

CHAPTER XIII

STATISTICAL INVESTIGATIONS, ESPECIALLY WITH REGARD TO ANTHROPOMETRY

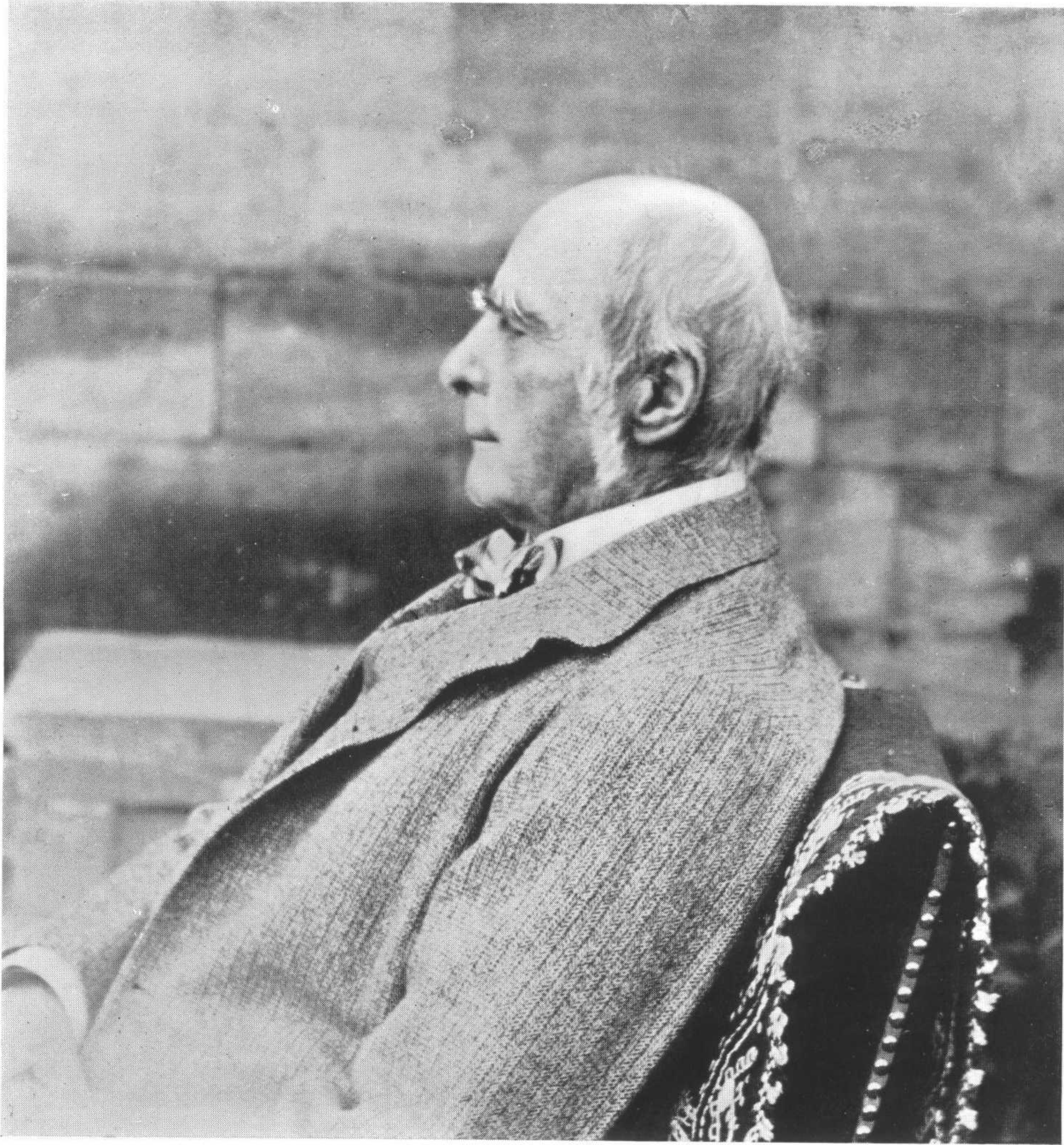
“Until the phenomena of any branch of Knowledge have been submitted to measurement and number it cannot assume the status and dignity of a science.” FRANCIS GALTON.

A. STATISTICS IN THE SERVICE OF ANTHROPOLOGY

THERE is no branch of knowledge to which Galton's remark applies more closely than anthropology; and there is certainly no field of research which owes more to Galton than that of anthropometry and in particular that branch of it which deals with craniometry. Here again as we have so often had occasion to remark Galton's contribution was essentially one of method, and lay in his insistence that the only way to permanent and safe deductions was the path of measurement and number. The reader has only to examine craniological papers of the 'sixties or 'seventies, even by such authorities of those days as Dr George Busk or Sir William Flower, to grasp how indefinite and inconclusive craniometry was before it became permeated with Galton's ideas of measurement and number. Half-a-dozen measurements on half-a-dozen skulls screened by a smoke-fog of vague remarks were considered an adequate basis for attack on the most elusive problems of racial differentiation. There was no conception of the number of individuals or of the number of characters which require to be measured before we can reach definite conclusions. Anthropology was considered as a field to be left for a recreation ground almost entirely to men busy in other matters, for it had developed no academic discipline of its own, until Galton's methods gave it the status and dignity of a real science.

What troubled Galton, when travel and geography in the wider sense had led him to anthropology, was not only the lack of quantitative method but the lack also of ample material¹. He at once set about supplying both in his own original way. Yet having reached some certainty himself, he proceeded, owing to the weakness of his brethren, in administering it only in homoeopathic doses. At the Brighton British Association of 1872, a recommendation was made by the General Committee, probably on Galton's suggestion, that brief forms of instruction should be prepared for travellers. Two years later the *Notes and Queries on Anthropology, for the Use of Travellers and Residents in uncivilised Lands*, drawn up by the Committee of the British Association (which included Lane Fox, Beddoe, Lubbock, Tylor, Galton and others), was issued. To the first edition of this handbook

¹ Neither lack was fully recognised even to Galton's death in many of the papers published by the representative English Anthropological Society; and I remember on more than one occasion his saying with a sigh: “Poor dear old Anthropological.” All his efforts had produced little if any impression upon its members.



Francis Galton in the late 'sixties.

Galton contributed the hundredth section entitled "Statistics." He opens with the characteristic sentence:

"The topics suitable to statistics are too numerous to specify; they include everything to which such phrases as 'usually,' 'seldom,' 'very often' and the like are applicable, which vex the intelligent reader by their vagueness and make him impatient at the absence of more precise data." (p. 143.)

He then refers to the necessity of homogeneity, the breaking up even of homogeneous groups when there is a variation largely governed by a dominant influence, e.g. age, and the need for a truly random selection. He says that precision varies as the square root of the number of observations, but that number must not be reached at the expense of accurate reporting. He then turns to the "law of deviations" and suggests the "ranking" of characters in individuals, and the measurement of the mid (500th), the 250th and the 750th individuals in ranks of a thousand, or what we now term the median and quartiles¹. The ranking gives him his so-called ogive curve, and his whole appeal to theory consists in the statement that when individual differences in a homogeneous population are due to many small and independent variable influences then the excess of the $(m+t)$ th individual if m be the mid number will equal the defect of the $(m-t)$ th individual from the mid individual. Galton does not enter into the mathematics of the matter. He says this

"law of deviations holds for the stature of men and animals, and apparently in a useful degree for every homogeneous group of qualities or compound qualities, mental or bodily, that can be named."

Galton gives no proof of the "normal curve of deviations," but suggests that it is mathematically deducible on making certain rather forced suppositions to render calculation feasible. Comparing fact, however, with theory

"wherever comparison is possible, it is found that they agree very fairly and in many cases surprisingly well." (p. 144.)

He concludes with the statement that a good book on these matters has yet to be written.

"Quetelet's Letters on the Theory of Probabilities is perhaps the most suitable to the non-mathematical reader." (p. 146.)

It will be clear that Galton was proceeding gradually, and the dose was a very small and simple one².

In the second edition we find Galton contributing some further sections.

¹ Galton then termed the 500th individual in a thousand the "average." The middle man is practically the 500th, but not so theoretically. The diagram in later editions disappeared.

² Other contributions by Galton to the first edition were No. xciv on "Population," which begins characteristically with "Count wherever you can," No. lviii on "Communications," reminiscent of the *Art of Travel*, No. liv "Causes that limit Population," No. xxi "Astronomy," with special reference to the seasons, and to steering by sun and stars. There is also (pp. 21-2) a note on heredity, giving a list of hereditary characters which admit of precise testing; "those who confuse the effects of nature and nurture give information that is of very little use." The first edition also contains a section (No. ix) on "Physiognomy," by Charles Darwin, who was collecting material for his work on Expression of the Emotions, a section which Dr Garson had the temerity to revise in later editions.

In a note on the "Physical Powers" he suggests taking three weights *A, B, C* of definite and increasing heaviness and noting how many fail to lift each, and so ranking the community. He suggests similar tests for running the same distance in specified times and for running three distances in a specified time, etc., etc. He says that such methods

"afford a complete and approximately correct picture of the distribution of the qualities tested, and not merely general averages." (p. 41.)

There is also in this edition a note on testing sight (pp. 43-4). The distance at which the tested can distinguish between two white squares on a black ground, one with the side and the other with the diagonal vertical, is recorded and used as a measure of acuity of vision.

"The testing must be performed when the light is perfectly good, but not dazzling. Always test yourself when you are testing others, because if your own efficiency comes up to its normal standard, it is fair evidence that the conditions of light etc. are normal also, otherwise very probably they are not."

Like Darwin's contribution, Galton's were "revised" in later editions, and ultimately they disappeared, perhaps desirably as they had been deprived of any characteristic value.

Galton's anthropometric projects were, however, far from being confined to travellers; he had much more comprehensive schemes in view. One of his earliest proposals was the establishment of anthropometric laboratories in schools¹, but here again he exhibited at first only the thinnest end of the wedge. He had realised that statistical material for such fundamental characters as height and weight did not exist for the British people.

"We do not know whether the general physique of the nation remains year after year at the same level, or whether it is distinctly deteriorating or advancing in any respects. Still less are we able to ascertain how we stand at this moment in comparison with other nations, because the necessary statistical facts are, speaking generally, as deficient with them as with ourselves." (p. 308.)

Galton's proposal was to take samples of reasonably homogeneous classes, and then by aid of the census to combine the returns in the proper proportions. He considered that homogeneous groups of boys, girls and youths already existed in several large schools, under conditions which offered extraordinary facilities for obtaining anthropometric data. He proposed to measure children in the great public schools, middle class schools and others down to those for pauper children (p. 311). Galton held that the masters in such schools were "trustworthy and intelligent in no common degree," that they knew their pupils well, and that the general organisation and discipline of the school was favourable to collecting full and accurate statistics. He believed that the school authorities might be induced in not a few instances to cooperate heartily and with great intelligence. Once the system of anthropometric measurement was established in schools it would spread elsewhere.

"The boys when they grow up into men would retain favourable recollections of the whole procedure, and application might then be made to Universities, Factories, and other large bodies of adults, with greater probability than at present of obtaining the required information." (p. 309.)

¹ "Proposal to apply for Anthropological Statistics from Schools," *Journal of the Anthropological Institute*, Vol. III, pp. 308-11, 1874. Paper read in 1873, proposals drafted 1872.

Galton confines his attention to the data for age, height and weight, and remarks:

“It seems to me better not to speak at present of the attractive and numerous problems that might be solved by a wider range of inquiry; because if we confine the attention of those we ask to few and simple questions, we are far more likely to have them well and thoroughly answered, than if we had issued a more ambitious programme.” (p. 310.)

Anthropometric measurements were soon taken at a number of schools and in some schools anthropometric laboratories established. From the schools they spread to the Universities (Cambridge, 1884; Oxford, 1908; London, Galton Laboratory, 1920). But on the whole there has been a tendency to take in routine fashion a few superficial measurements, and not use the anthropometric laboratory as a means of solving definite problems, physical or mental. They might still be of value if a little inspiration were thrown into their work and psychic or dynamic qualities measured rather than superficial static characteristics. One result of the proposal was those returns from the public schools, upon which Galton based his paper on the weight and height of boys in town and country schools discussed on our p. 125.

Another somewhat slender paper of this period is entitled: “Excess of Females in the West Indian Islands from documents communicated to the Anthropological Institute by the Colonial Office¹.” This paper gives statistics showing the excess of females in most of the West Indian colonies, although there is an excess of male births. The anomaly is partly due to mortality following dissipation in the young of the male sex, but more extensively to adult male emigration. The whole topic might now be rediscussed with fifty years additional statistics, and would not be without interest. As Galton remarked in 1874 each of the West Indian Islands is an individual social experiment, and each therefore deserves the pains of a separate and thorough statistical investigation.

The collection of statistical data was, however, not the only point that Galton had in view; he sought to make statistical theory simple and of easy application, and he risked the possibility that loss of refinement might involve decreased accuracy and a drawing of over hasty conclusions. His “Proposed Statistical Scale” was first given at a Royal Institution Friday evening discourse on February 27, 1874. He followed the lecture up by a letter to *Nature* on March 5, 1874². His communication embraced the idea of “ranks,” and the whole theory of ranks has been developed from this origin. It is easy to recognise that it is often less difficult to place two persons in order as to the intensities they possess of any physical or psychical character than actually to measure those intensities. A trained schoolmaster can “rank” his class for intelligence with very considerable accuracy. If a number of individuals be placed in order of their intensity for any character, they are arranged according to Galton on a “statistical scale” (S.S.). The grade of any individual is then determined by the percentage of the whole population who stand above that individual on the statistical scale. The middle man—or the man who would stand half-way between the two middle men if there were two—was later said

¹ *Jour. Anthropol. Inst.* Vol. iv, pp. 136–7, 1874. ² Vol. ix, p. 342 (abstract of lecture, p. 344).

by Galton to have the *median* value of the character (m). The two men with 75 % and 25 % of the population above them are said to have the lower and upper quartile values (q_1 and q_2). If the distribution be symmetrical about the median then $m - q_1$ and $q_2 - m$ will be equal; if the distribution obeys the so-called normal curve of deviations, then all the constants of the distribution can be found by measuring the intensity of the character in the median and in the quartile individuals. Thus Galton would place a hundred and one savages in a row, the curves formed by the apices of their heads would be his "ogive"¹ for their stature, and by measuring only the 25th, the 51st and 76th men he would obtain a reasonable distribution for the stature of adult men in that tribe.

Theoretically there are difficulties about Galton's "ogive," if we suppose it to correspond to a normal curve of deviations, in particular at the terminals. Galton endeavoured to get over these difficulties by replacing the normal curve by a symmetrical binomial, which has a finite range. He treats of this matter in a paper on "Statistics by Intercomparison with Remarks on the Law of Frequency of Error²." In this paper after mentioning that Quetelet had shown that a binomial to the 999th power was practically a normal curve of deviations, Galton goes on to indicate that the same holds very closely for symmetrical binomials of quite low powers. Thus he plots (p. 39) the Binomial Ogive of 17 elements against the Binomial Ogive of 999 equal elements, which is practically identical with the Exponential Ogive, and argues therefrom to the binomial of the 17th power being very close, indeed (which is a fact), to the normal curve³. Galton then passes to some suggestive remarks on the origin of the distribution of deviations according to the normal law. He rejects any idea of its source in a very large number of small and independent contributory causes. He supposes the exponential curve to arise because it nearly resembles the curve based upon a binomial of moderate power, i.e. he supposes that in nature the contributory cause-groups are relatively few; but he has to suppose in this case that nature works all her processes by equal additions or subtractions, i.e. prefers the mathematics of coin-tossing to those of the dice-box.

"I shall show," he writes, "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 elements [independent cause-groups as the individual coins of a combined toss] cannot exceed." (p. 40.)

Galton takes the mean (m) and divides it by the quartile deviation ($q_2 - m$ or $m - q_1$) and computes the ratio $m/(q_2 - m)$. In the case of the binomial $(a + b)^n$ this will be

$$nb/0.67449 \sqrt{nab} = 1.48257 \sqrt{\frac{nb}{a}},$$

¹ *Philosophical Magazine*, January, 1875, Vol. XLIX, pp. 33-46.

² If X be the abscissa, i.e. the rank, and Y the ordinate or value of the variate, m the mean, σ the standard deviation of the population = $1.48257 (q_2 - m)$, and N the total population, then the equation to Galton's "ogive" will be $X = \frac{1}{2} + \int_{-\infty}^Y \frac{N}{\sqrt{2\pi\sigma}} e^{-\frac{1}{2}(Y-m)^2/\sigma^2} dY$.

³ I have not been able to agree with the values given in the Table on p. 42.

and it will be clear that we could not determine n —the number of independent cause-groups in general—without a knowledge of a and b , for which we require higher moments than the first two. If we suppose with Galton that “nature tosses,” i.e. $a = b = \frac{1}{2}$, then clearly a knowledge of $m/(q_2 - m)$ will give \sqrt{n} and so determine the number of elements, or contributory cause-groups.

Galton obtains (p. 43) the following results:

Number of Elements or Contributory Groups	Value of $\frac{m}{q_2 - m}$	
	Galton	Above formula
17	5	6
32	10	8
65	15	12
107	20	15
145	25	18
186	30	20
999	48	47

The source of the divergence is two-fold. First, there is no theoretical means of discovering the quartiles in a binomial of discrete terms. Galton determined them by drawing a freehand curve through the tops of the plotted binomial blocks in order to reach a continuous ogive. This method is not capable of great accuracy. Secondly, although the standard deviation of a binomial is well known, the probable deviation (or quartile) will not be equal to .67449 multiplied by the standard deviation unless n is fairly large. Still the deviations seem too large to be wholly attributable to this cause.

I have enlarged on this matter because it provides an illustration of the cases in which a standard deviation can be determined and a quartile cannot. On the other hand there is more than an assumption of $a = b$ in Galton's method. If we are given any data, for example statures of a definite group, there appears to be no reason why the zero of stature should correspond to the start of the binomial; nature is more likely to take its additions and subtractions from some definite value, and zero stature to be not only a great improbability, but an impossibility. Further, nature's unit of addition or subtraction will not be that of the measurement of stature and this introduces another unknown. When we approach the problem with all these quantities— a , n and both the unit of addition (or subtraction) and the centre about which nature works—unknown, we can still solve the problem of fitting a binomial to our data. Experience shows, however, that in the great majority of cases the equations lead to imaginary values for our constants, or to such as are uninterpretable (e.g. n negative) on the basis of a simple binomial. In other words nature does not work on the basis of a finite number of *independent* cause-groups, such as are assumed in the binomial frequency;

it is more probable that the products of the contributory cause-groups are correlated. That is to say, that the first contribution influences later ones¹.

As far as I am aware, however, Galton was the first to endeavour to unriddle something of nature's method of working from the frequency distribution of a given variate. We may see now-a-days that his solution of 1874 is not valid, but we have to confess that we have not got much farther than he did².

In the remainder of the memoir Galton discusses

"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." (p. 42.)

He considers that small disturbing influences would weld the binomial blocks into a continuous ogive. He concludes by showing that the sum of three symmetrical binomials taken in certain proportions may lead to a result indistinguishable from a single binomial. He justifies the exponential law, or normal curve, on the ground that it is very close in the results it gives to any binomial ogive, and would propose to use it for intercomparison in cases where no scale of equal parts has been or can be applied. As we have endeavoured to show the paper is extremely suggestive, but not every reader will be induced by the arguments to accept its conclusions.

Galton, influenced by his own motto: "Whenever you can, count," seldom went for a walk or attended a meeting or lecture without counting something. If it was not yawns or fidgets, it was the colour of hair, of eyes or of skins. But the record of several characters involves a considerable effort of memory, and using a pencil invites attention to the work of the recorder. The Galton Laboratory possesses no less than five implements of a type which Galton later termed "registrators." One consists of a pair of cotton gloves; on the palmar face of one glove across the fingers is a pocket capable of containing a card, about the size of a gentleman's visiting card; just below the tip of the thumb is a thin piece of wood or metal sewn into the inside of the glove and carrying a needle point projecting very slightly through the material of the glove. If the thumb be pressed against the palmar surface of any one of the four fingers a fine hole is recorded on the card. "A great many holes may be pricked at haphazard close together without their running into one another or otherwise making it difficult to count them afterwards." Another registrator consists of a thimble which being pressed against a card or even a newspaper makes a pinhole by aid of a needle point which projects on the thimble being pressed. A third registrator is a single dotter and contains a guarded needle-point which on a slight squeeze stabs a strip of paper, the action of the instrument being such that a 'stab' slightly pulls the strip of paper forward, so that a line of dots is made; the instrument can be held in the palm of the hand in the pocket of an overcoat. Another simple pocket

¹ Nature prefers a hypergeometrical series to a binomial series!

² No stress whatever, in my opinion, can be laid on the results of those writers who believe that the direction of evolution of a character can be determined from the asymmetry or skewness of its distribution, or of those who assert that certain forms of distribution connote "instability" in the character.

recorder consists of a brass disk sliding with a range of about 3" vertically, and rather more than $\frac{1}{3}$ " horizontally, so that a needle which projects from the disk on pressing a spring is capable of holing about one square inch of visiting card supported on chamois leather. The range is adequate for the record of two, possibly three characters.

The most complete registrator was one made for Galton by Hawksley; the needle point is done away with, and the instrument records on five dials the number of separate pressures on five pins. These pins or stops communicate by a ratchet with a separate index-arm that moves round its own dial. The dials are covered by a plate which can be removed to read off the results. The instrument is $\frac{1}{4}$ " thick, 4" long and $1\frac{3}{4}$ " wide and it can be held unseen in either hand with a separate finger and thumb on each stop. When any finger is pressed on the stop below it the corresponding index-arm records a unit. Guides are placed to keep the fingers in their proper positions. The instrument may be used in the pocket or under a loose glove or other cover. "It is possible by its means to take anthropological statistics of any kind among crowds of people without exciting observation, which it is otherwise exceedingly difficult to do¹." I may remark that it requires some little training to press with the correct finger. With an instrument of this kind Galton recorded the percentage of attractive, indifferent and repellent looking women he met in his walks through the streets of various towns with the object of forming a "Beauty-map" of the British Isles—a project he never completed, although he held London to have most and Aberdeen fewest beautiful women of the towns he had observed. He once also remarked to me that he had found Salonika to be the centre of gravity of lying, though I have no direct evidence that he used a registrator to tick off liars and truth-speakers in his travels in Greece.

While busy with his *Hereditary Genius*, 1869, Galton had noticed how apt are the families of great men to die out and that genius has been asserted to be related to sterility. He endeavoured to explain the matter in the case of the judges and in the case of peers by special causes (see our pp. 93–96). De Candolle also referred to this topic in his *Histoire des Sciences*, four years later, and suggested without mathematical investigation that families in the *male* line must always tend to die out, the name becoming extinguished when a son failed to be born. He suggested that a mathematician ought to be able to solve this problem of the extinction of surnames. Galton saw the importance of the determination of the rate of extermination of surnames as a preliminary investigation to the inquiry as to the dying out of the families of men of ability, in whose cases heredity had been too often traced in the male line only—e.g. the extinction of peerages granted for great achievements—and this extinction of the line attributed to some unexplained sterility in able men. Galton accordingly propounded the problem in the *Educational Times*, and there it met with poor success at first—one erroneous solution. Ultimately the late H. W. Watson, a personal friend of Galton's,

¹ See the paper: "Pocket Registrator for Anthropological Purposes," *British Association Report*, Swansea, 1880, p. 625.

was persuaded to take it up and sent his discussion of it to the above Journal¹. His discussion with certain preliminary remarks by Galton was also published in the *Journal of the Anthropological Institute*². The kernel of Watson's paper is as follows: The symbols $t_0, t_1, \dots, t_s, \dots, t_q$ denote the chances of a man having no children or one, \dots, s, \dots, q children. Then the chance of a surname having s representatives in the next succeeding generation, if it has p in any generation, will be the coefficient of x^s in the multinomial

$$(t_0 + t_1x + t_2x^2 + \dots + t_qx^q)^p = T^p, \text{ say.}$$

Let ${}_u m_v$ be the fraction of N , the original number of distinct surnames, which in the u th generation have v representatives, then the number of surnames with s representatives in the v generation must be the coefficient of x^s in

$$\{ {}_{r-1} m_0 + {}_{r-1} m_1 T + {}_{r-1} m_2 T^2 + \dots + {}_{r-1} m_{q^{r-1}} T^{q^{r-1}} \} N = f_r(x) N, \text{ say.}$$

It follows that ${}_{r-1} m_1, {}_{r-1} m_2, \dots$ are the coefficients of x, x^2, \dots in the expression $f_{r-1}(x)$. As soon as the t 's are known, it should be possible, although laborious, to find the succession of functions given by

$$f_r(x) = f_{r-1}(t_0 + t_1x + \dots + t_qx^q).$$

As the numerical values of the t 's are not known, Watson takes two hypothetical systems. In the first he takes $q=2$ and $t_0 = t_1 = t_2 = \frac{1}{3}$. He finds by a brute-force expansion that out of a million distinct surnames 333,333 will disappear in the first, 148,147 in the second, 89,620 in the third, 70,030 in the fourth, and only 34,150 in the fifth generation. In this case the total male population is clearly constant and two-thirds of the surnames have disappeared in five generations. Watson's second hypothesis is that the t 's are the successive terms in the binomial

$$(\lambda_1 + \lambda_2)^q,$$

where $\lambda_1 + \lambda_2 = 1$. In this case

$$f_1(x) = (\lambda_1 + \lambda_2 x)^q, \text{ and } {}_1 m_0 = \lambda_1^q,$$

$$f_2(x) = \{ \lambda_1 + \lambda_2 (\lambda_1 + \lambda_2 x)^q \}^q,$$

and

$${}_2 m_0 = (\lambda_1 + \lambda_2 \lambda_1^q)^q$$

$$= (\lambda_1 + \lambda_2 {}_1 m_0)^q = \lambda_2^q \left(\frac{\lambda_1}{\lambda_2} + {}_1 m_0 \right)^q,$$

and generally

$${}_r m_0 = \lambda_2^q \left(\frac{\lambda_1}{\lambda_2} + {}_{r-1} m_0 \right)^q.$$

The extinctions in each generation can then be easily calculated. Watson takes the case of $\lambda_1 = \frac{3}{4}, \lambda_2 = \frac{1}{4}$ and $q=5$. In this case the t 's are

$$t_0 = \cdot 237, \quad t_1 = \cdot 396, \quad t_2 = \cdot 264, \quad t_3 = \cdot 088, \quad t_4 = \cdot 014, \quad t_5 = \cdot 001,$$

and the extinctions in the first ten generations of 1000 original distinct surnames:

$$237, \quad 109, \quad 65, \quad 40, \quad 27, \quad 18, \quad 14, \quad 10, \quad 7, \quad 6,$$

¹ *Educational Times*, Vol. xxvi, 1873, p. 17 and p. 115.

² Vol. iv, pp. 138-44, 1874.

or a total loss of 533 surnames. Here the population increases since

$$t_1 + 2t_2 + 3t_3 + 4t_4 + 5t_5$$

is greater than unity. As before the extinction rate is quick to begin with but soon slackens down, as the number of persons holding each surname increases, while the number of surnames diminishes. On the above hypothesis nearly a quarter of newly-created peerages would become extinct in the first generation and half of them by the sixth generation. With any such hypothesis there is no need to appeal to sterility as rendering rapidly extinct a large proportion of the peerages created for ability. It will be clear that if we take not the number of sons, but the number of children, in computing the t 's, the problem becomes that of the extinction of definite stirps; it is highly probable that families die out in approximately the same manner as they die out in the male line. If mankind has not sprung from a single pair, it seems possible that even the most numerous nation may tend with the ages to be the product of a very few stirps, if not of a single pair. The fable of Adam and Eve may be somewhat truer for an old world than for a young one!

Beside the data noted in the paper on the stature of boys from urban and rural schools¹, several schools provided material of a more extended kind, notably Marlborough School, which had established something like an anthropometric laboratory². The school medical officer and the natural science master took the measurements: namely weight, stature, horizontal circumference of the head, chest girth, girth of the flexed arm over the biceps muscle, girth of the leg over the calf, both the last two being the maximum measurements. The ages of the boys ranged from 10 to 19 and there were 550 of them. The authors of the paper give three correlation tables for age with stature, weight and head circumference, but make no reductions, citing merely in the case of the extreme boys in each measurement the other measurements of those boys. One remark deserves citing. The authors state that they "are unable to trace any distinct connection between intellectual vigour and head measurement; for although many of those who possess the higher girths of head are intelligent boys of considerable ability, it must be confessed that many boys whose heads measure less than 22 inches, are in ability, perseverance, and general culture, quite equal to those who possess the higher measurements." (p. 129.)

This remark bears on a point already referred to in this *Life* (p. 94).

Galton's short accompanying paper confines itself to one character, stature, and he tells us that he proposes to illustrate the statistical methods which will be adopted, when sufficient material of a homogeneous nature is available. He takes the boys for each year of age and finds their means, which give for the central ages $12\frac{1}{2}$, $13\frac{1}{2}$, etc. the law of growth. He thus obtains what we should now term the regression line. But here he strikes a new point: he finds that the arithmetical means of the arrays are not identical with

¹ See our p. 125.

² "On a Series of Measurements for Statistical Purposes, recently made at Marlborough College." By Walter Fergus, M.D., and G. F. Rodwell, F.R.A.S. *Journal of the Anthropological Institute*, Vol. iv, pp. 126-30. "Notes on the Marlborough School Statistics" by Francis Galton himself follow this paper, pp. 130-5.

what he terms the "typical means." The normal curves for the age-arrays are not fitted to the arithmetical means, but to the "typical means." Galton does not describe how he obtained either "typical means" or normal curves; probably but not certainly his "typical means" are not "modes," but what he later termed "medians," and his normal curves were then found from the quartiles. The median or typical mean in all cases corresponds to a less stature than the arithmetic mean; and there is thus some little evidence that the arrays are asymmetrical and are not normal curves. Galton emphasises the point that we have really not a mere system of arrays, but a continuous frequency surface. Further he points out that the variation of these arrays widens as the age increases, a condition we now know to be incompatible with a normal frequency surface. It is a striking fact that in this first anthropometric surface Galton should have actually run up against the line of medians generally diverging from the line of means, when the variability of the arrays is not constant, i.e. that he should have come across the asymmetrical frequency surface, which is still proving a hard nut to crack.

We have seen how Galton urged anthropologists to turn from the sole discussion of external physical characters to the mental characters in man, such as personal equation and rapidity of judgment¹. He early perceived the importance of the school not only for anthropometric physical but for anthropometric psychical measurements, and he endeavoured to enlist the schoolmaster in the service of psychical anthropometry. He rightly looked upon the school as not only an institution for educating the young, but as a laboratory for studying their mentality, and so by increased knowledge of psychical character improving education. In 1880 Galton wrote a strong letter on this point to *Nature*² as a result of his receiving from Mr W. H. Poole, then science master of Charterhouse, very valuable material on visual images (see our p. 237). It seemed to Galton—as it has often seemed to some of us who do not fully realise the pressure of school routine work—that other schoolmasters might emulate the exceptional Mr Poole.

"The observation I desire"—writes Galton—"to make is that as every hospital fulfils two purposes, the primary one of relieving the sick, and the secondary one of advancing pathology, so every school might be made not only to fulfil the primary purpose of educating boys, but also that of advancing many branches of anthropology. The object of schools should be not only to educate, but also to promote directly and indirectly the science of education. It is astonishing how little has been done by the schoolmasters of our great public schools³ in this direction, notwithstanding their enviable opportunities. I know absolutely of no work written by one of them in which his experiences are classified in the same scientific spirit as hospital cases are by a physician, or as other facts are by the scientific man in whose special line of inquiry they lie. Yet the routine of school work is a daily course of examination. There, if anywhere, the art of putting questions and the practice of answering them is developed to its highest known perfection. In no other place are persons so incessantly and for so long a time under close inspection. Nowhere else are the conditions of antecedents, age, and present occupation so alike as in the boys of the same form. Schools are almost ideally perfect places for statistical inquiries....If a schoolmaster were now and then found capable and

¹ Address to Section H, British Association, 1877: see our p. 228.

