loops

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only and in the fore-fingers only, and (3) of disregarding the slopes altogether. Also in each of these three cases taking account of—

(*a*) All the ten digits;
(*b*) the fore, middle, and ring-fingers of both hands;
(*c*) those same three fingers, but of the right hand only;
(*d*) the fore and middle fingers of the right hand.

TABLE X.

No. of different index-heads in 100 sets of Finger Prints.

No. of digits regarded.	Digits noted.	Account taken of		
		All slopes.	*i* and *o* in fore-fingers.	No slope.
10	All the 10 digits . .	82	76	71
6	Fore, middle, and ring-fingers of both hands	65	50	43
3	Of right hand only .	25	16	14
2	Fore and middle of right hand only . .	12	8	7

The column headed "all slopes" refers to the method first used with success, and described in my Memoir, already alluded to (*Proc. Roy. Soc.*, 1891), accompanied by a specimen index, from which the present one was derived. There the direction of the slope of every pattern that has one, is taken into account, and in order to give as much scope as possible to the method, the term Arch (I then called it a Primary) was construed somewhat over liberally (see p.). It was made to include the nascent-loop Fig. 12 (2) and Fig. 16 (48), so long as not more than a single recurved ridge lay within the outline of the pattern; therefore many of the so-called arches had slopes. It is not necessary to trouble the reader with the numerical nomenclature that was then used, the method itself being now obsolete. Full particulars of it are given in the Memoir.

A somewhat large experience in sorting finger prints in various ways and repeatedly, made it only too evident that the mental strain and risk of error caused by taking all slopes into account was considerable. The judgment became fatigued and the eye puzzled by having to assign opposite meanings to the same actual direction of a slope in the right and left hands respectively. There was also a frequent doubt as to the existence of a slope in large whorls of the tendril and circlet-in-loop patterns (Fig. 13, 23) when the impressions had not been rolled. A third objection is the rarity of the inner slopes in any other digit than the fore-finger. It acted like a soporific to the judgment not only of myself but of others, so that when an inner slope did occur it was apt to be overlooked. The first idea was to discard slopes altogether, notwithstanding the accompanying loss of index power, but this would be an unnecessarily trenchant measure. The slope of a loop, though it be on the fore-finger alone, decidedly merits recognition, for it differentiates such loops into two not very

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unequal classes. Again, there is little chance of mistake in noting it, the impression of the thumb on the one side and those of the remaining fingers on the other, affording easy guidance to the eye and judgment. These considerations determined the method I now use exclusively, and it is that to which the second column of Table X., headed "*i* and *o* in fore-fingers," refers.

The heading of the third column, "no slope," explains itself, no account having been there taken of any slopes whatever, so *i* and *o* disappear, having become merged under *l*.

The table gives a very favourable impression of the differentiating power of all these methods of indexing. By the "*i* and *o* fore-finger" method, it requires as many as 76 different index-headings to include the finger prints of 100 different persons; 195 of 300 persons, and 285 of 500.

The number of entries under each index-heading varies greatly; reference to the index of 100 sets showing no less than six entries (Nos. 60-65) under one of them, and four entries (Nos. 18-21, and 37-40), under each of two others. Thus, although a large portion of the 100 sets are solitary entries under their several headings, and can be found by a single reference, the remainder are grouped together like the commoner surnames in a directory. They are troublesome to distinguish, and cannot be distinguished at all except by supplementary characteristics, such as the number of ridges in some specified part of the pattern, or the character of the cores.

by which
Table IX was compiled and

In other respects the difference of merit between the three methods is not great, as is succinctly indicated by the next table.

TABLE XI.—*In 100 Sets.*

Number of Entries under the same head.	No. of different index-headings.		
	All slopes.	*i* and *o* fore-fingers only.	No slope.
1	71	63	58
2	10	7	9
3	1	3	1
4	...	2	2
5	...	...	...
6	1	...	...
13	...	...	1
Total . . .	83	76	71

Hence it is evident that the second method of "*i-o* fore-finger" is capable of dealing rapidly with 100 cases, but that the method of "no slope" will give trouble in twelve out of the hundred cases.

[TABLE XII.

TABLE XII.

Index-headings under which more than 1 per cent of the sets of Finger Prints were registered.

(500 sets observed.)

i and *o* in fore-fingers.			No slope.		
No. for reference.	Index-heading.	Frequency per cent.	No. for reference.	Index-heading.	Frequency per cent.
1	*all all ll ll*	1·2	I.	*all all ll ll*	1·2
2	*all ill* ,, ,,	1·6	II.	*all lll* ,, ,,	2·2
3	*ill ill* ,, ,,	2·8			
4	*oll ill* ,, ,,	1·4	III.	*lll lll* ,, ,,	9·2
5	*oll oll* ,, ,,	4·0			
6	*ill oll wl ll*	1·2	IV.	*lll lll wl ll*	3·2
7	*oll oll* ,, ,,	1·4			
8	*oll all ll ll*	2·2	V.	*lll all ll ll*	3·0
9	*olw ull* ,, ,,	2·0	VI.	*llw lll* ,, ,,	3·0
10	*wll wll* ,, ,,	1·2	VII.	*wll wll* ,, ,,	1·2
11	*www www ww ww*	1·4	VIII.	*www www ww ww*	1·4

The headings in the right half of the table include more cases than the left half, because a combination of two or more cases that severally contain less than 1 per cent of the finger prints, and are therefore ignored in the first half of the table, may exceed 1 per cent and find a place in the second half.

The entries in Table XII. are derived from a catalogue of 500 sets, and include all entries that appeared more than five times in other words, whose frequency exceeded 1 per cent. These are the index-headings that give enough trouble to deserve notice in catalogues of, say, from 500 to 1000 sets.

In the left half of Table XII. all the index-headings are given, under each of which more than 1 per cent of the sets fell, when the method of "*i* and *o* in fore-fingers" was adopted; also the respective percentage of the cases that fell under them. In the right half of the table are the corresponding index-headings, together with the percentages of frequency, when the "no slope" method is employed. These are distinguished by Roman numerals. The great advantage of the "*i* and *o* fore-finger" method lies in its power of breaking up certain large groups which are very troublesome to deal with by the "no slope" method. According to the latter as many as 9·2 per cent of all the entries fall under the index-heading marked III., but according to the "*i-o* fore-finger" method these are distributed among the headings 3, 4, and 5. The "all slopes" method has the additional merit of breaking up the large group Nos. 11 and VIII. of "all whorls," but its importance is not great on that account, as whorls are distinguishable by their cores, which are less troublesome to observe than their slopes.

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The percentage of all the entries that fall under a single index-heading, according to the "*i-o* fore-finger" method, diminishes with the number of entries at the following rate:—

TABLE XIII.

	Total number of entries.		
	100	300	500
Percentage of entries falling under a single head .	63	49·0	39·8

It may be that every one of the $4^2 \times 3^8$, or one hundred and five thousand possible varieties of index-headings, according to the "*i-o* fore-finger" method, may occur in Nature, but there is much probability that some of them may be so rare that instances of no entry under certain heads would remain in the register, even of an enormous number of persons.

Hitherto we have supposed that prints of the ten fingers have in each case been indexed. The question now to be considered is the gain through dealing in each case with all ten digits, instead of following the easier practice of regarding only a few of them. The following table, drawn up from the hundred cases by the "all slopes" method, will show its amount.

TABLE XIV.—*From* 100 *Sets.*

Digits.	No. of digits.	No. of different index-headings.		
		All slopes.	*i* and *o*, fore-finger.	No slope.
Fore and middle of right hand	2	11	8	7
Fore, middle and ring of right hand	3	23	16	14
Fore, middle and ring of both hands	6	65	50	45
All ten digits	10	83	76	73

The trouble of printing, reading off, and indexing the ten digits, is practically twice that of dealing with the six fingers; namely, three on each of the hands; the thumb being inconvenient to print from and having to be printed separately, even for a dabbed impression, while the fingers of either hand can be dabbed down simultaneously.

For a large collection the ten digit method is certainly the best, as it breaks up the big battalions; also in case of one or more fingers having been injured, it gives reserve material to work upon.

We now come to the great difficulty in all classifications; that of transitional cases. What is to be done with those prints which cannot be certainly classed as Arches, Loops, or Whorls, but which lie between some two of them. These occur about once in every forty digits, or once in every four pairs of hands. The roughest way is to put a mark by the side of the entry to indicate doubt, a better one is to make a mark that shall express the nature of the peculiarity; thus a particular eyed pattern (Plate 10, Fig 16, *n*) may be transitional between a loop and a whorl; under whichever of the two it is entered, the mark might be an *e* to show that anyhow it is an eye. Then when it is required to discover whether an index contains a duplicate of a given specimen in which a transitional pattern occurs, the two headings between which the doubt lies have to be searched, and the marked entries will limit the search. Many alternative ways of marking may be successfully used, but I am not yet prepared to propose one as being distinctly the best. When there are two of these marks in the same set, it seldom happens that more than two references have to be made, as it is usual for the ambiguity to be of the same kind in both of the doubtful fingers. If the ambiguities were quite independent, then two marks would require four references, and three marks would require nine. There are a few nondescript prints that would fall under a separate heading, such as Z. Similarly, as regards lost or injured fingers.

I have tried various methods of sub-classification, and find no difficulty in any of them, but doubt whether general rules are advisable; it seems best to treat each large group on its own merits.

One method that I have adopted and described in the *Proc. Royal Soc.*, is to sketch in a cursive and symbolic form the patterns of the several fingers in the order in which they appear in the print, confining myself to a limited number of symbols, such as might be used for printer's types. They sufficed fairly for some thousands of the finger marks upon which I tried them, but might I daresay be improved. A little violence has of course to be used now and then, in fitting some unusual patterns to some one or other of these few symbols. But we are familiar with such processes in ordinary spelling, making the same letter do duty for different sounds, as *a* in the words *as*, *ale*, *ask*, and *all*. The plan of using symbols has many secondary merits. It facilitates a leisurely revision of first determinations, it affords a pictorial record of the final judgment that is directly comparable with the print itself; it almost wholly checks blunders between inner and outer slopes. A beginner in finger reading will educate his judgment by habitually using them at first.

The cores give great assistance in breaking up the very large groups of all-loops (see Table XII., III.); so does an entry of the approximate number of ridges in some selected fingers, between the core and the upper outline of the loop.

The plan I am now using for keeping finger prints in regular order, is this:—In my principal collection, the prints of each person's ten digits are taken on the same large card; the four fingers of either hand being dabbed down simultaneously above, and all the ten digits rolled separately below. (Plate 2, Fig. 3.) Each card has a hole three-eighths of an inch in diameter, punched in the middle near to the bottom edge, and the cards are kept in trays, which they loosely fit, like the card catalogues used in many libraries. Each tray holds easily 500 cards, which are secured by a long stout wire passing like a skewer through the ends of the box, and the holes in the cards. The hinder end of the box being made to slope, the cards can be tilted back and easily examined; they can be inserted or removed after withdrawing the wire.

It will be recollected that the leading and therefore the most conspicuous headings in the index, refer to the fore, middle, and ring-fingers of the right hand, as entered in column A of the Specimen Register (Table IX.) The variety of these in the "*i* and *o* fore-finger" method, of which we are now speaking, cannot exceed thirty-six, there being only four varieties (*a*, *i*, *o*, *w*) in the fore-finger, and three varieties (*a*, *l*, *w*) in each of the other two; so their maximum number is $4 \times 3 \times 3 = 36$. The actual number of such index-headings in 500 cases, and the number of entries that fell under each, was found to be as follows:—

TABLE XV.

No. of entries in 500 cases, under each of the thirty-six possible index-letters for the fore, middle, and ring-fingers of the right hand by the "i-o fore-finger" method.

a a a	4	*i a a*	1	*o a a*	1	*w a a*	—
l	17	*l*	3	*l*	2	*l*	—
w	5	*w*	—	*w*	1	*w*	1
a l a	3	*i l a*	—	*o l a*	2	*w l a*	1
l	45	*l*	54	*l*	88	*l*	40
w	11	*w*	33	*w*	59	*w*	52
a w a	—	*i w a*	—	*o w a*	—	*w w a*	—
l	—	*l*	3	*l*	—	*l*	10
w	—	*w*	11	*w*	6	*w*	47

a = Arch.
i = Inward-sloped Loop on the fore-finger.
o = Outward-sloped Loop on the fore-finger.
l = Loop of either kind on the middle, or ring finger.
w = Whorl.

These 500 cases supply no entries at all to eleven of the thirty-six index-headings, less than five entries (or under 1 per cent) to ten others, and the entries are distributed very unevenly among the remaining fifteen. This table makes it easy to calculate beforehand the spaces required for an index of any specified number of prints, whether they be on the pages of a Register, or in compartments, or in drawers of movable cards.

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CHAPTER X

PERSONAL IDENTIFICATION

We shall speak in this chapter of the aid that finger prints can give to personal identification, supposing throughout that facilities exist for taking them well and cheaply, and that more or less practice in reading them has been acquired by many persons. A few introductory words will show this supposition to be reasonable. At the present moment any printer, and there are many printers in every town, who, at a small charge, would blacken a slab and take the prints effectively, being first warned to use very little ink, as described in Chapter III. The occupation of finger printing would, however, fall more naturally into the hands of photographers, who, in addition to being found everywhere, are peculiarly well suited for it, for, taken as a class, they are naturally gifted with manual dexterity and mechanical ingenuity. Having secured good impressions, they would be able to multiply them photographically, and enlarge when desired, while the ticketing and preservation of the negatives would fall into their usual business routine. As they already occupy themselves with one means of identification, a second means of obtaining the same result is allied to their present work.

Were it the custom for persons about to travel to ask for prints of their fingers when they were photographed, a familiarity with the peculiarities of finger prints, and the methods of describing and classifying them, would become common. Wherever finger prints may be wanted for purposes of attestation, and the like, the fact mentioned by Sir W. Herschel, p , as to the readiness with which his native orderlies learnt to take them with the ink of his office stamp must not be forgotten.

The remarks about to be made refer to identification generally, and are not affected by the fact that the complete process may or may not include the preliminary search of a catalogue; the two stages of search and of comparison will be treated separately towards the close of the chapter.