² May 6, 1880, Vol. XXII, p. 9.

³ The stress laid on the appointment of classical and clerical heads, to the neglect of scientific candidates, largely accounts for the matter.

willing to codify in a scientific manner his large experiences of boys, to compare their various moral and intellectual qualities, to classify their natural temperaments, and generally to describe them as a naturalist would describe the fauna of some new land, what excellent psychological work might be accomplished? But all these great opportunities lie neglected. The masters come and go, their experiences are lost, or almost so, and the incidents on which they were founded are forgotten, instead of being stored and rendered accessible to their successors; thus our great schools are like mediaeval hospitals, where case-taking was unknown, where pathological collections were never dreamt of, and where in consequence the art of healing made slow and uncertain advance.

Some schoolmaster may put the inquiry: What are the subjects fitted for investigation in schools? I can only reply: Take any book that bears on psychology, select any subject concerning the intellect, emotions, or senses in which you may feel an interest; think how a knowledge of it might best be advanced either by statistical questioning or by any other kind of observation, consult with others, plan carefully a mode of procedure that shall be as simple as the case admits, then take the inquiry in hand and carry it through."

I have cited Galton at length because in 1924 his words remain as true as in 1880, but I have faint hope that they will by repetition here reach a new generation of teachers more responsive than the old. In this country we have exceptional men who promulgate new ideas, but the average mind is an inert one. The school as laboratory, the factory as laboratory, the prison as laboratory, and the asylum as laboratory, these are essentially true conceptions, but their truth and their profit will be seen in America, in Germany—even in France—before they are grasped here! Galton scarcely realised that it required greater *ingenium* to discover a solvable problem than to carry it through when propounded, and that the average schoolmaster finds it easier to take prescribed measures of his boys—even to fill folios with them—than to discover an important problem and design new measurements to solve it. The school anthropometric laboratory must be futile if it be only a laboratory of record and not one of discovery. The fault lies rather with our current academic training than with the schoolmaster—for it lays greater stress on the average man solving set problems, than on finding novel problems himself.

The boy is never discouraged, and Galton retained his boyhood to the end. He could put on one side his teaching as to eugenics because the time was not ripe for it, and propound it with all his youthful enthusiasm nearly forty years later; the relative barrenness of the harvest resulting from his school anthropometric proposals did not cause him to despair of profits resulting from anthropometric inquiry in schools. In the eighty-third year of his life, thirty-three years after his first attempt, he returns to the charge, and with additional proposals, which would immensely increase the work—while needless to say they would enormously increase the utility—of school anthropometric laboratories.

In 1905, at the London Congress of the Royal Institute of Public Health, Galton gave an address on "Anthropometry at Schools¹."

"**Anthropometry**, or the art of measuring the physical and mental faculties of human beings, enables a shorthand description of any individual to be given by recording the measurements of a small sample of his dimensions and qualities. These will sufficiently define his bodily proportions, his massiveness, strength, agility, keenness of sense, energy, health, intel-

¹ *Journal of Preventive Medicine*, Vol. xiv, pp. 93-98. London, 1906.

lectual capacity, and mental character, and will substitute concise and exact numerical values for verbose and disputable estimates¹. Its methods necessarily differ for different faculties; some measurements are made by the foot-rule, others by scales, others by the watch; health is measured by the frequency and character of illness; the remainder by performances in the school or on the playground. Anthropometry furnishes the readiest method of ascertaining whether a boy is developing normally or otherwise, and how far the average conditions of pupils at one institution differ from those at others. Though partially practised at every school—for example in all examinations—its powers are far from being generally understood, and its range is much too restricted. But as an interest in anthropometry has arisen and progressed during recent years, it is to be expected that the good sense of school authorities, assisted by the expert knowledge of medical men, anthropologists, and statisticians, will gradually introduce improvements in its methods and enlargement of its scope.”

This passage is noteworthy as it indicates how fully Galton had come to realise that the complete anthropometric laboratory must take measurements not only of statical physique and psychical characters, but also of the dynamic workings of the body, and generally of its physiological and medical fitness. What a stage onwards from that thin end of the wedge which suggested a measurement of stature, and obtained some half-dozen statical characters! But when we have got all this information, what is its value? Galton was not bent on describing what the school anthropometric laboratory should do for the boy, but what it should do for the man into which he developed. He regretted the deplorable and widespread lack of knowledge of the true value of anthropometric forecasts. Who can answer the questions:

“How far does success or failure in youth foretell success or failure in later years? What is the prophetic value of anthropometry at school in respect to health, strength and energy in after-life?”

Indeed these matters are only yet on their trial: Will the data collected in a fully equipped anthropometric laboratory recording the physical, mental, medical or other characters be able to make a forecast of the best career for a young man, or the probable success or failure in after life of its examinees? It will take twenty to thirty years to correlate well-selected measurements with experience in after careers. Galton realised this and wished to prepare the way for obtaining a life-history of the boys who had been measured in the school anthropometric laboratories.

“The first conclusion to be emphasised is that no programme for anthropometry in any school can be considered complete unless it provides for the collection of data during the after-lives of their pupils.”

Every fourth year, Galton suggests, the “old boy” should receive a schedule and return it with an account of his doings in life, his health, vigour, his profession and achievements, his marriage and children. These four-yearly reports would be combined in one dossier with his school anthropometric measurements record. The schedule of these records would leave a space for one sheet of family history to be obtained from the boy’s parents when he was about to leave school, which he himself would verify later, and there would be space for a few photographs.

Such was Galton’s scheme in brief abstract. It will be seen to approach closely the eugenic record proposed many years previously, but now asso-

¹ It would be difficult to excel this passage as a description of anthropometry.

ciated with more detailed anthropometric measurements, and with the school as record office.

“The school authorities would rejoice in the possession of the whole history of those over whose early development they exercised large control. Anthropologists would know where to lay hands on a mass of material suitable for comparing the health, bodily qualities, and scholastic achievements in early life with the health, vigour and achievements afterwards. Statisticians would possess a four-yearly census, out of which unexpected conclusions would probably be derived. Lastly some few of the records would be invaluable to future biographers. . . . The effect of the present proposals would be . . . to prolong and intensify the kindly fellowship between past and present pupils and their school, and to make it serve more than sentimental purposes. The addition of a scientific motive could not fail to invest that relationship with a more durable and businesslike character, and to open a way to fields of research of no small importance that have hitherto been neglected.”

Galton suggests that the return of the four-yearly schedule should always be made on February 29th, thus associating the return with Leap Year. On this day there should be school regatherings¹ and thoughts of the old school and former friends should predominate.

“The celebration of the day in schools would be much concerned with the works of living men, who were formerly pupils, but then engaged in the battle of life. Their doings would be spoken of, and hearty sympathy evoked. Affection and duty should co-operate in maintaining the bands of fellowship between school and former scholars; in short, its maintenance should be considered a ‘pious’ object.”

“The child is father to the man,
And I would wish my days to be
Bound each to each by natural piety.”

Galton’s dream was a noble one, if the time for its fulfilment be not yet. Possibly it may one day be realised in ways the dreamer thought not of. I cite it here to show how rarely he let fall, rather more often amplified in his old age, the ideas of his younger days.

It must not be thought that Galton’s principle: “Count whenever you can,” led him to a slavish admiration of all types of statistics. There is a very striking illustration of the contrary. In 1877 the Council of the British Association had been much troubled by the proceedings of Section F (Economic Science and Statistics), and appointed a committee to report on

“the possibility of excluding unscientific or otherwise unsuitable Papers and Discussions from the Sectional Proceedings of the Association.”

While Galton reserved a final judgment the remarks he put before the committee were adverse to the maintenance of Section F. He analysed all the papers of the years 1873–75 and remarked that

“not a single memoir treats of the mathematical theory of Statistics, and it can hardly be doubted that if any such paper should be communicated to the Association, the proper place for it would be Section A.”

Galton admitted that Section F dealt with numerous and important matters of human knowledge, but such as are akin, for example, to History, not to Science, and are therefore inappropriate subjects for the British Association.

“Usage has drawn a strong distinction between knowledge in its generality and science, confining the latter in its strictest sense to precise measurements and definite laws, which lead

¹ Revisits of the old pupil to his school after a long interval might provide opportunities for recording a few simple measurements such as weight, stature, eyesight, strength.

by such exact processes of reasoning to their results, that all minds are obliged to accept the latter as true. It is not to be expected that these stringent conditions should be rigorously observed in every memoir submitted to a scientific meeting, but they must not be too largely violated; and we have to consider whether the subjects actually discussed in Section F do not depart so widely from the scientific ideal as to make them unsuitable to the British Association."

Galton's test of what constitutes science is clear—it is that of a mathematical physicist—and rigidly applied it would exclude large regions of biology, including possibly the doctrine of evolution.

But it emphasises exactly Galton's feeling with regard to much of what passed for statistics in 1877, that old type of statistics which had no theoretical basis, while Galton was working for a new type; he would willingly have transferred Section F to the Social Science Congress. But what could be said against Section F applied equally to and remained true till at least the end of the nineteenth century of the Royal Statistical Society itself. The opposite point of view was taken by Dr W. Farr; he cited a long list of *mathematical* statisticians from Halley to Poisson, who were undoubtedly men of science. But this was no real reply to Galton, for these men would have frequented Section A, and the atmosphere of Section F, or indeed of the Statistical Society, would have been as distasteful to them as to Galton. Probably the right procedure would have been to permeate Section F with the newer type of statisticians. This process has been more or less successful in the course of the last twenty years in the case of the Statistical Society. There is still opportunity for the modern school of statisticians to adopt a similar course with regard to Section F.

B. STATISTICS BY SCHEDULE-ISSUES.

We have seen that Galton had great hopes from the schoolmaster as a collector of statistical data, but he by no means confined himself to this source of information. We have also noted how he appealed to English men of science, and to his many personal friends and others, by issuing schedules in preparing his books. The *Galtoniana* contain numerous instances in which he issued inquiry schedules, and in some cases we possess considerable numbers of these filled in. As a rule, however, I cannot find that he published anything bearing on the subject of the proposed inquiry. Either he never issued the schedule after printing it, or having issued it he was discontented with the quantity or the quality of the returns, and so made no use of them. Yet several of these schedules are so suggestive of the workings of Galton's mind, that they deserve a brief notice here.

Before 1876 Galton was much interested not only in the inheritance of longevity, but also in the influence of the age of parents on the vigour of their offspring. The schedule he issued is entitled "Inquiry into the Relation between Vigour in the Offspring and Age in the Parents," and it is prefaced by the remark:

"Instances are sought of old persons of both sexes, who have retained their bodily vigour and activity in very advanced life. It is desired to know the ages of their fathers and mothers at the time of their birth."

The schedule contains spaces not only for the facts illustrating the special vigour in old age of the subject, but for the ages of his parents at his birth, the size of his co-fraternity and his position in it, and further for other instances of exceptional longevity in the kinship. Among the somewhat meagre data collected are several instances of marked hereditary longevity, and one of a man who above eighty became the father of healthy children. Galton was undoubtedly interested in this inquiry owing to the hereditary longevity in his own family¹, but the knowledge of this fact did not relieve him from having at times considerable anxiety as to his own health, and in the sense of the proverb "that cranky doors hang longest on their hinges," he was interested to know whether "a considerable proportion of aged persons have been more or less ailing through a great portion of their lives."

I am not sure whether a printed document I have found with the longevity dossier was issued with the schedule or prepared for some later inquiry; it bears evident traces of Galton's complete or co-operative production. It is so suggestive for an inquiry which apparently has never been made, and still might be made with great profit, that I have reproduced it bodily here.

Those who knew Galton personally will trace some of his beliefs and some of his doubts seeking statistical confirmation in this document.

An Inquiry concerning Persons who have attained or passed the Age of Eighty Years.

This inquiry, as will be seen by the card, is intended to be general, the object being to obtain by Collective Investigation on a large scale, information respecting the present and past condition, habits, and maladies, as well as the family history and other circumstances, of those who have attained to advanced periods of life, in order that we may be able to ascertain, with greater certainty than we now can, what are the circumstances which favour longevity, the means by which it may be promoted, and the maladies which are most, and those which are least incidental to it.

The following are some of the questions which arise in connection with this subject, and for answers to which we may look.

What bodily conformation, temperament, and habits, are most associated with, or conducive to, longevity?

Do women more frequently attain to great age than men, and have women somewhat below the ordinary stature the advantage in this respect?

Are the married or the unmarried, the stout or the spare, the active or the sedentary, the industrious or the idle, the indoor student or the outdoor workers, the well-to-do or the poor, the town dwellers or the country dwellers, the more likely to become octogenarians?

It is said that "small eaters and short sleepers are long livers." Is this so? Will the "early to bed and early to rise" maxim receive confirmation? What is the influence of alcohol?

It has been remarked that a considerable proportion of aged persons have been more or less ailing during a great part, or the whole, of their lives. Is that the case? It has also been remarked that many of them have been troubled with constipation, and that many have long been in the habit of resorting to aperient medicine.

The cartilages of the ribs and the trachea have been found soft and elastic in some very aged people, old Parr forming no exception in this. Should this be shown to be generally the case, the inference would follow that persons in whom they are not so are not destined to attain to great age.

Do octogenarians often suffer, or do they enjoy a comparative immunity from affections of

¹ See *Memories of my Life*, p. 7—a paragraph which contains the only reference I have seen to the age of Elizabeth Collier's mother at death, if it be not a slip for the age at death of Erasmus Darwin's mother.

the urinary and genital organs, and of the abdominal organs, also from malignant disease and scrofula?

Are they on the whole comparatively exempt from disease?

To what affections are they most liable, and to what morbid influences are they most susceptible? Do any maladies seem to have an influence in promoting longevity? What influence upon the longevity of an individual has the age of the parents at his birth? Do twins or the children of twins often attain great age?

Information, though not positive, yet of much interest and importance, upon these and other points, will accrue from the replies to the questions on the cards. It need hardly be said that the questions are by no means exhaustive, and that information upon other points which are judged to be of interest and importance by those who fill up the cards, will be valuable, as also information on any special points in particular cases which seem worthy of note.

Though the questions are such that they may for the most part be answered by the persons themselves, or by their friends, it is hoped that in most instances the observations will be made and the information given by medical men; and the person who fills up the card is in each instance requested to state whether he is a medical man or not.

It will be an additional advantage if some information can be gleaned respecting the succession of maladies in the same person, and in different individuals of the same family, or respecting the preservative influence upon the system of certain maladies against the inroads of others.

Something in the Hereditary Problem may be also learned respecting the cross-action and modifying influences of certain diseases. For instance, is there any foundation for the view that chronic gouty affections retard the development of other diseases.

The strength and enduring quality of the body, like that of a chain, must be measured at its weakest point; and though in it, more than in a chain, the strength and quality of some parts may compensate for deficiency in others, yet the very opposite may be the result. The stronger organs may relieve, but they may also oppress, the weaker members. A strong digestive system may overload a weakly circulation, and prove injurious to the liver, lungs, or kidneys, in fact a disturbing agent to the general nutrition. The requisite for longevity, therefore, we may expect to be not so much strength of organs as their enduring quality, a good mutual adjustment, in other words, their good balance. The replies relating to "plethora" and other features of general condition will have an important bearing on this view.

Another matter about which Galton's mind was much exercised was that of "social stability." He was anxious to know whether and to what extent individual stirps move up and down in the social scale, or whether our society is in the main made up of "castes," which stand fast by their grade of occupation and to their social position. To throw light on this matter he sought a comparison between the average social position of householders of all classes in the present day and in that of their fathers. He prepared accordingly a schedule—which appears never to have been issued—of inquiries concerning householders and their fathers; the age of the householder, his profession, trade, or employment, his position among men of the same occupation, and the corresponding data for the father of the householder were to be recorded, and Galton hoped to get material ample enough to provide a measure of social stability—the frequency with which sons advanced on, remained equal to or regressed from the father in social status. While this schedule deals with social stability in its narrower economic sense, the same dossier contains notes by Galton giving the term social stability a much wider significance; he notes that many view with alarm the progressive disappearance of those ancient landmarks—such as theological beliefs—by which conduct has been traditionally regulated and fear that mankind must sink into brutality. The motives which lead to social stability ought therefore to be measured and analysed, the main fact being that it is discoverable

among the most divergent populations and the most varied surroundings. The inquiry, Galton tells us, must be *statistical* in its character, referring to the acts of a population as a whole, and not regarding the units of which it is composed, for it is only in this way that we can neutralise and eliminate the effects of individual character and circumstances, which are far too numerous to be severally allowed for. Galton's notes then turn to the "valuation of motives," and he asserts that while they are of the most varied kind, they are yet *commensurable*; they may be equally efficacious in producing a particular result. "There are an indefinite variety of bribes, and experience shows the amount of bribe of each several sort that is necessary to produce a given average result¹." The attractive forces of each of many shows at a fair, appealing to many diverse tastes, are comparable in a statistical sense without any other reservation by the money they take. I will not venture to cite more of Galton's rough notes; he was thoroughly convinced that "motives" like other psychical characters are capable of statistical evaluation. To press the matter would be to call forth from some readers a protest similar to that which the editor of the *Spectator* made after Galton's Royal Institution Lecture of 1874, wherein he applied the method of "ranking" to psychical characters.

"We can only express our wonder, and repeat our belief that what Mr Galton has succeeded in doing, is in exposing the utter inapplicability of physico-scientific methods to intellectual and moral subjects... We can imagine no more profitless or idle task than the attempt to draw out a Statistical Scale (say) of Candour or of Power of Repartee, and to arrange the public men of this generation in it, except indeed doing the same thing for a considerable number of qualities, and giving the reasons for the place assigned in the biographies, which would be rendered unreadable by the process²."

There might be difficulty in "ranking" Gladstone and Disraeli for "Candour," but few would question John Morley's position relative to both of them in this quality. It would require an intellect their equal to rank truly in the quality of scholarship Henry Bradshaw, Robertson Smith and Lord Acton, but most judges would place all three above Sir John Seeley, as they would place Seeley above Oscar Browning. After all there are such things as brackets, which only make the statistical theory of ranking slightly less simple in the handling.

Drafted much about the same time was Galton's first circular on "Fatigue," by which he sought to measure any permanent ill effects of mental work. This again was a topic on which Galton felt strongly, having his own experience always in mind. The proposed circular was to be addressed apparently to the fellows and scholars in Cambridge (and possibly Oxford) Colleges, and related not only to mental overwork, but to its possible association with physical overstrain, in both school and college periods of life. He probably

¹ "The gingling of the guinea soothes the hurt that honour feels"—which is not exactly Tennyson. Galton was wont to say, on seeing a hilarious party of middle-aged persons, that it struck him as strange that notwithstanding their glee they were all of them orphans.

² See the *Spectator*, May 23, 1874, and Galton's letter with the editorial rejoinder May 30, 1874. "It is about time we drew the *Spectator* again," W. Kingdon Clifford would say, and Galton was only too apt to do so without malice prepense!

refrained from circulating his questionnaire, as so many of the recipients might reasonably associate "mental overstrain" and "mental breakdown" with a form of mental illness they would be unwilling to admit having suffered from. As we have seen¹, Galton took up the topic again in 1888, endeavouring to obtain the requisite data from school teachers.

The next circular I pick up is entitled: *Ethnological Inquiries on the Innate Character and Intelligence of Different Races*. By Francis Galton. The object of these inquiries is clear, they were intended to obtain statistical data upon which a judgment might be made as to how far racial character or training influences the mental characters. The "subjects" dealt with are to be those "who have been reared since childhood in European or American schools, families, asylums or missionary establishments. By this restriction, it is hoped to eliminate all peculiarities that are due to the abiding influence of early education, and to the manners and customs of their own people." The *standard* to be kept in mind in answering these questions is the *average* Anglo-Saxon character; paying strict regard to the influence of sex, age, education and social position. Where there is no *decided* divergence from this standard, it will be best to reply—"ordinary."

The *Galtoniana* contain no replies to this circular; I do not know whether it was ever issued in mass, nor have I anywhere seen a reference to it, nor to data obtained by its circulation. The origin of it may be connected with the idea conveyed by Galton's treatment of unlike twins under like environment (see our p. 126 *et seq.*). As we might suppose the questions are well chosen, and bear closely on Galton's own experience with uncivilised races. As the questionnaire would be distinctly helpful to anyone embarking on an inquiry of like kind—and one might be well worth pushing with more vigour than Galton seems to have given to the matter—I reproduce the questionnaire here:

1. Signature, title and full address of the sender of the information.
2. Name or initials, sex and age of individual whose character is described.
3. His (or her) country and race. State specifically if his race is known to be pure, if not describe the admixture.
4. Age at which he was removed from his parents and people, also particulars showing the extent to which he has since been separated from their influence.
5. What language, or languages, does he commonly speak? Does he retain the use of his native tongue?
6. State any circumstances that may or may not justify his being considered a good typical specimen of his race.
7. Is he capable of steady and sustained hard labour; or, is he restless and irregular in his habits?
8. Is he capable of filling responsible situations? Does he show coolness of temper when in difficulty? (It is said that Hindoos are incapable of steering large ships, that is, of acting as quartermasters; while in British vessels that duty is commonly performed by native Christians of the Philippines.)
9. Is he docile or obstinate?
10. Children of many races are fully as quick, and even more precocious than European children, but they mostly cease to make progress after the season of manhood. Their moral character changes for the worse at the same time. State if this has been observed in the present instance.

¹ Cf. our p. 276.

11. Has he any special aptitudes, or the reverse, such as in mimicry, sense of the ludicrous, taste in colours, music, poetry, dancing, calculating power, keenness of sight or hearing, quickness of observation, manual dexterity, horsemanship, ability to tend cattle?
12. Is he naturally polished and self-composed in manner or rude and awkward?
13. Is he modest and self-reliant, or servile and cringing? Is he vain?
14. Is he solitary or sociable; morose or cheerful?
15. Is the passion of sexual affection strongly developed in him, or the reverse?
16. Is he fond of children, and are children fond of him?
17. Does he cherish malice for long periods, or does he forgive frankly?
18. Is he liable to outbursts of rage?
19. Did he for long show uneasiness at the restrictions of civilised life, or did he readily accept them; such as keeping regular hours, acting on a steady system, wearing shoes and other clothing?
20. Children of savages, who have been reared in missionary families, have been known to throw off their clothes, and quit the house in a momentary rage, and to go back to their people, among whom they were afterwards found in apparently contented barbarism. State authentic instances of this, if you know of any, with full particulars.
21. Has he a strong natural sense of right and wrong, and a sensitive conscience?
22. Does he exhibit to his religious teachers any strong conviction of an original sinfulness in his nature, or the reverse?
23. Is he much influenced by ceremonial observances, such as those of the Roman Catholic Church?
24. Is he a willing keeper of the Sabbath?
25. Has he any strong religious instinct; is he inclined to quiet devotion?
26. Is he ascetic, self-mortifying and self-denying, or the contrary?
27. Is he inclined to be unduly credulous or unduly sceptical?
28. Is he active or impassive in social duties?
29. Is he much governed by superstitious feelings, such as [are indicated by the use of] charms or omens of good or ill luck?
30. Has he any tendency to be sanctimonious and hypocritical?
31. Is he honest, truthful and open, or cunning and intriguing?
32. Is he grateful or ungrateful?
33. Does he, in conversation, make frequent use of abstract terms? Does he adequately understand their meaning when he employs them?
34. Are there any other marked peculiarities in his character or intellect?

Please address copies to
 FRANCIS GALTON,
 42 Rutland Gate,
 London.

This is a schedule which—if the employers of native labour could be induced to fill it up accurately in large numbers—would still be certainly of much value.

Francis Galton's next venture was entitled:

*Inquiry into the alleged Darkening of the Hair of the English
 in the Present and Recent Generations.*

The explanation of the reason for the inquiry is given on the back of the schedule. It had been alleged that on the whole the hair of English children was darker than that of their parents, and it was asserted that the English race was gradually but surely becoming dark-haired. The object of the inquiry was to test the truth of this statement. Galton remarks that it is probable that the recent and rapid changes in English habits may have caused certain sub-types, that were previously repressed, to prevail in the

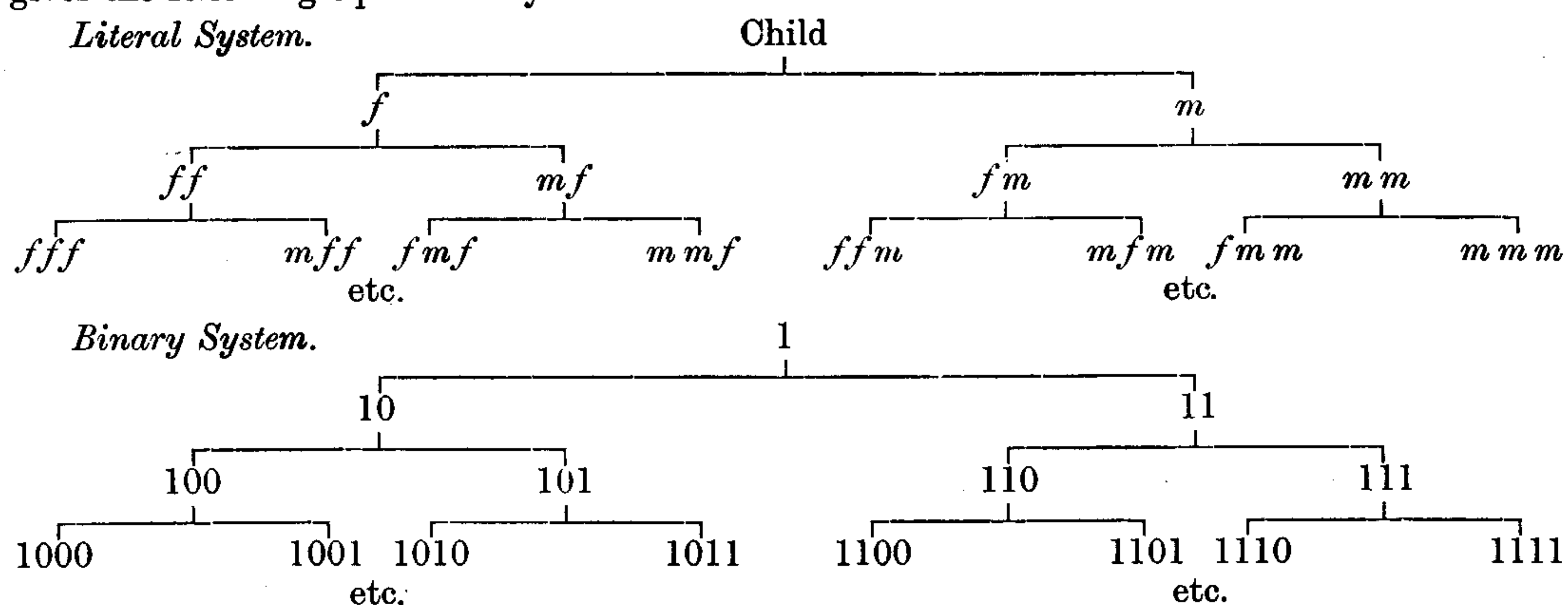
struggle for existence, and that it is of interest to know what these subtypes are. The colour of animals is often found to be intimately correlated with their power or incapacity to thrive under certain conditions, and it may well be the same in the case of man. Galton cites Baxter¹ to prove that in America, where the pressure of life peculiar to modern civilisation is even greater than with us, the black-haired persons are less liable to nearly every form of disease than the fair-haired. He observes, however, that it is needful at the same time to determine the relative fertility of the light and dark haired, and that it would be very important to distinguish between the children of a dark-haired man who had sprung from a light-haired stirp, and those of a similar man from a dark-haired stirp. The schedule is fairly straightforward and contains the first statement of Galton's system of numerals for relationship, i.e. child 1, parents 2, 3, grandparents 4, 5, 6, 7, etc., the even numbers standing for males and the odd for females (No. 1 excepted, which may have either sex); the number of any individual when doubled gives that of his father, and his mother's number is obtained by the addition of one to the number of his father². The characteristic Galtonian statement is made incidentally that:

"The inquiry will have the merit of being accompanied by incidental pleasures; it will be an excuse for corresponding with distant friends and relations on topics of common interest, and it is probable that not a few facts of family history much prized by its members will in many cases be incidentally brought to light by its means."

Galton himself was so interested in family history that he quite naively overlooked the fact that nine-tenths of humanity either fear to examine it or are frankly bored by it. Against that dead-weight of inertia Galton could effect little, and there is no evidence that these circulars were ever returned in sufficiently adequate numbers to serve as a basis for an answer to his inquiry.

¹ *Medical and Anthropological Statistics of the Provost-Marshal-General's Bureau*, Washington, 1875.

² Galton published a letter on this numerical system of relationship in *Nature*, Sept. 6, 1883, under the title: "Arithmetic Notation of Kinship." Taking f = father of, m = mother of, he gives the following equivalent systems of notation:



The Binary System is cumbersome but simple, we add a zero for the father and a unit for the mother of any individual to that individual's number. The decimal system is as follows:

The next schedule I have come across is termed a "Biographical Register." It starts with a genealogy of the subject as far as the grandparents and their descendants with a space for more distant relatives. Then follows the biographical register proper with a column for each age period of seven years, with spaces for education (class lists), amusements (tastes and pursuits), accidents and bad illnesses, anthropometric tests at various ages, and other characteristics. The "Notes" show that personal appearance, pigmentation, height, weight, etc. were to be included, and eventually marriage and children. There is not a doubt but that this was the original scheme from which the *Life-History Album* sprung. The interesting point is that this biographical register was designed for undergraduates. The returns were apparently to be preserved in the archives of the colleges for future statistical purposes and for the compilation of college histories.

"It is believed that a large collection of personal and family records such as these, would furnish important data for investigating the social and hereditary antecedents that are most favourable to success in college and after life. They will certainly protect from oblivion many facts that may hereafter prove of considerable biographical interest to the undergraduates themselves and to their families; possibly to a much wider circle."

Again there appears to have been no result from this schedule, even if it were ever issued to an undergraduate population. The author of this biography knows only too well—having collected with the aid of colleagues two long series of schedules from undergraduates—how hard is the task; each series took four to six years to collect even by those who were actually working and teaching among the population; and Galton had none of these advantages. The very wealth which enabled him to carry out effectively his experimental ideas, prevented him from seeking and holding a teaching post, whereby he could have created more quickly a school, and been able to collect adequate material. It would be hard to say whether the balance was one of gain or loss to the world. There were factors in Galton's character—his invariable courtesy and kindness, his love of simple methods, his sympathy with younger minds, and his suggestive enthusiasm—which would have made

Decimal System. We translate the binary into the ordinary scale. Thus :

Grade of kinship	Father's Side	Mother's Side
Child	1	
Parents	2	3
Grandparents ...	4 5	6 7
Great-grandparents etc.	8 9 10 11 etc.	12 13 14 15 etc.

A want of these systems is an expression for the sibship of any individual, his or her brothers and sisters, or again for his or her nephews and nieces, uncles and aunts. Perhaps decimal figures might be added.

him a great teacher, but a teaching post would probably have cost him that travel-experience and that leisure to ruminate on which so much of his scientific success depended. He loved to work and to play with absolute freedom, and fixed duties would probably have been irksome to him, even if his health could have stood the *opus cathedrae strepitusque*.

The "Biographical Register" was followed by a "Genealogical Table of the family of brothers and sisters that includes —." We need not linger over this, it was the immediate forerunner of the "Family Records," which when Galton hit upon the idea of offering money prizes for filling in schedules became at once a great success—the material source whence sprung his two books the *Life-History Album* and *Natural Inheritance*. The latter will be duly considered in our chapter on Galton's contributions to Heredity.

One remaining schedule may be noticed here—Galton's circular letter of March, 1882 entitled: "Application of Composite Photographic Portraiture to the Production of Ideal Family Likenesses." This circular is remarkable for its artistic printing and "get up." It is an appeal to amateur photographers to provide full-face and profile portraits of members of families, and contains a characteristic family composite. The "bribe" in this case was a print of the family composite together with the negative if they desired it. Galton also stated that he should await with great interest the family's opinion on the family likeness. The response to this circular was very considerable, and the ruins of the material—for most of the photographs have perished or are perishing—are still in the *Galtoniana*. The conditions Galton demanded for the composite are worthy of preservation:

1. The set of portraits must be all *absolutely* in full-face, looking straight at the camera just above the lens, or they must be all in profile, with the eyes directed straightforward along their own level.

2. The light must fall from the same direction in every case; it is best that the sitters should occupy successively the same seat.

3. The portraits of which the head alone is used, must be of about the size of the sketches on the previous page, that is, a little more than an inch from the chin to the top of the head¹.

Galton considered these three conditions essential,

"if the portraits differ in aspect the composite would be blurred; if the shadows fall differently they are mixed up with the lights and the composite becomes ineffective—it will be like a portrait taken in cross-lights; if the photographs are too small the difficulties of adjustment are greatly increased and success is uncertain....It is, however, important that they should be forcible and well contrasted in light and shade."

Galton adds that the composite is invariably softer and more regular than its components; this statement was, perhaps, the inducement which led to the dispatch of some of the originals now in the *Galtoniana*!

¹ More recent investigations have shown that far larger "standard" photographs can be advantageously employed.



Francis Galton's "Standard Photograph" of himself to illustrate the profile and full-face portraits which are desirable in the case of Family Records and Life-History Albums and are suitable for composite photography.

C. GALTON'S ANTHROPOMETRIC LABORATORIES

The above series of schedules will show how fertile were Galton's plans for collecting statistical data during the decade 1874-84. It was, however, only in the course of this schedule experience that he learnt how reluctant most people are to fill up a schedule. As a result of this experience Galton changed his method of action. Failing the establishment of school anthropometric laboratories Galton determined to set one up at his own cost, and catch the world when on its leisurely and inquisitive peregrinations. He called into existence the first Anthropometric Laboratory at the International Health Exhibition in London, 1884. On the closing of this exhibition the laboratory was removed to the Science Museum, South Kensington, and the total number of visitors measured before it was closed was well over 9000. These included both sexes and all ages from five to eighty years. This splendid material, which is only at the present time being fully reduced and utilised¹, together with Galton's "Family Records" embracing between three and four hundred families, some 150 'stirps,' provided him at last with the material he had so long sought. The discussion of this material furnished Galton with occupation for at least ten years; and the need for novel statistical methods, which its problems demanded, led him to the correlational calculus, the *fons et origo* of that far-reaching ramification—the modern mathematical theory of statistics. One quakes to think of what might have happened had Galton not obtained through that first anthropometric laboratory and his family records the data he needed! The latter led him at once to the quantitative measure of heredity—the correlation of kinsmen for any faculty—and the former showed him that the same problems repeat themselves in all statistical material, and that the conception of correlation is not peculiar to heredity, but embraces all recordable qualities which without being causally linked together yet vary more or less stringently one with the other. From that conception arose a new view of the universe, both organic and inorganic, which provides all branches of science with a *novum organum*, far wider-reaching in its effects than that of Bacon, and as characteristic of the last quarter of the nineteenth century as the fluxional calculus was of that of the seventeenth. I have sought in vain for any forerunner of Galton in this matter², and feel convinced that he was the first to grasp not only the need of measuring associated variations, but the first to provide any real measure of them. Galton wrote to Darwin on December 24, 1869 that the appearance of the *Origin of Species* had formed a real crisis in his life and freed him from his old superstition as if he had been roused from a nightmare (see Vol. I, Plate II). For some of us Galton's new calculus acted in precisely the same manner; it enabled us to reach real knowledge—"to submit phenomena to measurement and number"—in many branches of inquiry where

¹ See for example Koga and Morant, "On the Degree of Association between Reaction Times in the case of Different Senses," *Biometrika*, Vol. xv, pp. 346-72, 1923.

² See a paper by the present writer entitled "Notes on the History of Correlation," *Biometrika*, Vol. XIII, pp. 25-45, 1920.

opinion only had hitherto held sway. It relieved us from the old superstition that where causal relationships could not be traced, there exact or mathematical inquiry was impossible. We saw the field of scientific, of quantitative, study carried into organic phenomena and embracing all the things of the mind. It was for us the dawn of a new day, and we smiled indeed over the attempts of the *Spectator* to obscure such a daybreak by looking westward and asserting it was and must remain night.

To those who realise what Galton's work meant for some of us in the 'eighties, when fresh from Cambridge we encountered his papers, there is something of supreme interest in the path by which he reached his conceptions, his long failure to collect data and its final solution in the Anthropometric Laboratory.

The growth of Galton's plan for creating an Anthropometric Laboratory is fairly well exhibited in his papers. We have first the idea of very simple statistics being collected in schools, then the plan of a somewhat more extended school laboratory and in 1882 a paper in the *Fortnightly Review*¹, "The Anthropometric Laboratory." The points of present day importance in this paper are the following:

(a) Galton propounds the need of an institute where a man may from time to time get his family and himself measured physically and mentally and photographed according to a standardised method.

(b) He reasserts his conclusion that circumstances and education have very little to do with an individual's capacities. These are provided by his heredity, they form his stock-in-trade, the amount of which admits of definition, and by means of which he has to earn his living and play his part as a citizen. Just as far as we succeed in measuring them, so far we shall be able to forecast what a man is fit for, and what he may undertake with the least risk of disappointment. In other words we have the first foreshadowing of industrial or occupational anthropometry.

(c) He then proceeds to speak very briefly of the old type of anthropometric records (chiefly statical), height, weight, vital capacity, grip², pigmentation, etc.

He next turns to Energy and Endurance. He considers that the true tests would be physiological and very delicate, measuring excess of waste over repair. Just as a clothdealer tests a piece of cloth by moderate tension without tearing it, so the balance of the living system might be artificially disturbed by a definite small force and its stability under the influence of greater forces be thereby inferred. He admits that at present tests of a person's endurance under sustained bodily or mental work have not been adequately developed. But he recognises that dynamic tests—the functioning of the body—are far more important than static tests. He would have *agility* tested by gymnasium or athletic sports tests. Co-ordination of muscles and eye by measured skill in well-known games from racquets to billiards³.

¹ N.S. Vol. xxxi, pp. 332-8.

² Vital capacity and sustained grip belong rather to the dynamic characters.

³ Since 1884 balancing (a slender column on a flat board raised from hip to shoulder), maze

ANTHROPOMETRIC LABORATORY

For the measurement in various
ways of **Human Form and Faculty.**

Entered from the Science Collection of the S. Kensington Museum.

This laboratory is established by Mr. Francis Galton for
the following purposes:—

1. For the use of those who desire to be accurately measured in many ways, either to obtain timely warning of remediable faults in development, or to learn their powers.
2. For keeping a methodical register of the principal measurements of each person, of which he may at any future time obtain a copy under reasonable restrictions. His initials and date of birth will be entered in the register, but not his name. The names are indexed in a separate book.
3. For supplying information on the methods, practice, and uses of human measurement.
4. For anthropometric experiment and research, and for obtaining data for statistical discussion.

Charges for making the principal measurements:

THREEPENCE each, to those who are already on the Register.
FOURPENCE each, to those who are not:— one page of the Register will thenceforward be assigned to them, and a few extra measurements will be made, chiefly for future identification.

The Superintendent is charged with the control of the laboratory and with determining in each case, which, if any, of the extra measurements may be made, and under what conditions.

(d) Keeness and discrimination of the senses are next emphasised as indispensable tests.

(e) Reaction times and judgment times follow; memory of form and memory of number. These points are sufficient to indicate that Galton from the earliest time laid as much stress on the psychical as on the physical tests of an anthropometric laboratory. Nay, he went further; he asserted that:

(f) There is need for a medico-metric section in an anthropometric laboratory. This section would make as exact and complete a report of the physiological and medical status of an individual as is feasible in the present state of science by the help of the microscope, chemical tests and physiological apparatus.

Such a "medico-metric" laboratory Galton holds would be useful to the general practitioner who could send his patients to be examined in the same manner as physicists send their delicate instruments to Kew Observatory to have their errors ascertained. Great stress is laid on the physician writing case notes of the successive illnesses of private patients even as he takes clinical notes at the bedsides of his hospital patients. These notes should be preserved by the patient and accumulating with the years would form his medical life-history, and be a unit-contribution to the medical history of the family. Galton emphasises the value they would be as an heirloom to the children of the subject and to their medical attendants in future years by throwing light on hereditary peculiarities. In short Galton saw in the anthropometric laboratory a centre for standardised family records of biographical interest to all members of the family, of value from the medical point of view to each individual during his life, and to his descendants as suggesting hereditary dangers and vital probabilities. Lastly and perhaps for Galton himself the most important advantage was the material they would ultimately provide for much needed statistical research into human genetics.

For the race the value of such records is incontestible, but all men have not Galton's power of calm self-introspection, and the effect of studying his family medical history in the case of a neurotic subject might well be disastrous for the individual.

The idea of medical family histories was further developed by Galton in a paper entitled "Medical Family Registers" in the *Fortnightly* for August, 1883¹.

In this paper Galton defines more closely what he means by medical histories and states that he has consulted a number of eminent medical men (Simon, Beddoe, Duncan, Gull, Ogle, Ord, Richardson and Wilkes) who have approved the scheme. In this article he suggests for the first time—as far as I am aware—a system of monetary prizes.

"I have made arrangements to initiate the practice of compiling them [Medical Family Registers] through the offer of substantial prizes, open to competition among all members of the medical profession. The prizes will be awarded to those candidates who shall best succeed

(a pencil carried in measured time round the convolutions of a maze without touching the sides), needle (a fine knitting needle put through a series of holes of decreasing diameter without contact) and other similar tests have been introduced replacing skill in games.

¹ N.S. Vol. xxxiv, pp. 244-50.

in defining vividly, completely, and concisely the characteristics (medical and other) of the various members of their respective families, and in illustrating the presence or absence of hereditary influences."

We have seen how Galton grew from traveller to geographer, from geographer to ethnologist, from ethnologist to anthropologist, and now the last stage appears: he is chiefly interested in anthropometry because of the contributions he expects from it to heredity; the anthropologist becomes a geneticist. Looked at superficially Galton's work seems like a comprehensive but confused mosaic of many branches of science. Studied in relation to his life we see a definite pattern, a picture of a long-continued mental development; each branch of knowledge he acquired fell into its fitting place, and formed a stepping stone to a further advance.

His own interest in Medical Family Registers arises, he tells us, from

"all that can throw light on the physiological causes of the rise and decay of families, and consequently on that of races. Some diseases are persistently hereditary, and others are not; they are variously found in different varieties or subraces of men, and these have various other attributes including various degrees of fertility. We cannot as yet foretell, but we may hope hereafter to do so in a general way, which are the families naturally fated to decay and which to thrive, which are those who will die out and which will be prolific and fill the vacant space." (p. 245.)

In this paper Galton shows that he has realised more fully the difficulty about medical registers:

"Most men and women shrink from having their hereditary worth recorded. There may be family diseases of which they hardly dare to speak, except on rare occasions, and then in whispered hints or obscure phrases, as though timidity of utterance could hush thoughts and as though what they fondly suppose to be locked-up domestic secrets may not be bruited about with exaggeration among the surrounding gossips. It seems to me ignoble that a man should be such a coward as to hesitate to inform himself fully of his hereditary liabilities, and unfair that a parent should deliberately refuse to register such family hereditary facts as may serve to direct the future of his children, and which they may hereafter be very desirous of knowing. Parents may refrain from doing so through kind motives; but there is no real kindness in the end." (pp. 245-6.)

Still Galton recognised that the difficulty remains, that the majority of men do fall into his category of ignoble cowards and will not record their family secrets as to disease. Accordingly he proposed to get over the difficulty by inducing medical men, under the bribe of £500 in prizes, to give confidential records of their own families. He hoped that the custom of medical family records having been introduced in this way, doctors would thereafter be not infrequently called upon to draw them up for the satisfaction of the patients themselves, and—Galton adds naively as a lure—"at their expense"! The particulars Galton proposed should be included in the registers to be provided deal not only with medical details, but with race, conditions of life, marriage, fertility, vigour, keenness of sense, artistic capacity, intelligence, character, etc. He was clearly working up to much of the material he finally asked for from laymen in his "Family Records." A great part of the paper is taken up with the conditions under which the prizes he proposed would be given.

There is an interesting paragraph as to fraud :

"As regards the probable trustworthiness of the information received, I am perfectly aware that a modern De Foe or Swift might write an interesting romance, and make a register apparently true to life, wholly out of his own head; but De Foes and Swifts are not common, and such persons would be sure to find better occupation than that. Moreover they could not gain a prize without committing a downright fraud. Able men are generally above petty tricks, and there will be abundant internal evidence in every register to show whether the writer be able or not. It is almost needless to remark, that every statistician worthy of the name is wary and slow to accept startling conclusions without much indirect confirmation.... What I fear most is that the registers sent by many of the candidates will afford internal evidence of being little trustworthy, not through deliberate intent, but owing to the incapacity of the writers to state their cases clearly, and to support their statements with judiciously selected data." (pp. 248-9.)

Perhaps this latter remark was rather severe on the profession to whom Galton was appealing for aid; it failed to give due weight to what should be the effect of the clinical training in a hospital on a man's powers of observation, record and deduction. Yet our poor hero saw with yearning those 23,000 qualified medical men and thought here at last was a source of the material essential to his work!

"I should hope that the examination would be complete after some three months' labour of myself and the examiners. The prizes being allotted and done with, it will remain to work up the results....The statistical meal will be a large one; I gloat over it in anticipation, and know that it will take long to digest. I cannot doubt that new ideas will be derived from a careful study of so unique a collection, enough I hope to justify to myself the cost and time spent on it. When I shall have done with this collection, its ultimate destination will probably be as a gift to some appropriate medical or anthropological institution. It will then be in the form of anonymous documents bearing mottoes, but with no mark by which any one of them could be distinguished....Considering that prizes for essays usually attract numerous competitors, although the pains taken in working for them are rather barren of result except to the winners, I conclude that similar prizes leading to inquiries beneficial in every case, and from many points of view, ought to attract yet more numerous candidates, and to result in producing shelves full of family histories of unprecedented completeness and concentration, and of extreme value for a long time to come to medical and anthropological investigators." (p. 250.)

What killed this scheme of prizes to the doctors for medical registers of their families? I can find but one further reference to it (see our p. 367). All letters for the year 1883 seem to be wanting; so that we cannot trace the causes which led Galton to drop the emphasis on the medical register and offer his prizes for family records to laymen as well as medical men. Three extracts from *L. G.'s Record* may be given here as conveying about all the knowledge we have of the matter:

"1882. Frank began his book on *Human Faculty* early in the year and it has gone on through the year, and was a great pastime to him during our summer ramble on the Rhine, in the Black Forest, Constance, and lastly Axenfels. Bad weather haunted us, but we were happy and I kept well and began sketching again. It was such a boon not to be kept by a British Association Meeting this summer. Mr Darwin's death in May had cast a deep gloom over us....Besides what I have mentioned as to Frank's work during the year he gave a Lecture at Eton on Anthropometric Registers and Life Histories, and wrote a paper in the *Fortnightly* on the same, and gave a lecture to the Committee of the Medical Association¹. He was invited to lecture at the Lowell Institution in America, but refused. In Meteorology he designed a clock for cumulative temperature. He is elected on to the Council of the Royal Society and was begged to accept the presidency of the Anthropological, but refused.

¹ Probably the Committee of the B.M.A. for Collective Investigation.

1883. Frank went to the British Association at Southport in September. In the early part of the year he corrected proofs of his *Inquiries into Human Faculty*, which was published by Easter. He also worked, helped by Croom-Robertson, at means of measuring the sensations. In the August *Fortnightly* he wrote an article on Medical Life Histories, offering Prizes up to £500. He spent much time on the details. His *Record of Family Faculties* has just come out, also the *Life-History Album*, which he edited. He was Chairman of the Anthropometric Committee of the British Association, which published this year its fourth and final Report, and also Chairman of the Local Scientific Societies Committee.

1884. More and more home seems the most fitting place for me and for Frank as he is always full of occupation. This last year he has been chiefly occupied in abstracting and collating from the Family Records for which he offered prizes to the amount of £500. About 150 of these Records were sent in by May 15th, and the statistics they afford will be food for many a month and possibly years. Then Frank took great interest in establishing an Anthropometric Laboratory at the International Health Exhibition, South Kensington, and gave no end of time and money to its prosecution; he gained, however, full recognition from Sir James Paget and others, whose opinion he cared about....In the summer my great desire to go abroad was stopped by cholera in the south of France and after long debate Frank settled on the English Lakes, and I was quite willing in default of my pet scheme, which I especially had cherished, as there was no English British Association to spoil our holiday, it being held at Montreal."

The *Record* tells us indeed as much as we know of the whole matter, namely, that in some way the medical profession disapproved of the prizes, and that they were then offered to laymen, who to a considerable extent responded¹.

The interval, however, between the *Fortnightly* paper of 1883 and the appeal to laymen for family records was well employed, and Galton having got in touch with several leaders of medical opinion over the medical registers obtained their aid in preparing his *Life-History Album*. It may be well to deal with these matters before we return to the Anthropometric Laboratory.

¹ In the announcement of the prizes of December, 1883, this note occurs after the statement of conditions, etc.: "The above conditions are in lieu of those provisionally sketched out by Mr Galton in the *Fortnightly Review* of August, 1883, for the purpose of eliciting suggestions, and which were subsequently submitted in a more elaborate form to many members of the medical profession. Their present shape is fixed in accordance with the balance of opinions elicited by these preliminaries, which was in favour of throwing them open to general competition and not to medical men only, as at first intended." This is obviously only a formal explanation, and does not indicate the nature of the opinions against confining the scheme to men who were in a much better position than the layman to record family ailments and the causes of death. Possibly the article in the *Fortnightly* rather impeded than aided his plans; possibly the medical profession of those days resented the intrusion of an outsider into what they might consider to be their own domain, even if they had not cultivated this portion of it, and showed no haste to do so. There is also another point which has much weight with me: Galton even to the time of his death had a great belief in working his projects through committees. I think it arose by reason of an innate modesty which was always seeking advice from others and accepting their opinions as worth more than his own. These committees often became unwieldy, were composed of incongruous and irresponsible elements, and on more than one occasion perverted or destroyed Galton's original scheme. It is conceivable that the Life-History Sub-Committee of the Collective Investigation Committee of the British Medical Association, of which Galton was Chairman, contained elements of this character. Anyone who has endeavoured like the writer to pick out from Galton's *Record of Family Faculties* a definite disease like phthisis by aid of its numerous lay synonyms or rather intentional pseudonyms will be rapidly convinced that the widening of the field of candidates was not an unmixed advantage. I had already written this note when I chanced to turn up Galton's preface to the second edition of the *Life-History Album* (p. ix). He there admits to the full the evil of working through a committee, but alas! it did not cure him of the habit.

D. THE RECORD OF FAMILY FACULTIES AND THE LIFE-HISTORY ALBUM

In the advertisement of the prize competition Galton suggested that information should be collected with regard to the child, its parents, its grandparents and great-grandparents, Numbers 1 to 15 of his scale of relationship, and as far as possible of all the collaterals of these, i.e. members of the same sibships as these, or about 70 to 90 individuals. Cousins Galton omitted, although we now know that, both on Mendelian theory and by actual observation, they exhibit as much of the constitution of the common stirp as aunts, uncles or grandparents and more than great-grandparents¹. The prizes were to be given to those who filled in the blank spaces of the *Record of Family Faculties* most completely and perspicuously. The *Record* was published by Macmillans in 1884 and has been long out of print. A new and somewhat modified edition of it is certainly needed. It records, for the family of an individual—his stirp—what the *Life-History Album* of the same year does more copiously for the individual himself. In other words, the *Record of Family Faculties* could be extracted from the separate Life-History Albums of its units, but the inverse process is not possible. The one gives—except for the medical section—a brief account of the *adult* characteristics of each unit of the stirp, the other traces the unit through all phases of growth.

In the *Record* the following questions are asked: (1) Date of Birth; (2) Occupation, Birthplace, Residences; (3) Age at Marriage of individual, number of sons and daughters alive and their ages, and the same for those deceased with age at death; (4) Age at Marriage of spouse; (5) Mode of life so far as affecting growth or health; (6) Was early life laborious? why and how? (7) Adult height, adult colour of hair—colour of eyes; (8) General appearance; (9) Bodily strength and energy, if much above or below the average; (10) Keeness or imperfection of sight and other senses; (11) Mental powers and energy, if above or below the average; (12) Character and temperament; (13) Favourite pursuits and interests, artistic aptitudes. Then comes the medical history: (14) Minor ailments to which there was special liability (*a*) in youth, (*b*) in middle age; (15) Graver illnesses, (*a*) in youth, (*b*) in middle age; (16) Cause and date of death, and age at death. There are pages for male and female relatives of whom little is known but the age at and cause of death. There are pages for summaries of the anthropometric and medical characteristics of the stirp, and two Appendices to be devoted respectively to the Biological History of the Father's and of the Mother's Family. By 'biological history' Galton understood the constitutional history and hereditary peculiarities of mind and body on either father or mother's side. A third appendix deals with an examination of the way in which the faculties of the father and mother are blended or otherwise combined in the child.

¹ This result of theory and observation always troubled Galton, but I do not think there is any doubt of its accuracy.

The whole work is prefaced with an account by Galton of how the *Record* should be filled in. It contains many characteristic statements. A few of these may be cited here, as the book is very scarce.

"This book is designed for those who care to forecast the mental and bodily faculties of their children, and to further the science of heredity. The natural gifts of each individual being inherited from his ancestry, it is possible to foresee much of the latent capacities of a child in mind and body, of the probabilities of his future health and longevity, and of his tendencies to special forms of disease, by a knowledge of his ancestral precedents. When the science of heredity shall have become more advanced, the accuracy of such forecasts will doubtless improve; in the meantime we may rest assured that fewer blunders will be made in rearing and educating children, under the guidance of a knowledge of their family antecedents, than without it."

In the third paragraph Galton rightly points out that it is needful to study as many ancestral lines as possible, and that the book gives no countenance to the vanity that prompts most family historians to trace their pedigree to some notable ancestor and to pass over the rest in silence. Galton remarks that

"one ancestor who lived at the time of the Norman Conquest, twenty-four generations back, contributes (on the supposition of no intermarriage of kinsfolk) less than one part in 16,000,000 to the constitution of a man of the present day." (p. 1.)

This is rather a theoretical than an observational result. It is true a man may have 20 to 30 generations back 16,000,000 direct line ancestors if so many were available, but it is equally true that a distinguished man of that day might have several million descendants, and, if any system of alternative or factorial inheritance be true, the *distinguished* individuals among those descendants may owe their nature to that distinguished ancestor. It does mean something to trace even in one line—and there are four or five—a link between Darwin or Galton and Alfred the Great. It signifies nothing to trace the same link between a mediocrity and Alfred the Great¹. Galton suggests that we need not go back beyond our great-grandparents, and this is absolutely true of characters which blend. But when he tells us that if an alien element of race or disease has been introduced into a family—a touch of Hebrew, of Huguenot (or even negroid) blood—it may be traced far further, he seems to me to be contradicting his previous statement. Albinism for example may remain latent through far more than three generations. But Galton recognises fully this latency at other points.

"Brothers and sisters are alike in blood, but it commonly happens that one of them exhibits some faculty in a conspicuous degree, which exists only in a latent form in another, and which the latter is, perhaps, equally capable of transmitting to his children. Therefore records of the faculties of the brothers and sisters of direct ancestors are of great value in disclosing hidden characteristics." (p. 2.)

I think it would be more just to say that the limitation to great-grandparents is only a question of the limitation of knowledge in the case of most families; and without being conclusive a great deal may still be learnt, if we

¹ The illustration can be given in another form: Some 15 years ago piebaldism appeared in my stirp of dogs and soon disappeared. After 7 or 8 generations it has reappeared. The piebald ancestor means little to the average dog of my stock; he means everything to the isolated piebald puppies of to-day.

cannot get beyond the three generations and their collaterals. We must make a start somewhere and if we record three generations now our great-grandchildren will be able to consider *six*. What we need is to store data for the future.

"The advance of the science of heredity is seriously delayed through the want of such data. We do not yet know whether any given group of different characteristics which may converge by inheritance upon the same family will blend, neutralise or intensify one another, nor whether they will be metamorphosed and issue in some new form. Our ignorance is especially great in hereditary maladies, where much alarm undoubtedly exists which inquiry will dispel. It is possible that the different disease tendencies of different ancestors may in some cases neutralise one another, and on the other hand, that some ancestral combinations may be more hurtful to the descendants than we have at present any idea of....Our present ignorance of the conditions by which the level of humanity may be raised is so gross, that I believe if we had some dictator of the Spartan type, who exercised absolute power over marriages, assigning *A* to be the wife of *B*, and *C* to be the wife of *D*, and who acted with the best intentions, he might possibly do even more harm than good to the race."

Which remark I commend to the good Mr Chesterton who believes that he is better able than the Founder of Eugenics to appreciate what Eugenists propose.

Galton discusses the questionnaire of the *Record* showing how the answers will bear on such vital problems as the relative influences of nature and nurture, as the effect of overstrain in the parents on the robustness of their offspring, as the possibility of secular changes in the English race, and as to the influence of various racial types on fertility.

Considering the senses Galton remarks:

"Keeness of sensation in each of its forms is a valuable natural gift; unfortunately no means are as yet easily accessible for testing it in different persons; there are no anthropometric laboratories as yet in existence to which any one may go, and on payment of a small fee have all his faculties measured and registered by the various ingenious appliances known to modern science. We must therefore be content for the present with such definite facts bearing on the keeness or imperfection of the various senses as may have been incidentally observed." (p. 9.)

Galton demonstrated even in the same year as he wrote these lines what an anthropometric laboratory could achieve in measuring the keeness of the senses. Fitly attached to the Laboratory in the University of London, which now bears his name, is an Anthropometric Laboratory with complete equipment for testing not only the keeness of the senses but for measuring many other physical, psychical and physiological characters. The difficulty does not now lie in the absence of means of testing, but in the discovery of suitable ways of reaching those who are desirous of being tested, or who ought to be tested.

When Galton comes to the medical record he gives a list of "minor ailments" to which an individual may be subject and also one of the "graver illnesses." These lists may be repeated here as they are still of value in considering hereditary transmission of disease.

"*Minor Ailments.* Colds in the head or throat, sick headaches, sleeplessness, boils, quinsy, enlarged glands in the neck, bleeding at the nose, indigestion, bilious attacks (state whether accompanied by jaundice, vomiting or headache), constipation, skin eruptions (their nature should be stated if known), varicose veins, etc.

Graver Illnesses. Gout, rheumatism, consumption, spitting of blood, struma (scrofula), cancer (and other forms of tumour), bronchitis, asthma, paralysis (state whether of both legs or of one side), epilepsy, insanity, heart disease, dropsy of abdomen, general dropsy (Bright's disease), diabetes, stone, goitre, fistula, the peculiar liability to bleed seriously from slight cuts, etc."

It will be noted that Galton omits such things as the tendencies to zymotic diseases which undoubtedly run in families, or those to hereditary eye diseases such as cataract, *retinitis pigmentosa*, etc. He does state that malformations which are extremely hereditary should be included.

Then follows a brief but useful list of sources from which family information may be obtained and he concludes as follows:

"Whatever may be the value of these results, the facts incidentally obtained during the course of the inquiry will form a separate document much prized by the family. The scientific importance of each investigation will, however, be soon appreciated by the author of it, for his researches will lay bare many far-reaching biological bonds that tie his family into a connected whole, whose existence was previously little suspected. Few, if any, have seriously studied the facts of heredity without being impressed with the conviction that no man stands on an isolated basis, but that he is a prolongation of his ancestry in no metaphorical sense, and I shall be surprised if the compilation of these registers does not extend this conviction very widely." (p. 13.)

We now turn to the *Life-History Album*. The first edition appeared in 1884 and bears on the title-page the words: "Prepared by direction of the Collective Investigation Committee of the British Medical Association. Edited by Francis Galton, F.R.S., Chairman of the Life-History Sub-Committee."

The original proposal seems to have been the return at fixed periods of the records in the Album to the Collective Investigation Committee. I think this proposal was not carried out, for I know of no publication of results from Life-History Album data ever being issued. The foreword to the owner of the book concludes with Galton's words from the *Fortnightly Review* article of January, 1882 (see our p. 358).

"The life-histories of our relatives are, therefore, more instructive to us than those of strangers; they are especially able to forewarn and to encourage us, for they are prophetic of our own futures."

The second edition of this work appeared in 1902 and there has recently been a reissue by the Cambridge University Press for the Galton Laboratory Publications. The second edition bears the sub-title "Tables and Charts for recording the Development of Body and Mind from Childhood upwards with Introductory Remarks. Rearranged by Francis Galton, D.C.L., F.R.S."

In the preface to the second edition Galton refers to the enthusiasm in the production of the work of the late Dr Mahomed, who had firmly persuaded himself that a work of the kind would be favoured and promoted by medical men throughout the country; and this idea led to its being produced under the auspices of the Collective Investigation Committee of the British Medical Association. Dr Mahomed

"made it a further condition that my name should appear as editor, I being known at that time to be much occupied in such matters. To this I agreed with some reluctance, for I wished to bear the entire responsibility or none at all. So a small committee of medical men was

formed who met frequently at my house, where the book was mostly composed. But the result did not at all satisfy myself, neither do I think it satisfied the others. It was too bulky and ill-arranged. In fact it was emphatically a case of too many cooks. Each had his own views, and I believe that any one of us acting alone would have produced a better balanced book than we did working together."

Soon after the issue of the first edition Dr Mahomed died and none of the remaining medical members of the committee concerned himself much further with the work. Accordingly when Galton in 1902 was pressed for a second edition he largely rearranged and rewrote it. Thus the second edition and not the first really represents the book as Galton had originally planned it nearly twenty years earlier. It alone will be considered here¹.

While the author in his *Introductory Remarks* states that the *Album* may be started at any age in life and if then filled in as far as records are available will be of considerable interest, he notes that the book is especially suitable as a present in readiness for infants expected to be born, or for very young children. It should be handed over by the parent to the child only when the latter is old enough to appreciate its importance, to take charge of it and to continue the record for himself.

"The future of each man is mainly a direct consequence of the past—of his own biological history, and of those of his ancestors. It is therefore of high importance when planning for the future to keep the past under frequent review, all in its just proportion, and this is exactly what this album is intended to help him to do." (p. 2.)