In civilised lands, honest citizens rarely need additional means of identification to their signatures, their photographs, and to personal introductions. The cases in which other evidence is wanted are chiefly connected with violent death through accident, murder, or suicide, which yield a constant and gruesome supply to the Morgue of Paris, and to the corresponding institutions in other large towns, where the bodies of unknown persons are exposed for identification often in vain. But when honest persons travel to distant countries where they have few or no friends, the need for a means of recognition is more frequently felt. The risk of death through accident or crime is increased, and the probability of subsequent identification diminished. There is a possibility not too remote to be disregarded, especially in times of war, of a harmless person being arrested by mistake for another man, and being in sore straits to give satisfactory proof of the error. A signature may be distrusted as a forgery. There is also some small chance, when he returns to his own country after a long absence, of finding difficulty in proving who he is. But in civilised lands and in peaceable times, the chief use of a sure means of identification is to benefit society by detecting rogues, rather than to establish the identity of men who are honest. Is this criminal an old offender? Is this new recruit a deserter? Is this professed pensioner personating a man who is dead. Is this upstart claimant to property the true heir, who was believed to have died in foreign lands?

In India and in many of our Colonies the absence of satisfactory means for identifying persons of other races is seriously felt. The natives are mostly unable to sign; their features are not readily distinguished by Europeans; and in too many cases they are characterised by a strange amount of litigiousness, wiliness, and unveracity. The experience of Sir W. Herschel, and the way in which he met these unfavourable conditions by the method of finger prints, has been briefly described in p. L. Lately Major Ferris, of the Indian Staff Corps, happening to visit my laboratory during my absence, and knowing but little of what Sir W. Herschel had done, was greatly impressed by the possibilities of finger prints. After acquainting himself with the process, we discussed the subject together, and he very kindly gave me his views for insertion here. They are as follow, with a few trifling changes of words :—

"During a period of twenty-three years, eighteen of which have been passed in the Political Department of the Bombay Government, the great need of an official system of identification has been constantly forced on my mind.

"The uniformity in the colour of hair, eyes, and complexion of the Indian races renders identification far from easy, and the difficulty of recording the description of an individual, so that he may be afterwards recognised, is very great. Again, their hand-writing, whether it be in Persian or Devanagri letters, is devoid of character and gives but little help towards identification.

"The tenacity with which a native of India cleaves to his ancestral land, his innate desire to acquire more and more, and the obligation that accrues to him at birth of safeguarding that which has already been acquired, amounts to a religion, and passes the comprehension of the ordinary Western mind. This passion, or religion, coupled with a natural taste for litigation, brings annually into the Civil Courts an enormous number of suits affecting land. In a native State at one time under my political charge, the percentage of suits for the possession of land in which the title was disputed amounted to no less than 92, while in 83 per cent of these the writing by which the transfer of title purported to have been made, was repudiated by the former title-holder as fraudulent and not executed by him. When it is remembered that an enormous majority of the landholders whose titles come into court are absolutely illiterate, and that their execution of the documents is attested by a mark made by a third party, frequently, though not always apparently interested in the transfer, it will be seen that there is a wide door open to fraud, whether by false repudiation or by criminal attempt at dispossession.

"It has frequently happened in my experience that a transfer of title or possession was repudiated; the person purporting to have executed the transfer asserting that he had no knowledge of it, and never authorised any one to write, sign, or present it for registration. This was met by a categorical statement on the part of the beneficiary and of the attesting witnesses, concerning the time, date, and circumstances of the execution and registration, that demolished the simple denial of the man whom it was sought to dispossess. Without going into the ethics of falsehood among Western and Eastern peoples, it would be impossible to explain how, what is repugnant to the one as downright lying, is very frequently considered as no more than venial prevarication by the other. This however is too large a subject for present purposes, but the fact remains that perjury is perpetrated in Indian Courts to an extent unknown in the United Kingdom.

"The interests of landholders are partially safe-guarded by the Act that requires all documents effecting the transfer of immovable property to be registered, but it could be explained, though not in the short space of this letter, how the provisions of the Act can be, and frequently are, fulfilled in the absence of the principal person, the executor.

"Enough has been said to show that if some simple but efficient means could be contrived to identify the person who has executed a bond, cases of fraud such as these would practically disappear from the judicial registers. Were the legislature to amend the Registration Act and require that the original document as well as the copy in the Registration Book should bear the imprint of one or more fingers of the parties to the deed, I have little hesitation in saying that not only would fraud be detected, but that in a short time the facility of that detection would act as a deterrent for the future. [This was precisely the experience of Sir W. Herschel.—F.G.] In the majority of cases, the mere question would be, is the man A the same person as B, or is he not? and of that question the finger marks would give unerring proof. For example, to take the simplest case, A is sued for possession of some land, the title of which he is stated to have parted with to another for a consideration. The document and the Registration book both bear the imprint of the index finger of the right hand of A. A repudiates, and a comparison shows that whereas the finger pattern of A is a whorl, the imprint on the document is a loop; consequently A did not execute it.

In the identification of Government pensioners the finger print method would be very valuable. At one period, I had the payment of many hundreds of military pensioners. Personation was most difficult to detect in persons coming from a distance, who had no local acquaintances, and more especially where the claimants were women. The marks of identification noted in the pension roll were usually variations of:—"Hair black—Eyes brown—Complexion wheat colour—Marks of tattooing on fore-arm"—terms which are equally appropriate to a large number of the pensioners. The description was supplemented in some instances, where the pensioner had some distinguishing mark or scar, but such cases are considerably rarer than might be supposed, and in women the marks are not infrequently in such a position as to practically preclude comparison. Here also the imprint of one or more finger prints on the pension certificate, would be sufficient to elucidate any doubt as to identity.

"As a large number of persons pass through the Indian gaols not only while undergoing terms of imprisonment, but in default of payment of a fine, it could not but prove of value were the finger prints of one and all secured. They might assist in identifying persons who have formerly been convicted, of whom the local police have no knowledge, and who bear a name that may be the common property of half a hundred in any small town."

Whatever difficulty may be felt in the identification of Hindoos, it is experienced in at least an equal degree in that of the Chinese residents in our Colonies and Settlements, who to European eyes are still more alike than the Hindoos, and in whose names there is still less variety. I have already referred, p. , to Mr. Tabor, of San Francisco, and his proposal in respect to the registration of the Chinese. Remarks showing the need of some satisfactory method of identifying them, have reached me from various sources. The *British North Borneo Herald*, August 1, 1888, that lies before me as I write, alludes to the difficulty of identifying coolies, either by photographs or measurements, as likely to become important in the early future of that country.

For purposes of registration, the method of printing to be employed, must be one that gives little trouble on the one hand, and yields the maximum of efficiency for that amount of trouble on the other. Sir W. Herschel impressed simultaneously the fore and middle fingers of the right hand. To impress simultaneously the fore, middle, and ring-fingers of the right hand ought however to be better, the trouble being no greater, while three prints are obviously more effective than two, especially for an off-hand comparison. Moreover, the patterns on the ring-finger are much more variable than those on the middle finger. Much as rolled impressions are to be preferred for minute and exhaustive comparisons, they would probably be inconvenient for purposes of registration or attestation. Each finger has to be rolled separately, and each separate rolling takes more time than a dab of all the fingers of one hand simultaneously. Now a dabbed impression of even two fingers is more useful for registration purposes than the rolled impression of one; much more is a dabbed impression of three, especially when the third is the variable ring-finger. Again, in a simultaneous impression, there is no doubt as to the sequence of the finger prints being correct, but there may be some occasional bungling when the fingers are printed separately.

For most criminal investigations, and for some other purposes also, the question is not the simple one just considered, namely, "is A the same person, or a different person to B?", but the much more difficult problem of "who is the unknown person X?; is his name contained in such and such a register?" We will now consider how this question may be answered.

Registers of criminals are kept in all civilised countries, but in France they are indexed according to the method of M. Alphonse Bertillon, that admits of an effective search being made through a large collection. We shall see how greatly the differentiating power of the French or any other system of indexing might be increased, by including finger prints in the register.

M. Bertillon has described his system in three pamphlets:—

(1) *Une application pratique de l'anthropometrie*, Extrait des Annales de Démographie Interne. Paris 1881. (2) *Les signalements anthropometriques*, Conference faite au Congrès Penitentiare International de Rome, Nov. 22, 1885. (3) *Sur le fonctionnement du service des signalements*, all published by Masson, 120 Boulevard, St. Germain, Paris. To these must be added a very interesting but anonymous pamphlet, based on official documents, and which I have reason to know is authorised by M. Bertillon, namely (4) *L'anthropometrie Judiciare en Paris, en* 1889: G. Stenheil, 2 Rue Casimir-Delavigne, Paris.

Besides these a substantial volume is forthcoming, which may give a satisfactory solution to some present uncertainties.

The scale on which the service is carried on, is very large. It was begun in 1883, and by the end of 1887, no less than 60,000 sets of measures were in hand, but thus far only about one half of the persons arrested in Paris were measured, owing to the insufficiency of the staff. Arrangements were then made for its further extension. There are from 100 to 150 prisoners sentenced each day by the Courts of Law in Paris, to more than a few days' imprisonment, and every one of these is sent to the Dépôt for twenty-four hours. While there, they are now submitted to *bertillionage*, a newly coined word that has already come into use. This is done in the forenoon, by three operators and three clerks; six officials in all. About half of the prisoners are old offenders, of whom a considerable proportion give their names correctly, as is rapidly verified by an alphabetically arranged catalogue of cards, each of which contains photographs in front and profile, and measurements. The remainder are examined strictly, all their bodily marks are recorded according to a terse system of a few letters, and they are variously measured. All this occupies seven or eight minutes. They are then photographed. From sixty to seventy-five prisoners go through this complete process every forenoon. In the afternoon the officials are engaged in making numerous copies of each set of records, one of which is sent to Lyon, and another to Marseille, where there are similar establishments. They also classify the copies of records that are received from those towns and elsewhere in France, of which from seventy to one hundred arrive daily. Lastly, they search the Registers for duplicate sets of measures of those, whether in Paris or in the provinces, who were suspected of having given false names. The entire staff consists of ten officials. It is difficult to rightly interpret the figures given in the pamphlet (4) at pp. 22-24, as they appear to disagree, but as I understand them, 562 prisoners who gave false names in the year 1890 were recognised by *bertillionage*, and only four other persons were otherwise discovered to have been convicted previously, who had escaped recognition by its means.

I had the pleasure of seeing the system in operation in Paris a few years ago, and was greatly impressed by the deftness of the measuring, and with the swiftness and success with which the assistants searched for the cards containing entries similar to the measures of the prisoner then under examination.

It is stated in the *Signalements* (p. 12), that the basis of the classification are the four measurements (1) Head-length, (2) Head-breadth, (3) Middle-finger-length, (4) Foot-length, their constancy during adult life nearly always [as stated] holding good. Each of these four elements severally is considered as belonging to one or other of three equally numerous classes—small, medium, and large; consequently there are 3^4 or 81 principal headings, under some one of which the card of each prisoner is in the first instance sorted. Each of these primary headings is successively subdivided, on the same general principle of a three-fold classification, according to other measures that are more or less subject to question, namely, the height, the span, the cubit, the length and breadth of the ear, and the height of the bust. The eye colour alone is subjected to seven divisions. The general result is (pp. 19, 22) that a total of twelve measures are employed, of which eleven are classed on the three-fold principle, and one on the seven-fold, giving a final result of $3^{11} \times 7$, or more than a million possible combinations. M. Bertillon considers it by no means necessary to stop here, but in his chapter (p. 22) on the "Infinite Extension of the Classification," claims that the method may be indefinitely extended.

The success of the system is considered by many experts to be fully proved, notwithstanding many apparent objections, one of which is the difficulty due to transitional cases: a belief in its success has certainly obtained a firm hold upon the popular imagination in France. Its general acceptance elsewhere seems to have been delayed in part by a theoretical error in the published calculations of its efficiency, the measures of the limbs which are undoubtedly correlated being treated as independent, and in part by the absence of a sufficiently detailed account of the practical difficulties experienced in its employment. Thus in the *Application pratique*, p. 9, "We are embarrassed what to choose, the number of human measures varying independently of each other, being considerable." In the *Signale-*

ments, p. 19, "It has been shown" (by assuming this independent variability), "that by seven measurements, 60,000 photographs can be separated into batches of less than ten in each." (By the way, even on that assumption, the result is somewhat exaggerated, the figures having been arrived at by successively taking the higher of the two nearest round values.) In short, the general tone of these two memoirs is one of enthusiastic belief in the method based, so far as is there shown, almost wholly on questionable *theoretic* grounds of efficiency.

To learn how far correlation interferes with the regularity of distribution, causing more entries to be made under some index-heads than others, as was the case with finger prints, I have classified on the Bertillon system, 500 sets of measures taken at my laboratory. It was not practicable to take more than three of the four primary measures, namely, the head-length, its breadth, and the middle-finger-length. The other measure, that of foot-length, is not made at my laboratory, as it would require the shoes to be taken off, which is inadmissible since persons of all ranks and both sexes are measured there; but this matters little for the purpose immediately in view. It should however be noted that the head-length and head-breadth have especial importance, being only slightly correlated, either together or with any other dimension of the body. Many a small man has a head that is large in one or both directions, while a small man rarely has a large foot, finger, or cubit, and conversely with respect to large men.

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The following set of five measures of each of the 500 persons were then tabulated; (1) head-length; (2) head-breadth; (3) span; (4) bust, that is the height of the top of the head from the seat on which the person sits; (5) middle-finger-length. The measurements were to the nearest tenth of an inch, but in cases of doubt, half-tenths were recorded in (1), (2) and (5). With this moderate minuteness of measurement, it was impossible so to divide the measures as to give better results than the following, which show that the numbers in the three classes are not as equal as desirable. But they will enable us shortly to arrive at an approximate idea of the irregular character of the distribution.

TABLE XVI.

Dimensions measured.	Medium measures in inches and tenths.	Nos. in the three classes respectively.			
		– below.	0 medium.	+ above.	Total.
1. Head-length	7·5 to 7·7	101	191	208	500
2. Head-breadth	6·0 „ 6·1	173	201	126	500
3. Span . . .	68·0 „ 70·5	137	165	198	500
4. Bust . . .	35·0 „ 36·0	139	168	193	500
5. Middle-finger	4·5 „ 4·6	180	176	144	500

The distribution of the measures is shown in Table XVII.

[TABLE XVII.

TABLE XVII.

Distribution of 500 sets of measures into classes. Each set consists of five éléments; each element is classed as + or above mediocrity; M, or mediocre; –, or below mediocrity.

(Total number of classes is $3^5 = 243$.)