Much of what Galton writes about filling in the details of the individual in the *Album* need not detain us; it is more or less a development of the information given in the *Record of Family Faculties*. Ample space is left for photographs in the standard positions and even for pictures of the homes and for finger-prints. The actual scale of coloured wools is omitted in this edition, although the heap of bits of variously coloured wools is still suggested as a test for colourblindness. An appendix gives tests for keenness of vision. Two paragraphs to illustrate the wise inferences that our sage of eighty could draw from his experiences may be reproduced here. The first relates to memory:

"Memory alone is an imperfect and deceitful guide; it preserves only a trifling part of the events of early life, and that part far from correctly. The extreme vividness with which a few childish incidents are usually recalled gives a very exaggerated view of the power of its grasp. Anybody who attempts to compile a sustained history of his early years will soon be persuaded of the truth of this remark, for he will surely become aware of huge gaps of time that he is totally unable to give account of. Every autobiography that I have seen testifies to these lapses of memory. Again when one happens to meet a friend not seen since early life, and to compare recollections, it is astonishing to find how differently the two memories have behaved. The one man fails to recall a multitude of events that have strongly impressed the other. Even as to those they alike remember as wholes, it will often occur that their memories disagree in essential details. In fact, the experience gained by such interviews is commonly humiliating to both." (p. 2.)

The second quotation I shall make relates to veracity in recording the nature of disease. Galton had come up against that hesitation to be literally

¹ The second edition differs not only in quality but quantity, for instead of carrying on the record only to the seventy-fifth year, it continues it to the hundredth. Galton was himself eighty when it was issued.

truthful which at once meets all of us when we attempt to deal with the heredity of pathological conditions. In the humbler classes of society, who form the bulk of hospital patients, there is sometimes almost a pride in the possession of malformations and of pathological conditions. Many of the women are only too ready to talk about them, and to exhibit any children who may possess them. It is possibly the great interest which they observe is excited among medical men by pathological states, which leads them to be pleased at being noteworthy even for a deformity¹. Unfortunately the family records and traditions in hospital cases are often woefully scanty. The great-grand-parents are rarely to be traced, and only too often little if anything is known of the families of uncles or aunts. On the other hand, when we turn to social classes where the knowledge of ancestry and collaterals is considerable, we are too often met by an indignant refusal to give information, even if it be needed for scientific purposes. No retreat for the insane or sanatorium for the tuberculous designed for patients of middle or upper class status is able to provide adequate material for heredity. The inquirer is solemnly informed that the grandfather died of inflammation following a chill, that the sister pined away after the death of her fiancé, or that the father was at times eccentric, the aunt had attacks of the nerves, or the cousin brain-fever due to overwork. It does not profit to give way to discouragement, or feel sore before rebuffs, if you want to study human heredity. The only resource is to try and educate the so-called educated classes and produce if possible a more reasonable attitude towards hereditary matters amongst them. Galton achieved much in this direction and if it is still difficult to make rapid progress, it is certainly easier than it was before he started his campaign against the folly, not to say crime, which would screen family history not only from the future wife, but from adult children. Galton writes on this second point in 1902 with the sagacity of old age; he has learnt that to brand such action as "ignoble cowardice"¹ may be absolute truth, but it will hardly obtain what we need from the cowards. He says:

"It is too much to expect that even the most scrupulously kept records will be written throughout with perfect veracity. Healthy minded persons are seldom disposed to lay themselves wholly bare in written words. There will be omissions in every Album, sometimes of matters of fact and at other times of the real inwardness of events, that are of high importance to the right understanding of a life-history. The writer of the Album will mentally supply the omissions and interpret the misleading euphemisms when he refers to its pages; other persons who read his records must be prepared for their existence. Thus in matters of disease, an unsurmountable prejudice exists in many sensitive persons against ascribing cancer and insanity to their ancestors in direct terms. They shrink from the thought of recording hereditary possibilities that might destroy the peace of mind of their descendants, and perhaps work their own fulfilment. The duty of parents to be truthful histographers seems overborne by what they consider to be a still more pressing duty to their children. It is almost useless to attempt to calm hypersensitive feelings by pointing to the fact that healthful tendencies are just as heritable as morbid ones, and that every child is sure to be endowed with both. So I will confine myself to the mention of an instructive

¹ The psychology of these cases is similar to that of many persons, who being connected with some appalling criminal trial seem pleased with the momentary notoriety which carries their portraits into the cheap illustrated daily papers, possibly the one chance of publicity in their lives. I am by no means certain that it is not the same human frailty, under a thin covering of veneer, which causes the bride of another class to send her photograph to the 'society' papers.

experience of my own, which was gained while working at the family histories of a multitude of individuals. They were so tabulated that the medical history of any individual could be concealed by the hand or by a sheet of paper, while those of all his immediate ancestors, namely, parents, grandparents, uncles, and aunts, were exposed. I experimented frequently in guessing at the cause of death of a deceased individual from the knowledge of those of his ancestry, and I found my guesses to be on the whole grossly incorrect¹. But though the stated cause of death could not be predicted with any approach to a useful degree of accuracy, the inheritance of minor ailments was conspicuously manifest. For this reason some stress is laid in the Album upon recording them. For my own part, I find no reason why the diseases of ancestors should be described otherwise than with perfect honesty, especially as a knowledge of them may induce their descendants to take reasonable precautions against inborn tendencies, instead of taking no precautions at all and doing themselves irreparable injury out of pure ignorance." (p. 3.)

The publication of the *Record of Family Faculties* and the *Life-History Album* brought Galton into touch with many families and individuals who were deeply interested in heredity. These books also brought him material, even if it was not quite as adequate as he had hoped for; further, they gave him an acknowledged position with regard to human heredity, which enabled him to prosecute more widely his inquiries, and to focus a great deal of the somewhat scattered attempts which were then being made in this field. Nor did his works fail to find competitors. Mr Jonathan Hutchinson, who had been a member of the British Medical Association Committee, published in 1888 *The Life Register*, which we might suppose to have derived some ideas from Galton's work, did not the author tell us that it was in no sense a copy, "having been in existence long before the Album was published." It is less elaborate than Galton's and fails in any adequate description of how and what entries are to be made. Quite recently much more ambitious *Albums* have been published in America demanding entries far more numerous than Galton's book. On the whole that work takes the *juste milieu*, and combines anthropometric with medical details in fair proportions. Galton was undoubtedly first in the field with a published volume; that volume shows his characteristic talents, and still remains the best *Life-History Album* available². The insignificant annual sale of the work is, however, fairly conclusive evidence of how little the many persons who talk about eugenics and enrol themselves in societies for the advance of eugenics do to carry out Galton's fundamental idea that every individual should keep, in a standardised form, as perfect a register as possible of his own and his stirp's genetic possibilities. That is still a want for the future to supply; and as without its fulfilment we must ourselves fail in eugenic conduct, few of us have a right to preach eugenic morality to others.

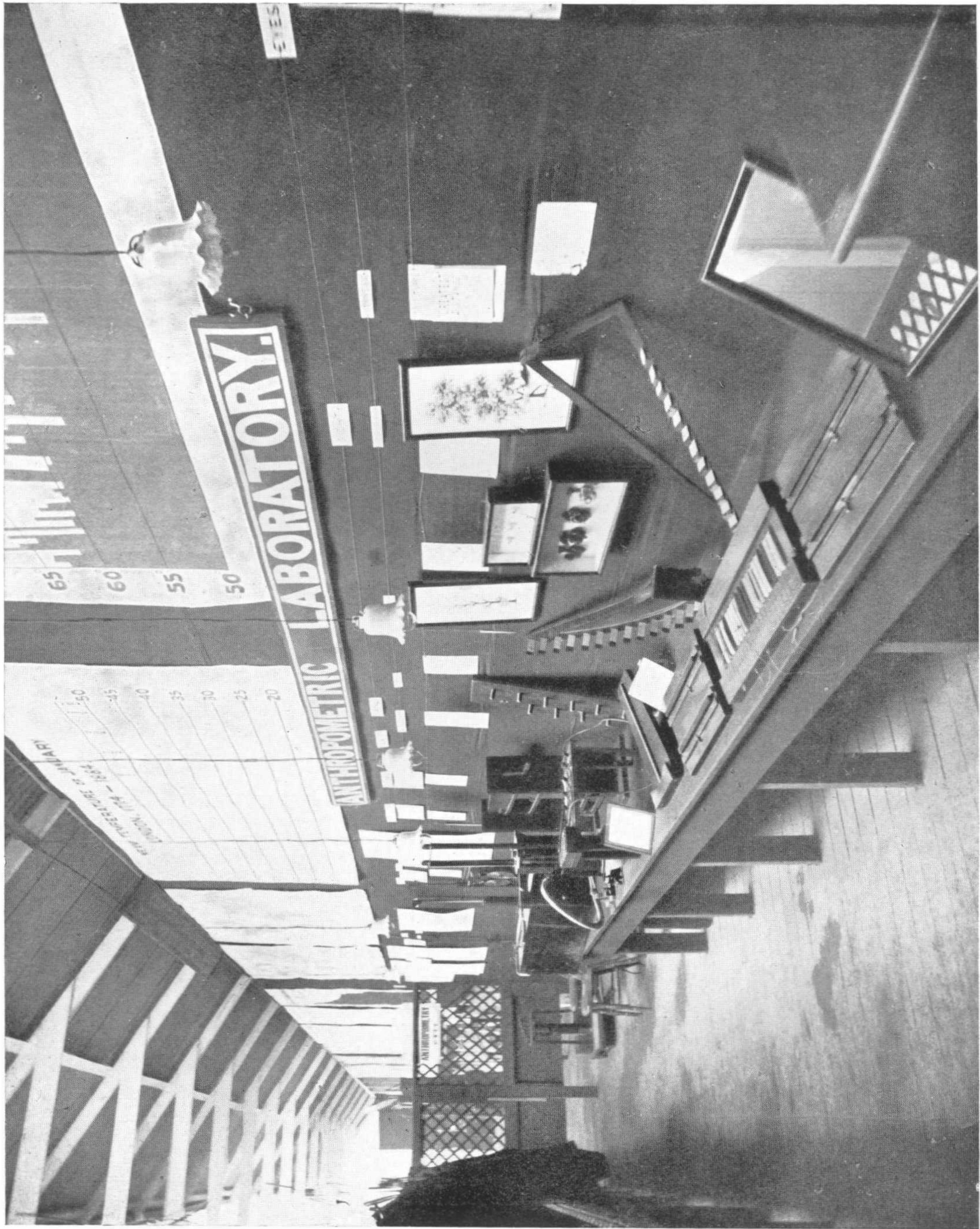
¹ One may hope shortly to see this result tested on actual tabulated material. One of the chief difficulties lies in the fact that the lives of relatives are not exposed to equal risk, the environments and occupations may be so different; probably the risk in the case of sisters will be more nearly similar and give more conclusive results. Undoubtedly there are many pedigrees in which it would be safe to predict from the ancestral generations that at least 20% of the new generation will be affected with a given deformity or pathological condition. Would it be reasonable by predicting for isolated individuals to say that one's prediction was grossly incorrect if four individuals out of five were free of the defect? I think prediction must proceed on an average or probability basis, and that in Galton's test where apparently only *one* individual's cause of death was considered the *probability* of death from a particular form of disease was not really incorrect because that individual failed to die of it.

² It is still published by the Cambridge University Press, Fetter Lane, E.C. 4.

The *Record of Family Faculties* and the offer of prizes supplied Galton with the material upon which to work for the next five years, and the resulting memoirs and his book *Natural Inheritance* of 1889 will be discussed in a later chapter. When Galton came to distributing the prize money he had offered, he was in some difficulty as to measuring relative values and finally assigned forty £7 and forty-five £5 prizes, so that eighty-five competitors shared the £505. The awards were made on June 24, 1884, and were published in the *Times*, *Nature*, *British Medical Journal* and *Guardian*. Of the returns it is of interest to note that 70 were made by men, 80 by women; (i) 20, or rather fewer, concerned titled persons and the landed gentry; from (ii) Army and Navy, (iii) Church (various denominations), (iv) Law, (v) Medicine, (vi) Commerce (higher), (vii) Commerce (lower) there were 110 returns in all. The remaining twenty were from land agents, farmers, artisans, literary men, schoolmasters, clerks, students, and one domestic servant—a fairly representative collection. The original records were returned by Galton to their authors after he had made statistical abstracts.

E. THE ANTHROPOMETRIC LABORATORY (*continued*)

We have already indicated the labour Galton gave to the construction of instruments for this laboratory and described some of the psychometric and other measurements he took. We have further discussed the paper on the "Outfit for an Anthropometric Laboratory" which he drew up in conjunction with the late Professor Croom-Robertson and privately circulated (see our p. 212). Other papers actually deal with the instruments and measurements made at the first Anthropometric Laboratory (International Health Exhibition). The first is a penny pamphlet of some fourteen pages, "Issued by Authority," and intended to be sold to visitors to the Exhibition. It is entitled: "Anthropometric Laboratory arranged by Francis Galton, F.R.S., for the Determination of Height, Weight, Span, Breathing Power, Strength of Pull and Squeeze, Quickness of Blow, Hearing, Seeing, Colour Sense, and other Personal Data. The Laboratory is situated in the East Corridor Annexe, Entrance from South Gallery. Admission to the Laboratory 3d., for which a schedule filled up with the above details will be furnished. London, William Clowes and Sons. International Health Exhibition, 1884." The pamphlet describes the purpose of the Laboratory, namely to show to the public the great simplicity of the instruments and methods by which anthropometric measurements are taken. It then states what measurements are taken and how they are taken. It further indicates two uses of such measurements, namely (i) the personal use, to ascertain whether the growth of child or youth is proceeding normally, and to draw attention to defects with a view to their being remedied; and (ii) the statistical use, to discover the efficiency of a nation as a whole and of its several parts, and the direction in which it is changing, whether for better or worse. Galton then notes the need there is for a more systematic registration of physical measurements.



Francis Galton's First Anthropometric Laboratory at the International Health Exhibition, South Kensington, 1884-5.

"Their value is indisputable, the cost of making them is trifling, and the facility of registration in any permanent institution is obvious. It seems strange that they should be neglected at any school or university." (p. 4.)

Dealing with the question of hair and eye colour, Galton remarks that:

"The British nation is partly a blend and partly a mosaic of very distinct types. The short black-haired ancient British race unites imperfectly with the tall fair-haired Danish or Scandinavian. Their union resembles what druggists call an emulsion, that is a mixture of oil and water, so well shaken together that they form an apparently homogeneous substance; but the compound is not durable. Leave the emulsion alone and after a longer or shorter time it will separate into its component elements. Types are stable, but the forms of their mongrel offspring are not; and whenever the external features of the old types are found in something of their original purity, it is reasonable to suppose that their inward characteristics are present also." (p. 5¹.)

Galton notes that Baxter has shown from an analysis of 330,000 to 340,000 reported cases of invalidism in the medical examinations for the American army during the Civil War, that the light-haired men suffered more than the dark-haired from every form of disease except chronic rheumatism². It is to solve problems of this kind that a record of pigmentation in individuals and families as well as a record of disease is desirable.

The actual floor space of the Laboratory was only 6 feet wide by 36 feet long. It was fenced off from the side of a gallery by open lattice-work, through which the public could see what was going on without impeding the examinees. These entered by a door at one end and left by a door at the other; and some 90 individuals were passed through in the course of a day. The pamphlet gives an account of what the visitor is expected to do at each station and the nature of the instrument used for the test. A more elaborate account of the Laboratory was published in the *Journal of the Anthropological Institute* for 1885³. It is entitled: "On the Anthropometric Laboratory at the late International Health Exhibition." The memoir begins by stating that the exhibition being over it is desirable that some account should be given of the methods and experiences of the Anthropometric Laboratory, and that the author wishes to invite criticism and suggestion. He states that the Laboratory aroused considerable interest, 9337 persons were measured, each in 17 different ways⁴. Duplicates of the instruments had been ordered by executive officers in foreign countries, and much interest had been expressed in the apparatus by many places of education. As it was most desirable that for comparative purposes there should be a standard set of instruments Galton brought his apparatus and the attendants who had supervised the measuring before the Institute⁵. Galton remarks that the total expenditure having been covered by a charge of 3d. per head, he

¹ More recent research indicates little association between external and psychical characters after hybridisation.

² Baxter, J. H. *Medical and Anthropological Statistics of the Provost-Marshal-General's Bureau*, Washington, 1875.

³ Vol. xiv, pp. 205-21.

⁴ The schedules containing these measurements are now in the Galton Laboratory and work on their complete reduction is in progress.

⁵ All the work was done by Serjeant Williams, Mr Gammage, an optical instrument maker, and a doorkeeper, of course under Galton's supervision.

did not see why a similar periodic anthropometric examination should not be made in all the great schools of the country; and he makes the excellent suggestion of an itinerant anthropometric laboratory, which should circulate from school to school. The idea is a very good one, and would keep the school laboratory, where such existed, up to modern standards of methods, tests and apparatus.

Discussing what should be measured, Galton observes:

“One object is to ascertain what may be called the personal constants of mature life. This phrase must not be taken in too strict a sense, because there is nothing absolutely constant in a living body. Life is a condition of perpetual change. Men are about half an inch shorter when they go to bed than when they rise in the morning. Their weight is affected by diet and habit of life. All our so-called personal constants are really variables, though a large proportion of their actual variations may lie between narrow limits. Our first rule then is, that the trouble of measurement is best repaid when it is directed upon the least variable faculties.” (p. 207.)

He then touches on a point which still troubles the anthropometrician, especially in the case of mental tests:

“There are many faculties that may be said to be potentially constant in adults though they are not developed, owing to want of exercise. After adequate practice, a limit of efficiency would in each case be attained and this would be the personal constant; but it is obviously impossible to guess what that constant would be from the results of a single trial. No test professes to do more than show the efficiency of the faculty at the time it was applied, and many tests do even less than this, being so novel to the person experimented on that he is maladroit, and fails to do himself justice¹; consequently the results of earlier trials with ill-devised tests may differ considerably from those of later ones. The second rule then is that the actions required by the tests should be as familiar as possible².”

Galton makes a suggestion as to practice which might be worth following up, although great care would have to be taken in experimenting between sessional and secular variations. The sessional variation, or variation in a sitting (or in one closely contracted series of tests), may be largely a result of chance, partly of practice, and partly of fatigue, while the secular variation may show the marked effect of continual practice³. The suggestion runs:

“There is some hope that we may in time learn to eliminate the effect of an unknown amount of previous practice by three or more distinct sets of trials. There exists a rough relation between practice and proficiency which ought to be apparent wherever progress is not due to acquiring a succession of new knacks, but proceeds regularly. When no practice has previously taken place, the progressive improvement will be very rapid; then its rate will smoothly decrease until it comes to an entire stop. I suspect that a curve might be drawn between proficiency and practice, and that the data afforded by at least three successive series of tests would roughly

¹ I think Galton has conceivably overlooked a point here. One test of fitness to environment is the readiness with which an individual can adapt himself to new conditions and respond promptly to the everchanging experiences of his life. In many cases therefore a *novel* test is a truer test of mental agility, than one which has become a familiar routine.

² This rule appears to be stated too generally; when familiarity comes in, then there will be a correlation between length of practice and efficiency, of which we do not know the intensity. When I was a boy I was taught to dovetail and learnt to do so creditably; any mediocre carpenter would excel my work now both in speed and neatness. On the other hand I should probably copy out far more clearly and rapidly than he would a page of letterpress. Any test to determine our relative powers of mentally controlling the rapidly moving hand ought, I think, to be made in a manner which should be as unfamiliar to us both as possible.

³ The reader may consult a paper by E. S. Pearson on the “Variations in Personal Equation, etc.,” *Biometrika*, Vol. XIV, pp. 23-102.

determine the position in the curve of the person who was being tested. They would show what he was capable of at the time, and approximately how much conscious or unconscious practice he had already gone through, and the maximum efficiency to which the faculty under test admitted of being educated." (p. 208.)

On the same page Galton raises a protest against pursuing, in anthropometric measurements, the strict methods appropriate to psycho-physical investigations.

"We do not want to analyse how much of our power of discriminating between two objects is due to this, that or the other of the many elementary perceptions called into action. It is the total result that chiefly interests us."

Galton is content if the judgment finally made rests upon a blend of many different factors.

A good deal of the paper is taken up with a discussion of the means of passing people rapidly through a long series of tests with a minimum of supervision. This was on account of the finite period for which the laboratory was open and the cost of attendants, a necessity in Galton's case, but it is, I think, open to a certain amount of reasonable criticism. Our own plan in the Galton Laboratory is to have as far as possible one supervisor for each test or at least kindred group of tests. He is able to explain what is needed, assist the examinee when in doubt and save delicate instruments from rough handling. If such supervision is feasible I feel certain that examinees pass through at the maximum speed, and there is greater accuracy in the records.

In Galton's first laboratory no head measurements were made, and he discusses the omission on p. 210. They were purposely omitted under the peculiar circumstances of a mixed crowd.

"I feared it would be troublesome to perform on most women on account of their bonnets, and the bulk of their hair, and that it would lead to objections and difficulties¹."

However, Galton actually designed a spanner to take the height of the head from the auricular passages, and it came into use in Cambridge in 1885. In the case of this instrument he measures the maximum height

"above the plane that passes through the upper edges of the orbits and the orifices of the ears." (p. 210.)

Presumably he means the central line of the orifices and the *upper* border of the orbits. It is clear that Galton's standard plane differs on both counts from the Frankfurt horizontal plane².

The reader will find on pp. 211 and 217 descriptions of Galton's first instrument for measuring the swiftness of a blow (see our p. 220) and of what

¹ It may be worth noting that we feared the same trouble in our anthropometric laboratory at University College, but it has proved an idle fear. It is true bonnets no longer exist and hair is often 'bobbed.' Even if bulky it lies now-a-days low on the head and the head length is generally taken without disturbing it. Occasionally the tape measurements are a difficulty, but if help is asked for, I have found it most readily given as in passing the tape under the hair knot, or releasing the knot if necessary. I have been told occasionally that the hair will be let down if required, but there is usually a little justifiable pride about this offer, and a practised operator can take all the usual measurements fairly accurately without accepting it!

² The change to the *lower* border of the orbits was made at Cambridge later, but I do not know when.

appears to be an ingenious instrument for measuring delicacy of touch on the principle of the Roberval balance on p. 212. "The action of the instrument seems perfect, but it exists as yet only as a working model." I do not know whether any further account was published of it, nor how it exactly functioned physiologically.

There are two Appendices to the paper; the first contains chiefly extracts from the Exhibition pamphlet already discussed. The second indicates the methods Galton was using for the reduction of his material, and gives a certain number of his results¹. In this Appendix he says that he is prepared to admit that the persons who applied to be measured were not possibly a random sample of those who attended the Exhibition, nor the crowd who visited the Exhibition a random sample of the British population, but he considers that such a criticism must not be pushed unreasonably far. Probably the data afford materials for testing the relations between various bodily faculties, and the influence of occupation and birthplace. Although in this paper he is dealing chiefly with statical anthropometric characters—i.e. those of least interest—such treatment was a necessary preliminary to further discussion and served to exemplify Galton's method of the statistical scale, i.e. the use of percentiles. He considered—and at that time he was justified in considering—that he was

"presenting in a compact and methodical form a great deal more concerning the distribution of the measurements of man than has hitherto been attempted in a numerical form." (p. 275.)

Galton deals only with adult males and adult females, of whom there were 4726 of the former and 1657 of the latter.

His first table gives the maximum or highest records among these adult cases for seven characters.

Character	Highest Record	
	4726 Adult Males	1657 Adult Females
Stature without shoes ...	6 ft. 7·5 in.	5 ft. 10·3 in.
Weight	22 st. 0 lbs.	15 st. 8 lbs.
Vital Capacity	354 in. ³	270 in. ³
Strength of Pull	148 lbs.	89 lbs.
Strength of Squeeze	112 lbs.	86 lbs.
Swiftness of Blow	29 ft. per sec.	20 ft. per sec.
Keeness of Sight, distance of reading diamond type}	39 in.	40 in.

In all characters but sight the male had a higher record, and if we deal with the median values even there the male is higher than the female. Galton concludes that

"the female differs from the male more conspicuously in strength than in any other particular, and therefore that the commonly used epithet of 'the weaker sex' is appropriate." (p. 278.)

The lady, however, who could give a squeeze of 86 lbs.² is not to be despised,

¹ See "Some Results of the Anthropometric Laboratory," *J. A. I.* Vol. xiv, pp. 275-87.

² This is slightly in excess of the squeeze of the *median* adult male. Galton remarks that

and the idea of her tickled the fancy of *Mr Punch*, who in the issue that followed the publication of Galton's paper thus apostrophises her:

The Squeeze of 86.

Maiden of the mighty muscles,
There recorded, you would be
Famous in all manly tussles,
And its very clear to me,
That if in the dim hereafter
Any husband should play tricks,
You would with derisive laughter,
Give a "Squeeze of 86."

Husbands be it sadly stated,
Have been known their wives to whack,
You, unless you're over-rated,
Could give such endearments back.
Yours the task to try correction,
Till your husband and your "chicks,"
Had a lively recollection
Of your "Squeeze of 86."

Punch, April 15, 1884.

Galton's second table exhibits the full advantages of his method of percentiles, but presents also its disadvantages, as it does not allow us to deduce the usual frequency constants with any reasonable accuracy. At the same time it is easily understood by the non-statistical, who can readily determine from it their rank for any character. Thus suppose a man between 23 and 26 years of age to have a vital or breathing capacity of 190 cubic inches, he sees at once from the table that he is surpassed by 70 % of men (199) and himself surpasses 20 % (187). A simple rule of three sum then indicates to him that he lies $\frac{3}{12}$ or $\frac{1}{4}$ of the way from 80 to 70 or at 77.5. In other words he would rank between the 77th and 78th men in a population of 100 young adults similar to those who visited the anthropometric laboratory.

The ages and numbers in the table¹ on the following page possibly require some justification. All Galton says is that he had

"groups of appropriate cases extracted from the duplicate records by Mr J. Henry Young of the General Registry Office. I did not care to have the records exhausted, but requested him to take as many as seemed in each case to be sufficient to give a trustworthy result for these and certain other purposes to which I desired to apply them. The precise number was determined by accidental matters of detail that in no way implied selection of the measurements." (p. 278.)

Now-a-days we should consider it needful to keep the probable errors in view, which are occasionally somewhat large for small series treated by the method of percentiles. Galton deduced his percentiles from the frequency distributions by summing, plotting, drawing a curved line through the plotted points and then interpolating for the actual percentiles by graphical interpolation².

"the population of England hardly contains enough material to form even a few regiments of efficient Amazons."

¹ This table with the description of its method of preparation was also published in *Nature*, January 8, 1885 (Vol. xxxi, pp. 223-5), a month or two before its appearance in the *Journal of the Anthropological Institute*.

² The method with more detail was discussed by Galton under the title "The Application of a Graphic Method to Fallible Measures" in a paper communicated to the Jubilee Congress of the Royal Statistical Society and published in the *Jubilee Volume of the Statistical Society*, 1885, pp. 262-5. The method consists in drawing an ogive curve from the data and interpolating for the percentiles. The same end could be reached by integrating the frequency histogram and dividing its final ordinate into equal parts corresponding to the percentiles. This may, of course, be rapidly done by the integrator.

Anthropometric Percentiles¹.

Values Surpassed and Values Unreached, by various percentages of the persons measured at the Anthropometric Laboratory at the late International Health Exhibition.

Values surpassed by percents. below

Character measured	Age	Unit of measurement	Sex	No. of Persons	95%	90%	80%	70%	60%	50%	40%	30%	20%	10%	5%
					Values unreached by percents. below										
					5%	10%	20%	30%	40%	50%	60%	70%	80%	90%	95%
Height standing without shoes	23—	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
	51	"	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—	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
	51	"	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—	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
	51	"	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—	Pounds	M.	520	121	125	131	135	139	143	147	150	156	165	172
	26	"	F.	276	102	105	110	114	118	122	129	132	136	142	149
Vital or Breathing Capacity	23—	Cubic	M.	212	161	177	187	199	211	219	226	236	248	277	290
	26	Inches	F.	277	92	102	115	124	131	138	144	151	164	177	186
Strength of Pull as archer with bow	23—	Pounds	M.	519	56	60	64	68	71	74	77	80	82	89	96
	26	"	F.	276	30	32	34	36	38	40	42	44	47	51	54
Strength of Squeeze with strongest hand	23—	Pounds	M.	519	67	71	76	79	82	85	88	91	95	100	104
	26	"	F.	276	36	39	43	47	49	52	55	58	62	67	72
Swiftness of Blow	23—	Feet per second	M.	516	13.2	14.1	15.2	16.2	17.3	18.1	19.1	20.0	20.9	22.3	23.6
	26	"	F.	271	9.2	10.1	11.3	12.1	12.8	13.4	14.0	14.5	15.1	16.3	16.9
Keeness of Sight by distance of reading diamond type	23—	Inches	M.	398	13	17	20	22	23	25	26	28	30	32	34
	26	"	F.	433	10	12	16	19	22	24	26	27	29	31	32

Taking the table, it can be shown by plotting in reverse directions that the male and female ogive curves, for squeeze of hand say, intersect at 7%, or if we had to select the 100 strongest individuals out of 100 men and 100 women taken at random, we should select 93 men and seven women. On this Galton makes the comment:

"Very powerful women exist, but happily perhaps for the repose of the other sex, such gifted women are rare." (p. 278.)

It is probable that if Galton had taken the fisherfolk of Aberdeen, the peasantry of the Tyrol, or any primitive people, he would have found a considerably greater percentage of women in his 100 strongest individuals. He was largely dealing with a town population. Our experience in the Galton Laboratory shows that there are quite a few characters in which the absolute values reached by the women equal or excel those of the men, notably in Memory of Form, Discrimination of Colour, Steadiness of Hand, and Speed-Accuracy of Hand.

¹ The value that is unreached by $n\%$ of any large group of measurements and surpassed by $(100 - n)\%$ is called the n th percentile.

At this point in his paper Galton draws attention to a "Common Error in Statistics¹," which he says has not hitherto attracted attention. It occurs in a classification, such as "65—" for stature, which is assumed to mean 65 inches and less than 66 inches. He points out that the centre of this group is usually taken as 65.5 inches, but that this is most often erroneous, and that the true centre of the group (if we arrange the group uniformly over its range) may differ very considerably from this. The true centre depends on the fineness of our readings. If, for example, we read stature to the nearest half-inch only, then all measurements in excess of 64.5 would be included in 65, and all measurements in excess of 65.5 in 66. The former would all appear in "65—" and the latter would not. Thus the centre of "65—" would be actually the centre of the range 64.5 to 65.5 or 65 and not 65.5. In other words, there would be half an inch error in the resulting mean. Most tyros in statistics are now aware of this point, i.e. that the centre of a subrange depends upon the fineness or coarseness of the readings. It is, therefore, startling to think that Galton was the first to point out this pitfall even as late as 1884!

The next paragraphs are of some historical interest. Galton is seeking to find a relation between various characteristics, and considering the old idea of the ratios of anatomical measurements as constants. He notes that in certain cases these ratios are not constant (p. 281). Galton also gives rough growth curves for vital capacity, but has missed the point of contraflexure in the neighbourhood of puberty, which his own data treated more in detail exhibit. These are cases of what we should now term skew regression. Galton, we notice, had not yet reached in 1884 the full conception of correlation. He gives (p. 285) what we should now term a correlation table between vital capacity and strength of squeeze for 522 males of ages 23, 24 and 25. He says that he was surprised to find no close relation between these two characters.

"The importance of a large breathing capacity to a man who expends force rapidly, as to a runner or mountain climber, is undoubted, but for a strain of short duration it seems comparatively non-essential." (p. 284.)

He reaches his conclusion by comparing the means for strength of squeeze corresponding to vital capacities of 150 and 300 cubic inches. He says there is a difference of 17 lbs. But without a measure of the variabilities we could not deduce any measure of the association of the two characters. Now-a-days we should say that the coefficient of correlation was certainly not negligible: it is $.40 \pm .02$ from his table; but we are able to express the intensity of the association in this simple manner, only because Galton later taught us how to do it².

Some data are given for the relationship of right and left sides. Thus Galton found the left hand to be about 6% weaker than the right. The right and left eyes on the average had equal keenness of sight. Galton

¹ See p. 280 of the present paper. Galton also contributed a paper on the same point to the *Jubilee Volume of the Statistical Society*, June 22-24, 1885, p. 261.

² Galton's wording is rather vague, but I think there is more correlation than his phrase would suggest.

investigated whether there was any relation between superior strength of the right or left hand and superior reading power of the right or left eye. Presumably he made a correlation table; he tells us that he found no association, but he considers that an association would be less improbable had he been able to compare the difference in skill of right and left hands with the difference in vision of the right and left eyes.

The paper concludes with the data on highest audible note referred to on our p. 221.

A study of this paper suggests at once the road along which Galton was being carried towards the conception of correlation; we shall see soon how he recognised that the numerical association between two anthropometric variates and the quantitative measure of the intensity of the hereditary factor were one and the same mathematical problem.

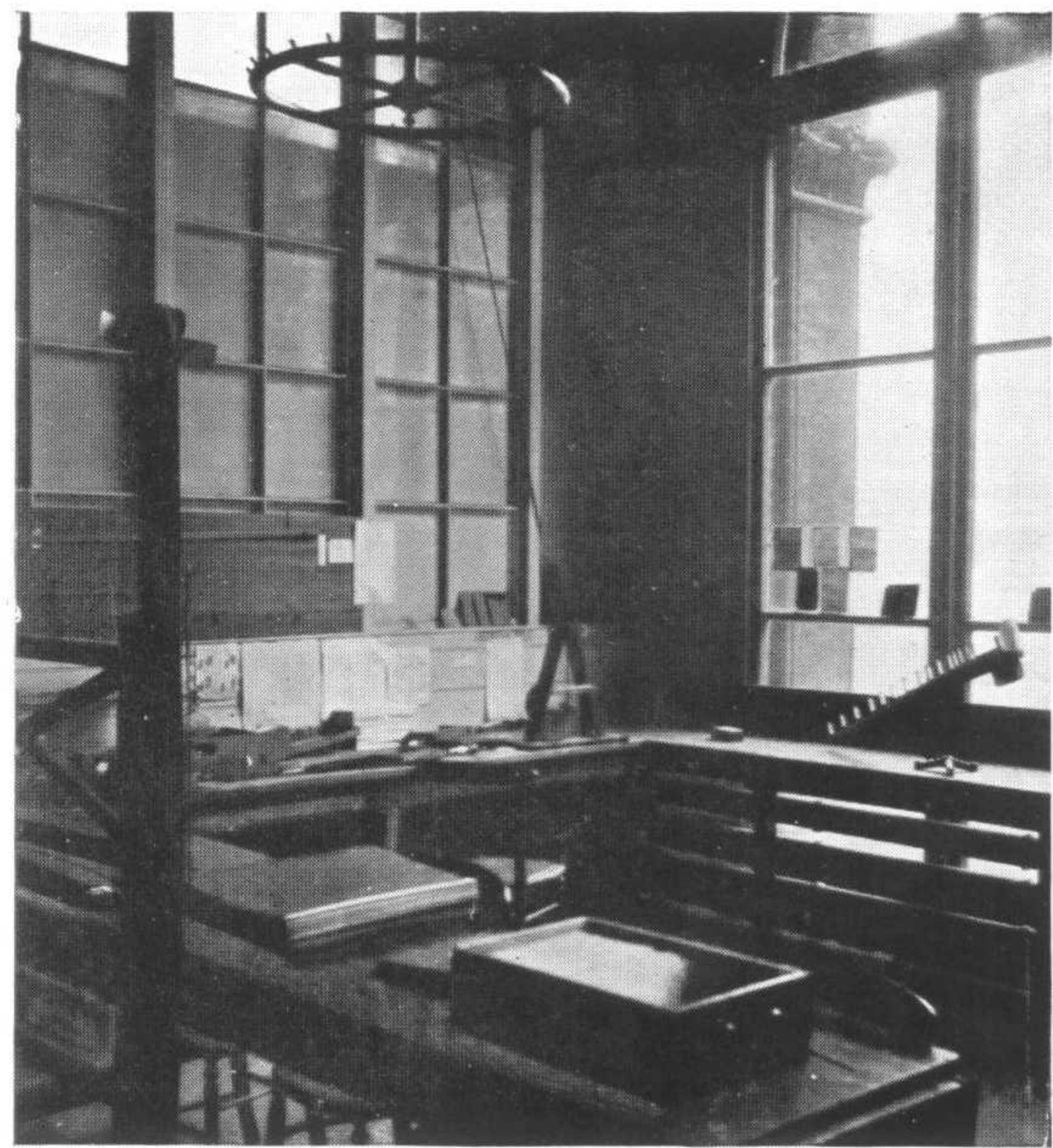
While Galton began immediately to use his data for the problem of correlation he did not hesitate to place portions of it before other investigators that they might if they liked reduce it by other methods. Thus in 1889 he published¹ data for 400 of the 518 individuals, extracted by Mr J. H. Young (see our p. 375). These data are for adults aged 23, 24 and 25, and for other individuals than those he himself used in his 1888 Royal Society memoir on *Correlation*. They give Age, Status (married or single), Eye Colour, Birthplace, Occupation, Residence, Vital Capacity, Squeeze of both hands, Span, Sitting Height, Stature and Weight in ordinary indoor clothing. As far as I know they have never been reduced, and would prove rather inadequate in number if allowance were made for birthplace and occupation. For a really full discussion of these matters, it would be needful to return to the much fuller material in the far more numerous original records.

Two further papers remain to be touched on, although of a much later period than the first Anthropometric Laboratory. It will, I think, be wiser to take the later of these first, because it gives us more of the history of his laboratories. It is entitled: "Retrospect of Work done at my Anthropometric Laboratory at South Kensington," and was published in 1891².

Galton's Anthropometric Laboratory on the closing of the Health Exhibition in 1885 had been transferred to a piece of vacant ground, which later was taken over by the Imperial Institute. Here the laboratory, under Serjeant Randal as Superintendent, continued its work for five years, and an additional 3678 persons were measured. New measurements and observations were made, including the wonderful collection of finger-prints now in the *Galtoniana*. But Galton after six years' experience had begun to realise that an Anthropometric Laboratory cannot remain stationary either in its methods or instruments. It must always be starting new inquiries, and needs for this purpose a scientific research staff. He had himself used his laboratory for various special researches, such as the Finger-Print inquiry, the question

¹ *Journal of the Anthropological Institute*, Vol. xviii, pp. 420-30.

² *Ibid.* Vol. xxi, pp. 32-5. The Laboratory was dismantled in February 1891, and reopened August 3, 1891. Galton states in his *Retrospect* that the South Kensington Museum authorities had offered to place a larger and better lighted space at his disposal under their own roof.



Francis Galton's Second Anthropometric Laboratory, South Kensington Museum, 1891-95.

of marks for physical tests, and the problem of the symmetry of the two sides of the body. Such inquiries required an ever available scientific director, and Galton himself was far too busy with the multitude of claims on his energy and executive skill to undertake such duties. He writes:

"In brief, what little has been accomplished at the laboratory during the three years of its existence justifies to my own mind the trouble and expense I have been put to in building, equipping and maintaining it. But it never reached to my ideal. Besides the objects already named, I was almost equally desirous of establishing a place where the keenness of the senses and other faculties in any individual who applied, might be measured with all the accuracy and painstaking that is achieved by the few biologists who occupy themselves seriously in such pursuits. To effect this it would be necessary to secure the occasional services of a skilled experimenter and to ensure at the same time that a sufficiency of persons should come to be measured. The time did not seem to have arrived for such an enlargement of the existing methods, though I hoped and still hope that it may not be far distant, as the utility of the laboratory becomes more widely appreciated. The measurements thus far employed are of a comparatively rude, but not ineffective, character. It would give me pleasure at any time to receive suggestions as to new and useful special inquiries, such as might be carried out and brought to conclusion without a too serious expenditure of time and effort."

But although Galton was very modest about what his laboratory could achieve in the future without a scientific staff, it really had accomplished a great deal.

"Persons of all ranks went to it¹, a knowledge of its existence was extending, and it was becoming increasingly frequented up to the day of its closure. Many correspondents in the United Kingdom, in America, and elsewhere, have more or less adopted its methods, and it was, I may add, a great consolation to me to receive, on the very day that I began to dismantle it, the proof sheets of the register, and other forms in many respects like my own, that are to be used in the laboratory at Dublin, which has been set on foot through the efforts, and will be carried on under the superintendence, of Professors Cunningham and Haddon." (p. 32.)

Immediately following² Galton's *Retrospect* is a paper by Cunningham and Haddon entitled "The Anthropometric Laboratory of Ireland." They say that Mr Galton, who has given them every encouragement in their work, proposed that they should give some account of the steps they were taking to introduce anthropometric work into Ireland and their aims in doing so.

"We need hardly explain in the Institute where the important and interesting results obtained by Mr Galton in this field of inquiry have been so largely made known, that it was these that stimulated us to endeavour to do likewise in Ireland." (p. 35.)

Directly or indirectly Galton's Laboratory was the parent of Anthropometric laboratories at Eton, Dublin and Cambridge; indeed of the much later work also of Schuster at Oxford. Just as Galton generally transferred his laboratory to the varying loci of the British Association, so Cunningham and Haddon proposed peregrinations for their laboratory during the Long

¹ Among whom we may note Mr W. E. Gladstone, whose head measurements afterwards were as serviceable to Mr Brock, as those of the Biometric Laboratory on the head of Professor Weldon were to Mr Hope Pinker—both being used for posthumous portraiture. Gladstone was amusingly insistent on the size of his head, saying that hatters often told him that he had an Aberdonian head—a fact which he did not forget to tell his Scottish constituents. It was not, however, of very great circumference and rather low (like Sir Thomas Browne's and Bentham's). It was less than Spottiswoode's, Sharpey's and Galton's own. "Have you ever seen as large a head as mine?" Gladstone said to Galton, on which the latter observed: "Mr Gladstone, you are very unobservant!"

² *Journal of the Anthropological Institute*, Vol. XXI, pp. 35-9.

Vacations to special "ethnic islands" in Ireland in the hope of unravelling "the tangled skein of the so-called 'Irish Race.'" The peripatetic principle of Galton was again adopted by Gray and Tocher in their Scottish surveys. If the people will not come to you, you must go to the people, and stand beside the itinerant dentist and the cheapjack in the market-place and measure their crowds.

Looking back on those years during which the Anthropometric Laboratory existed we ask what definitely was achieved? Well, (i) An immense amount of material was collected, which only forty years later is being adequately reduced. (ii) From small portions of it Galton deduced the foundations of the correlational calculus. Here Galton evolved an entirely new principle described in his paper of 1889, yet to be placed before the reader. This was in itself a very big achievement, much bigger in the light of what has followed than it might have appeared to casual observers of that day. It reduced

"all forms of correlation, including hereditary qualities, to one simple law, namely that of the relation between two variables partly dependent on a common set of influences."

(iii) It led to laws of growth and development whose study had hitherto been impossible, and it enabled investigators to take account of social status and of occupations. (iv) The limitations of such systems of personal identification as that of Bertillon became apparent and this prepared the way for Galton's finger-print system combining indexing with identification. (v) It provided material by which many interesting problems could be solved, such as diurnal changes in measurement, fallibility of the measurer, and the limits of change after adult age in various anthropometric "constants."

Lastly the Laboratory gave a most vigorous push to anthropology; it indicated what might be achieved by anthropometry taken seriously and the impetus is yet far from exhausted. There are still anthropologists who believe that great racial problems can be solved by juggling with a few cephalic indices. They do not recognise that a human being is a vast congeries of faculties, and that only certain of these are highly correlated. Others are practically independent and are largely modified by intermixture in each new generation. It is wholly impossible to define an individual, still less a race or associated group of individuals, on the basis of a single character. It requires a great many measurements to describe with moderate accuracy an individual, and quite as many to characterise, or provide the type of, a local group of men. Anthropometry whether physical, psychical or medical has this end in view: the definition of Type—in particular racial type—by the measurement of a fairly representative system of characters. Anthropologists learnt from Galton's Anthropometric Laboratories not only the importance of their task, but how to set about it. They learnt for the first time to what extent characters are correlated and how to measure the degree of their association. That could only result, when the investigator learnt to deal with the whole system of variations and did not occupy himself simply with the averages of characters. The last quarter of the nineteenth century revolutionised anthropology, and Galton was the main mover in those momentous changes.

We now turn to the second memoir, published in 1890, entitled: *Anthropometric Laboratory, Notes and Memoirs*, No. I. This seems to have been printed at Galton's expense and was probably sold (price 3*d.*) at the second Anthropometric Laboratory in the Science Museum, South Kensington. The pamphlet, now unpurchasable, consists of four chapters, thirty-two pages in all. The first chapter bears for title: "Why do we measure mankind?" Galton first answers the supposed reader, who may say, "I do not care for science, why then should I go and be measured?" For this "very cynical but not quite imaginary speaker" Galton writes as follows:

"The cost of being measured has been proved to amount to something between 3*d.* and 1*s.*, and the real question is whether it is worth your while to pay a shilling at a maximum to have yourself or your boys and girls measured."

Galton indicates first the advantage to non-adults; given age, sex and social position, we are able to tell them how they rank among their contemporaries.

The value of testing sight is specially emphasised and it is pointed out how a knowledge of mischief may lead to the removal of its source. Then Galton notes how often colour-blindness has been discovered late in life and after possibly a failure has been made in a profession where it is an absolute bar. He cites the case of a widow bringing a son of 18 years to the laboratory and getting quite angry with him for his supposed stupidity in blundering between reds and greens, quite unconscious that there was such a natural incapacity as colour-blindness!

It is then pointed out how important to an adult is a knowledge of his strength and his vital capacity, and how valuable a warning may be, not only in the selection of an occupation, but in the case of an incapacity arising from unrecognised causes.

"A register of measurements resembles a well-kept account-book. It shows from time to time the exact state of a man's powers, as the account-book shows that of his fortune. Whatever may be whispered by the inner voices of vanity or of envy, no sane and experienced person can doubt the enormous difference between the natural gifts of different men, whether in moral power, in taste, in intellect, or in physical endowments. Those who have frequently pitted themselves fairly against others, doing their very best to succeed, must have often known what it is to be utterly beaten by their natural superiors. It is only those who have kept aloof from contest who can possibly flatter themselves with the belief that their failures are wholly due to circumstance and in no degree to natural incapacity. Such persons will quickly be awakened from their self-conceit by submitting themselves to measurement and thereby ascertaining their exact rank in each several respect. They will be sure to receive a good moral lesson from the results." (p. 8.)

This passage explains to a considerable extent why Galton preferred the method of grades to the use of frequency curves. He liked a system in which the uninitiated could put his finger on a single number and say: "I am worse than so many per cent. of men and better than so many per cent." That is what the method of Anthropometric Percentiles achieves and the fundamental diagram reproduced on our p. 390 appears on p. 23 of this reprint.

¹ It also appears in *Lippincott's Magazine* for February, 1890.

The next advantage of anthropometry Galton finds in its *industrial* value, a matter which some of our moderns believe they have discovered for the first time.

“Employers of labour might often find it helpful to require a list of laboratory measurements when selecting between many candidates who otherwise might be equal in merit. Certainly a man who was thereby shown to be measurably much more highly endowed than the generality of his class with physical efficiency, would have a corresponding high chance of being selected for any post in which physical efficiency of the kind tested was of advantage. I have great hope of seeing a system of moderate marks for physical efficiency introduced into the competitive examinations of candidates for the Army, Navy and Indian Civil Services.” (p. 8.)

We have here a point to which we must shortly return—the question of marks for physical proficiency, which Galton strongly advocated. I might be inclined to go somewhat further and suggest that when there is doubt—and there often is—between the intellectual merits of two candidates the *mental* tests of a well-organised anthropometric laboratory would effectively discriminate. The County Council educational authorities are annually in difficulty in the award of their secondary scholarships, not about the boys or girls at the top, but those at the bottom of the selected list, where there are numbers on a nearly dead level, with no adequate examinational differences to guide the judgment. It would be a valuable and justifiable procedure to further separate out the better of these candidates by well-chosen anthropometric tests, which would be certain to appeal to faculties whose differentiation could not be achieved by a written examination.

Another advantage Galton finds in anthropometry is the registration of individuals for identification. Honest men may need identification as well as rogues, and the measurements, especially if finger-prints are included, would suffice to identify any one between the ages of 20 and 60.

Apart from the—shilling’s worth of—advantage to the individual, Galton emphasises the scientific ends which can be attained by anthropometric laboratories. He refers to problems such as whether the promises of youth are fulfilled in adult life; if a boy is of high rank among his compeers is it an indication of superior future efficiency in the man? Another problem is that of the influence of education or practice upon both mental and physical characters. Again the influence of environment could be tested as soon as we have precise measures of faculties. Galton notes that even if there be a rapid rise in the efficiency of any factor due to training or environment, it soon reaches a condition in which the daily improvement lessens and at last stands still; the limit of perfectibility has been reached.

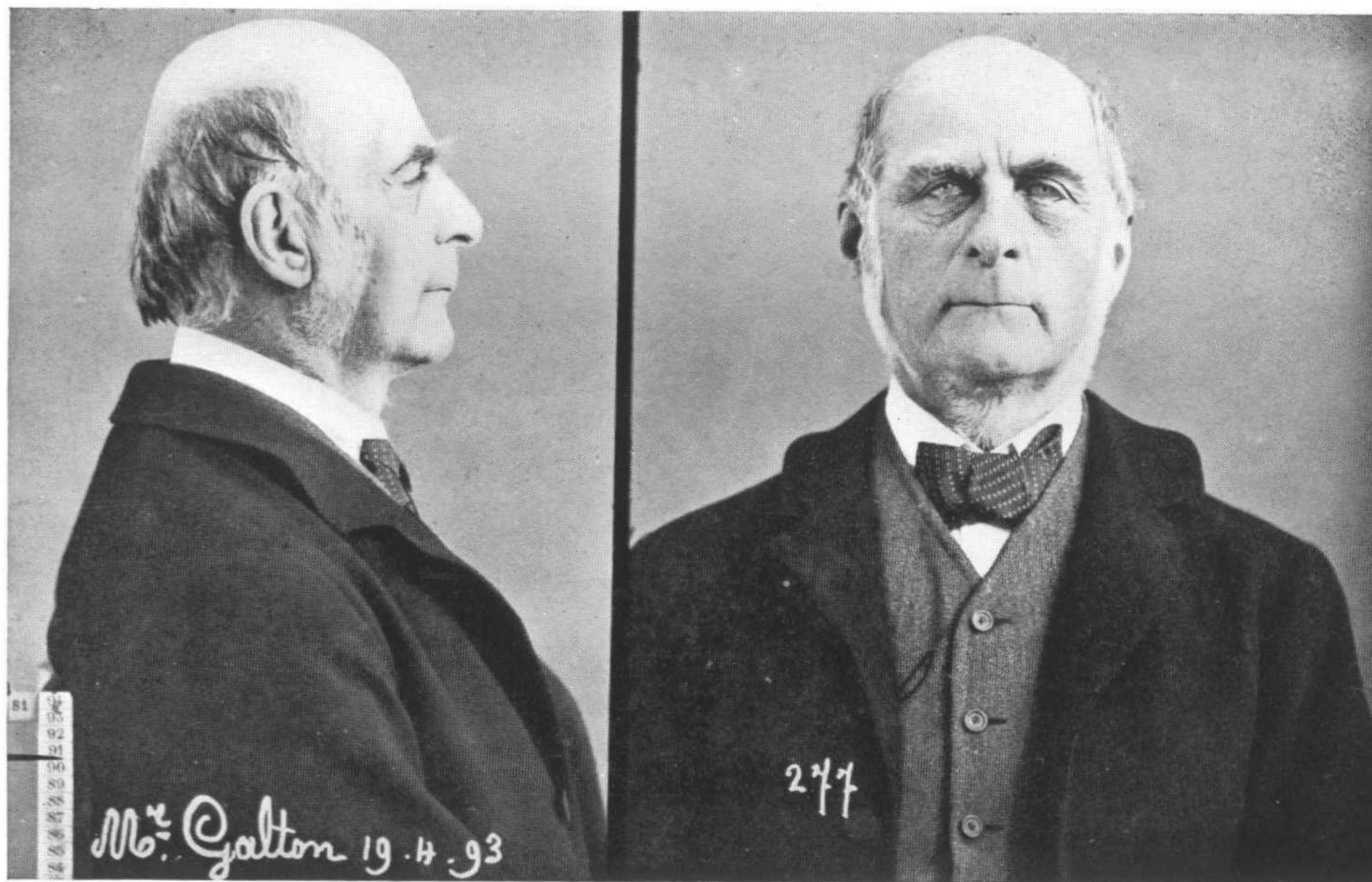
“Experiences of this kind on a large enough scale to give trustworthy results would have a direct bearing on the science of education.” (p. 10.)

Lastly Galton deals with the educational value that a habit of measurement has in promoting accuracy in ideas and language:

“The present vague way in which men mostly estimate and describe the performances of themselves or others, testifies to much muddleheadedness and to a sad lack of expression.... There is a world of interest hidden from the minds of the great majority of educated men, to whom the conceptions and laws of the higher statistics are unknown. A familiarity with these conceptions would soon be gained by the habit of dealing with human measurements, as by the

Taille 1 ^m	Oreille dr. / Iete.	Long ^r	Pied g.	Coul ^r de l'iris } N° de cl. Aur ^{le}	Agé de
Voûte		Larg ^r	Médius g.		né le:
Enverg 1 ^m		Long ^r	Auric ^{le} g.		a
Buste 0,		Larg ^r	Coudée g.		Part ^{ie}

(Réduction photographique 1/7.)



Front.	Inclin ^a	Sez.	Racine (cavité)	Oreille droite.	Bord. o. s. p. f.	Barbe	Color ^a (pig ^{me})
			Dos Base		Lob. c. a. m. d.	Cheveux	Color ^a / sang ^a
			Haut ^r Saillie. Larg ^r		A. trg. i. p. r. d.	Car Ceint.	Autres traits caractéristiques :
			Larg ^r l l		Pli. f. s. h. E	Part ^{ie}	Sig ^{et} dressé par M.

Francis Galton, aged 71, photographed as a criminal on his visit to Bertillon's Criminal Identification Laboratory in Paris, 1893.

assignment of rank in a class, or by making other deductions that I have not space to refer to here, such as the numerical values by which the nearness of different degrees of kinship may be expressed, or the closeness of correlation between different parts of the body. There is no intrinsic difficulty in grasping the conceptions of which I speak, but they are foreign to present usage and look strange at first sight. They are consequently very difficult to express briefly and intelligibly to those to whom they are wholly new."

It will be seen that as early as 1889 Galton was fully assured that the ideas of a correlational calculus opened a new world of thought, not only to the trained scientist, but to every man of education who could master his natural inertia and endeavour to grasp the new conceptions.

Chapter II of the pamphlet is entitled "Human Variety." This was Galton's final address to the Anthropological Institute at the end of the four years during which he had held the office of president¹. The paper opens with a paragraph on finger-prints which shows that Galton was working at the subject, and already fully recognised its importance for identification. He especially refers to Sir William Herschel's use of the method in India, and suggests its use in North Borneo for identifying the coolies, and in other cases where there may be fraud from impersonation of pensioners and annuitants. Galton then turns to correlation,

"a very wide subject indeed. It exists wherever the variations of two objects are in part due to common causes; but on this occasion I must only speak of such correlations as have an anthropological interest."

He tells us that the particular problem he first had in view was to ascertain the practical limitations of the ingenious method of anthropometric identification due to M. Alphonse Bertillon, which was then in habitual use in the criminal administration of France. Correlation between the various measurements would obviously be a serious defect of the Bertillon system, and Galton suspected strongly the existence of this source of error². An element of history is now revealed:

"The first results of the inquiry, which is not yet completed, have been to myself a grateful surprise. Not only did it turn out that the measure of correlation between any two variables is exceedingly simple and definite, but it became evident almost from the first that I had unconsciously explored the very same ground before. No sooner did I begin to tabulate the data than I saw that they ran in just the same form as those that referred to family likeness in stature,

¹ Delivered Jan. 22, 1889. See *Journal of the Anthropological Institute*, Vol. xviii, pp. 401-19.

² The first complete analysis of these correlations was given by the late Dr W. R. Macdonell and he indicated how an index could be constructed of artificial functions of the Bertillon measurements in which this difficulty of correlation would be satisfactorily surmounted, *Biometrika*, Vol. I, 1902, pp. 177-227, "On Criminal Anthropometry and the Identification of Criminals." He shows that the correlations of the Bertillon measurements are high, far too high for indexing purposes. Galton first expressed his doubts on this point after Bertillon's discourse on his system before the Anthropological Institute. "There may be room for reasonable doubt among anthropologists whether the precision with which the living body can be measured is quite as great, and whether its dimensions are quite as permanent, as they are considered to be by M. Bertillon; and again there may be some hesitation in believing that a very large collection of measures would admit of being so surely catalogued on the Bertillon system as to be ransacked with a promptitude at all corresponding to that with which a word may be found in a huge dictionary" (*Journal of the Anthropological Institute*, Vol. xx, p. 198). This was one of the early clashes in the contest between the Bertillon and Galton systems, which was to end ultimately with the victory of the latter.

and which were submitted to you two years ago. A very little reflection made it clear that family likeness was nothing more than a particular case of the wide subject of correlation, and that the whole of the reasoning already bestowed upon the special case of family likeness was equally applicable to correlation in its most general aspect." (p. 14.)

This passage justifies the assertion that Galton came first to correlation from the study of heredity; that, when testing Bertillon's system of indexing anthropometric measurements, he discovered correlation afresh and then saw that both associations were capable of identical treatment, and only special cases of a far wider conception.

But there was another point Galton now recognised, in dealing with heredity he had compared the *same* characters; he now saw that in comparing different characters we must take each in its own unit of variation, and that when this is done the relations become strictly reciprocal¹. In this paper Galton states, probably for the first time, that measured in units of variation, there is always 'regression,' i.e. that in these units the proportion that one average deviation of one character is of a given deviation of a second is always a proper fraction. (p. 15.)

The next section of this chapter is headed "Variety," and Galton laments that, while an immense amount of trouble is taken over measurements, anthropologists devote their inquiries solely to the means of groups, passing over the variety of the individuals in those groups with "contented neglect." The whole section is so thoroughly characteristic of Galton's attitude to anthropology and of his genial sagacity that I cannot refrain from reproducing it almost entirely; there is much still to be learnt from its perusal even forty years after it was written.

"It seems to be a great loss of opportunity when, after observations have been laboriously collected, and been subsequently discussed in order to obtain mean values from them, that the small amount of extra trouble is not taken, which would determine other values whereby to express the variety of all the individuals in those groups. Much experience some years back, and much new experience during the past year, proved to me the ease with which variety may be adequately expressed, and the high importance of taking it into account. There are numerous problems of special interest to anthropologists that deal solely with variety.

There can be little doubt that most persons fail to have adequate conceptions of the orderliness of variability, and think it is useless to pay scientific attention to variety, as being, in their view, a subject wholly beyond the powers of definition. They forget that what is confessedly undefined in the individual may be definite in the group, and that uncertainty as regards the one is in no way incompatible with statistical assurance as regards the other. Almost everybody is familiar now-a-days with the constancy of the average in different samples of the same large group, but they do not often realise the completeness with which a similar statistical constancy permeates the whole of the group. The mean or the average is practically nothing more than the middlemost value in a marshalled series. A constancy analogous to that of the mean characterises the values that occupy any other fractional position that we please to name such as the 10th per cent., or the 20th per cent.; it is not peculiar to the 50th per cent., or middlemost. Still less do they realise the fact that all Variety has a strong family likeness, by approximating more or less closely to the normal type, which is that which mathematicians prove must be the consequence of Variety being due to the aggregate effect of a very large number of small and independent influences.

Greater interest is attached to individuals who occupy positions towards either of the ends of a marshalled series than to those who stand about its middle. An average man is morally

¹ In modern language Galton recognised that with standard deviations as units, the 'regression' is equal to the correlation coefficient.

and intellectually an uninteresting being. The class to which he belongs is bulky and no doubt serves to help the course of social life in action. It also affords, by its inertia, a regulator that, like the fly-wheel to the steam-engine, resists sudden and irregular changes. But the average man is of no direct help towards evolution, which appears to our dim vision to be the goal of all living existence. Evolution is an unresting progression; the nature of the average individual is essentially unprogressive. Consider the interest attached to the Hebrew race, whose average value is little worthy of note, but which is of special importance on account of its variety. Its variability in ancient and modern times seems to have been extraordinarily great. It has been able to supply men, time after time, who have towered high above their fellows, and left enduring marks on the history of the world.

Some thoroughgoing democrats may look with complacency on a mob of mediocrities, but to most other persons they are the reverse of attractive. The absence of heroic gifts among them would be a heavy set off against the freedom from a corresponding number of very degraded forms. The general standard of thought and morals in a mob of mediocrities must necessarily be mediocre, and, what is worse, contentedly so. The lack of living men to afford lofty examples, and to educate the virtue of reverence, leaves an irremediable blank. All men would find themselves at nearly the same dead average level, each being as meanly endowed as his neighbour. These remarks apply with obvious modification to variety in the physical faculties. Peculiar gifts, moreover, afford an especial justification for division of labour, each man doing what he can do best." (pp. 15-16.)

Could any man but Galton have written a sermon like the above to bring out the essential meaning to humanity of—let us say—the standard deviation or the quartile? Could any man have written thus but Galton without laying himself open to the charge of self-conceit? He did not regard himself as one whose faculties gave him rank in the extreme tail of a frequency distribution. He would have recognised such a position for half his friends, before he thought of it for himself. He pictured life statistically, and with the naiveness of a child spoke the truth, forgetting that he was talking to a crowd the bulk of whom must have been sufficiently introspective to doubt whether they were not themselves mediocrities. Perhaps there is little to wonder at, if Galton's words fell on barren ground, and his brother anthropologists have continued for more than thirty years to neglect variation and cultivate mediocrity by ascertaining means.

Galton in his next section, "The Measurement of Variety," proceeds to describe his method of reaching the median M and the quartile Q from *any* two sets of observations, i.e. from any two percentiles, thus generalising his previous results, for directly finding M and Q . He suggests that a traveller among savage peoples might test them by finding what percentages could stretch two (or better three) bows, the bows having been previously tested by the numbers of pounds' weight which were required to stretch them to the full¹. Similar tests might be easily applied to the delicacy of hearing and eyesight, to swiftness in running, accuracy of aim, with spear, arrow, sling, etc.

¹ Let x_1 and x_2 be the unknown distances from the median m , of the two known test values t_1 and t_2 . Then $x_1' = x_1/\sigma = (m - t_1)/\sigma$ and $x_2' = x_2/\sigma = (t_2 - m)/\sigma$, where σ is the standard deviation, can be found at once from the tables of the probability integral, taking as standard case, one percentage above and one below the median (50%). Solving for m and σ we have:

$$m = (x_2' t_1 + x_1' t_2)/(x_1' + x_2') \text{ and } \sigma = (t_2 - t_1)/(x_1' + x_2'),$$

which solve the problem. Galton works with the quartile instead of σ , but assumes the normal relation of the two.