3 Span.	4 Body-height.	5 Middle-finger.	1 Head-length, 2 Head-breadth.								
			1 2 – –	1 2 – M	1 2 – +	1 2 M –	1 2 M M	1 2 M +	1 2 + –	1 2 + M	1 2 + +
–	–	–	14	7	4	14	11	5	3	3	2
		M	–	2	–	2	4	1	–	2	4
		+	–	–	–	1	–	–	–	–	–
–	M	–	5	2	2	7	4	2	1	4	3
		M	–	2	–	3	1	3	2	3	–
		+	–	–	–	–	–	–	–	–	2
–	+	–	2	–	–	1	1	1	–	–	1
		M	–	2	–	–	–	–	–	1	1
		+	–	–	–	1	–	–	–	1	–
M	–	–	4	–	1	3	4	3	1	2	2
		M	3	2	–	3	2	3	2	4	–
		+	–	–	–	–	1	2	–	1	–
M	M	–	1	3	1	4	3	2	4	4	3
		M	5	3	–	7	5	2	2	6	5
		+	2	1	1	1	1	–	1	4	2
M	+	–	2	1	1	5	2	–	–	2	2
		M	2	2	–	3	3	1	1	6	7
		+	–	–	1	2	–	–	3	2	2
+	–	–	–	–	1	–	1	–	–	–	–
		M	1	–	–	1	2	–	1	3	–
		+	1	2	–	1	1	–	–	–	2
+	M	–	1	–	1	3	2	–	–	–	2
		M	2	–	1	1	4	–	3	2	4
		+	2	1	–	2	4	1	4	6	3
+	+	–	1	2	–	1	–	1	1	2	2
		M	–	1	–	5	10	3	3	8	9
		+	2	2	2	11	10	3	9	24	19

The frequency with which 1, 2, 3, 4, etc., sets were found to fall under the same index-heading, is shown in Table XVIII.

TABLE XVIII.

No. of sets under same index-heading.	Frequency of its occurrence.	No. of entries.
0	83	0
1	47	47
2	47	94
3	25	75
4	16	64
5	7	35
6	3	18
7	4	28
8	1	8
9	2	18
10	2	20
11	2	22
14	2	28
19	1	19
24	1	24
Total entries . . .		500

No example was found of 83, say of one-third, of the 243 possible combinations. In one case no less than 24 sets fell under the same head; in another case 19 did so, and there were two cases in which 14, 11 and 10 did the same. Thus, out of 500 sets (see the five bottom lines in the last column of the above table) no less than 113 sets fell into four classes, each of which included from 10 to 24 entries.

The 24 sets whose Index-number is +M, + + + admit of being easily subdivided and rapidly sorted by an expert, into smaller groups, paying regard to considerable differences only, in the head-length and head-breadth. After doing this, two comparatively large groups remain, with five cases in each, which require further analysis. They are as follow, the height and eye colour being added in each case, and brackets being so placed as to indicate measures that do not differ to a sufficient amount to be surely distinguished. No two sets are alike throughout, some difference of considerable magnitude always occurring to distinguish them. Nos. 2 and 3 come closest together, and are distinguished by eye colour alone.

TABLE XIX.

Five cases of Head-length 8·0, and Head-breadth 6·1.

	Span.	Body.	Finger.	Height.	Eye-colour.
1.	72·4	38·0	4·8	71·2	br. grey
2.	72·6	37·0	4·7	71·4	br. grey
3.	72·7	36·7	4·7	71·4	blue
4.	73·9	36·4	5·0	70·7	brown
5.	75·3	37·9	4·8	73·4	blue

Five cases of Head-length 7·8, and Head-breadth 6·0.

	Span.	Body.	Finger.	Height.	Eye-colour.
6.	70·8	37·8	4·7	70·0	brown
7.	71·9	36·2	4·7	69·3	blue
8.	72·4	37·2	4·7	68·4	brown
9.	74·8	37·8	5·0	73·1	blue
10.	79·9	37·3	5·3	75·6	blue grey

This is satisfactory. It shows that each one of the 500 sets may be distinguished from all the others by means of only seven elements; for if it is possible so to sub-divide twenty-four entries that come under one index-heading, we may assume that we could do so in the other cases where the entries were fewer. The other measures that I possess—strength of grasp

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Ven. Archdeacon Lewis
WBF.

Sydney Bottacki
Hon Bay
Miss Black.
Miss Robertson
Mrs Goldsmid
Mrs Lewis & Arabella.
Miss Arleen Rattigan
Miss Rattigan

—

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and breathing capacity—are closely correlated with stature and bulk, while eye-sight and reaction-time are uncorrelated, but the latter are hardly suited to test the further application of the Bertillon method.

It would appear, from these and other data, that a purely anthropometric classification, irrespective of bodily marks and photographs, would enable an expert to deal with registers of considerable size.

Bearing in mind that mediocrities differ less from one another, than members of either of the extreme classes, and would therefore be more difficult to distinguish, it seems probable that with comparatively few exceptions, *at least* two thousand adults of the same sex might be individualised, merely by means of twelve careful measures, on the Bertillon system, making reasonable allowances for that small change of proportions that occurs after the lapse of a few years, and for inaccuracies of measurement. This estimate may be far below the truth, but more cannot, I think, be safely inferred from the above very limited experiment.

The system of registration adopted in the American army for tracing suspected deserters, was described in a memoir contributed to the "International Congress of Demography," held in London in 1891. The memoir has so far been only published in the *Abstracts of Papers*, p. 233 (Eyre and Spottiswoode). Its phraseology is unfortunately so curt as sometimes to be difficult to understand; it runs as follows:—

Personal identity as determined by scars and other body marks by Colonel Charles R. Greenleaf and Major Charles Smart, Medical Department, U.S. Army.

Desertions from United States army, believed to greatly exceed deserters, owing to repeaters.

Detection of repeaters possible if all body marks of all recruits recorded, all deserters noted, and all recruits compared with previous deserters.

In like manner men discharged for cause excluded from re-entry.

Bertillon's anthropometric method insufficient before courts-martial, because possible inaccuracies in measurement, and because of allowable errors.

But identity acknowledged following coincident indelible marks, when height, age, and hair fairly correspond.

That is, Bertillon's collateral evidence is practically primary evidence for such purposes.

There is used for each man an outline figure card giving anterior and posterior surfaces, divided by dotted lines into regions.

These, showing each permanent mark, are filed alphabetically at the Surgeon-General's office, War Department.

As a man goes out for cause, or deserts, his card is placed in a separate file.

The cards of recruits are compared with the last-mentioned file.

To make this comparison, a register in two volumes is opened, one for light-eyed and one for dark-eyed men. Each is subdivided into a fair number of pages, according to height of entrants, and each page is ruled in columns for body regions. Tattooed and non-tattooed men of similar height and eyes are entered on opposite pages. Recruits without tattoos are not compared with deserters with tattoos; but recruits with tattoos are compared with both classes.

On the register S T B M etc., are used as abbreviations for scar, tattoo, birth-mark, mole etc.

One inch each side of recorded height allowed for variation or defective measurement.

When probability of identity appears, the original card is used for comparison.

Owing to obstacles in inaugurating new system, its practical working began with 1891 [?], and, to include May 1891, out of sixty-two cases of suspected fraud sixty-one proved real. [= 5 months. FG]

There was some interesting discussion, both upon this memoir and on a verbal communication concerning the French method, that had been made by M. Jaques Bertillon the statistician, who is a brother of its originator. It appeared that there was room for doubt whether the anthropometric method had received a fair trial in America, the measurements being made by persons not specially trained, whereas in France the establishments, though small, are thoroughly efficient.

There are almost always moles or birth-marks, serving for identification, on the body of every one, and a record of these is, as already noted, an important though subsidiary part of the Bertillon system. Body-marks are noted in the English registers of criminals, and it is curious how large a proportion of these men are tattooed and scarred. How far the body-marks admit of being usefully charted on the American plan, it is difficult to say, the success of the method being largely dependent on the care with which they are recorded. The number of persons hitherto dealt with on the American plan appears not to be very large. As observations of this class require the person to be undressed, they are unsuitable for popular purposes of identification, but the marks have the merit of being independent of growth, which the measurements of the limbs have not.

It seems strange that no register of this kind, so far as I know, takes account of the teeth. If a man on being first registered, is deficient in certain teeth, they are sure to be absent when he is examined on a future occasion. He may, and probably will in the meantime, have lost others, but the fact of his being without specified teeth on the first occasion, excludes the possibility of his being afterwards mistaken for a man who still possesses them.

We will now separately summarise the results arrived at, in respect to the two processes that may both be needed in order to effect an identification.

First, as regards *search in an Index.*—Including the bodily marks and photographs, let us rate the Bertillon method as able to cope with a register of 20,000 adults of the same sex, with a small and definable, but as yet unknown, average dose of difficulty, which we will call x. Some sets of measures will give trouble, but the greater proportion can apparently be catalogued with so much certainty, that if a second set of measures of any individual be afterwards taken, no tedious search will be needed to hunt out the former set.

A catalogue of 500 sets of finger prints easily fulfils the same conditions. I could lay a fair claim to much more, but am content with this. Now the finger patterns have been shown to be so independent of other conditions that they cannot be notably, if at all, correlated with the bodily measurements or with

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any other feature, not the slightest trace of any relation between them having yet been found, as will be shown at p. , and more fully in Chapter XII. For instance, it would be totally impossible to fail to distinguish between the finger prints of twins, who in other respects appeared exactly alike. Finger prints may therefore be treated without the fear of any sensible error, as varying quite independently of the measures and records in the Bertillon system. Their inclusion would consequently increase its power fully five-hundred fold. Suppose one moderate dose of difficulty is enough for dealing with the measurements, etc. of 20,000 adult persons of the same sex by the Bertillon method, and a similar dose of difficulty with the finger prints of 500 persons, then two such doses could deal with a register of 20,000 × 500, or 10,000,000.

We now proceed to consider the second and final process, namely that of identification by *Comparison*. When the data concerning a suspected person are discovered to bear a general likeness to one of those already on the register, and a minute comparison shows their finger prints to agree in all or nearly all particulars, the evidence thereby afforded that they were made by the same person far transcends in trustworthiness any other evidence that can ordinarily be obtained, and vastly exceeds what can be derived from any number of ordinary anthropometric data. *By itself it is amply sufficient to convict.* Bertillionage can rarely supply more than grounds for very strong suspicion, the method of finger prints affords certainty. It is easy however to understand, that while the peculiarities of finger prints are not generally understood, a juryman would be cautious in accepting their evidence, but it is to be hoped that attention will now gradually become drawn to their marvellous virtues, and that after their value shall have been established in a few conspicuous cases, it will be popularly recognised.

So long as/ Correl

Let us not forget two great and peculiar merits of finger prints; they are self-signatures, free from all possibility of faults in observation or of clerical error; and they apply throughout life.

An abstract of the remarks made by M. Herbette, Director of the Penitentiary Department of the Ministère de l'Interieur, France, at the International Penitentiary Congress at Rome, after the communication by M. Alphonse Bertillon had been read, may fitly conclude this chapter.

"Proceeding to a more extended view of the subject and praising the successful efforts of M. Bertillon, M. Herbette pointed out how a verification of the physical personality, and of the identity of people of adult age, would fulfil requirements of modern society in an indisputable manner under very varied conditions.

"If it were a question for instance, of giving to the inhabitants of a country, to the soldiers of an army, or to travellers proceeding to distant lands, notices or personal cards as recognisable signs, enabling them always to prove who they are; if it were a question of completing the obligatory records of civil life by perfectly sure indications, such as would prevent all error, or substitution of persons; if it were a question of recording the distinctive marks of an individual in documents, titles or contracts, where his identity requires to be established for his own interest, for that of third parties, or for that of the State,—

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there the anthropometric system of identification would find place.

"Should it be a question of a life certificate, of a life assurance, or of a proof of death, or should it be required to certify the identity of a person who was insane, severely wounded, or of a dead body that had been partly destroyed, or so disfigured as to be hardly recognisable, from a sudden or violent death due to crime, accident, shipwreck, or battle—how great would be the advantage of being able to trace these characters, unchangeable as they are in each individual, infinitely variable as between one individual and another, indelible at least in part, even in death.

"There is still more cause to be interested in this subject when it is a question of identifying persons who are living at a great distance, and after the lapse of a considerable time, when the physiognomy, the features, and the physical habits may have changed from natural or artificial causes, and to be able to identify them without taking a journey and without cost, by the simple exchange of a few lines or figures that may be sent from one country or continent to another, so as to give information in America as to who any particular man is, who has just arrived from France, and to certify whether a certain traveller found in Rome is the same person who was measured in Stockholm ten years before.

"In one word to fix the human personality, to give to each human being an identity, an individuality that can be depended upon with certainty, lasting, unchangeable, always recognisable and easily adduced, this appears to be in the largest sense the aim of the new method.

"Consequently, it may be said that the extent of the problem as well as the importance of its solution, far exceeds the limits of penitentiary work and the interest, which is however by no means inconsiderable, that penal action has excited amongst various nations. These are the motives for giving to the labours of M. Bertillon and to their practical utilisation the publicity they merit.

These full and clear remarks seem even more applicable to the method of finger prints, than to that of anthropometry.

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CHAPTER XI

HEREDITY

SOME of those who have written on finger marks affirm that they are transmissible by descent, others assert the direct contrary, but no inquiry hitherto appears to justify a definite conclusion.

Chapter VIII. shows a close correlation to exist between the patterns on the several fingers of the same person. Hence we are justified in assuming that the patterns are partly dependent on constitutional causes, in which case it would indeed be strange if the general law of heredity failed in this particular case.

After examining many prints, the frequency with which some peculiar pattern was found to characterise members of the same family convinced me of the reality of an hereditary tendency. The question was how to submit the belief to numerical tests; particular kinships had to be selected, and methods of discussion devised.

It must here be borne in mind that "Heredity" implies more than its original meaning of a relationship between parent and child. It includes that which connects children of the same parents, and which I have shown (*Natural Inheritance*) to be just twice as close in the case of stature as that which connects a child and either of its two parents. Moreover, the closeness of the fraternal and the filial relations are to a great extent inter-dependent, for in any population whose faculties remain *statistically* the same during successive generations it has been shown that a simple algebraical equation must exist, that connects together the three elements of Filial Relation, Fraternal Relation, and Regression, by which a knowledge of any two of them determines the value of the third. So far as Regression may be treated as being constant in value, the Filial and the Fraternal relations become reciprocally connected. It is not possible briefly to give an adequate explanation of all this now, or to show how strictly observations were found to confirm the theory; this has been fully done in *Natural Inheritance*, and the conclusions will here be assumed.