The method is very suggestive and easy, but it assumes the normal distribution of frequency to be the rule in the variation of anthropometric characters. To demonstrate this we should have to compare our percentiles in each case with the theoretical distribution. There is no doubt that a large number of physical measurements in man follow accurately enough for practical work the normal distribution but the rule is far from universal. Galton found when his deviations for all characters were reduced to their respective quartiles, that the average value of all the deviations at each of the grades in the eighteen series closely corresponded to the normal series, though individually they differed more or less from it, some in one way, some in another. He gives the following table of deviations:

	5°	10°	20°	30°	40°	50°	60°	70°	80°	90°	95°
Normal Frequency	-2.44	-1.90	-1.25	-0.78	-0.38	0	0.38	0.78	1.25	1.90	2.44
Average of observed	-2.44	-1.87	-1.24	-0.77	-0.40	0	0.38	0.75	1.21	1.92	2.47

This, while not demonstrating the truth of the normal distribution for every anthropometric measurement, suggests at least that on the average there is no persistent deviation from it.

Galton concludes this chapter by stating that the properties of the law of frequency of error are

“largely available in anthropometric inquiry. They enable us to define the trustworthiness of our results, and to deal with such interesting problems as those of correlation and family resemblance, which cannot be solved without its help. Anthropologists seem to have little idea of the wide fields of inquiry open to them as soon as they are prepared to deal with individual variety and cease to narrow their view to the consideration of the Average.” (p. 21.)

F. MARKS FOR BODILY EFFICIENCY

Galton's Chapter III is entitled: “On the Advisability of Assigning Marks for Bodily Efficiency in the Examination of Candidates for those Public Services in which Bodily Efficiency is of Importance¹.” We reach at this point a topic in which Galton had a great and persistent interest, namely the desirability of giving marks in competitive examinations for physical as well as mental proficiency. The development of anthropometric laboratories with more or less standardised tests rendered such a proposal fairly feasible, and for a time Galton pushed it energetically. It is of course related to his earlier proposals for the selection of eugenic youths for endowment, these being determined by both intellectual and physical examination.

Galton read his paper before the Anthropological Section of the British Association in 1889, and obtained a Report from the Council of that body in favour of the proposal in the following year. He wrote at this time several articles urging its importance and he lectured on the subject. Galton says in his *Memories* that he became convinced that although the proposal had strong

¹ This is part of the British Association memoir of 1889: see *Report*, pp. 471-3.

à priori claims to consideration, it did not merit acceptance, and therefore he gave it up. But it is difficult to discover a really strong answer to his papers, and it will undoubtedly be revived some day, when the official world is slightly more enlightened. Probably marks will be given for a complete series of anthropometric tests, mental as well as physical. There can be no better remedy against cramming than examinations of this kind wherein it is quite easy to vary the tests, and to prevent anything but general intelligence and good physique scoring.

In 1878 a Joint Committee of the War Office and Civil Service Commissioners had been appointed to inquire into the question

“whether the present literary examinations for the army should be supplemented by physical competition.”

The recommendations of this Joint Committee deal principally with marks to be assigned for athletic performance, and very reasonable objection was taken to the proposals. In a report of 1889 the Civil Service Commissioners stated that the War Office were satisfied with the physique of the young men selected by their examinations, but the Commissioners remark that should any department of the public service be desirous of testing the physical qualifications of its officers more severely than at present, they anticipated no more difficulty in determining the relative capacities of the individual candidates in this respect than is experienced in the literary examination.

“Moreover encouragement would be given generally to candidates to maintain a good state of health while preparing for the literary examinations, and any tendency to overpressure would thereby be diminished.” (p. 24; *B. A. R.* p. 472.)

This is the point from which Galton starts, and he remarks that to define bodily efficiency and to measure it both in individuals and races is the special task of the anthropologists, who concern themselves with the practice of human measurements, and devise tests that give warning whether growth and development are or are not proceeding normally. Galton at once states that what he has in view has no relation whatever to the pass-examination now made by medical men to eliminate candidates who are absolutely unfit. These pass-examinations are obviously a necessity. The reform asked for consists in giving additional marks to those youths who are not only fit for service, but exceptionally well fit as far as bodily efficiency is concerned.

“The curious and hardly accountable disregard of bodily efficiency in those examinations through which youths are selected to fill posts in which exceptional bodily gifts happen to be peculiarly desirable, must strike the attention of anthropologists with special force, and they of all people are best able to appreciate how much is sacrificed by its neglect.” (p. 24; *B. A. R.* p. 472.)

Galton next cites the reduction by Dr Venn¹ of the data from the Cambridge Anthropometric Laboratory to show there is no significant asso-

¹ *Journal of the Anthropological Institute*, Vol. xviii, pp. 140-54. Galton provided the instruments for the Cambridge Anthropometric Laboratory and when over a thousand students had been measured Venn reduced the data with the above-mentioned result. Galton supplemented Venn's paper with some discussion of the association of intelligence with size of head (pp. 155-56), and considered that it was possible to show that there was increased intelligence with increased head size. This matter is of such importance that a number of years ago I got

ciation between physique and intellect, and we cannot therefore argue from examinational success to any physical fitness. The high honours men, the low honours men and the poll men were alike in their average bodily efficiency, except for slightly worse vision in the high class honours men.

"The intellectual differences are usually small between the candidates who are placed, according to the present literary examinations, near to the dividing line between success and failure. But their physical differences are as great as among an equal number of other candidates taken at random. It seems then to be most reasonable whenever two candidates are almost on a par intellectually, though one is far superior physically, that the latter should be preferred. This is practically all I propose. I advocate no more at present than the introduction of new marks on a very moderate scale sufficient to save from failure a few very vigorous candidates for the Army, Navy, Indian Civil Service, and certain other Government appointments in which high bodily powers are of service. I would give the places to them that would be occupied under the present system by men who are far their inferiors physically, and very little their superiors intellectually. I am sure that every successful employer of men would assign as much weight as this to bodily efficiency, even among the highest class of those whom he employs, and that Government appointments would be still better adjudged than they now are if considerations of high bodily efficiency were taken into some account." (p. 25; *B. A. R.* p. 473.)

Galton considers that the desirable tests should involve measures of strength, vital capacity, agility or promptness, keenness of eyesight and of hearing. We could now add a number of tests, such as have been applied recently to candidates for the Air Force. The chapter concludes with the remark:

"It would certainly be grateful to many parents who now lament the exclusively bookish character of the examinations, and are wont to protest against a system that gives no better chance to their own vigorous children of entering professions where bodily vigour is of high importance than if they had been physically *just not unfit* to receive an appointment." (p. 26; *B. A. R.* p. 473.)

Chapter IV is entitled: "On the Principle and Methods of Assigning Marks for Bodily Efficiency¹." Galton starts by saying that we may either give marks for ranks or for achievements, and apparently proposes in each

permission to have copies taken of some of the schedules and worked out the actual correlations for over 1000 cases with the following results:

Length of Head and Intelligence	+ .111 ± .020,
Breadth of Head and Intelligence	+ .097 ± .021.

Schoolboys and schoolgirls at age twelve gave similar results. (See "Relationship of Intelligence to Shape and Size of Head," *Biometrika*, Vol. v, p. 120.) Thus Galton was *formally* correct in saying there was association, but the correlation is so low as to be absolutely idle for any individual prognosis.

Galton's interest in the Cambridge Anthropometric Laboratory was very great and in association with Mr (now Sir) Horace Darwin not only new anthropometric instruments were devised, but the old ones improved or modified. The Cambridge Scientific Instrument Company produced "A Descriptive List of Anthropometric Apparatus, consisting of Instruments for Measuring and Testing the Chief Physical Characteristics of the Human Body. Designed under the Direction of Mr Francis Galton." Of this, the fourth issue (May, 1890) lies before me; it is rather a reasoned account of the meaning of an anthropometric laboratory, of the measurements which may be taken and of the methods of taking them, than the price list of a manufacturing firm.

¹ This was also contributed as a memoir to the British Association, see *Report*, 1889, pp. 474-8. The diagrams of the pamphlet fail in this B.A. memoir. They were, however, given in a paper in *Nature*, October 31, 1889, Vol. xI, pp. 650-51. On the other hand the *B.A. Report* has the paper by A. A. Somerville discussing Eton experiments on the relative reliability of physical tests and of literary examinations. A system of marking having been devised for physical tests and for medical fitness in certain directions two medical men examined independently 32

case to mark from the mean or median. It is not quite clear in this case why there should not be negative marks for those who fell short of mediocrity. Galton then provides marks for each rank above 50% or again for the deviate. In the latter case the full number of marks is supposed to be reached by a grade 99.99. Next he draws up a table in which the total marks for physical efficiency are assumed to be 10 and are supposed to be assigned in different proportions to rank and to absolute achievement, i.e. to deviate from mediocrity. His table (*B. A. R.* p. 476) is as follows:

Percentage of marks assigned to		Rank					
Rank	Deviate	50°	55°	75°	95°	99°	99.99...°
%	%						
100	0	0	1.0	5.0	9.0	9.8	10.0
50	50	0	0.7	3.5	6.9	8.4	10.0
33.3	66.6	0	0.6	3.0	6.2	8.0	10.0
25	75	0	0.5	2.7	5.8	7.7	10.0
0	100	0	0.4	2.0	4.8	7.0	10.0

It is clear that Galton is compounding what every schoolmaster has to consider, namely: "place in class" with "marks in examination," or indeed "place" and "marks in examination." If the distribution were normal the marks would be readily deducible from "place" in examination. Considering what wide differences occur between the top individuals—for example in the old Cambridge Mathematical Tripos system—and what slight differences between mediocre individuals, one is inclined to doubt the legitimacy of marking by rank. This point was recognised, I think, by Galton later when he endeavoured to estimate the average differences between individuals arranged according to rank. However the table is suggestive and we leave it to the consideration of the educationalist.

Galton supposes the chief physical measurements to have been reduced for the class under examination to percentile scales, such as in the table on our p. 376. He gives a rather clear diagram of the absolute values for males and females at each rank for seven characters on p. 29¹. In some of the characters it would be desirable for rapid and safe interpolation to insert a few more values of the deviate.

boys. Their average difference in judgment was 8.75% of the maximum marks. Nineteen of the boys were subsequently examined in English Essay, and the essays submitted to two independent examiners. The average difference was now 16.7% of the maximum marks. The experiments seem to have been undertaken at Galton's suggestion and are used by him as an argument that physical test marks can be as accurately determined as marks for literary work. Some years ago the present writer reported on the marking statistics of the London Matriculation Examination. He was startled to find that the relative personal equation of examiners in history, languages and literature was very greatly larger than the relative personal equation of examiners in branches of science; and that no systematic method of correcting for this personal equation had been adopted. Thus a candidate's chance of passing depended largely on the examiner to whom his paper was allotted! It would appear therefore that the choice of an English essay for comparison of marking differences was rather unfortunate.

¹ See *Nature*, Vol. XL, pp. 650 and 651.

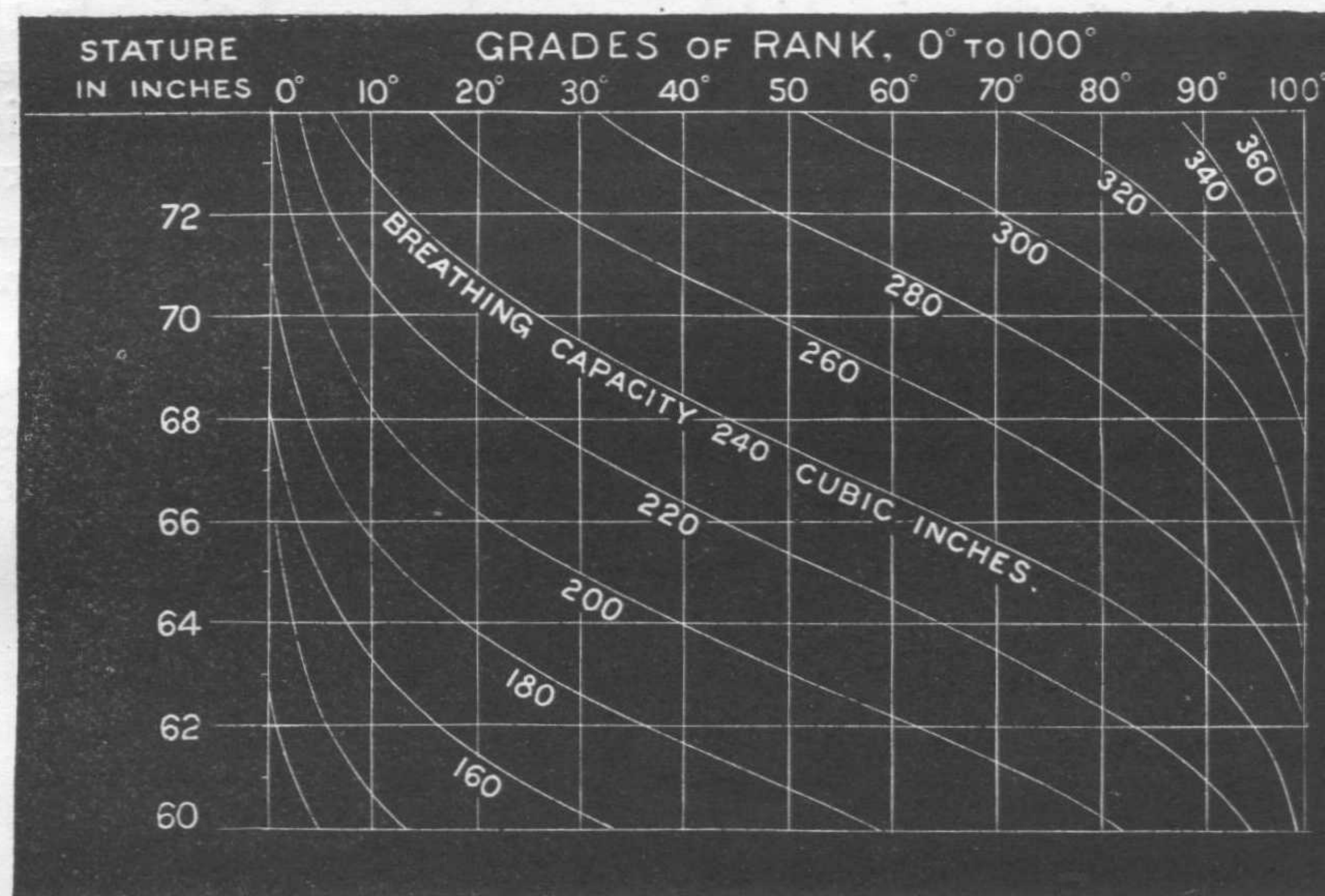
At this point Galton recognises two important matters, namely: (i) that these physical characters are in certain cases highly correlated, and that

		GRADES OF RANK, 0° TO 100°												
		0°	10°	20°	30°	40°	50°	60°	70°	80°	90°	100°		
		STATURE, WITHOUT SHOES.												
MALES		63	64	65	66	67	68	69	70	71	72	73	INCHES.	
FEMALES		59	60	61	62	63	64	65	66	67	68			
		HEIGHT SITTING, ABOVE SEAT OF CHAIR.												
MALES		33	34	35		36	37				38	39	INCHES.	
FEMALES		32		33		34			35		36			
		SPAN OF ARMS BETWEEN OPPOSITE FINGER-TIPS												
MALES		64	65	66	67	68	69	70	71	72	73	74	75	INCHES.
FEMALES		58	59	60	61	62	63	64	65	66	67	68		
		WEIGHT IN USUAL INDOOR CLOTHING												
MALES		120		130		140		150		160		170	LBS.	
FEMALES		100		110		120		130		140		150		
		BREATHING CAPACITY												
MALES		170	180	190	200	210	220	230	240	250	260		CUBIC INCHES.	
FEMALES		90	100	110	120	130	140	150	160	170	180			
		KEENNESS OF EYESIGHT. DISTANCE OF READING DIAMOND TYPE.												
MALES		10	15	20			25			30		35	INCHES.	
FEMALES		10		15	20		25			30		35		
		STRENGTH OF GRASP												
MALES		65	70	75	80	85	90	95	100	105			LBS.	
FEMALES		30	35	40	45	50	55	60	65	70	75			
		0°	10°	20°	30°	40°	50°	60°	70°	80°	90°	100°		

Diagram of Absolute Values at each Rank.

accordingly it is scarcely accurate to mark them independently, and (ii) that from the fitness point of view a vital capacity, for example, for a small man may be amply adequate, while the same vital capacity would be insufficient for a big man. If we judged independently by rank for vital capacity and stature, say

the big man might be more heavily marked than the small man, although his combined system was far less efficient. To throw light on these matters Galton constructs "mixed" correlation tables in which he gives the rank of one variate and the deviate of the other and draws contour lines for the first. Thus for each value of stature—i.e. for every two inches—he constructs a scale of deviate of vital capacity to each grade. He then combines these to give a single diagram and joins the points of equal vital capacity. He thus obtains what he terms *isograms* at every 20 cubic inches of vital capacity. He gives a similar system of isograms for strength of squeeze and weight. We reproduce one of these diagrams.



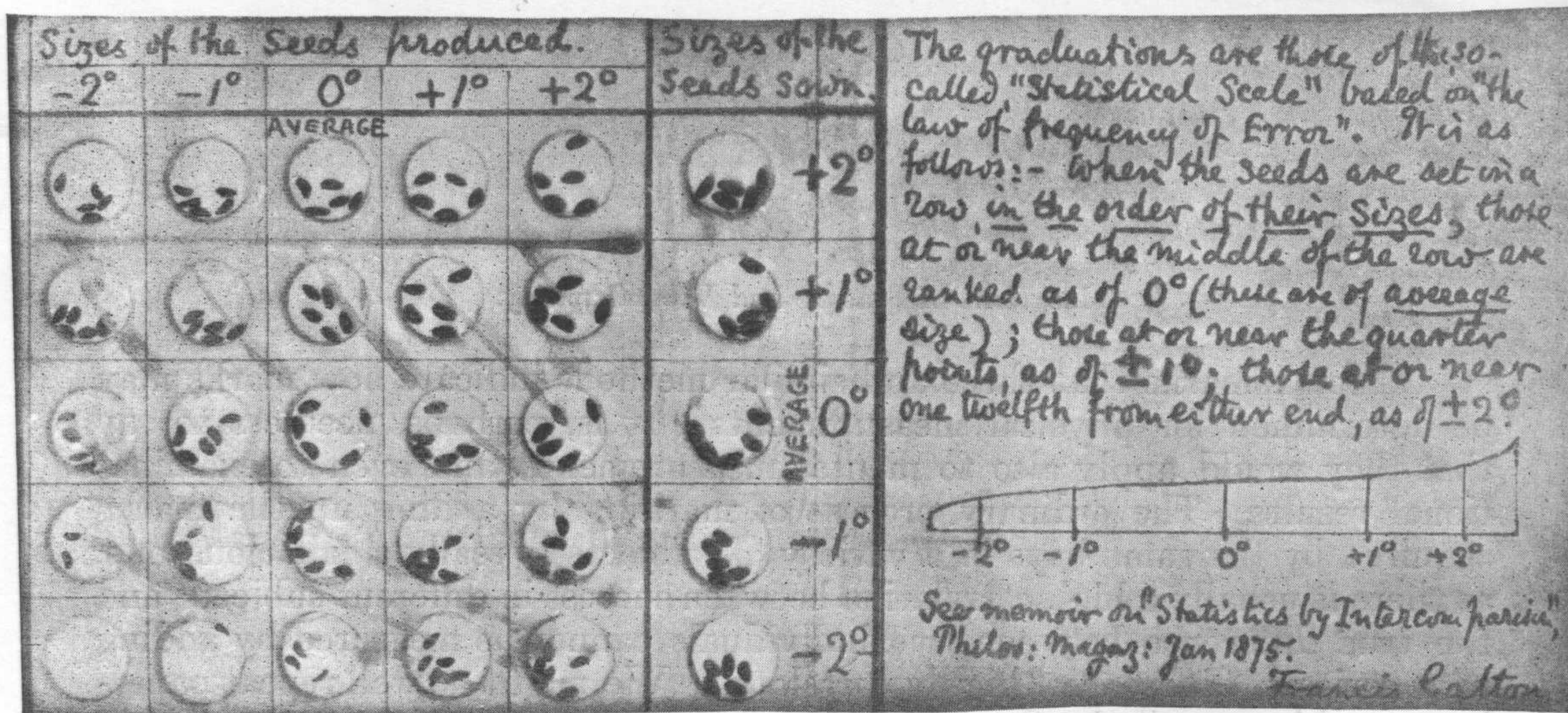
Isograms of Stature and Vital Capacity.

Galton remarks that these and similar methods indicate how marks may be determined for physical measurements. It is, perhaps, needless to say that they would apply also to mental measurements, and even to examination results. The ordinary process of marking by adding up the marks acquired by the same boys in a variety of subjects is really a fallacious one, as there is usually a high correlation between success in different subjects, and the true difference between two individuals cannot be measured by adding up a series of differences, highly correlated. We might as well repeat papers in one subject until the first of two boys had multiplied his excess marks a hundred times, but this deviate difference would certainly not be the measure of their relative intelligence. Their true ranking would have to be obtained from the multiple correlation surface, allowing correlation between subjects. The theory of examination marking sadly needs scientific treatment¹. Galton himself concludes as follows his discussion of marking:

¹ Ranking in multiple marking can be thrown back, through multiple correlation, on the P and χ^2 of the 'Goodness of Fit' tables.

"It is now the part of those who have to fix the scales of marks to determine the weight to be given respectively to relative rank and to absolute performance in examinations of each different kind of service." (p. 32; *B. A. R.* p. 477.)

A point may be noted here before we consider Galton's further contributions to the marking problem. In the paper just discussed Galton illustrates by *isograms* his method of exhibiting graphically the correlation of the rank for one character with the deviate for a second associated character. He undertook, however, the construction of a diagram of isograms for the case in which rank of one character was correlated with rank of a second. For all three cases—rank with rank, rank with variate, and variate with variate—Galton constructed isograms or contour lines. There is little doubt that rank with rank was the first way in which he approached correlation. In 1875 (see our p. 187) Galton was dealing with the inheritance of size in sweet-pea seeds, but before he obtained his data for sweet-peas, he appears to have tried what he could do with much smaller seed, apparently that of cress. The correlation of the seed of mother and daughter plants was dealt with by the method of his memoir of 1875, and that is, I think, the probable date of his first crude correlation table, which he obtained from five groupings of each size of seed; the isograms are represented by ink lines on the sheet of glass covering the little compartments which contain the ranked seeds of the daughter-plants. These isograms have been smudged and almost obliterated by the wear and tear of fifty years, but can still be traced. The facts (a) that he



Galton's first illustration of Correlation, circa 1875. From the *Galtoniana*.

uses the word *average* when he later used *median*, (b) that he divides not into the percentiles of his later work, but into quartile and double-quartile¹

¹ Galton himself says one *twelfth* from the end of the range, but if the reader looks at Galton's table on p. 42 of his paper on "Statistics by Intercomparison" (see our p. 338), he will find 82 in 1000 given for two quartile units, which Galton takes approximately as $\frac{1}{2}$.

groups suffice to indicate that the model must have been made about the time of his paper of 1875. It is, I think, sufficient evidence that Galton dealt with the correlation of ranks before he even reached the correlation of variates, and the claim that it is a contribution of the psychologists some thirty or forty years later to the conception of correlation does not seem to me valid. Galton, we may with high probability suggest, had satisfied himself that the correlation of ranks was more cumbersome than the correlation of variates, because in the simplest case, that of the normal distribution, it fails to provide linear regression, but gives a non-integrable curve, which can only be plotted by aid of the probability integral table.

Galton undoubtedly first attacked the problem of correlation from the standpoint of ranks, and it naturally did not lead him to any simple expression for the relation between relatives. In his *Memories* (p. 300) he tells us how:

“As these lines are being written, the circumstances under which I first clearly grasped the important generalisation that the laws of heredity were solely concerned with deviations expressed in statistical units [for Galton the quartile values] are vividly recalled to my memory. It was in the grounds of Naworth Castle¹, where an invitation had been given to ramble freely. A temporary shower drove me to seek refuge in a reddish recess in the rock by the side of the pathway. There the idea flashed across me, and I forgot everything else for a moment in my great delight.”

That ‘recess’ deserves a commemorative tablet as the birthplace of the true conception of correlation!

Galton continued his campaign in favour of marks for physical efficiency. He published a paper in the *Nineteenth Century*, Feb. 1889 (pp. 303–8), entitled, “The Sacrifice of Education.” The title does not seem well chosen, and I expect it was an editorial choice; Galton himself has written “Tests of Physical Capacity” above the paper and so indexed it. The object of the paper is to show the ease with which certain anthropometric measurements can be made, and the cost of making them. With most of the facts stated in the paper the reader will be already familiar. The tests are described, the time taken in making them, and the cost of running an anthropometric laboratory.

“The problem is to give marks for physical qualifications just as they are now given for intellectual ones, in order to pass those candidates who being a little under par intellectually are far above par bodily; conversely to weed out those other candidates who, not being particularly fit in respect to their brains, are at the same time of decidedly inferior physique. The relative weight to be assigned for intellectual and bodily excellences is a question of detail, most important no doubt, but one that need not be discussed here.”

Galton states the various criticisms of the proposal that have been made, but points out that they apply not only to marks for physical capacity, but to all examinations whatsoever. We can never test *all* the faculties even of the mind, and again we can only test the examinee on the special occasion on which he presents himself. It is admitted that the examination of *any* faculty, physical or mental, is a difficult art, and one not to be perfected offhand.

¹ Naworth Castle is in Cumberland, north-east of Carlisle. The Galtons went north on Aug. 12 and lodged at Wetheral in the neighbourhood of Naworth; later, on Sept. 10, they travelled to Newcastle and stayed with the Spence Watsons at Bensham Grove during the B.A. meeting at which Francis Galton read his paper on marks for physical efficiency. “Frank has been busy all the year on problems connected with correlation but has published nothing yet about them.” (*L. G's Record* under 1889.)

The next effort of Galton was a lecture before the *Society of Arts* on November 26, 1890¹, entitled: "Physical Tests in Examinations." This lecture tells us a little more of the history of the movement. Galton's proposal to give marks for physical efficiency was sent by the Anthropological section to the Council of the British Association, and the latter drew up a report on the subject which was distinctly favourable, although cautiously expressed; finally the recommendations reached were submitted to various government departments. Not improbably the original draft was due to Galton. The recommendations ran:

(1) That an inquiry should be held as to the best system of assigning marks for physical qualifications, on the double basis of inspection and anthropometry, with a view to its early establishment as a temporary and tentative system.

(2) That the marks to be given under this temporary system should be small, so as to affect the success of those candidates only, who would be ranked by the present examinations very near to the dividing line between success and failure, and whose intellectual performances would consequently be nearly on a par, though they would differ widely in their physical qualifications.

(3) That a determination should be expressed to reconsider the entire question after the experience of a few years.

The replies received from the government departments were more or less of the usual type.

"The Civil Service Commissioners, moved thereto by the India Office, are now engaged in considering the practicability of the proposals."

They probably would wait to see what force of public opinion was behind them.

Galton states that—subject to such reservations as that training for physical efficiency must not distract the candidate from his books, that it should not exhaust his energy, and that it should not displace any of the usual examination subjects or the medical pass-examination—it was a truism to say that physical efficiency ought to be taken into account in selecting men to fill posts where high physical powers are advantageous. Our lecturer next dealt with the means of testing physical efficiency. Athletic competitions he discarded as in no way suitable; he wanted to test natural capacities and not these capacities after a severe course of training. He then turned to inspection, and suggested that medical examiners might not only pass men, but mark them. Galton appeared to lay some stress on judgment of physical efficiency by inspection and cited the manner in which horse dealers and slave dealers rapidly reach on inspection fairly accurate estimates². Galton next turns to physical tests and mentions those with which this book has rendered the

¹ *Journal of the Society of Arts*, Vol. xxxix, pp. 19–27.

² Galton illustrated this from an experience of his own when travelling in 1846 in the Soudan. "An Egyptian, who possessed little besides a sword, had attached himself to the caravan with which I was travelling. He was on his way to join a slave-raiding expedition on the borders of Abyssinia, and he had, I found out, considerable experience in slave markets. I asked him many questions, from time to time, about the valuing of slaves, and at last begged

reader familiar, also his latest instruments and the need for considering the correlation of characters.

Then Galton passes to objections. It had been stated that many great commanders and strategists had a poor physique, and that a Nelson might be excluded. But the proposal did not exclude anybody, that was for the pass medical examination to do. Really able men would not be excluded by the marks for physical efficiency, which it was proposed should touch only borderline cases.

Another objection was that anthropometric tests did not reach the important quality of energy, which includes pluck, strong will and endurance. Galton admits *lacunae* here, as in mental testing.

“We must be content with what we can get. It is not impossible that practical tests of energy in some of its forms may yet be discovered. It must be associated with physiological signs that we have not yet had the wit to discover.” (p. 24.)

The third objection Galton discusses is the supposed untrustworthiness of the examination he proposes. There is, he says, no reason to suppose that it would be more untrustworthy than a literary examination, and he cites for variability of judgment in a literary examination Edgeworth's paper in the *Journal of the Statistical Society* of the same year¹.

Among problems which the lecturer held could be ultimately settled by tests of this physical kind were the following :

(1) Whether the proposed tests of physical efficiency confirmed the results of athletic competitions.

(2) Whether physical efficiency in youth corresponded to achievement in after life.

(3) What type of physique was best suited to tropical climates.

“It is cruel and costly to tempt youths to the tropics who are less constitutionally capable than others of thriving there. If we could distinguish those who are fitted for life in hot countries, we should select them even though in other respects they may be somewhat wanting. The tropical possessions of England are become so large that it is a matter of national importance to investigate this question thoroughly. It may yet be possible to find varieties of our race who are capable of permanently establishing their families in those climates.” (p. 25.)

Thus the far-seeing Galton ; we are no nearer a settlement of these problems now than we were when he urged their importance, and the reason of it lies in the fact that the data still fail us. One of his reasons for establishing tests of physical efficiency is the stimulus it would give to the collection of trustworthy records.

him as a favour to price myself, just as if I was a light-coloured African; for I was curious to know my worth as an animal. He took evident pains, and I think was fairly honest, though with a bias towards flattery. Having regard to the then high state of the market, he estimated my worth, on the spot, at a number of piastres that was about equal to £20.” (p. 20.)

The price of adult negro slaves in 1690 (Davenant) was £26. Sir William Petty after elaborate calculations valued an Englishman at £69, which King some twenty years later reduced to £65. The most recent valuation that I have come across is that of the average American citizen at about £400 (Davenport). It must be admitted therefore that Galton, aged 23, would have been a distinctly cheap bargain at £20.

¹ Vol. LIII, pp. 460-75, 644-63, 1890, and compare Vol. LI, pp. 599-635, 1888, and *Phil. Mag.* 1890, pp. 171-88.

The discussion which followed the lecture was of the usual character and not very profitable. The lecture, however, shows that Galton himself had not weakened in his faith in the advantages of his proposal. That the matter was dropped in 1890, without any published criticism of a damaging kind, will make it more difficult for success to be achieved when the matter comes up for discussion again, as it evidently must; we shall be told that the proposer himself abandoned the scheme in 1890, and why should it be resurrected? The only answer can be that we really do not know why Galton gave up the fight. His reasons given twenty years after the campaign are not conclusive¹. Who was the high authority of the War Office who wrote a careful minute, and what evidence is there that he possessed the requisite anthropological and physiological knowledge? Galton, as we have said before, was very apt to assume that other men's judgments, whatever their real intelligence, must certainly be better than his own, and the present was probably a case in point.

G. PRESIDENTIAL ADDRESSES TO THE ANTHROPOLOGICAL INSTITUTE

During the four years 1886–1889 Galton was President of the Anthropological Institute, and gave not only four presidential addresses, but took a considerable part in its proceedings, and worked for its welfare². The first of his presidential addresses (1886) was on the inheritance of stature, and will be dealt with in our next chapter. The second presidential address (1887) is of a mixed character: it describes the then recent progress of anthropology, and it gives some suggestions and thoughts arising therefrom³. It is followed by an obituary notice of Dr George Busk, which I happen to know was written by Galton himself⁴. Among other matters he refers to the anthropological collections recently established in London and Oxford, to the projected Imperial Institute and to the foundation of the International

¹ *Memories*, p. 214. Some experiments undertaken at Marlborough and reported by Meyrick and Eve (*Marlborough College Natural History Society*, 1889) seem to me very wide of the point. The actual anthropometric measurements were placed before eleven masters who were asked to mark them according to their own arbitrary opinion having regard to the boys' ages. Probably none of those masters had any idea of the variations of and the correlations between the measurements; it is hardly likely that they had the physiological or medical knowledge adequate for appreciating the relative values of the tests, nor any idea of how the individual boys would rank in a population of a like class. The conclusion that "there is probably greater vagueness in this examination than in most school examinations" was probably perfectly correct as applied to an examination for physical fitness conducted in such a manner.

² The *Report* of the Council for 1888 contains the words: "Mr Francis Galton's second term of office, as President, has now expired, and the Council desires to put on record its sense of the valuable services which he has rendered to the Institute and to the cause of Anthropological Science in general, during the past four years. The many ways in which Mr Galton has promoted the interests of the Institute demand, in an exceptional manner, the grateful acknowledgment of the members." *Journal of the Anthropological Institute*, Vol. xviii, p. 400.

³ *Journal of the Anthropological Institute*, Vol. xvi, pp. 387–402.

⁴ Galton wrote a considerable number of notices of dead friends, Spottiswoode, Marianne North, Herbert Spencer and George Busk, among others. They are graceful tributes to his friends' work.

Statistical Institute by Sir Rawson W. Rawson. Speaking about the new Anthropological Society of Japan and its memoirs, Galton writes:

“No doubt some of the more valuable papers in this journal will hereafter appear in one or other of the chief European languages. The curse of the Tower of Babel, in whatever sense we may employ the phrase, has long pressed heavily upon scientific men in Europe; the contemplation of the additional burden on our descendants of having possibly to learn Japanese, Russian and Chinese as well as the Western European languages can hardly be indulged in with equanimity.” (p. 394.)

Galton next turns to the white man in the tropics—a favourite topic with him—and noting that it is the temperature of the living rooms—especially the bedrooms—which is the difficulty, he enters fairly fully into the possibilities, mechanical and economical, of refrigerating apparatus for use in tropical climates. This leads Galton to consider the possibility of an acclimatised strain of Englishmen, and thus prompted he discusses alternate inheritance:

“Much has recently been written on the difficulty of any rare accidental variety of animal or plant establishing itself, when it has unrestricted opportunity of intercrossing with the parent stock. It is urged that the peculiarity would be halved in each successive generation, and would very soon cease to be apparent in the descendants. It seems to me that this argument is sometimes pressed too far. It cannot be a general truth that characteristics blend, else to take a conspicuous example, there would be a growing tendency in every mixed population for the eye-colour to become of a uniform hazel or brown gray tint, through the intermarriage of persons whose eye-colours differ widely. On the contrary I have lately shown by a considerable body of statistics¹ that among the English, the proportions between the eye-colours, as sorted under seven headings, have not changed at all during four generations. The fact is the heritages are only partially liable to be blended together; partially they are mutually exclusive. No case of inheritance probably falls under either of these opposed extreme conditions, but some approximate to one, and others to the other. I am not aware that the respective results of these two extreme conditions have yet been put forward quite as forcibly as they admit of being and deserve to be.... Suffice it to conclude that the establishment of a somewhat rare variety, as that of white men naturally suited to thrive and multiply in tropical climates, is not so great an improbability as those anticipate, who lay exclusive stress on the tendency of rare peculiarities to disappear in a very few generations, through free intermarriage with the ordinary members of the stock.” (pp. 400-402.)

Galton's presidential address of 1888² turns chiefly on Alphonse Bertillon's system of criminal identification. The address begins with a reference to Galton's short course of lectures on “Heredity and Nurture” given at the South Kensington Museum in December, 1887, under the auspices of the *Institute*.

“Their object was to test the reality of a supposed demand for information on such subjects, and so far as it was possible to judge from the results, there seemed to be a widely spread interest in the matter.... These lectures have led to at least one tangible result. I took the opportunity to reiterate my often expressed regret that no anthropometric laboratory existed in this country, at which children and adults of both sexes could at small cost have their faculties measured by the best methods known to science, and a record kept for future use. I explained how difficult it would be to maintain such a laboratory, and to make it effective, except under the shelter of some important institution that was daily frequented by the class of persons likely to make use of it.” (p. 346.)

As a result of Galton's reiteration he was given the wooden building associated with the South Kensington Museum which formed his second

¹ We shall return to Galton's 1886 paper on this subject in the following chapter.

² *Journal of the Anthropological Institute*, Vol. xvii, pp. 346-54.

anthropometric laboratory, and this arrangement lasted till 1890. He insists largely on the value of such anthropometric records for the identification of individuals, and cites the claims made for them by Bertillon, Topinard and Herbette. He describes further the index-system of Bertillon:

“Whether all that was claimed for the power of M. Bertillon’s system, on purely theoretical grounds and in his earlier publications, can be sustained, may fairly be questioned; but there can be no doubt that a series of measurements must be of considerable service as supplementary evidence, either that a person is really the man he professes to be, or negatively that he is not the man for whom he is taken. In speaking of these matters it is impossible not to allude to the Tichborne trial, and the enormous waste of money, effort, and anxiety which might have been spared, had Roger Tichborne passed through an anthropometric laboratory before he went abroad. It would be a reasonable precaution for every person about to leave his country for a long time, having regard to the various accidents of good or ill fortune, to be properly measured, and to leave a copy of his measurements in the safe keeping of an anthropometric laboratory.” (p. 252.)

“Another and very important question is as to the degree in which the several bodily proportions that are measured may be looked upon as independent variables. The stature is related with the length of the foot, and with that of the forearm, and we should expect a still closer relation to exist between any two of these taken together, and the third. We have yet to learn the proportion between the number of the elements measured and their value for purposes of identification. The supposition that they may be treated as independent variables, which lies at the bottom of some of the earlier estimates, such as that on page 22 of the Conference at Rome¹ headed ‘*Étendue infinie de la Classification*,’ cannot be accepted as correct.

The whole subject of ‘Personal Identification and Description’ forms an important chapter of anthropological research, and it is one on which I hope before long to be in a position to offer some views of my own.” (p. 354.)

The careful reader of this passage will note how Galton is beginning to realise the problems of multiple regression, and to see that with a large number of *correlated* variables, there is a limit to the intensity of the multiple correlation coefficient, which cannot be indefinitely increased by increasing the number. We also see how he is studying the correlation of bodily characters and gradually advancing beyond the indexing by such characters to a method of his own—the identification by finger-prints.

Galton’s fourth presidential address, that of 1889, we have already discussed (see our p. 383), and the reader who turns back to our account will have some appreciation of how the ideas of the 1888 address had been developed in the interval. Besides working out the fundamental ideas concerning correlation and heredity to which Galton was led by his “Family Records” and the data from his Anthropometric Laboratory, he contributed numerous short papers on anthropometric and statistical topics to various journals during this period and later at intervals.

H. MISCELLANEOUS STATISTICAL PAPERS

(i) *The Horse*. The horse had always been a favourite animal with him, notwithstanding his experiences with the camel in Syria and the ox in Damaraland, and no less than four of his papers treat of the horse under various conditions in addition to his paper on standard photographs of horses

¹ Louis Herbette and Alphonse Bertillon: “Les Signalements Anthropométriques,” *Conférence faite au Congrès Pénitentiaire de Rome*, Masson, Paris, 1886.

for measurement¹. One of these papers deals with heredity and will be considered in the following chapter. The first of the remaining three belongs to Galton's work on composite photography. It is entitled: "Conventional Representation of the Horse in Motion²." In this paper Galton endeavours to construct the conventional attitude of sculpture or of painting from composites of Muybridge's photographs of the "Horse in Motion." The final result ought to represent a mule, for it is certainly a very hybrid structure.

"The first composite shows the hind legs distinctly, the second shows the fore legs distinctly; and if duplicates of the first and second woodcuts are each divided into two halves and the best defined halves of each are united (in a way that might have occurred to Baron Munchausen if a second rider's horse had suffered as his own, and there had been a mistake in piecing them) a result is produced that shows a very fair correspondence with a not uncommon representation in sculpture."

The second paper is entitled: "The American Trotting Horse³." The interest of this paper lies in its great value as indicating the influence of long continued selection on a character. Galton, dealing with the statistics of the speed of American trotters, shows that every three years from 1871 to 1880 the speed of the best horse increased about two seconds, or in the nine years there was an improvement from about a mile in 2 mins. 17 secs. to a mile in 2 mins. 11 secs. Perhaps the most noteworthy point is, however, that not only the speed of the fastest horse thus improved, but the *first hundred* horses maintained their *relative* speeds, or all increased by about the same two seconds. Galton's final table is as follows:

Number of Seconds and Tenths of Seconds in Excess of Two Minutes that are required for Running One Mile by the Horses, whose order in the Rate of Running in each Year is given at the Top of the Column.

Year	100th	50th	20th	10th	Year	100th	50th	20th	10th
1874	25.1	23.4	20.5	18.8	1880	20.8	19.3	17.6	16.0
1875	24.1	22.5	19.9	18.2	1881	20.4	18.8	17.2	15.7
1876	23.5	21.6	19.5	17.7	1882	19.9	18.4	17.0	15.4
1877	22.9	21.0	19.0	17.4	Anticipated 1890	16.8	15.5	14.4	13.4
1878	22.1	20.2	18.5	17.0					
1879	21.3	19.6	18.0	16.6					

Mem. The first horse runs the mile in about five or six seconds less than the tenth horse.

Galton's anticipation for 1890 is obtained by noticing that the values in the vertical columns are nearly linear and then extrapolating.

"Supposing the conditions to be maintained, I should anticipate that in 1890 there will be about 15 horses that will run a mile in 2 minutes 15 seconds or less, and that the fastest horse of that year will run a mile in about 2 minutes 8 seconds⁴."

¹ See our p. 317 *et seq.*

² *Nature*, Vol. xxvi, pp. 228-9, July 6, 1882.

³ *Nature*, Vol. xxviii, p. 29, May 10, 1883.

⁴ Actual best values for 1891, 1900, 1910, and 1922 were: 2' 8".25, 2' 3".25, 1' 58".75 and 1' 56".75 respectively, so that Galton's prediction was exact, if we take 1891 for 1890.

The third memoir on horses also related to American trotters, but it is 14 years later, 1897¹. In this paper Galton draws attention to the value of the speeds of the "trotters" and "pacers" in *Wallace's Year Book of American Trotters* and suggests their importance for heredity. He notes, however, that it is very difficult to obtain the speeds of the parents and grandparents. No horse can theoretically obtain admittance to the trotting register unless it can trot a mile in under 2 mins. 30 secs. The data, however, seem to indicate either that some grace is given to horses who trot the distance of a mile in a little over the limit time, or else that a good many owners do not care to press their horses beyond the limit of admission. In either case Galton was justified in discarding the 2 mins. 29 secs. to 2 mins. 30 secs. entries as not homogeneous with the remainder. Dealing with the remainder Galton fits the distribution by rather rough methods² with a *portion* of a normal curve of deviations, and thus is able to determine not only the mean speed but the variation in the speed of American trotters. Thus he obtains for the median and quartile of the series of 982 records for 1893, a median speed of 2 mins. 26 secs. with a quartile of 5 secs. (2 mins. 21 secs.). The paper is of considerable value as it was, I believe, the first occasion on which an attempt had been made to fit incomplete series.

(ii) *The Median*. There are a number of short papers by Galton which are, perhaps, most suitably dealt with in this chapter. A good many of them appeared in the pages of *Nature*, a ready means of attracting immediate attention, but too often at the cost of later oblivion. Several of these papers concern really important points, which have, since their publication, been again and again overlooked.

In the first place we may turn to a group dealing with the median or the mid-character in a series. It is well-known that the median is subject to a larger probable error than the mean and this has discouraged its use in statistical inquiries dealing with carefully recorded observations. But Galton realised that while its chief value in such cases was the rapidity with which it could be ascertained³, yet there existed certain cases in which the median may be said to be far more reliable than the mean. In a paper of 1907 entitled "One Vote, One Value⁴," Galton draws attention to how misleading a use of the *average* may be. He cites as instances of importance and frequent occurrence: (i) the assessment by a jury of damages, (ii) the determination by the council of a society or by a committee of a sum of money suitable for some particular purpose. Each voter, whether of jury or council, ought to have equal authority with each of his colleagues. How can the right conclusion be drawn from the many individual estimates? Galton

¹ *Royal Soc. Proc.* Vol. LXII, pp. 310-15. See also *Nature*, Vol. LVII, p. 333, Feb. 3, 1898.

² More exact methods have since been applied to the data: see *Biometrika*, Vol. II, pp. 2-6. In the 1893 case a mean of 2 mins. 28 secs. and a quartile of 5.96 secs. were found.

³ That Galton used median and quartiles so frequently even on careful records must, I think, be attributed to his great love of brief analysis. He found arithmetic in itself irksome; he would prefer to interpolate by a graph rather than by a formula, and while his rough approximations were as a rule justified, this was not invariably the case.

⁴ *Nature*, Vol. LXXV, p. 414, Feb. 28, 1907.

holds, and surely he is right, that the *middlemost* estimate, the median, is the correct one. Every other estimate has a majority of the voters against it as either too low or too high. The correct estimate cannot be the *average* for, as Galton puts it, the average "gives a voting power to 'cranks' in proportion to their crankiness." The average allows crankiness to swamp reasonable judgment. For such reasons Galton laid considerable stress on the median, and on various contrivances for rapidly determining it.

I have already referred (pp. 336, 385) to the use Galton made of two bows or two weights to test the strength of a group, and how he determined his median from the resulting percentages. This point is more fully dealt with in a paper on "The Median Estimate" read at the Dover meeting of the British Association in 1899¹. In this paper Galton applies the two weights test to determine the proper damages by a jury or a suitable grant by a committee. Two sums A and B , B being greater than A , are fixed on and then three shows or counts of hands are taken, (i) for a sum less than A , (ii) for a sum between A and B , and (iii) for a sum greater than B . The individuals have thus not to determine actual amounts, but only inequalities. Galton now assumes the "normal" distribution of judgments and proceeds to determine the median in the manner of our footnote, p. 385². To expedite the determination he published a table of percentiles giving the ordinates in terms of the quartile. This table is also reproduced in a paper of the following year and originally appeared in his book *Natural Inheritance* of 1889. It can still be used although it only gives three significant figures (two decimals), when the quartile is preferred. It has, however, been superseded for most purposes by the table of five significant figures (four decimals) provided by Dr W. F. Sheppard at the suggestion of Galton, who wrote a prefatory note to the table³. This table gives the deviate in terms of the standard deviation and proceeds by permilles not percentiles. The prefatory note is a remarkable one considering that Galton was then aged 85; he there broke a last lance for the use of the ogive curve and the median, which he had introduced 40 years earlier. He took his present biographer's data for the intelligence of Cambridge graduates and represented it on a percentile scale and not on the biographer's "normal" scale; and he made a very good defence of his method.

¹ *British Association Report*, 1899, pp. 638-40.

² If b and a be the fractions of the total assessors who vote "above B " and "below A " respectively, then the ordinates of the probability curve corresponding to b and a , in terms of the standard deviation as unit, can be found from a table of permilles (see *Tables for Statisticians and Biometricians*, Table I). If these be α and β the median will be

$$m = A + a \frac{B - A}{a + \beta} = B - \beta \frac{B - A}{a + \beta}.$$

Here we suppose a and b both less than 50 per cent. of the total number of assessors. This is the better way of determining m ; a slight modification is needed, if m be greater (or less) than both A and B . The values of a and b should correspond to more than 5 per cent. of the assessors for reasonable accuracy.

³ *Biometrika*, Vol. v, pp. 400-6. "Grades and Deviates (including a Table of Normal Deviates corresponding to each millesimal grade in the length of an array, and a figure)."

He sums up its merits as follows:

“(1°) It establishes a centesimal scale of precedence, into which the order of any individual, in any array of individuals and of any length, can be easily translated, and it gives the normal deviate at the grade which the individual occupies.

(2°) It easily defines the limiting values of successive classes of given numbers in a normal array.

(3°) It classifies objects that can be arrayed by judgment, though not by actual measurement.

(4°) It gives by inspection the value of σ [the standard deviation] in a normal series, and that of the probable-error in any series, whether normal or not.

(5°) It exhibits processes under their real forms, and so is free from the danger of errors in principle, to which those unpractised in statistics are liable.

(6°) It affords an excellent criterion as to whether an observed array is or is not normal, and of the degree of its departure from normality.” (p. 104.)

There is no doubt that Galton's method of grades and deviates will retain a permanent position in statistics, chiefly as a means of illustrating in a simple manner statistical results; but as a fundamental method of tabulation it cannot be used. The ogive curve has no simple mathematical expression and data described in this way do not readily lend themselves to further quantitative discussion. This will be obvious to anyone who endeavours to determine the correlation coefficient from doubly-graded data, instead of from a frequency table¹. Yet there was something fine about Galton's defence of his first statistical method in his 85th year! It did not, perhaps, convince the younger school, but it made them reconsider, and possibly judge more favourably and use more frequently, Galton's mode of representation.

Some years earlier Galton reduced his method of determining the median to a very simple process². He transformed his “ogive curve” to a straight line by altering its horizontal or percentile scale. He was thus applying to a special case the conception of Lalanne's anamorphic geometry. In Galton's case the scale of percentiles is so chosen that the vertical ordinate up to an arbitrary sloping straight line represents the deviate in terms of the quartile or standard deviation as unit. Percentiles 5° to 95° limit the practical range of working. Any sloping straight line on this chart will be an ogive curve, i.e. correspond to some one or other normal distribution. If we know that p_a per cent. of individuals have a character less than A , and p_b per cent. a character less than B , we plot A and B upwards at the percentiles p_a and p_b respectively, join by a straight line the tops of these ordinates, and the point in which this line meets the 50° percentile gives by its ordinate the median. I do not know whether Galton ever prepared an accurate chart (“abac”) of his ogive transformed to a line—I have not come across it—but it would not be hard to do with considerable accuracy on a large scale, which might then be reduced by photography to reasonable dimensions.

Galton shows that linear interpolation on the ogive itself is very imperfect unless p_a and p_b are equally distant from the 50° point.

¹ Even the criterion suggested in (6°) is one rather of appearance than actual measure of “goodness of fit.”

² “A Geometric Determination of the Median Value of a System of Normal Variants, from two of its Centiles.” *Nature*, Vol. LXI, pp. 102-4, Nov. 30, 1899.

Some good illustrations of the merit and defect of the ogive-median method may be found in a further paper published in 1907.

In "Vox populi"¹ Galton begins by stating that

"in these democratic days any investigation into the trustworthiness and peculiarities of popular judgments is of interest,"

and proceeds to illustrate the "Vox populi" by discussing the 787 answers given in a weight-judging competition at the West of England Annual Fat Stock Show at Plymouth. The judgments turned on what a selected fat ox would weigh after being slaughtered and dressed. Galton considers that the entrance fee of 6*d.* and the hope of a prize deterred practical joking and that the judgments would be largely those of butchers and farmers experienced in the matter.

"The judgments were unbiased by passion and uninfluenced by oratory and the like... The average competitor was probably as well fitted for making a just estimate of the dressed weight of the ox, as an average voter is to judge the merits of most political issues on which he votes."

Galton gives the following table of results and the diagram on page 404:

Distribution of the estimates of the dressed weight of a particular living ox, made by 787 different persons.

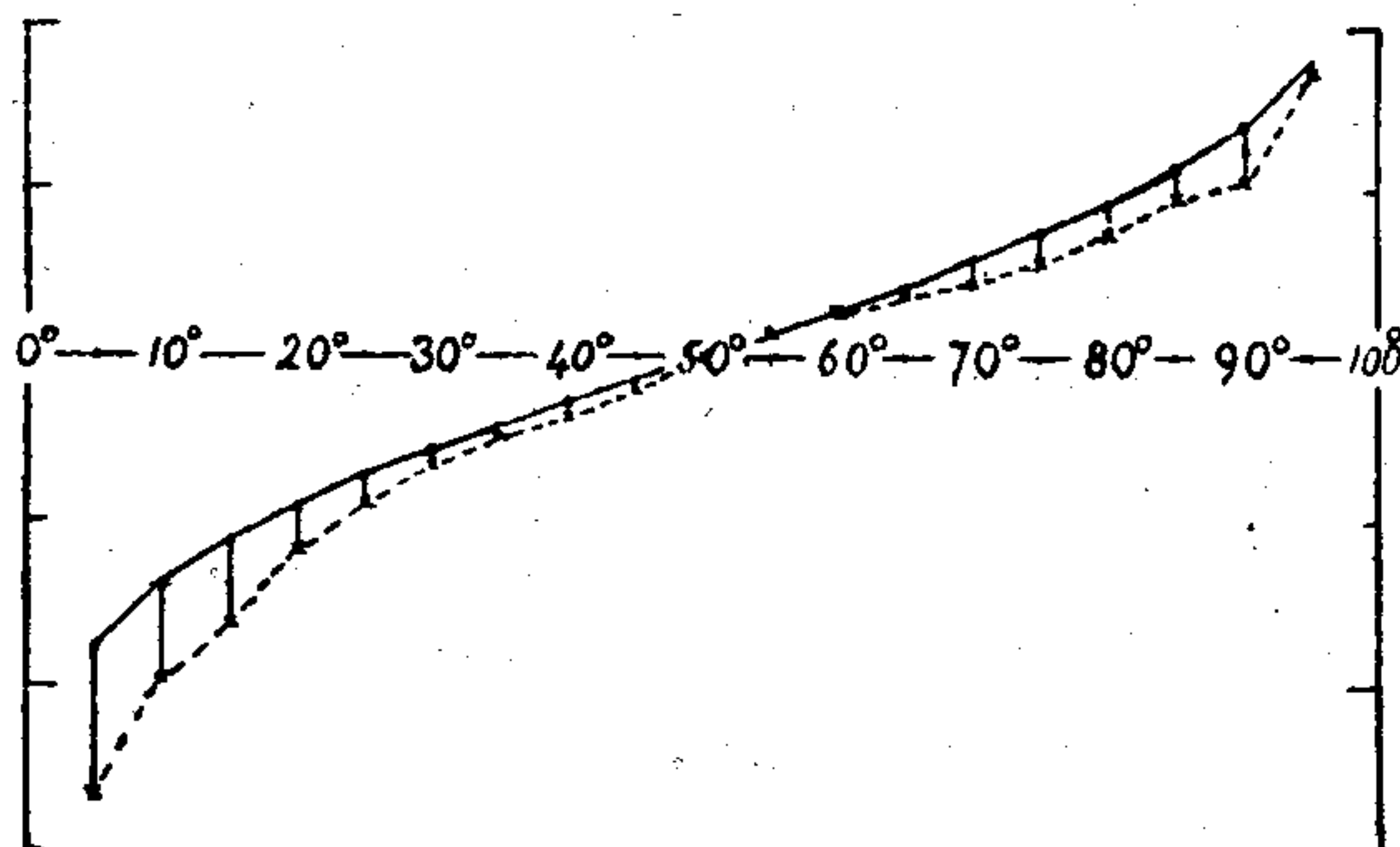
Degrees of the length of Array 0°—100°	Estimates in lbs.	Centiles		Excess of Observed over Normal	
		Observed deviates from 1207 lbs.	Normal p.e. = 37	Francis Galton	Karl Pearson
0					
5	1074	- 133	- 90	+ 43	- 23
10	1109	- 98	- 70	+ 28	- 11
15	1126	- 81	- 57	+ 24	- 9
20	1148	- 59	- 46	+ 13	+ 1
<i>q</i> ₁ 25	1162	- 45	- 37	+ 8	+ 5
30	1174	- 33	- 29	+ 4	+ 7
35	1181	- 26	- 21	+ 5	+ 7
40	1188	- 19	- 14	+ 5	+ 5
45	1197	- 10	- 7	+ 3	+ 6
<i>m</i> 50	1207	0	0	0	+ 8
55	1214	+ 7	+ 7	0	+ 7
60	1219	+ 12	+ 14	- 2	+ 4
65	1225	+ 18	+ 21	- 3	+ 1
70	1230	+ 23	+ 29	- 6	- 2
<i>q</i> ₃ 75	1236	+ 29	+ 37	- 8	- 5
80	1243	+ 36	+ 46	- 10	- 8
85	1254	+ 47	+ 57	- 10	- 9
90	1267	+ 52	+ 70	- 18	- 11
95	1293	+ 86	+ 90	- 4	- 8

*q*₁, *q*₃, the first and third quartiles, stand at 25° and 75° respectively.

m, the median or middlemost value, stands at 50°.

The dressed weight proved to be 1198 lbs.

¹ *Nature*, Vol. LXXV, pp. 450-51, March 7, 1907.

Diagram, from the tabular values.

The continuous line is the normal curve with p.e. = 37.

The broken line is drawn from the observations.

The lines connecting them show the differences between the observed and the normal.

According to this method of dealing with the matter the "Vox populi" was only wrong nine pounds (1207 against 1198), or 0·8 per cent. Galton considers that the judgments were not distributed normally and that negative errors were magnified and positive errors minimised by the competitors. But what if Galton be not fitting the best curve to his data? It is not hard to show that the judgment of the middlemost man is not the best median—paradoxical as it may seem! Almost any pair of symmetrical percentiles gives a result with less probable error. For example, the median of the quartiles $\frac{1}{2}(1162 + 1236)$ is 1199, only 1 lb. out. Other medians are:

20° and 80°	30° and 70°	35° and 65°	40° and 60°
1195 lbs.	1202 lbs.	1203 lbs.	1203 lbs.

—all better than the middlemost value.

Again the 25° and 75° are far from being the best percentiles to obtain the "probable error" from, i.e. the quartile does not give the quartile best, strange as that may appear. If we calculate the quartile from the 15° and 85° percentiles it is $\frac{1}{2}(73 + 55) \times 1/1\cdot54 = 41\cdot5$ and this is nearly the best position for determining its value, on the assumption of a normal distribution¹. With median at 1199 and quartile or probable error 41·5, a much more reasonable distribution is found, and there is far less need to assume as Galton did that the individual judgments are abnormally distributed; it is no longer true to say that errors in defect have been exaggerated, although errors in excess are still minimised. Whether the "fit" is a reasonable one it is not possible to determine when the data are thus given in percentiles. I have dwelt on the matter, because Galton's use of the values at 25°, 50° and 75° to determine the median and quartiles is not the best, and may lead, as

¹ Unfortunately the percentile method of tabulation does not permit of very ready determination of the mean and standard deviation and so of getting the best normal distribution. But I find after some labour: mean 1197, standard deviation 61·895, leading to a probable error or quartile value of 41·75. These give a far better fit than Galton's median and quartile values. I have inserted a column on the right of the table giving my results.

in this case, to an erroneous conclusion. The study of popular judgments and their value is an important matter and Galton rightly chose this material to illustrate it. The result, he concludes, is more creditable to the trustworthiness of a democratic judgment than might be expected, and this is more than confirmed, if the material be dealt with by the "average" method, *not* the "middlemost" judgment, the result then being only 1 lb. in 1198 out.

Among other matters which much interested Galton was the verification of theoretical laws of frequency by experiment. He considered that dice were peculiarly suitable for such investigations¹, as easily shaken up and cast. As an instrument for selecting at random there was he held nothing superior to dice². Each die presents 24 equal possibilities, for each face has four edges, and a differential mark can be placed against each edge. If a number of dice, say four, are cast, these can without examination be put, by sense of touch alone, four in a row, and then the marks on the edges facing the experimenter are the random selection. Galton uses another die, if desirable, to determine a plus or minus sign for each of the inscribed values. On the 24 edges of this die he places the possible combinations of plus and minus signs four at a time (16), and of plus and minus signs three at a time (8). Then, when he has copied out in columns his data from the facing edges of the first type of dice, he puts against their values the plus or minus sign according to the facing edge of the sign-die, which gives either three or four lines at a cast. The paper is somewhat difficult reading, and there are a good many pitfalls in the way of those who wish experimentally to test theories of frequency, especially those of small sampling. The importance of distinguishing between hypergeometrical and binomial distributions, between sampling from limited and from unlimited or very large populations, and the question of the returning or not of each individual before drawing the next, are matters which much complicate experimental work with dice.

Galton, however, was not unconscious of the many pitfalls which beset the unwary student of the theory of chance. There is an interesting short paper by him on "A plausible Paradox in Chances," written in 1894³. The paradox is as follows: Three coins are tossed. What is the chance that the results are all alike, i.e. all heads or all tails?

"At least two of the coins must turn up alike, and as it is an even chance whether a third coin is heads or tails, therefore the chance of being all alike is 1 to 2 and not 1 to 4."

If the reader can *distinctly* specify off-hand, without putting pen to paper, wherein the fallacy lies, he has had some practice in probability or has a clear head for visualising permutations. We leave the solution to him

¹ Ordinary dice do not follow the rules usually laid down for them in treatises on probability, because the pips are cut out on the faces, and the fives and sixes are thus more frequent than aces or deuces. This point was demonstrated by W. F. R. Weldon in 25,000 throws of 12 ordinary dice. Galton had true cubes of hard ebony made as accurate dice, and these still exist in the *Galtoniana*.

² "Dice for Statistical Experiments." *Nature*, Vol. XLII, pp. 13-14, 1890.

³ *Nature*, Vol. XLIX, pp. 365-6, Feb. 15, 1894.

remarking that Galton confirmed the accuracy of the 1 to 4 result by an experiment with his favourite dice. The paradox is

“a good example of the pitfalls into which persons are apt to fall, who attempt short cuts in the solution of problems of chance instead of adhering to the true and narrow road.”

For Galton that “true and narrow road” was the study of the possible permutations, the road followed by the early masters of the doctrine of chances.

We have seen how the “Vox populi” was—at any rate in the judgment of the meat-weight of fatted oxen—not so far from the truth. Galton, in a paper of twelve years earlier, endeavoured to test the “Vox judicum¹,” the reasonableness of the judgments of a presumably educated and trained class of minds. Galton expected that the various terms of imprisonment awarded by judges would fall into a continuous series. He limited his data to sentences on males without option of a fine, and he dealt with 830 sentences for terms of years, 10,540 for terms of months and 43,500 for terms of weeks. All these data give what we now term *J*-curves—i.e. frequency distributions similar to those of cricket scores, of incomes or rents—the shortest sentences in each case being the most numerous, the longest the least frequent. This is probably the nature of criminality in the population—or as Galton would put it of “true penal deserts.” But Galton does not lay stress on this remarkable deviation from the normal curve of distribution. He is concerned with another phase of this distribution of criminality, namely that it is extraordinarily irregular; there are marked preferences for certain terms of imprisonment. Thus when sentences are reckoned in months, the maxima occur at 3, 6, 9, 12, 15, 18 and 24 months. In 10,540 sentences in months there are none at 17 months, hardly any at 11 or 13 months. Galton argues that three months or a quarter of a year is a round figure that must commend itself to a judge by its simplicity. He suggests that if our year had been divided into 10 periods, then $2\frac{1}{2}$ periods, the equivalent of 3 months, would not have been used in its place, or the same penal deserts would have been treated differently from what they are now. Again, in the distribution of sentences in years, he draws attention to the emphasis on sentences of 3, 5, 7 and 10 years, showing a tendency at first to a unit of 2 years and then, presumably guided by a habit of decimal notation, a jump from 7 to 10 years. Galton remarks that while there were 7 sentences for 20 years and 6 for 15 years, there were absolutely none for 19, 18, 17 or 16 years. Terms of weeks are distributed with equal irregularity. Galton argues that the powerful cause of disturbance which interferes with the orderly distribution of punishment in conformity with penal deserts lies in the personal fancies of judges for certain series of numbers.