The fraternal relation, besides disclosing more readily than other kinships the existence or non-existence of heredity, is at the same time more convenient, because it is easier to obtain examples of brothers and sisters alone, than with the addition of their father and mother. The resemblance between those who are twins is also an especially significant branch of the fraternal relationship. The word "fraternities" will be used to include the children of both sexes who are born of the same parents; it being impossible to name the familiar kinship in question either in English, French, Latin, or Greek, without circumlocution or using an incorrect word, thus affording a striking example of the way in which thought outruns language, and its expression is hampered by the inadequacy of language. In this dilemma I prefer to fall upon the second horn, that of incorrectness of phraseology, subject to the foregoing explanation and definition.

The first preliminary experiments were made with the help of the Arch-Loop-Whorl classification, on the same principle as that already described and utilised in Chapter VIII., with the following addition. Each of the two members of any couplet of fingers has a distinctive name—for instance, the couplet may consist of a finger and a thumb; or again, if it should consist of two fore-fingers, one will be a right fore-finger and the other a left one, but the two brothers in a couplet of brothers rank equally as such. The plan was therefore adopted of "ear-marking" the prints of the first of the two brothers that happened to come to hand with an A, and that of the second brother with a B; and so reducing the questions to the shape:—How often does the pattern on the finger of a B brother agree with that on the corresponding finger of an A brother? How often would it occur between two persons who had no family likeness? How often would it correspond if the kinship between A and B were as close as it is possible to conceive? Or transposing the questions, and using the same words as in Chapter VIII., what is the relative frequency of (1) Random occurrences, (2) Observed

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occurrences, (3) Utmost possibilities? It was shown in that chapter how to find the value of (2) upon a centesimal scale in which "Randoms" ranked as 0° and "Utmost possibilities" as 100°.

The method there used of calculating the frequency of the "Random" events will be accepted without hesitation by all who are acquainted with the theory and the practice of problems of probability. Still, it is as well to occasionally submit calculation to test. The following example was sent to me for that purpose by a friend who, not being mathematically minded, had demurred somewhat to the possibility of utilising the calculated "Randoms."

The prints of 101 (by mistake for 100) couplets of prints of the right fore-fingers of school children were taken by him from a large collection, the two members, A and B, being picked out at random and formed into a couplet. It was found that among the A children there were 20 arches, 50 loops, and 29 whorls, and among the B children 25, 34, and 42 respectively, as is shown by the *italic* numerals in the last column, and again in the bottom row of Table XX. The remainder of the table shows the number of times in which an Arch, Loop, or Whorl of an A child was associated with an Arch, Loop, or Whorl of a B child.

[Table XX.

TABLE XX.

Observed Random Couplets.

B children.	A children.			Totals in B children.
	Arches.	Loops.	Whorls.	
Arches . .	5	12	8	*25*
Loops . . .	8	18	8	*34*
Whorls . .	9	20	13	*42*
Totals in A children .	*20*	*50*	*29*	101

TABLE XXI.

Calculated Random Couplets.

B children.	A children.			Totals in B children.
	Arches.	Loops.	Whorls.	
Arches . .	5·00	12·50	7·25	*25*
Loops . . .	6·80	17·00	9·86	*34*
Whorls . .	8·40	21·00	12·18	*42*
Totals in A children .	*20*	*50*	*29*	101

The question, then, was how far calculations from the above data would correspond with the contents of Table XX. The answer is that it does so admirably. Multiply each of the italicised A totals into each of the italicised B totals, and after dividing each result by 101, enter it in the square at which the column that has the A total at its base, is intersected by the row that has the B total at its side. We thus obtain Table XXI.

We will now discuss in order the following relationships: the Fraternal, first in the ordinary sense, and then in the special case of twins of the same set; Filial, in the special case in which both parents have the same particular pattern on the same finger; lastly, the relative influence of the father and mother in transmitting their patterns.

Fraternal relationship.—In 105 fraternities the *observed* figures were as in Table XXII. :—

TABLE XXII.

Observed Fraternal Couplets.

B children.	A children.			Totals in B children.
	Arches.	Loops.	Whorls.	
Arches . . .	5	12	2	*19*
Loops . . .	4	42	15	*61*
Whorls . . .	1	14	10	*25*
Totals in A children .	*10*	*68*	*27*	105

The squares that run diagonally from the top at the left, to the bottom at the right, contain the double events, and it is with these that we are now concerned. Are the entries in those squares larger or not than the randoms, calculated as above, viz.:—the values of 10 × 19, 68 × 61, 27 × 25, all divided by 105? The calculated Randoms are shown in the first line of Table XXIII., the third line gives the greatest feasible number of correspondences which would occur if the kinship was as close as possible, subject to the reservation explained in p. . As there explained, the *lower* of the A and B values is taken in each case, for Arches, Loops, and Whorls respectively.

TABLE XXIII.

	A and B both being		
	Arches.	Loops.	Whorls.
Random	1·7	37·6	6·2
Observed	5·0	42·0	10·0
Utmost feasible	10·0	61·0	25·0

In every instance, the Observed values are seen to exceed the Random.

Many other cases of this description were calculated, all yielding the same general result, but these results are not as satisfactory as can be wished, owing to their dilution by inappropriate cases, the A.L.W. system being somewhat artificial.

With the view of obtaining a more satisfactory result the patterns were subdivided under fifty-three heads, and an experiment was made with the fore, middle, and ring-fingers of 150 fraternal couplets (300 individuals and 900 digits) by Mr. F. Howard Collins, who kindly undertook the considerable labour of indexing and tabulating them.

The provisional list of standard patterns published in the *Phil. Trans.* was not appropriate for this purpose. It related chiefly to thumbs, and consequently omitted the tented arch; it referred to the left hand, but in the following tabulations the right hand has been used; and its numbering is rather inconvenient. The present set of fifty-three patterns has faults, and I should be sorry to offer it as final, but it was convenient for our purposes and may be convenient to others; as Mr. Collins worked wholly by it, it may be distinguished as the "C. set." The banded patterns, 24-31, are almost non-existent in the fingers, but being common in the thumb were retained, on the chance of our requiring the introduction of thumb patterns into the tabulations. The numerals refer to the patterns as seen in impressions of the *right hand* only. [They would equally be true for the patterns as seen on the *fingers themselves* of the left hand.] For impressions of the left hand the numerals up to 7 inclusive would be the same, but those of all the rest would be changed. These are arranged in couplets, the one member of the couplet being a reversed picture of the other, and those in each couplet are distinguished by severally bearing an odd and an even number. Therefore, in impressions of the left hand, 8 would have to be changed into 9, and 9 into 8; 10 into 11, and 11 into 10; and so on, up to the end, viz., 52 and 53. The numeral 54 was used to express nondescript patterns.

The finger prints had to be gone through repeatedly, some weeks elapsing between the inspections, and under conditions which excluded the possibility of unconscious bias; a subject of frequent communication between Mr. Collins and myself. Living at a distance apart, it was not easy at the time they were made, to bring our respective interpretations of transitional and some other of the patterns, especially the invaded loops, into strict accordance, so I prefer to keep his work, in which I have perfect confidence, independent from my own. Whenever a fraternity consisted of more than two members, they were divided, according to a prearranged system, into as many couplets as there were individuals. Thus, while a fraternity of three individuals furnished all of its three possible varieties of couplets, (1, 2), (1, 3), (2, 3), one of four individuals was not allowed to furnish more than four of its possible couplets, the two italicised ones being omitted, (1, 2), (1, 3), *(1, 4)*, *(2, 3)*, (2, 4), (3, 4), and so on. Without this precaution, a single very large family might exercise a disproportionate and even overwhelming statistical influence.

It would be essential to exact working, that the mutual relations of the patterns should be taken into account; for example, suppose an arch to be found on the fore-finger of one brother and a nascent loop on that of the other; then, as these patterns are evidently related, their concurrence ought to be interpreted as showing some degree of resemblance. However, it is impossible now to take cognizance of partial resemblances, the mutual relations of the patterns not having as yet been determined with adequate accuracy.

The completed tabulations occupied three large sheets, one for each of the fingers, ruled crossways into fifty-three vertical columns for the A brothers, and fifty-three horizontal rows for the B brothers. Thus, if the register number of the pattern of A was 10, and that of B was 42; then a mark would be put in the square limited by the ninth and tenth horizontal lines, and by the forty-first and forty-second vertical ones. The marks were scattered sparsely over the sheet. Those in each square were then added up, and finally the numbers in each of the ~~bands~~ rows and in each of the columns were severally totalled.

If the number of couplets had been much greater than they are, a test of the accuracy with which their patterns had been classed under their appropriate heads, would be found in the frequency with which the same patterns were registered in the corresponding finger of the A and B ~~classes~~ brothers. The A and B groups are strictly homogeneous, consequently the frequency of their patterns in corresponding fingers ought to be alike. The success with which this test has been fulfilled in the present case, is passably good, its exact degree being shown in the following paragraphs, where the numbers of entries under each head, are arranged in as orderly a manner as the case admits,

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the smaller of the two numbers being the one that stands first, whether it was an A or a B. All instances in which there were at least five entries under either A or B, are included; the rest being disregarded. The result is as follows:—

I. Thirteen cases of more or less congruity between the number of A and B entries under the same head:—5-7; 5-7; 5-8; 6-8; 7-10; 8-9; 8-12; 9-12; 10-10; 11-13; 12-16; 14-18; 72-73. (This last refers to loops on the middle finger.)

II. Six cases of more or less incongruity. 1-7; 6-12; 14-20; 14-22; 22-35; 39-50.

The three Tables, XXIV., XXV., XXVI., contain the results of the tabulations and the deductions from them.

[Table XXIV.

Table XXIV.

Comparison of three Fingers of the Right Hand in 150 *Fraternal Couplets.*

Index No. of Pattern.	Fore-fingers.			Middle fingers.			Ring-fingers.		
	Down columns.	Along lines.	Double events.	Down columns.	Along lines.	Double events.	Down columns.	Along lines.	Double events.
	A	B	A and B	A	B	A and B	A	B	A and B
1	15	12	4	8	5	2	7	5	1
2	3	2	...	3	2	...	...	...	...
6	2	2	1	...	...	...	2	4	...
7	...	2	...	2	1	...	7	5	1
8	...	...	...	...	...	...	...	1	...
9	1	7	...	4	1	1	7	1	...
12	1	...	...	2	...	...	...	...	...
13	...	...	...	2	1	...	...	...	...
14	4	3	...	4	4	1	20	14	1
15	16	12	3	4	2	...	3	4	...
16	2	3	...	2	3	...	10	7	2
17	4	3	...	3	...	...	...	...	...
18	...	...	...	4	1	...	18	14	6
19	3	3	...	2	5	...	1	...	...
20	...	...	...	...	...	...	1	3	1
21	...	1	...	...	...	...	...	...	...
22	...	4	...	1	8	...	1	2	...
23	1	...	...	1	...	...	6	...	...
27	1	...	...	...	...	...	...	...	...
32	1	...	...	1	3	...	4	4	...
33	3	1	1	1	...	...	3	3	1
34	3	2	...	4	1	...	...	...	...
35	2	3	...	...	5	...	9	12	2
38	2	1	...	...	...	...	...	...	...
39	4	...	...	3	1	...	...	...	...
40	13	11	1	14	22	6	9	8	...
41	12	8	...	1	3	...	...	1	...
42	22	35	5	73	72	35	39	50	16
43	10	10	3	4	1	...	...	3	...
44	2	1	...	...	2	...	...	2	...
45	1	1	...	...	...	...	...	...	...
46	8	6	1	3	1	...	...	1	...
47	3	4	...	...	...	...	...	...	...
48	6	12	1	4	6	...	2	3	...
49	1	1	...	...	...	...	...	...	...
52	...	...	...	...	...	...	1	...	...
53	..	...	...	...	...	...	...	1	...

Table XXV.

Comparison between Random and Observed Events.

Fore.		Middle.		Ring.	
Random.	Observed.	Random.	Observed.	Random.	Observed.
1·20	4	0·26	2	0·23	1
0·08	...	0·11	1	0·05	...
1·28	3	0·05	...	0·23	
0·08	...	0·07	...	1·87	1
0·06	...	0·05	...	0·08	...
0·95	1	2·05	6	0·46	2
0·64	...	34·08	35	1·68	6
5·18	5	0·16	...	0·11	...
0·67	3			0·06	1
0·32	1			0·72	2
0·08	...			0·48	...
0·48	1			13·00	16
All others.					
0·29	2	0·28	1	0·12	1
11·31	20	37·11	45	19·09	31

Table XXVI.

Centesimal Scale (to nearest whole numbers).

150 fraternal couplets.	Random.	Observed.	Utmost possibilities.	Reduced to lower limit = 0.			Reduced to upper limit = 100.		
							Centesimal scale.		
Fore-finger	11·31	20	115	0	9	104	0°	9°	100°
Middle	37·11	45	117	0	10	80	0°	10°	100°
Ring	19·09	31	118	0	12	99	0°	12°	100°
				Mean . .			0°	10°	100°
50 additional couplets.									
Middle finger only .	8·2	11	22	0	3	14	0°	21°	100°
Loops only, and on middle finger only.									
150 couplets . . .	34·0	35	72	0	1	72	0°	$1\frac{1}{4}$°	100°
50 couplets . . .	6·4	7	14	0	0·6	8	0°	8°	100°

Table XXIV. contains all the Observed events, and is to be read thus, beginning at the first entry. Pattern No. 1 occurs on the right fore-finger fifteen times among the A brothers, and twelve times among the B brothers; while in four of these cases both brothers have that same pattern.

Table XXV. compares the Random events with the Observed ones. Every case in which the calculated expectation is equal to or exceeds 0·05, is inserted in detail; the remaining group of petty cases are summed together and their totals entered in the bottom line. For fear of misapprehension or forgetfulness, one other example of the way in which the Randoms are calculated will be given here, taking for the purpose the first entry in Table XXIV. Thus, the number of all the different combinations of the 150 A with the 150 B individuals in the 150 couplets, is 150 × 150. Out of these, the number of double events in which pattern No. 1 would appear in the same combination, is 15 × 12 = 180. Therefore in 150 trials, the double event of pattern No. 1 would appear upon the average, on 180 divided by 150, or on 1·20 occasions. As a matter of fact, it appeared four times. These figures will be found in the first line of Table XXV.; the rest of its contents have been calculated in the same way.

Leaving aside the Randoms that exceed 0, but are less than 1, there are nineteen cases in which the Random may be compared with the Observed values; in all but two of these the Observed are the highest, and in these two the Random exceed the Observed by only trifling amounts, namely 5·18 Random against 5·00 Observed; 1·87 Random against 1·00 Observed. It is impossible, therefore, to doubt from the steady way in which the Observed values overtop the Randoms, that there is a greater average likeness in the finger marks of two brothers, than in those of two persons taken at hazard.

Table XXVI. gives the results of applying the centesimal scale to the measurement of the average closeness of fraternal resemblance, in respect to finger prints, according to the method and under the reservations already explained in page 125. The average value thus assigned to it, is a little more than 10°. The values obtained from the three fingers severally, from which that average was derived, are 9°, 10°, and 12°; they agree together better than might have been expected. The value obtained from a set of fifty additional couplets of the middle fingers only, of fraternals, is wider; it is 21°. Its inclusion with the rest raises the average of all to between 10 and 11.

In the pre-eminently frequent event of loops with an outward slope on the middle finger, it is remarkable that the Random cases are nearly equal to the Observed ones; they are 34·08 to 35·00. It was to obtain some assurance that this equality was not due to statistical accident, that the additional set of fifty couplets were tabulated. They tell however the same tale, viz. 6·4 Randoms to 7·0 Observed. The loops on the fore-fingers confirm this, showing 5·18 Randoms to 5·00 Observed; those on the ring-finger have the same peculiarity though in a slighter degree, 13 to 16: the average of other patterns show much greater differences than that. I am unable to account for this curious behaviour of the loops, which can hardly be due to statistical accident, in the face of so much concurrent evidence.

Twins.—The signs of heredity between brothers and sisters, ought to be especially apparent between twins of the same sex, who are physiologically related in a peculiar degree and are sometimes extraordinarily alike. More rarely, they are remarkably dissimilar. The instances of a moderate family resemblance between twins of the same sex are much less frequent than between ordinary brothers and sisters, or between twins of opposite sex. All this has been discussed in my *Human Faculty*. In order to test the truth of the expectation, I procured prints of the fore, middle, and ring-fingers of seventeen sets of twins, and compared them, with the results shown in Table XXVII.

[TABLE XXVII.

TABLE XXVII.

17 SETS OF TWINS (A and B).

Comparison between the patterns on the Fore, Middle, and Little fingers respectively of the Right hand.

Agreement (=), 19 cases; partial (· ·), 13 cases; disagreement (×), 19 cases.

	A B	A B	A B	A B	A B
Fore . . .	42 = 42	21 = 21	40 = 40	6 = 6	1 = 1
Middle . .	42 = 42	8 = 8	32 × 42	15 · · 32	42 = 42
Ring . . .	42 = 42	8 = 8	42 = 42	33 = 33	40 × 19
Fore . . .	42 = 42	43 × 15	1 = 1	15 × 34	2 · · 42
Middle . .	42 = 42	42 · · 40	1 × 40	42 = 42	42 = 42
Ring . . .	42 · · 46	35 = 35	40 · · 42	14 × 32	42 × 14
Fore . . .	49 · · 14	15 × 49	15 · · 16	1 × 42	1 × 15
Middle . .	42 = 42	23 × 14	19 × 42	42 · · 48	32 × 22
Ring . . .	9 · · 32	14 · · 16	6 · · 18	42 × 8	18 × 23
Fore . . .	48 × 33	(loop) × 9			
Middle . .	42 × 22	48 × 22			
Ring . . .	14 · · 6	9 · · 35			

The result is that out of the seventeen sets (=51 couplets), two sets agree in all their three couplets of fingers; four sets agree in two; five sets agree in one of the couplets. There are instances of partial agreement in five others, and a disagreement throughout in only one of the seventeen sets. In another collection of seventeen sets, made to compare with this, six agreed in two of their three couplets, and five agreed in one of them. There cannot then be the slightest doubt as to the strong tendency to resemblance in the finger patterns in twins.

This remark must by no means be forced into the sense of meaning that the similarity is so great, that the finger print of one twin might occasionally be mistaken for that of the other. When patterns fall into the same class, their general forms may be conspicuously different (see p. 74), while their smaller details, namely, the number of ridges and the minutiæ, are practically quite independent of the pattern.

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It may be mentioned that I have an inquiry in view, which has not yet been discussed, owing to the want of a sufficiency of data; namely to determine the minutest biological unit that may be hereditarily transmissible. The minutiæ in the finger prints of twins, seem suitable subjects of observation for this purpose.

Children of like-patterned Parents.—When two parents are alike, the average resemblance in stature, at all events, which their children bear to them, is as close as the fraternal resemblance between the children, and twice as close as that which the children bear to either parent separately, when the parents are unlike.

The fifty-eight parentages affording fifty couplets of the fore, middle, and ring-fingers respectively give $58 \times 3 = 174$ parental couplets in all; of these, 27 or 14 per cent are alike in their pattern, as shown by Table XXVIII. The total number of children to these twenty-seven pairs is 109, of which 59 (or 54 per cent) have the same pattern as their parents. This fact requires analysis, as on account of the great frequency of loops, and especially of the pattern No. 42 on the middle finger, a large number of the cases of similarity of pattern between child and parents would be mere random coincidences.

Table XXVIII.—*Children of like-patterned Parents.*

		F.	M.	Sons.	Alike.	Total sons.	Daughters.	Alike.	Total daughters.	Total children.	Alike.
1	Fore . . .	1	1	1	1	1	1, 1	2	2	3	3
2		34	34	34	1	1	42, 48	...	2	3	1
3		40	40	41	...	1	2, 40	1	2	3	1
4		42	42	48	...	1	42	1	1	2	1
5	Middle . .	40	40	40	1	1	40	1	1	2	2
6		42	42	42	1	1	...	...	...	1	1
7		42	42	42	1	1	40	...	1	2	1
8		42	42	42, 38, 42, 42	3	4	40, 1	...	2	6	2
9		42	42	42	1	1	40, 42	1	2	3	2
10		42	42	48, 48, 42, 14	1	4	42, 42, 48, 42, 42	4	5	9	5
11		42	42	42	1	1	1, 40	...	2	3	1
12		42	42	40	...	1	42, 42, 42, 42	4	4	5	4
13		42	42	1	...	1	...	...	...	1	...
14		42	42	42	1	1	42, 42, 42	3	3	4	4
15		42	42	42, 46, 42	2	3	42, 42, 42, 42, 42, 42, 42	7	7	10	9
16		42	42	34, 42	1	2	33, 42	1	2	4	2
17		42	42	42	1	1	40, 42, 1	1	3	4	2
18		42	42	...	...	...	42, 42 (twins)	2	2	2	2
19	Ring . .	14	14	33, 42, 14	1	3	32, 40	...	2	5	1
20		14	14	42, 16	...	2	16, 14, 42, 42	1	4	6	2
21		14	14	6	...	1	9, 35, 48, 32, 14	1	5	6	1
22		42	42	40	...	1	40	...	1	2	...
23		42	42	42, 42, 42	3	3	40, 42	1	2	5	4
24		42	42	...	...	...	40, 42	1	2	2	1
25		42	42	42, 42	2	2	42, 40, 42	2	3	5	4
26		42	42	49, 14	...	2	42, 42, 42	3	3	5	3
27		46	46	48, 40, 40, 16	...	4	16, 38	...	2	6	...
					22	44	Daughters . . .	37	65		
							Sons	22	44		
							Total children . .	59	109	109	59

There are nineteen cases of both parents having the commonest of the loop patterns, No. 42, on a corresponding finger. They have between them seventy-five children, of whom forty-eight have the pattern No. 42, on the same finger as their parents, and eighteen others have loops of other kinds on that same finger, making a total of sixty-six coincidences out of the possible 75, or 88 per cent, which is a great increase upon the normal proportion of loops of the No. 42 pattern in the fore, middle, and ring-fingers collectively. Again there are three cases of both parents having a tendrilled-loop No. 14, which ranks as a whorl. Out of their total number of seventeen children, eleven have whorls and only six have loops.

Lastly there is a single case of both parents having an arch, and all their three children have arches; whereas in the total of 109 children in the table, there are only four other cases of an arch.

This partial analysis accounts for the whole of the like-patterned parents, except four couples, which are one of No. 34, two of No. 40, and one of No. 46. These concur in telling the same general tale, recollecting that No. 46 might almost be reckoned as a transitional case between a loop and a whorl.

The decided tendency to hereditary transmission cannot be gainsaid in the face of these results, but the number of cases is too few to justify quantitative conclusions. It is not for the present worth while to extend them, for the reason already mentioned, namely an ignorance of the allowance that ought to be made for related patterns. On this account it does

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not seem worth while to print the results of a large amount of tabulation bearing on the simple filial relationship between the child and either parent separately, except so far as appears in the following paragraph.

Relative Influence of the Father and the Mother.—Through one of those statistical accidents which are equivalent to long runs of luck at a gaming table, a concurrence in the figures brought out by Mr. Collins suggested to him the existence of a decided preponderance of maternal influence in the hereditary transmission of finger patterns. His further inquiries have however cast some doubt on earlier and provisional conclusions, and the following epitomises all of value that can as yet be said in favour of the superiority of the maternal influence.

The fore, middle, and ring-fingers of the right hands of the father, mother, and all their accessible children, in many families, were severally tabulated under the fifty-three heads already specified. The total number of children was 389, namely 136 sons and 219 daughters. The same pattern was found on the same finger, both of a child and of one or other of his parents, in the following number of cases:—

TABLE XXIX.

Relative Influence of Father and Mother.

	Fore.	Middle.	Ring.	Totals.	Corrected Totals.	
Father and son .	17	35	28	80	80	} 149
„ „ daughter	29	52	30	(111)	69	
Mother and son .	18	50	26	94	94	} 186
„ „ daughter	38	75	35	(148)	92	

The entries in the first three columns are not comparable on equal terms, on account of the large difference between the numbers of the sons and daughters. This difference is easily remedied by multiplying the number of daughters by $\frac{136}{219}$, that is by 0·621, as has been done in the fifth column headed Corrected Totals. It would appear from these figures, that the maternal influence is more powerful than the paternal in the proportion of 186 to 149, or as 5 to 4; but, as some of the details from which the totals are built up, vary rather widely, it is better for the present to reserve an opinion as to their trustworthiness.

CHAPTER XII

RACES AND CLASSES

The races whose finger prints I have studied in considerable numbers, are English, pure Welsh, Hebrew, and Negro; also some Basques from Cambo in the French Pyrenees, twenty miles south-east of Bayonne. For the Welsh prints I am primarily indebted to the very obliging help of Mr. R. W. Atkinson, of Cardiff, who interested the masters of schools in purely Welsh-speaking mountainous districts, on my behalf; for the Hebrew prints to Mr Isidore Spielman who introduced me to the great Hebrew schools in London, whose head-masters gave cordial assistance, and for the Negro prints to Sir George Taubman Goldie, Dep. Governor of the Royal Niger Co., who interested Dr Crosse on my behalf, from whom valuable sets of prints were received, together with particulars of the races of the men from whom they were made. As to the Basques, I printed them myself.

It requires considerable patience and caution to arrive at trustworthy conclusions, but it may emphatically be said that there is no *peculiar* pattern which characterises persons of any of the above races. There is no particular pattern that is special to any one of them, which when met with, enables us to assert, or even to suspect, the nationality of the person on whom it appeared. The only differences as thus far observed, are statistical, and cannot be determined except through patience and caution, and by discussing large groups.

I was misled at first by some accidental observations, and as it seems so reasonable to expect to find racial differences in finger marks, the inquiries were continued in varied ways until hard fact had made hope no longer justifiable.

After preliminary study, I handed over the collection of racial finger prints to Mr F. Howard Collins, who kindly undertook the labour of tabulating them in many ways, of which it will be only necessary to give an example. Thus, at one time attention was concentrated on a single finger and a single pattern, the most instructive instance being that of arches on the right fore-finger. They admit of being defined with sufficient clearness, for they have only one doubtful frontier of much importance, namely that at which they begin to break away into nascent-loops, etc. They also occur with considerable frequency on the fore-finger, so the results from a few hundred specimens ought to be fairly trustworthy. It mattered little in the inquiry at what level the limit was drawn, to separate arches from nascent-loops, so long as the same limit was observed in all races alike. Much pains were taken to secure uniformity of treatment, and Mr. Collins selected two limits, the one based on a strict and the other on a somewhat less strict interpretation of the term "arches," but the latter was not so liberal as that which I had used myself in the earlier inquiries (see p. 114). His results showed no great difference in the proportionate frequency of arches in the different races, whichever limit was observed; the following table refers to the more liberal limit:—

TABLE XXX.

Frequency of Arches in the Right Fore-Finger.

No. of Persons.	Race.	No. of Arches.	Per Cents.
250	English	34	13·6
250	Welsh	26	10·8
1332	Hebrew	105	7·9
250	Negro	27	11·3
	Hebrews in detail—		
500	Boys, Bell Lane School . .	35	7·0
400	Girls, Bell Lane School . .	34	8·5
220	Boys, Tavistock St. & Hanway St.	18	8·2
212	Girls, Hanway Street School .	18	8·5

The two contrasted values here are the English and the Hebrew. The 1332 cases of the latter give a percentage result of 7·9, which differs as may be seen less than 1 per cent from that of any one of the four large groups upon which the average is based. The 250 cases of English are comparatively few, but the experience I have had of other English prints is so large as to enable me to say confidently, that the percentage result of 13·6 is not too great. It follows, that the percentage of arches, in the English and in the Hebrew, differs in the ratio of 13·6 to 7·9, or nearly as 5 to 3. This is the largest statistical difference yet met with. The deficiency in arches among the Hebrews, and to some extent in loops also, is made up by a superiority in whorls, chiefly of the tendril or circlet-in-loop patterns.

It would be very rash to suppose that this relative infrequency of arches among the Hebrews was of fundamental importance, considering that such totally distinct races as the Welsh and the Negro have them in an intermediate proportion. Still, why does it occur? The only answer I can give is that the patterns being in some degree hereditary, accidental preponderances that may have existed among a not very numerous ancestry might be perpetuated. I have some reason to believe that local peculiarities of this sort exist in England, the children in schools of some localities seeming to be statistically more alike in their patterns, than English children generally.

Another of the many experiments was the tabulation separately by Mr. Collins of the fore, middle and ring-fingers of the right hand of fifty persons of each of the five races above-mentioned: English, Welsh, Basque, Hebrew, and different groups of Negroes. The number of instances ~~are~~ is of course too ~~few~~ small for statistical deductions, but they served to make it clear that no very marked characteristic distinguished the races. The impressions from Negroes betray the general clumsiness of their fingers, but their patterns are not, so far as I can find, different from those of others, they are not simpler as judged either by their contours, or by the number of origins, embranchments, islands, and enclosures contained in them, so far as I have counted. Still, whether it be from pure fancy on my part, or from the way in which they were printed, or from some real peculiarity, the general aspect of the Negro print strikes me as characteristic. The width of the ridges seems more uniform, their intervals more regular, and their courses more parallel than with us. In short they give an idea of greater simplicity, due to causes that I have not yet succeeded in submitting to the test of measurement.

The above are only a few examples of the laborious work so kindly undertaken for me by Mr. F. H. Collins, but it would serve no useful purpose to give more in this book, as no positive results have as yet been derived from it other than the little already mentioned.

The most hopeful direction in which this inquiry admits of being pursued is among the Hill tribes of India, Australian blacks, and other diverse and so-called aboriginal races. The field of ethnology is large, and it would be unwise as yet to neglect the chance of somewhere finding characteristic patterns.

Differences between finger prints of different classes might continue to exist although those of different races are inconspicuous, because every race contains men of various temperaments and faculties, and we cannot tell, except by observation, whether any of these are correlated with the finger marks. Several different classes have been examined both by Mr. Collins and myself. The ordinary laboratory work supplies collections of persons of much culture, and of many students both in the Art and in the Science schools. I took a large number of prints from the worst idiots in the London district, through the obliging assistance of Dr. Fletcher Beech, of the Darenth Asylum; my collections made at Board Schools are numerous, and I have one of field labourers in Dorsetshire and Somersetshire. There is no notable difference in any of them. For example; the measurements of the ridge-interval gave the same results in the art-students and in the science-students, and I have prints of eminent philosophers and of eminent statesmen that can be matched by those of congenital idiots.[1] No indications of temperament, character, or ability are to be found in finger marks, so far as I am able to discover.

Of course these conclusions must not be applied to the general shape of the hand, which as yet I have not studied but which seems to offer a very interesting field for exact inquiry.

[1] The results arrived at by M. Féré in a Memoir (*Comptes Rendus, Soc. Biologie*, July 2, 1891; Masson, 120 Boulevard St. Germain, Paris) may be collated with mine. The Memoir is partly a review of my paper in the *Phil. Trans.*, and contains many observations of his own. He has, by the way, curiously misinterpreted my views about symmetry. His data are derived from epileptic and other patients mentally affected.

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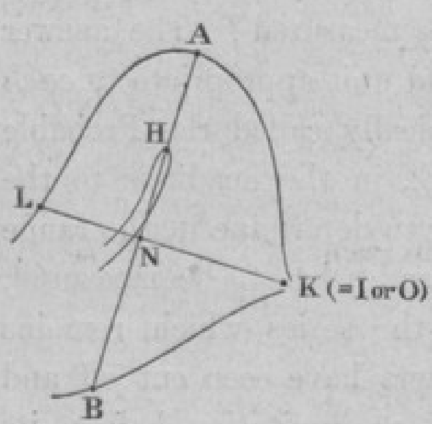
CHAPTER XIII

GENERA

THE same familiar patterns recur in every large collection of finger prints, and the eye soon selects what appear to be typical forms; but are they truly "typical" or not? By a type I understand an ideal form around which the actual forms are grouped, very closely in its immediate neighbourhood, and becoming more rare with increasing rapidity at an increasing distance from it, just as is the case with shot marks to the right or left of a line drawn vertically through the bull's eye of a target. The analogy is exact; in both cases there is a well-defined point of departure; in both cases the departure of individual instances from that point is due to multitude of independently variable causes. In short, both are realisations of the now well-known theoretical law of Frequency of Error. The problem then is this:—take some one of the well-marked patterns, such as it appears on a particular digit,—say a loop on the right thumb; find the average number of ridges that cross a specified portion of it; then this average value will determine an ideal centre from which individual departures may be measured; next, tabulate the frequency of the departures that attain to each of many successive specified distances from that ideal centre; then see whether their diminishing frequency as the distances increase, is or is not in accordance with the law of frequency of error. If it is, then the central form has the attributes of a true type, and such will be shown to be the case with the loops of either thumb. I shall only give the data and the results, not the precise way in which they are worked out, because an account of the method employed in similar cases will be found in *Natural Inheritance*, and again in the Memoir on Finger Prints in the *Phil. Trans.*; it is too technical to be appropriate here, and would occupy too much space. The only point which need be briefly explained and of which non-mathematical readers might be ignorant, is how a single numerical table derived from abstract calculations, can be made to apply to such minute objects as finger prints, as well as to the shot marks on a huge target; what is the common unit by which departures on such different scales are measured? The answer is that it is a self-contained unit appropriate to *each series severally*, and technically called the Probable Error, or more briefly, P.E., in the headings to the following tables. In order to determine it, the range of the central half of the series has to be measured, namely, of that part of the series which remains after its two extreme quarters have been cut off and removed. The series had no limitation before, its two ends tailing away indefinitely into nothingness, but, by the artifice of lopping off a definite fraction of the whole series from either end of it, a sharply-defined length, call it PQ, is obtained. Such series as have usually to be dealt with are fairly symmetrical, so the position of the half-way point M, between P and Q, corresponds with rough accuracy to the average of the positions of all the members of the series, that is to the point whence departures have to be measured. MP, or MQ, — or still better, $\frac{1}{2}$ (MP + MQ) is the above-mentioned Probable Error. It is so called because the amount of Error, or Departure from M of any single observation, falls just as often within the distance PE, as it falls without it. In the calculated tables of the Law of Frequency, PE (or a multiple of it) is taken as unity. In each observed series, the actual measures have to be converted into another scale, in which the PE of that series is taken as unity. Then observation and calculation may be compared on equal terms.

Observations were made of the loops of the right and left thumbs respectively. AHB is taken as the primary line of reference in the loop; it is the line that, coinciding with the axis of the *uppermost portion*, and that only, of the core, cuts the summit of the core at H, the upper outline at A, and the lower outline, if it cuts it at all, as



it nearly always does, at B. K is the centre of the single triangular plot that appears in the loop, which may be either I or O. KNL is a perpendicular from K to the axis, cutting it at N, and the outline beyond at L. In some loops N will lie above H, as in Plate 4, Fig. 8; in some it may coincide with H. (See Plate 6 for numerous varieties of loop.) These points were pricked in each print with a fine needle; afterwards the print was turned face downwards and careful measurements made between the prick holes at the back. Also the number of ridges in AH were counted, the ridge at A being reckoned as 0, the next ridge as 1, and so on up to H. Whenever the line AH passed across the neck of a bifurcation, there was necessarily a single ridge on one side of the point of intersection and two ridges on the other, so there would clearly be doubt whether to reckon the neck as one or as two ridges. A compromise was made by counting it as $1\frac{1}{2}$. After the number of ridges in AH had been counted in each case, any residual fractions of $\frac{1}{2}$ were alternately treated as 0 and as 1. Finally, six series were obtained; three for the right thumb, and three for the left. They referred respectively (1) to the Number of Ridges in AH; (2) to $\frac{KL}{NB}$; (3) to $\frac{AN}{AH}$, all the three being independent of stature. The number of measures in each of the six series varied from 140 to 176; they are reduced to percentages in Table XXXI.

We see at a glance that the different numbers of ridges in AH do not occur with equal frequency, that a single ridge in the thumb is a rarity, and so are cases above fifteen in number, but those of seven, eight, and nine are frequent. There is clearly a rude order in their distribution, the number of cases tailing away into nothingness, at the top and bottom of the column. A vast amount of statistical analogy assures us that the orderliness of the distribution would be increased if many more cases had been observed, and later on, this inference will be confirmed. There is a sharp inferior limit to the numbers of ridges, because they cannot be less than 0, but independently of this, we notice the infrequency of small numbers as well as of large ones. There is no strict limit to the latter, but the trend of the entries shows that forty, say, or more ridges in AH are practically impossible. Therefore, in no individual case can the number of ridges in AH depart very widely from seven, eight, or nine, though the range of possible departures is not sharply defined, except at the lower limit of 0. The range of variation is *not* "rounded off," to use a common but very inaccurate expression, often applied to the way in which genera are isolated. The range of possible departures is not defined by any rigid boundary, but the rarity of the stragglers rapidly increases with the distance at which they are found, until no more of them are met with.

The values of $\frac{KL}{NB}$, and of $\frac{AN}{AH}$, run in a less orderly sequence, but concur distinctly in telling a similar tale. Considering the paucity of the observations, there is nothing in these results to contradict the expectation of increased regularity, should a large addition be made to their number.

TABLE XXXI.

No. of ridges in AH.	No. of cases reduced to per cents.		KL/NB	No. of cases reduced to per cents.		AN/AH	No. of cases reduced to per cents.	
	Right.	Left.		Right.	Left.		Right.	Left.
	171 cases.	166 cases.		149 cases.	140 cases.		176 cases.	163 cases.
1	1	...	0·3-0·4	3	2	0·1-0·2	2	1
2	2	1	0·5-0·6	8	11	0·3-0·4	7	3
3	2	3	0·7-0·8	9	14	0·5-0·6	11	3
4	2	5	0·9-1·0	21	18	0·7-0·8	9	9
5	3	5	1·1-1·2	16	23	0·9-1·0	22	15
6	4	18	1·3-1·4	24	7	1·1-1·2	15	13
7	8	14	1·5-1·6	8	10	1·3-1·4	12	12
8	8	16	1·7-1·8	3	6	1·5-1·6	11	14
9	11	10	1·9-2·0	5	6	1·7-1·8	8	10
10	9	8	2·1-2·2	1	1	1·9-2·0	1	5
11	14	10	above	2	2	2·1-2·2	...	...
12	11	8	...	...	...	2·3-2·4	1	6
13	10	2	...	...	...	2·5-2·6	...	4
14	7	...	...	...	...	2·7-2·8	...	3
15	6	...	...	...	...	2·9-3·0	...	1
above	2	...	...	...	...	above	1	1
	100	100		100	100		100	100

TABLE XXXII.

Abscissae reckoned in centesimal parts of the interval between the limits of the scheme. 0° to 100°.	Ordinates to the six schemes of Distribution, being the ordinates drawn from the base of each scheme at selected centesimal divisions of the base.											
	No. of ridges in AH.				Values of $\frac{KL}{NB}$				Values of $\frac{AN}{AH}$			
	Right.		Left.		Right.		Left.		Right.		Left.	
	Observed.	Calculated from M = 10·4 p.e. = 2·3	Observed.	Calculated from M = 7·8 p.e. = 1·9	Observed.	Calculated from M = 1·15 p.e. = 0·25	Observed.	Calculated from M = 1·10 p.e. = 0·31	Observed.	Calculated from M = 1·08 p.e. = 0·30	Observed.	Calculated from M = 1·36 p.e. = 0·36
5	3·8	4·8	3·8	3·2	0·54	0·54	0·49	0·35	0·36	0·32	0·58	0·48
10	5·5	6·0	4·8	4·2	0·64	0·67	0·59	0·51	0·50	0·48	0·74	0·68
20	7·3	7·5	5·8	5·4	0·85	0·84	0·78	0·71	0·66	0·67	0·96	0·91
25	7·9	8·1	6·1	5·9	0·91	0·90	0·83	0·79	0·79	0·75	1·00	1·00
30	8·5	8·6	6·4	6·3	0·99	0·95	0·89	0·86	0·87	0·82	1·04	1·08
40	9·5	9·5	7·1	7·4	1·05	1·05	1·00	0·98	0·98	0·93	1·21	1·22
50	10·5	10·4	7·8	7·8	1·15	1·15	1·10	1·10	1·04	1·05	1·37	1·36
60	11·3	11·3	8·4	8·2	1·29	1·25	1·18	1·22	1·18	1·17	1·48	1·50
70	12·1	12·2	9·3	9·3	1·33	1·35	1·32	1·34	1·31	1·28	1·66	1·64
75	12·5	12·7	9·9	9·7	1·41	1·40	1·46	1·41	1·39	1·35	1·73	1·72
80	13·0	13·3	11·0	10·2	1·45	1·46	1·53	1·49	1·48	1·43	1·90	2·81
90	14·3	14·8	11·5	11·4	1·77	1·63	1·73	1·69	1·69	1·62	2·23	2·04
95	15·0	16·0	12·2	12·2	2·00	1·76	1·80	1·85	1·81	1·78	2·48	2·24

Table XXXIII.

Abscissae reckoned in centesimal parts of the interval between the limits of the curve. 0° to 100°.	Ordinates to the six curves of distribution, drawn from the axis of each curve at selected centesimal divisions of it. They are here reduced to a common measure, by dividing the observed deviations in each series by the probable error appropriate to the series, and multiplying by 100. For the values of M, whence the deviations are measured, and for those of the corresponding probable error, see the headings to the columns in Table II.						Observed.	Calculated.
	No. of Ridges in AH.		Values of $\frac{KL}{NB}$		Values of $\frac{AN}{AH}$		Mean of the corresponding ordinates in the six curves after reduction to the common scale of p.e. = 100. 965 observations in all.	Ordinates to the normal curve of distribution, probable error = 100.
	Right.	Left.	Right.	Left.	Right.	Left.		
5	−291	−211	−244	−196	−230	−217	−231	−244
10	−213	−158	−204	−164	−183	−172	−182	−190
20	−135	−105	−120	−103	−130	−111	−117	−125
(P) 25	−109	− 84	− 92	− 87	− 87	−100	− 93	−100
30	− 83	− 74	− 64	− 68	− 60	− 89	− 73	− 78
40	− 44	− 37	− 44	− 31	− 23	− 42	− 37	− 38
(M) 50	+ 4	0	0	0	0	0	+ 1	0
60	+ 39	+ 31	+ 56	+ 23	+ 43	+ 33	+ 38	+ 38
70	+ 74	+ 79	+ 72	+ 68	+ 87	+ 83	+ 77	+ 78
(Q) 75	+ 91	+116	+104	+116	+113	+103	+107	+100
80	+113	+168	+120	+138	+143	+150	+139	+125
90	+170	+200	+248	+203	+213	+242	+213	+190
95	+200	+231	+340	+225	+253	+311	+260	+244

Table XXXII. is derived from Table XXXI. by a process described by myself in many publications, more especially in *Natural Inheritance*, and will now be assumed as understood. Each of the six pairs of columns contain side by side the Observed and Calculated values of one of the six series, the data on which the calculations were made, being also entered at the top. The calculated figures agree with the observed ones very respectably throughout, as can be judged even by those who are ignorant of the principles of the method. Let us take the value that 10 per cent of each of the six series falls short of, and 90 per cent exceed; they are entered in the line opposite 10; we find for the six pairs successively,

Obs.:	5·5	4·8	0·64	0·59	0·50	0·74
Calc.:	6·0	4·2	0·67	0·51	0·48	0·68

The correspondence between the more mediocre cases is much closer than these, and very much closer than between the extreme cases given in the table, namely, the values that 5 per cent fall short of, and 95 exceed. These are of course less regular, the observed instances being very few; but even here the observations are found to agree respectably well with the proportions given by calculation based upon the ~~necessary~~ supposition of an infinite number of cases having been included in the series.

As the want of agreement between calculation and observation must be caused in part by the paucity of observations, it is worth while to make a larger group, by throwing the six series together, as in Table XXXIII., making a grand total of 965 observations. Their value is not so great as if they were observations taken from that number of different persons, still they are equivalent to a large increase of those already discussed. The six series of observed values were made comparable on equal terms by first reducing them to a uniform PE and then by assigning to M, the point of departure, the value of 0. The results are given in the last column but one, where the orderly run of the observed data is much more conspicuous than it was before. Though there is an obvious want of exact symmetry in the observed values, their general accord with those of the calculated values is very fair. It is quite close enough to establish the general proposition, that we are justified in the conception of a typical form of loop, different for the two thumbs, ~~and that~~ the departure from the typical form ~~is~~ usually small, sometimes rather greater, and rarely greater still.

I do not see my way to discuss the variations of the arches, because they possess no distinct points of reference. But their general appearance does not give the impression of clustering around a typical centre. They suggest the idea of a head to a stream, that begins to ~~diverge~~ from the first.

As regards other patterns, I have made many measurements altogether, but the specimens of each sort were comparatively few, except in whorled patterns. In all cases where I was able to form a well-founded opinion, the existence of a typical centre was indicated.

It would be tedious to enumerate the many different trials that I have made for my own satisfaction, in order to assure myself that the variability of the several patterns is really of the quasi-normal kind just described. In my first trial I measured in various ways the dimensions of about 500 enlarged photographs of loops, and about as many of other patterns, and found that the measurements in each and every case formed a quasi-normal series. I do not care to submit these results, because they necessitate more explanation and analysis than the interest of the corrected results would perhaps justify, to eliminate from them the effect of variety of size of thumb, and some other uncertainties. Those measurements referred to some children, a few women, many youths, and a fair number of adults; and allowance has to be made for variability in stature in each of these classes.

The proportions of a typical loop on the thumb are easily ascertained if we may assume that the most frequent values of its variable elements, taken separately, are the same as those that enter into the most frequent combination of the elements taken collectively. This would necessarily be true if the variability of each element separately, and that of the sum of them in combination, were all strictly normal, but as they are only quasi-normal the assumption must be tested. I have done so by making the comparisons (*A*) and (*B*) shown in Table XXXIV., which come out correctly to within the first decimal place.

TABLE XXXIV.

		Right Thumb.	Left Thumb.
(a)	Median of all the values of KL	12·5	10·1
(b)	Median of all the values of NB	10·1	8·9
(A)	Value of a/b	1·24	1·11
	Median of all the fractions $\frac{KL}{NB}$	1·15	1·10
(c)	Median of all the values of AN	4·6	4·6
(d)	Median of all the values of AH	4·4	3·3
(B)	Value of c/d	1·05	1·40
	Median of all the fractions $\frac{AN}{AH}$	1·08	1·36

It has been shown that the patterns are hereditary, and we have seen that they are uncorrelated with race or temperament or any other noticeable peculiarity, inasmuch as groups of very different classes are alike in their finger marks. The very existence both of the ridges and of the patterns they form has been almost overlooked, because they are too small to have attracted attention, or to have been thought worthy of notice. We therefore possess in respect to the patterns a perfect instance of promiscuity in marriage, or, as it is now called, panmixia, in respect to these patterns. We might consequently have expected them to be hybridised. But that is not the case; they *refuse to blend.* Their classes are as clearly separated as those of any of the genera of plants and animals. They keep pure and distinct, as if they had severally descended from a thorough-bred ancestry, each in respect to its own peculiar character.

As regards other forms of natural selection, we know that races are kept pure by the much more frequent destruction of those individuals who depart the more widely from the typical centre. But natural selection is wholly inoperative in respect to individual varieties of patterns, and unable to exercise the slightest check upon their vagaries. Yet, for all that, the loops and other classes of patterns are isolated from one another just as thoroughly and just in the same way as are the genera or species of plants and animals. There is no statistical difference between the form of the law of distribution of individual Loops about their respective typical centres, and that of the law by which, say, the Shrimps described in Mr. Weldon's recent memoirs (*Roy. Soc. Proc.*, 1891 and 1892) are distributed about theirs. In both cases the distribution is in quasi-accordance with the theoretical law of Frequency of Error, this form of distribution being entirely caused in the patterns, by *internal* conditions, and in no way by natural selection in the ordinary sense of that term.

It is impossible not to recognise the fact so clearly illustrated by these patterns in the thumbs, that natural selection has no monopoly of influence in the construction of genera, but that it could be wholly dispensed with, the internal conditions acting by themselves being sufficient to form them. When the internal conditions are in harmony with the external ones, as they appear to be in all long-established races, their joint effects will curb individual variability more tightly than either could do by itself. The normal character of the distribution about the typical centre will not be thereby interfered with. The probable divergence (=probable error) of an individual taken at random, will be lessened, and that is all.

Not only is it impossible to substantiate a claim for natural selection, that it is the sole agent in forming genera, but it seems, from the experience of artificial selection, that it is scarcely competent to do so by favouring mere *varieties*, in the sense in which I understand the term.

My contention is that it acts by favouring small *sports*. Mere varieties from a common typical centre blend freely in the offspring, and the offspring of every race whose *statistical* characters are constant, necessarily tend, as I have often shown, to regress towards their common typical centre. Sports, on the other hand, do not blend freely; they are fresh typical centres or subspecies, which suddenly arise we do not yet know precisely through what uncommon concurrences of circumstance, and which observations show to be strongly transmissible by inheritance.

A mere variety can never establish a sticking point in the forward course of evolution, but each new sport affords one. A substantial change of type is effected, as I conceive, by a succession of small changes of typical centre, each more or less stable, and each being in its turn favoured and established by natural selection, to the exclusion of its competitors. The distinction between a mere variety and a sport is real and fundamental. I argued this point in *Natural Inheritance*, but had then to draw my illustrations from non-physiological experiences. I could not at that time find an appropriate physiological one. The want is now excellently supplied by observations of the patterns on the digits.

THE END

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test by the
of the finger
ing
(brothers)
within ridges
a rod or staple

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THE END

Printed by R. & R. Clark, *Edinburgh.*

SUPPLEMENTARY CHAPTER TO 'FINGER PRINTS'

DECIPHERMENT

OF

BLURRED FINGER PRINTS

BY

FRANCIS GALTON, F.R.S., ETC.

London
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DECIPHERMENT OF BLURRED FINGER PRINTS

THE registration of finger prints of criminals, as a means of future identification, has been thought by some to be of questionable value on two grounds—first, that ordinary officials would fail to take them with sufficient sharpness to be of use; secondly, that no jury would convict on finger print evidence. These objections deserve discussion, and would perhaps by themselves have justified a supplementary chapter to my book. It happens, however, that there are strong concurrent reasons for writing it. I have lately come into possession of the impressions of the fore and middle fingers of the right hand of eight different persons, made by ordinary officials, in the first instance in the year 1878 and secondly in 1892. They not only supply a text for discussing both of the above objections, but they also afford new evidence of the persistence of the *minutiæ*, that is of the forks, islands, and enclosures, found in the capillary ridges. It will be recollected that the evidence of their persistence, published or alluded to in my book, was derived from impressions made by only fifteen different persons, at the beginning and end of considerable intervals of

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time, varying from twelve to thirty-one years. Consequently, the data that have been derived from eight other persons, well deserve to be recorded.

The question now before us has nothing to do with classification, but is merely this: would such impressions as ordinary officials are likely to take, afford evidence strong enough to convince a jury that two submitted finger prints had or had not been made by the same finger? It is, of course, supposed that the cogency of the finger print argument will be presented to the jury in that lucid and complete form in which it is the business of barristers to state and support their case, when they are satisfied of the integrity of the evidence on which it is based.

The following paragraph occurs in *Finger Prints* (p. 89): "It would be well worth while to hunt up and take the present finger prints of such of the Hindoos as may now be alive, whose impressions were taken in India by the instructions of Sir W. J. Herschel, and are now preserved."

Sir William thereupon was so good as to write privately to Mr. Cotton, Secretary to the Government in Bengal, explaining more particularly what was wanted and where to look for it. The result has been that a letter or memorandum was received by him from Mr. Duke, the present Joint Magistrate of Hooghly, forwarding a batch of authenticated impressions. Mr. Duke remarks in it that all persons who executed documents, and registered them in the Hooghly Registration Office in the year 1878, were made to give impressions of the fore and middle fingers of their right hands in a separate book kept for that purpose, and that although the practice has been discontinued, the book of impressions has been preserved. He caused the Register of the Deeds executed in that year, to be searched for the names of persons who were still alive and resident at or near Hooghly, when it was found that the number of those who fulfilled both conditions was small. The Special Sub-Registrar of Hooghly, Babu Ram Gati Bannerjee, was then asked to visit eight of these persons (I do not know how many more were available), and to take fresh impressions of their fingers. He was so good as to do so with much care, impressing them successively on the same sheet of paper, and adding to each the name, age (actual or estimated), and date. Not only did he do this, but having been also given slips, cut out of the book that contained the original finger prints, he took a new impression on each slip by the side of the old one. Thus a collection was formed of impressions of the fore and middle fingers of eight different persons, one set in each case having been made in 1878, and two sets in each case having been made in 1892. (Some details were added in the accompanying notes concerning the impressions, that are not worth recording here.) All these were sent to Sir W. Herschel, and have been made over by him to me, and are now in my possession. The documents are characteristically Oriental; they are on a common kind of apparently native-made paper, worm-eaten with many holes, and abundantly subscribed with attestations, names, ages, and dates, partly written in English and partly in native characters. They refer to the persons in the following list. The numbers in the first column are those of the Deeds executed in 1878 by each of the persons whose finger prints form the collection in question.

	1878. No. of Deed.	In 1892. Age (about).
I. Saburan Bibi	162	65
II. Dwārikā Nath Banerji	28	64
III. Girish Chandra Rāy	43	52
IV. Beehā Rām Dās Adhikari	22	42
V. Sri Nāth Set	51	62
VI. Gagan Set	58	52
VII. Mādhab Chandra Rāy	54-56	54
VIII. Ghirish Chandra Pandit	379	46

My heartiest thanks are due, and are here gratefully rendered, to all the persons who have contributed to put me into the possession of such interesting materials.

The only thing that I cannot commend is the method of printing, which is by using dye or water-colour. It is very difficult in that way to ensure good impressions, because if the blackened pad on which the finger is pressed be wet or even moist, instead of being merely damp, the dye fills the furrows as well as the ridges; the finger is blackened all over, and on being pressed on paper it leaves a blot. Though the pad be in good order, a finger that is damp with its own perspiration will similarly make a blur or blot. Moreover the *grain* of a pad is too coarse for fine printing.

The impressions obtained from these eight persons cannot compare in clearness with those regularly and rapidly taken in large numbers at my laboratory by means of printer's ink, in the way fully described in Chapter III. of *Finger Prints.* The ink is spread uniformly and *very thinly* on a smooth slab by a printer's roller, the fingers are pressed on this and then on paper, and the results are uniformly of a high-level order of goodness. If the fingers are wet with perspiration they should be dried first with a rag. I do not overlook the fact that much of the indistinctness of the Hooghly impressions is due to disintegration of the skin, owing to the advanced ages of the persons in 1892 (*op. cit.* 58). Three of the eight persons were then more than sixty years old, and three more of them were over fifty, which I presume corresponds to greater ages among ourselves, since Indian children are more precocious than ours. Neither should the fact be forgotten that hard manual labour of certain kinds may injure the sharpness of the ridges (p. 59). Nevertheless, after making full allowance for these two causes, there remains a large amount of indistinctness which is unquestionably due to what experience has convinced me to be a faulty method of printing, and by no means to lack of adequate skill or zeal in the officials.

The practice of always taking the impression of a single digit of criminals is, I understand, now under trial by the Bengal police; it is to be hoped that the trial will not be limited to the process with dye or water-colour.

These criticisms have been made principally in order to establish the points upon which this pamphlet is based. It will soon be shown how great is the value even of these blurred Hooghly prints for purposes of identification, and it is evident from what has just been said, that the value of impressions made by the method of printer's ink, but otherwise under similar conditions, would be considerably greater.

Reproduction of the Prints.—I photographed each set of fore and middle finger prints that were impressed in 1878, upon a "quarter plate," enlarging them at the same time to a two and a half scale, which was as much as the quarter plate could conveniently hold. I did the same with *the best* of the two sets of 1892. Slightly better results would have been obtained by sometimes selecting the fore finger from one of the two sets and the middle finger from the other, but the gain by doing so seemed not to be worth the trouble. These prints have been photo-lithographed, and they appear in Plates I. to IV.

Experience having convinced me of the necessity of considerably enlarging prints that are intended to be submitted to an unpractised eye, I selected a seven-fold scale for use in this book, which is none too large, because there ought to be space for legible notes and marks being written between the lines. The whole of each impression on a seven-fold scale would, however, require too many plates for publication in this pamphlet, so I selected a portion of one finger, fore or middle as the case might be, in each set, and was contented to discard the rest. The selected portions always included the most characteristic part of the pattern, namely its core, together with that portion of the adjacent parts that seemed in each case to be most suitable. After completing the enlargements and putting the apparatus by, I found that the selected portion of No. V. was by no means the best that might have been made. In fact it could hardly be deciphered at all. It was therefore discarded altogether and its place supplied by the second of No. VII. set. Some further remarks about No. V. will be made in their place, among the rest.

Though only about half of the impression of a single finger has been discussed on the seven-fold scale, it will be found that even this much, which is bounded by the sides of a rectangle 2 inches wide and $3\frac{1}{4}$ deep, ought in most cases to convince a jury. The whole of the palmar surface of the hands and of the soles of the feet contains similar material. Their total extent of surface may be taken to be 100 square inches, which on a seven-fold scale would cover forty-nine times that area, or no less than thirty-four square feet. If the Hooghly method were improved and regularly adopted, the whole of the impressions of the bulbs of each of two fingers would be available. Then, there would usually be upwards of thirty points of reference in each finger print, or of sixty points altogether. Even in these half portions of single impressions, which are far from being good, it will be seen that there are on an average between fifteen and sixteen points of reference in each.

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Before comparing the 1878 and 1892 impressions in turns, it is necessary to strongly insist on a point on which great stress was laid in *Finger Prints* (p. 91, illustrated in p. 80). It concerns the particular kind of resemblance that is to be looked for. The one important point in making comparisons, is to remark the *place* where a new ridge begins to make its appearance, because the *manner* in which it seems to begin is quite secondary. A new ridge is usually connected with one or other of the ridges between which it rises, or it may be with both of them, by means of a low neck, or a *col* as mountaineers would call it. An impression of this *col* may be purely the result of using overmuch ink, or overmuch pressure. Again, after the lapse of many years, it is quite possible that the height of the *col* may have become slightly raised or depressed, and that it may be always, or else never, printed. It follows that when a fork in one finger print corresponds to the sudden appearance of a new ridge in the other, the event is to be counted as an agreement and not as a disagreement. There are several good instances of this apparent incongruity, but real agreement, in the present collection, to two or three of which the attention of the reader will be directed. I have made the practice in all these cases, of representing the *event* in both of the prints by the same symbol, whether it be a fork or a terminal circle, such as might seem most suitable to the particular case.

Confining our attention now to the prints on the seven-fold scale, it will be seen that they are here presented in three different manners. Plates V. to VIII. are the simple untouched enlargements. In IX. to XII. these same enlargements are shown in orange, with the axes of the ridges as I have deciphered them, printed in a different and darker colour. It would be the business of a barrister to so examine an expert as to elicit the value of the decipherment in each particular. Plates XIII. to XVI. are skeleton charts; they show the axes of the ridges alone, with numbers attached to each point of reference, for facilitating comparison and explanation. The whole of the materials for forming a judgment are given here, in a shape somewhat similar to that which, I presume, would be put into the hands of each member of the jury and others concerned with the case. The set of three might be photo-lithographed as these are, but on the same sheet, or they might be submitted as ordinary photographic prints. The first set shows the original data on a seven-fold scale, the third set shows their decipherment, while the second set combines the first and the third. The numbers printed by the sides of the points of reference were not inserted in the second set, lest its general effect should be spoiled by them. When the decipherment of Plates IX. to XII. has been accepted as correct, the comparison of the skeleton charts is as simple a matter as that of two plans, drawn on the same principle and to the same scale, with the view of ascertaining whether they refer to the same district. The fact of the practically infinite variety of skeleton charts would be impressed on the jury by the exhibition of numerous specimens. After a few causes had been tried in which finger print evidence was adduced and commented on, this fact would become popularly accepted, and its proof would give no further trouble to the counsel.

In deciphering an impression, it is a very good plan to lay the print against a pane of the window (or on a photographic retouching frame), and to pencil the axes of its ridges on the back. Of course, tracing paper may be used, but I think the other plan is better, at least for a moderate draughtsman. There is no possible slipping of flimsy paper; the facts and their interpretation are both contained on the same sheet, one on the one side of it and the other on the other, while they do not interfere, unless the print is viewed as a transparency, in which position it might (especially if rendered temporarily transparent by a volatile oil) be so photographed as to correspond with the figures in Plates IX. to XII.

The axes should be drawn with a finely pointed pencil, and with care, down the middle of the ridges. Slap-dash attempts are almost sure to be failures. It is advisable to take pains to determine a common starting point, before beginning to draw any lines at all; then to proceed from point to point in the two prints alternately, at first with wariness but afterwards much more freely.

It is very likely that the correctness with which some of the more blotted parts of these charts have been deciphered, may be open at first to question. The absurd mistakes sometimes made by antiquaries in their interpretation of defaced inscriptions might be quoted as a warning.

There is considerably less risk of blunder in deciphering somewhat defaced finger prints than in making out equally defaced inscriptions, the conditions to be fulfilled being more exacting. The continuous course of every line has to be made out from beginning to end, and the lines must nowhere be too crowded or too wide apart, and they must all flow in easy and appropriate curves; also as much regard must be paid to such blanks as are not obviously due to bad printing, as to the markings. The general effect of these conditions is that a mistake in deciphering any one part of the impression nearly always introduces confusion at some other part, where the lines refuse to fit in. A study of the eight doublets in succession from Plates V. to VIII., in connection with those in Plates IX. to X., is the best guarantee for the truth of these remarks.

Whoever reads these pages with a desire of thoroughly understanding the art of decipherment should practise on the Plates V. to VIII., using tracing paper for the purpose, and of course beginning with the easier prints. If he should hereafter have occasion to compare other enlargements, he will discover the necessity of orienting them alike and of carefully confining his attention to the part common to both. This has been done for him in Plates V. to VIII.

I. FORE FINGER.—The print of 1892 is unfortunately of the tip rather than of the bulb of the finger, and so just fails to include four of the excellent points of reference marked with a cross (X) in the skeleton chart of 1878. Nine of the ten points that have numbers at their sides, are clear and call for only two special remarks. No. 2 is drawn with a terminal circle to express an abrupt ending, which is the case in the print of 1878; it might with equal propriety have been drawn as a fork, according to the print of 1892. This is a good example of the ambiguity, so strongly insisted upon above, that the *manner* in which a new ridge appears to begin is of secondary importance; the primary fact to be noted is that a new ridge comes *somehow*—never mind how—into existence between two particular ridges and at a particular spot, where they have separated to permit its interpolation. No. 6 is very imperfectly shown in the 1892 impression, because it falls almost wholly outside its limits; it is consequently not counted. The existence of a fork at that place is easily verified by turning to the ampler impression, on a smaller scale, in Plate I.

Total.—Nine points of agreement; none of disagreement.

II. Middle Finger.—This is a striking example of the possibility of deciphering under great difficulties. Three points of reference in the print of 1878 could alone be seized upon as being more or less dependable; they were the summit of the innermost loop 5, as determined by the central furrow, and the enclosure 6, 7, whose blurred mass in the 1878 print has an outline fairly similar to that of the well-marked enclosure in the print of 1892. The blur is distinctly separated from the next ridge outwards, and indistinctly but sufficiently from the next ridge inwards, and its central furrow is faintly indicated.

Starting from 5, 6, and 7, the bifurcations 2 and 3 were clearly made out. Nothing more can be said in favour of 1, 4, and 8 than that they are congruous with those of the 1892 print in width and position; they are too much blotted to afford further evidence, and will not be counted at all.

Total.—8 less 3, or 5 points of agreement; none of disagreement.

III. Middle Finger.—There is abundant evidence in this case of the identity of the finger that impressed the two prints. The cross (X) indicates that a fork in the 1878 print does not fall within the limits of the seven-fold enlargement of the portion of 1892. The finger had become broader in the interval. The fork is clearly seen in the ampler but less enlarged impression in Plate II. No. 6 in the 1878 print falls in the middle of a worm-eaten hole in the original paper, but the accident is of little importance, because the hole is small, and a ridge that is clearly seen to come out of its boundary has not entered it from the other side; consequently it begins somewhere within the limits of the hole, either as a fork or else suddenly, as is here drawn in order to correspond with the skeleton chart of 1892. Owing to the badness of the 1892 impression a novice might be puzzled to decipher the core, but there are so many clear points of agreement between the two prints that by taking, say, Nos. 16, 8, and 15 as three independent starting points, it is impossible to go wrong.

Total.—Twenty-one points of agreement; none of disagreement.

IV. Fore Finger.—Here the results are most satisfactory, though owing to one cause or another it gave me a little trouble to decipher the core. The persistence of the small scar made by a cut is interesting, as is also the good example it affords of the small difficulty caused by a deep cut in determining the original connections between the ridges that it severed and permanently dislocated.

Total.—Nineteen points of agreement; none of disagreement.

(V.) Though no seven-fold enlargement is given of either of the two fingers in this case, for reasons mentioned at p. 7, it will be well to turn to Plate III. and to study the minutiæ found at the base of the print of the middle finger. Place a card over the left part of the 1892 impression, so as to cut off the part of it that does not appear in the impression of 1878. Three points of reference, at least, will be easily made out, and should be used as starting points for further analysis. The upper portion of the impression of 1892 is very troublesome; still, I can trace a few points of apparent incongruity, though unable to rely on any one of them. The whole of the 1892 impression of the fore finger is equally difficult to analyse. The badness of the print is here mainly due to disintegration through age, and probably through hard manual labour also. The person who made it, is stated to have been sixty-two years old.

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VI. Fore Finger.—All the seven points of reference are distinctly seen in both prints. No. 1, in the print of 1892, is an excellent instance of the ambiguity caused by bad printing, as to the manner in which a new ridge really begins, although the fact of its beginning at a particular place is certain. In this instance there is nothing to tell whether it commences as a fork of the upper ridge, or of the lower ridge, or if it is connected with both, or whether it has an altogether independent origin.

Total.—Seven points of agreement; none of disagreement.

VII. Fore Finger.—The markings are very characteristic in this case, and the points of reference are numerous; they are totally different from those in II. middle finger and V. fore finger, though the *patterns* of all three finger prints have much the same general appearance. The island marked 5, 6, is not trustworthy; it is worth remarking, but is not intended to be counted one way or the other. On the other hand, there are two small and interesting islands, 14, 19, which not only should be counted, but should each of them be marked with two numbers, one for its beginning and one for its end. The subtraction of two numbers in the first case, and the addition of two in the second, leaves the total number of points of reference the same as the highest number that is recorded, viz. 19.

Total.—Nineteen points of agreement; none of disagreement.

VII. Middle Finger.—This is introduced in the place of one of the set V. Its core is an example of the monkey type of lineations, and gives me the opportunity of saying that the few instances I possess of prints of this type were taken from persons who did *not* seem to be of low or degenerated organisation. One of my specimens is taken from the finger of a lady who strikes me as being both physically and intellectually considerably above the average of her sex.

Total.—Fifteen points of agreement; none of disagreement.

VIII. Fore Finger.—The disintegrated lineations of 1892 were a little difficult to unravel, but the whole has been effected satisfactorily, except the hopelessly broken portion between 5 and 6. Of these, 5 has no value as evidence of identity; it is merely inserted for convenience of explanation and is not intended to be counted. The two ridges that issue from 9 seem to unite in the one that ends in 12, but no clear idea can be formed from either print of the ending of the ridge that issues from 8.

Total.—Thirty points of agreement; none of disagreement.

SUMMARY OF POINTS OF AGREEMENT AND DISAGREEMENT BETWEEN THOSE PARTS OF THE IMPRESSIONS OF 1878 AND 1892 THAT ARE PRINTED ON A SEVEN-FOLD SCALE.

No. of the Person.	Finger printed from.	Number of points of Agreement.	Number of points of Disagreement.	Pattern.
I.	Fore	9	none	loop
II.	Middle	5	none	loop
III.	Middle	21	none	whorl
IV.	Fore	19	none	whorl
VI.	Fore	7	none	loop
VII.	Fore	19	none	loop
VII.	Middle	15	none	loop
VIII.	Fore	30	none	whorl
Total . . .		125	none	
Average . . .		15.6		

The evidence discussed thus far has been drawn from the correspondence of the minutiæ, and not of the general pattern. A few words must now be said about the latter. They are chiefly in warning. No person could possibly mistake a decided whorl for a decided loop, but lesser differences are often deceptive to an untrained eye, especially when the impression includes only a portion of the pattern. Many, for example, might hurriedly translate the "II. fore finger" in the 1892 impression as a "tented arch" instead of a loop. Until the time comes when finger prints are popularly cared for, it would require an f118j✓

expert to give a trustworthy account of the patterns of imperfect and blurred impressions.

It will be recollected by those who have read *Finger Prints* that the varieties of pattern—arch, loop, whorl—found on the several ten digits of the same person goes some way, and in many cases a long way, towards identifying him. The same may be said in a lesser but still considerable degree of the patterns on the first three fingers of either hand, six in all. But when only two fingers of the right hand are taken into account, the evidence that their patterns can afford towards identification is usually small. Of course, the *negative* evidence they may give is absolutely trustworthy; thus the man who impressed I. could not possibly be the man who impressed IV.

The table in p. 132 of *Finger Prints* shows that some few combinations of patterns in these two fingers are rare, so the evidence that an occurrence of those combinations could give, would be proportionately strong. Using the notation of that table, we see that in 100 cases there was no occurrence of *ao*, *ia*, *io*, *oo*, *ow*, *wa*, or *wo*, and only one occurrence of *oa*.

A few words should be added on the recent considerable experience I have had in photographing finger prints. Mawson's photo-mechanical plates are the only ones that have given satisfactory density, being especially made for the reproduction of black and white drawings of mechanism. If an enlarged print is wanted hurriedly, and the light is not good, the best economy of time is to take a negative of the same size as the original, and to use it as a transparency to enlarge from, on paper, in the camera. A seven-fold enlargement direct from a print is a very tedious operation in the wintry daylight of London. If the person be accessible from whom an enlarged impression is wanted, the inked or smoked glass, from which his finger has been blackened, affords a transparent negative for use in the camera. Should it be the intention to store the negative, recollect that a glass inked with printer's ink takes some two or three days to dry hard, and that when dried hard it is liable to no injury from rough packing, but *it will not bear water.* It is curious to notice how quickly water will find its way between dried printer's ink and the glass, and detach it. Consequently ink negatives that are intended for storing had better be varnished; those on smoked glass must be varnished also. Of course no varnish is needed before using them in the camera. The negatives can be enlarged immediately after the finger has been raised from the blackened glass.

A camera that shall enlarge to any uniform scale, or even to one or other of two or three alternate scales, is most easily made by fitting a box or boxes to the body of a camera. I worked for some time with a home-made apparatus of this sort, of a very rude kind. In the apparatus I now use the print is adjusted to a frame, removable from the camera, under the guidance of a card that can be dropped into a recess in the middle of the frame, which it neatly fits. The card has a rectangular hole in its middle that exactly defines the portion of the print that will fall within the limits of the photograph.

After the adjustment is completed the print is clamped, the card is removed, and the frame is replaced on the camera. It fits into its place with accuracy, and is clamped there. The desired portion of the print is then sure to occupy the proper position in front of the lens. I often photograph many prints in succession without caring to verify either the focusing or the adjustment, having found that there is rarely any sensible error.

Photographic enlargements save a great deal of petty trouble. It is far easier to deal exhaustively with them than it is with actual impressions viewed under a magnifying glass. In the latter case, a few marked correspondences, or the reverse, can readily be picked out, and perhaps noted by the prick of a fine needle, the point of a pin being much too coarse. It is thus easy to make out whether a suspicious print deserves the trouble of photographic enlargement, but without previous enlargement a *thorough* comparison between two prints is difficult even to an expert, and no average juryman could be expected to make it.

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