“It would be interesting to tabulate the sentences passed by the several judges since their appointments, to discover their respective peculiarities and personal equations, all who exercise extensive jurisdiction in criminal cases being included under the title of judge.”

There is no doubt that the idiosyncrasies of some judges in the matter of sentences are as well recognised in the legal profession as by the habitual criminal himself, but is there not another source of the results observed by

¹ “Terms of Imprisonment.” *Nature*, Vol. LII, pp. 174–6, June 20, 1895.

Galton of as great influence as the vagaries of the judicial mind? His material is heterogeneous, in that it covers a great variety of classes of crime from misdemeanours to felonies. For many of these offences a period of imprisonment, or at least a maximum period of imprisonment, has been fixed by the legislature itself, and however much a judge might consider an offender to deserve a longer period of imprisonment he could not inflict it. I think the irregularity which undoubtedly manifests itself in these results is due as much to the 'vox legislatorum' as to the 'vox judicum.' This might be easily ascertained by discussing the returns separated out into individual classes of crime. Whatever may be the exact origin of the anomalies—which are certainly present if we hold that anti-social conduct is a continuous variate—we may safely conclude with Galton:

“by moralising on the large effects upon the duration of a prisoner, that flow from such irrelevant influences as the associations connected with decimal or duodecimal habits and the unconscious favour or disfavour felt for particular numbers. These trifles have been now shown on fairly trustworthy evidence to determine the choice of such widely different sentences as imprisonment for 3 or 5 years, of 5 or 7, and of 7 or 10, for crimes whose penal deserts would otherwise be rated at 4, 6 and 8 or 9 years respectively.”

There is a passage in this memoir which would have delighted the heart of the “Passionate Statistician.” It runs:

“We test the acquirements of youths by repeated examinations, but do not as yet employ the methods of statistics to test the performances of professional men. Examiners, for example, should themselves be tested in this way¹, and I have a fancy that a discussion of the clinical reports at the various large hospitals might enable a cautious statistician to express with some accuracy the curative capacities of different medical men, in numerical terms. Before putting oneself into the hands of any new professional adviser, it would certainly be a grateful help to know the indices of capacity of those among whom the choice lay, not such as might be inferred from their performances in school and undergraduate days, or by their unchecked professional repute, but as they really are in their mature and practical life.” (p. 176.)

What a readjustment of values there would be if those “indices of capacity” were found one morning attached to the brass plates of Harley Street or inscribed in the more sober black and white of the passages in Lincoln's Inn!

Two further contributions of Francis Galton may be just mentioned. On the death of Dr Samuel Haughton he wrote² pointing out that amid Haughton's many-sided activities he had introduced the “long drop,” as the most painless death by hanging by the neck. Haughton had experimented on the tensile strength of the spine and muscles of the neck and published a formula for the length of drop dependent on the height and weight of the culprit. Galton believed Haughton to have omitted a small factor in the increased section of the muscles of the neck in fat men. The matter, if an unpleasant one, still needed scientific investigation as a death by beheading—which in one case occurred—was not carrying out the sentence of the law.

¹ All universities ought to take periodic stock of their examiners in the manner suggested. I have no hesitation in asserting that in many cases the success or failure of candidates is not a measure of their intelligence, but of their choice of subject and still more of the particular examiner in that subject who has marked their script.

² *Nature*, Vol. LVII, p. 79, November 25, 1897.

Those who recognise the relative mercy of the long drop may wonder why capital punishment has not been still further modified in the direction of scarcely less painful, but more seemly, methods of disposing of the socially abhorrent.

Galton further contributed a letter to the discussion on "Corporal Punishment" which developed in the *Times* in 1898. He considered that the writers on the subject had overlooked two important points:

"The first is, the worse the criminal the less sensitive he is to pain, the correlation between the bluntness of the moral feelings and those of the bodily sensations being very marked. The second relates to the connection between the force of the blow and the pain it occasions, which do not vary at the same rate, but approximately, according to Weber's law, four times as heavy a blow only producing about twice as much pain. In a Utopia the business of the Judge would be confined to sentencing the criminal to so many units of pain in such and such a form, leaving it to anthropologists skilled in that branch of their science to make preliminary experiments and to work out tables to determine the amount of whipping or whatever it might be that would produce the desired results. Really these latter considerations might even now be made the subject of a solid scientific paper of no small interest, but they cannot be more than hinted at in a short letter like this, which has to be written in non-technical language."

The unit of pain—quite apart from corporal punishment—seems no more incapable of measurement than a unit of intelligence. The threshold of the sensation of pain might be determined in a number of ways, and then correlated with other mental and physical characters of the individual. Like all Galton's writings, this brief letter suggests unexhausted fields for the persistent and cautious investigator.

Galton's fertility of statistical ideas may be further illustrated by two papers, one belonging to 1894 when he was seventy-two years of age, and the other written fifteen years later when he was 87. The first was contributed to the *Proceedings of the Royal Society*¹ and deals with the important problem of the fertility of marriages according to the ages of father and mother. The fertility is measured by percentage of families which have a child when the husband and wife are of the given ages. If a chart be formed of which one variate is age of father, the second age of mother, and the percental offspring be inscribed for each pair of ages, then Galton proposes to represent the *loci* of equal percentages by contours, which he terms *isogens*. By a fairly simple, if somewhat rough process he constructs these isogens for Körösi's Budapest data, and we reproduce them below.

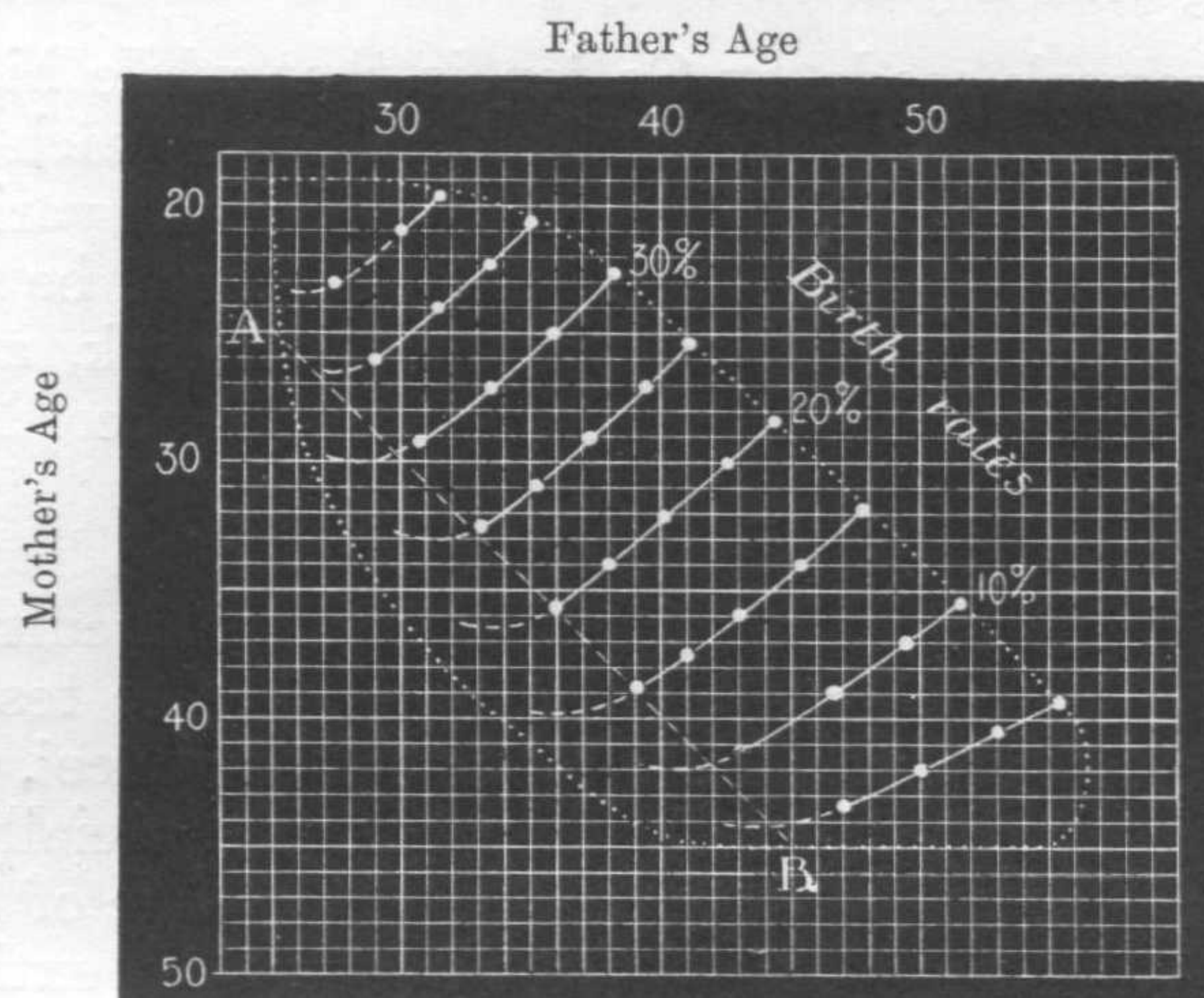
The diagram indicates that the form of the isogens as long as the husband is older than the wife is very closely a system of straight equidistant and diagonal lines. As a result of this Galton concludes that the fertility of a husband of age a_H and a wife of age a_w will be closely given by

$$p = 93 - a_H - a_w,$$

provided that (i) the wife is not older than the husband, and (ii) she is not less than 23 nor (iii) more than 40 years of age.

¹ Vol. LV, pp. 18–23. "Results derived from the Natality Table of Körösi by employing the method of Contours or Isogens."

Isogens or Lines of Equal Fertility.



Illustrations of the accuracy of the Isogens.

Wife	Husband	Calculated	Actual
23	27	43 %	42 %
23	37	33 %	30 %
27	33	33 %	31 %
31	35	27 %	26 %
35	41	17 %	16 %
39	49	5 %	6 %

Probably a little adjustment would give a better fitting plane to the portion of the surface under consideration, but Galton's rule gives at any rate a first approximation of a quite serviceable kind.

Our author notes the curious change in the direction of the isogens when the wife is older than the husband. This seems to me to indicate that when the wife is older than the husband, the age of the latter is of minor importance. Galton interprets it as follows:

“When she [the wife] is from thirty to thirty-eight she certainly seems to be appreciably more fertile with a husband of her own age or somewhat older than she is [than] with one who is younger¹. I should hesitate to ascribe this to physiological causes without corroborative

¹ The following results drawn from his data do not seem to confirm Galton's views :

Age of Wife	29	31	33	35	37	39	41	43	45
Percentage Children :									
Husband of same age ...	35	22	22	19	17	15	10	6	3
Mean for husbands younger	34	25	26.5	21.3	17.3	16.3	11.7	6.3	3.3

evidence derived from breeders of stock. It is very possible that indifference on the part of young husbands to aging wives may have something to do with it." (p. 22.)

An important remark made by Galton is that while his table and the smoothed isogons give the mean percentile fertilities at each age of husband and wife, they fail to measure the degree of individual variation from this mean; the nature of this variation, he remarks, could be found from the original observations with a moderate amount of work, and it would be of much interest to determine whether it varies in accordance with some definite law.

The second paper deals with a problem which Galton had special delight in handling. Nothing pleased him more than to dispel a current superstition by statistical criticism. We have already seen how he tested in this way the objective efficacy of prayer. In the present instance¹, with Mr Edgar Schuster's aid, he attacked the belief widely spread, especially among Roman Catholics, that Church property sequestrated at the time of the Reformation carried a curse with it; the effect of the curse was to extinguish the line of the owner by the death before inheritance of his sons, especially the eldest son. The phrase "Church property" is applied to estates which were in whole or part ecclesiastical previous to the dissolution of the monasteries under Henry VIII, and "Not Church property" to those that were not.

Survival of Eldest Sons.

Total number of Owners	Number who were Eldest Sons			
Not Church property 459	241	52.5 %
Church property ... 464	240	51.7 %

Length of Tenures.

	Mean Length
Not Church property ...	27.2 years
Church property ...	27.4 years

There is therefore no appreciable effect produced by the curse in either thwarting the inheritance of eldest sons, or in shortening the tenure of the ownership of Church property. It was found, however, that Church properties changed hands much more frequently than non-Church properties; transmissions by purchase were almost three times as frequent. Galton was inclined to ascribe this to the comparative unsuitability to modern require-

¹ "Sequestrated Church Property." *Nature*, Vol. LXXIX, p. 308, January 14, 1909.

ments of abbeys, etc., as dwelling houses, to their low situations and bad drainage, so that those who had experience of them would be the more ready to sell when the picturesqueness and romance of old buildings created a fictitious market value. Although this, Galton admits, is pure speculation, it may indicate that mundane reasons and not supernatural interferences really account for the excess of transmissions by purchase¹.

I have reserved one statistical paper because of its special importance for a last illustration of Galton's statistical *ingenium*. It appeared in August 1902, when he was more than 80 years old². Its title conveys no idea of its value and would suggest that it dealt only with a minor if interesting point. But Galton's method of proportioning first and second prizes demands a knowledge of the average interval between the first, the second and the third place men in a competition between a number n of individuals. Hitherto statistical frequency had been looked upon as a *continuous* distribution, which could be represented by the equation to a mathematical curve. I know no one before Galton, in 1902, who had proposed to consider a population for what it really is—a *discontinuum* with finite intervals between its individual members when arranged in order of their intensities with regard to a given character. What are the average values of these finite intervals, and how do they vary? What we now term Galton's *Individual Difference Problem* created at once a whole set of new conceptions and suggested questions, a few only of which have as yet been answered. But the explanation of certain well-recognised phenomena flowed at once. Taking only 100 individuals supposed to follow a normal distribution for any character the interval between the 50th and 51st is only one-tenth of the interval between the 99th and 100th, or between the 1st and 2nd. Given very large populations indeed mediocrity is crowded together; the exceptional are

¹ It is clearly impossible to give a summary of *all* Galton's many contributions to *Nature*, or of his letters to the daily press, although so many of them contain thoughts or suggestions, which it is sad should not be put on more permanent record. The curious may care to read *inter alia* (a) a long letter to the *Times*, October 6, 1887, entitled "The Proposed Imperial Institute: Geography and Anthropology,"—the word "Statistics" might have been added. We fear the existing Institute, largely deprived by cheeseparing governments of its own buildings, falls far short of Galton's ideal. (b) *Nature*, Vol. xxxvi, pp. 155–7 (June 16, 1887) contains a paper on "North American Pictographs." These interested Galton from more than one side. He thought the pictographic calendars of the Indians might be modernised for family records, and for some years was wont to have a small medallion drawn illustrating his own family occurrences in the year. He considered it possible to think by aid of pictographs as Laura Bridgeman had found an adequate basis for the exercise of a considerable amount of reasoning in the unassisted sense of touch. So also dogs may be occasionally "carrying out some real act of thought by aid of imagined and symbolic odours." This will seem fantastic only to those who have not observed what a Pekingese dog does when introduced for the first time to a new house with unknown occupants. (c) "A New Step in Statistical Science" (*Nature*, Vol. LI, p. 319, Jan. 31, 1895) deserves mention, as typical of Galton's splendid generosity to the younger generation when it came knocking at the door. Such innate generosity appears also in (d) a paper on "Bertillon's System of Identification" (*Nature*, Vol. LIV, pp. 569–70, Oct. 8, 1896), where in the "Signalitic Instructions" there was a not very happy attempt to claim finger-print identification as a French discovery.

² "The Most Suitable Proportion between the Values of First and Second Prizes." *Biometrika*, Vol. I, pp. 380–90.

widely separated from each other. I cannot describe better my sense of the importance of Galton's Difference Problem than by citing, with but slight additions, the words I used about it more than twenty years ago¹:

"Now, of course, the normal distribution in a general sort of way indicates that the differences between modal or what the biologists term 'normal' individuals are very small. But Mr Galton's Difference Problem enables us for the first time to appreciate quantitatively how much wider the differences are between the extreme (biologists' 'abnormal' or atypical individuals) and modal ('normal' or typical) individuals. Now the range of a distribution being somewhat about six times the standard deviation, we see that extreme individuals, even in a population of only 100, may be separated by as much as $\frac{1}{17}$ th of the range, while modal individuals have only a difference of $\frac{1}{250}$ th of the range and even individuals at the quartile only a difference of $\frac{1}{200}$ th of the range. The relative differences become much greater in populations of several millions.

It is not possible to pass over the general bearing of such results on human relations. If we define 'individuality' as difference in character between a man and his immediate compeers, we see how immensely individuality is emphasised as we pass from the average or modal individuals to the exceptional men. Differences in ability, in power to create, to discover, to rule men do not go by uniform stages. We know this by experience—our Shakespeares, our Newtons, our Napoleons have no close compeers in the populations of their own generations—but we see a reason for the gulf which separates the genius from ourselves, the phenomenon flows from a characteristic and familiar chance distribution. We ought not to be surprised, as we frequently are, at the results of competitive examinations, where the difference in marks between the first men is so much greater than occurs between men towards the middle of the list. In the same way the marked individuality of extreme criminality, and the appalling differences in stupidity and imbecility at the lower end of the moral and intellectual scales, receive their due statistical appreciation.

We stand in a better position to discriminate the pathological from the merely exceptional; mere isolation no longer leads us *à priori* to question the position of an outlying observation or of an exceptional individual.

In short Galton's Difference Problem leads us to look upon samples of populations, and even on populations themselves, no longer as arrays of individuals with continuously varying characters, but as systems of discrete units. We see discontinuity in every sample and in every population. We obtain a new and most valuable conception of a normal or standard population. It is one in which each individual is separated from his immediate neighbours,—when the whole is arranged according to any character,—by definite calculable intervals. These intervals are, of course, the *average* intervals which would be found by taking the mean of many such samples or populations, but they are none the less of extreme suggestiveness. Just as the *continuous* representation by a frequency curve is only an ideal representation of the observed facts, so we now reach an ideal representation of the actual *discontinuity* in the given population. As in the case of many physical investigations, so we find in statistical theory both continuous and discontinuous representations of the phenomena equally important and equally valid within the legitimate limits of interpretation."

Did Galton immediately recognise all that flowed from his treatment of the proper proportions of first and second prizes? Possibly not; he took some years to realise all that must eventually flow from his conception of correlation. But is not this failure to grasp immediately all that results from a new standpoint the essential peculiarity of the creative mind, whether it be that of a great scientist or of a great poet? Galton has himself so well described the workings of the exceptional mind that I need not labour this point². The mine discovered by Galton more than twenty years ago is far

¹ *Biometrika*, Vol. 1, p. 398.

² See the first footnote on p. 236 of this volume. The inspiration is the product of the subconscious mind. The man who has reached a truth knows it to be true, wrote Spinoza truly

from being exhausted, indeed its treasures are hardly yet touched; and probably will not be until some laborious German breaks by accident into its vein of wealth¹.

Admitted that Galton did not see the whole bearing of his conception, yet as pioneer he blazed the track. His method of approach was simple as usual, a rough but adequate approximation for the purpose he had in hand. He took as his average distribution of n individuals the normal curve divided into n equal areas, and he took as the character value of his individuals that corresponding to the areal bisector of each of these equal areas. He might have taken the means of those equal areas. Neither is in accordance with the more accurate method of approach which results from a fuller application of the theory of probability. But the difference is not very important unless the number of individuals be small; thus for a sample of 100 we find:

In terms of Standard Deviation	Method of Median (Galton)	Method of Mean	Accurate Theory
Distance between first and second	.406	.488	.360
Distance between second and third	.210	.215	.202

The ratios of first to second prizes given by the three methods are respectively: Median 74.6 to 25.4, Mean 76.6 to 23.4, Actual 73.6 to 26.5.

Thus the three methods do not lead to widely different results as far as the prize ratio is concerned; the first prize may, as Galton observes, be adequately taken as three times the second. The value for 10 competitors is 71.9 to 28.1 and for 1000 is 74.1 to 25.9. Hence the greater the number of competitors the more accurate is Galton's ratio of 3 to 1. But, as he remarks, the number in most competitions is limited by the fact that many individuals know it would be futile to enter, so that it is reasonable to proportion the prizes on numbers far larger than are actually entered². Galton's inspiration certainly divined that a better result could be obtained by using the median rather than the mean in this case!

As far as I know no attention has been paid to Galton's ratio in settling the monetary value of prizes in athletic competitions³, agricultural shows and scholastic awards. Its importance is obscured by the fact that it is merely an *average* value, and we are only to-day discovering how the system of differences—for example, between the first and second individuals—is

but really undemonstrably. What he did not write was that the applications of a truth are rarely fully recognised by its first discoverer. It is just because the exceptional mind does not and cannot exhaust the utility of its own creations that lesser minds value its teachings and recognise a master-builder.

¹ A prophecy which has come unexpectedly true! See von Bortkiewicz's recent paper on the range or distance between first and last individuals in a series.

² The first and second horses in the Derby are not merely the best out of many horses that actually run, but out of the very much larger number of potential runners.

³ I am informed that the 3:1 ratio is in use in at least one public school, but the origin of it is obscure.

distributed *round* the mean; we are not even clear as to the frequency of brackets in competitions of different sizes. There is still much work to be done on the lines of Galton's Difference Problem.

One conclusion, however, may be safely drawn, namely, that in this contribution of the octogenarian Galton to the theory of statistics there was no sign of a failure of that fertile suggestiveness which had led the sexagenarian Galton twenty years earlier to develop his far-reaching ideas on correlation.

Readers of this chapter will have observed that I have avoided almost entirely any reference to Galton's work on correlation, which is so essential a part of his contributions to statistical theory. This has been done purposely because it would not only have overweighted an already lengthy chapter, but its development belongs peculiarly to Galton's statistical studies of heredity. Both topics will form the subject of chapters in the remaining volume of his *Life, Letters and Labours*. It suffices in this chapter to see Galton deeply interested in almost every branch of statistics, but especially in their bearing on anthropometry. We have seen him pass from geography to ethnology, from ethnology to anthropometry, and from anthropometry to statistical theory. In the course of a long and crowded life, his contemporaries recognised him as a master-builder and as a pioneer in one branch of science after another. Space does not permit of our citing the innumerable questions and problems propounded to Galton by scientific correspondents from all quarters of the world. Galton's replies would indeed be a *repertorium* of information and suggestion, but in the majority of cases the recipients are now dead—for Galton outlived his generation—and I found the quest for his own letters a hopeless task. One such quest was, however, fruitful, and forms a fitting theme with which to close this chapter and volume.

I. THE PROPOSED PROFESSORSHIP OF APPLIED STATISTICS

Florence Nightingale has been usually estimated by that gracious phase of her life which appealed to the emotional sympathies of a little-instructed public. For that public she is the "Lady of the Lamp." Sympathy with suffering is, however, of small avail—no more so than charity—unless it be accompanied by administrative insight, and this side of Florence Nightingale's character has been too often overlooked. She was a great administrator, and to reach excellence here is impossible without being an ardent student of statistics. Florence Nightingale has been rightly termed the "Passionate Statistician." Her statistics were more than a study, they were indeed her religion. For her, Quetelet was the hero as scientist, and the presentation copy of his *Physique Sociale* is annotated by her on every page¹. Florence Nightingale believed—and in all the actions of her life acted upon that belief—that the administrator could only be successful if he were guided by

¹ Presented to the Galton Laboratory by Miss Nightingale's niece, Mrs Vaughan Nash, and now placed beside Darwin's gift of the *Origin of Species* to Galton and Tyndal's gift of his Belfast Address to Herbert Spencer.



Francis Galton at Haslemere in 1907.

“When the desired fullness of information shall have been acquired, then and not till then, will be the fit moment to proclaim a ‘Jehad,’ or Holy War, against customs and prejudices that impair the physical and moral qualities of our race.”

statistical knowledge. The legislator—to say nothing of the politician—too often failed for want of this knowledge. Nay, she went further: she held that the universe—including human communities—was evolving in accordance with a divine plan; that it was man's business to endeavour to understand this plan and guide his actions in sympathy with it. But to understand God's thoughts, she held we must study statistics, for these are the measure of his purpose. Thus the study of statistics was for her a religious duty.

Those who have drawn from the earlier chapters of this volume some idea of Galton's religion, will realise how close must have been the sympathy of ideas. For Galton the world was developing; at present under stern forces a mentally and physically superior human type was being evolved, and it was the religious duty of man to assist these changes, but for effective action we must study the laws of evolution, we must know and *statistically* know before the pace could be hastened.

“When the desired fullness of information shall have been acquired, then and not till then, will be the fit moment to proclaim a ‘Jihad,’ or Holy War, against customs and prejudices that impair the physical and moral qualities of our race¹.”

And again:

“The ideas have long held my fancy that we men may be the chief, and perhaps the only executives on earth. That we are detached on active service with it may be only illusory powers of free-will. Also that we are in some way accountable for our success or failure to further certain obscure ends to be guessed as best we can. That though our instructions are obscure they are sufficiently clear to justify our interference with the pitiless course of Nature, whenever it seems possible to attain the goal towards which it moves, by gentler and kindlier ways².”

Thus it came about that for Galton, and for Florence Nightingale, the end and the means were the same: men must study the obscure purpose of an unknown power,—the tendency behind the universe; and the manner of our study must be statistical. Therein, according to Francis Galton, lay the way to that unsolved riddle of “the infinite ocean of being”; therein, according to Florence Nightingale, lay the cipher by which we may read “the thoughts of God.” Men of the twentieth century may fail to appreciate the doctrine of either great Victorian, but of one thing they may be sure, the belief in both of them amounted to a religion. And what was a religion to both became at once in both a motive for action. Galton was not content with the office of teacher, he devoted a large portion of his fortune to the foundation of a school of eugenics on the basis of probability, that is of the modern theory of statistics. Statistics were to be applied in gleaning information as to Nature's immediate purpose, which undoubtedly lies in the evolution of man's mind, body, and character towards increased energy, and more efficient co-adaptation. Florence Nightingale strove—possibly with less scientific insight, but with a wider administrative experience and with no less religious earnestness—towards the like end. She sought, before Galton, but with smaller economic resources, to establish a university chair of “Applied Statistics.” I have often wondered how far the final form of

¹ *Probability the Foundation of Eugenics*: Herbert Spencer Lecture. Oxford, 1907. p. 30.

² *Ibid.* p. 9.

Galton's foundation was influenced by his correspondence with Florence Nightingale concerning this chair of applied statistics. When I sought a name in 1911 for the new department which should combine the Biometric and Galton Eugenics Laboratories, no fitter and more historically worthy name occurred to me than that of "Applied Statistics." Were I a man of wealth I would see that Florence Nightingale was commemorated, not only by the activities symbolised by the "Lady of the Lamp," but by the activities of the "Passionate Statistician." I would found a Nightingale Chair of Applied Statistics to carry out the ideal expressed in the letters below.

The first reference of Florence Nightingale to Francis Galton occurs in a very characteristic letter of hers to Captain (later Sir) Douglas Galton. It is dated August 7, 1867. In this letter she refers to a Standing Committee which was being appointed to consider contrivances for dealing with the wounded after a battle. The keynote to her letter is that appliances kill: Do away with all huts and marquees, give the wounded plenty of air and tend them on the battlefield. For every man that dies of his wound five or six die of the doctors and the removing; as to medicines, make the doctors swallow them all, all that is wanted is a little brandy and a great deal of water. But if the wounded are to be tended in extemporised shelters on the battlefield somebody must be on the committee who understands rough shelters.

"The only person who has written anything worth having on travelling apparatus is Mr Francis Galton, a cousin of yours I believe; I should put *him* on the Standing Committee, if possible."

It is not till twenty-four years later that Florence Nightingale again seeks the advice of Francis Galton; he was then 69 and she over 70. She was reviving one of the great dreams of her younger days and he, with no sign yet of age, was then actively contributing not a little towards its realisation.

10 SOUTH STREET, PARK LANE, W. Feb. 7, '91.

Scheme of Social Physics Teaching.

DEAR SIR, Sir Douglas Galton has given me your most kind message; saying that if I will explain in writing to you what I think needs doing, you will be so good as to give it the experienced attention without which it would be worthless. By your kind leave, it is this:

A scheme from someone of high authority as to what should be the work and subjects in teaching Social Physics and their practical application in the event of our being able to obtain a Statistical Professorship or Readership at the University of Oxford.

I am not thinking so much of Hygiene and Sanitary work, because these and their statistics have been more closely studied in England than probably any other branch of statistics, though much remains to be desired: as e.g. the result of the food and cooking of the poor as seen in the children of the Infant Schools and those of somewhat higher ages. But I would—subject always to your criticism and only for the sake of illustration—mention a few of the other branches in which we appear hardly to know anything, e.g.

A. The results of Forster's Act, now 20 years old. We sweep annually into our Elementary Schools hundreds of thousands of children, spending millions of money. Do we know:

(i) What proportion of children forget their whole education after leaving school; whether all they have been taught is *waste*? The almost accidental statistics of Guards' recruits would point to a large proportion.

(ii) What are the results upon the lives and conduct of children in after life who don't forget all they have been taught?

(iii) What are the methods and what are the results, for example in Night Schools and Secondary Schools, in preventing primary education from being a *waste*?

If we know not what are the effects upon our national life of Forster's Act is not this a strange gap in reasonable England's knowledge?

B (1). The results of legal punishments—i.e. the deterrent or encouraging effects upon crime of being in gaol. Some excellent and hardworking reformers tell us: Whatever you do keep a boy out of gaol—work the First Offenders' Act—once in gaol, always in gaol—gaol is the cradle of crime. Other equally zealous and active reformers say—a boy must be in gaol once at least to learn its hardships before he can be rescued. Is it again not strange in practical England that we know no more about this?

B (2). Is the career of a criminal from his first committal—and for what action—to his last, whether (a) to the gallows, or (b) to rehabilitation, recorded? It is stated by trustworthy persons that no such statistics exist, and that we can only learn the criminal's career from himself in friendly confidence—what it has been from being in gaol, say for stealing a turnip for a boys' feast, or for breaking his schoolroom window in a temper because he has been turned out of school for making a noise—to murder or to morality.

In how many cases must all our legislation be experiment, not experience! Any experience must be thrown away.

B (3). What effect has education on crime?

(a) Some people answer unhesitatingly: As education increases crime decreases. (b) Others as unhesitatingly: Education only teaches to escape conviction, or to steal better when released.

(c) Others again: Education has nothing to do with it either way.

C. We spend millions in rates in putting people into Workhouses, and millions in charity in taking them out. What is the proportion of names which from generation to generation appear the same in Workhouse records? What is the proportion of children de-pauperised or pauperised by the Workhouse? Does the large Union School, or the small, or 'boarding out' return more pauper children to honest independent life? On girls what is the result of the training of the large Union Schools in fitting them for honest little domestic places—and what proportion of them falling into vice have to return to the Workhouse? Upon all such subjects how should the use of statistics be taught?

D. India with its 250 millions—200 millions being our fellow-subjects, I suppose—enters so little into practical English public life that many scarcely know where this small country is. It forms scarcely an element in our calculations, though we have piles of Indian statistics. [As to India the problems are:]

(i) Whether the peoples there are growing richer or poorer, better or worse fed and clothed?

(ii) Whether their physical powers are deteriorating or not?

(iii) Whether fever not only kills less or more, but whether it incapacitates from labour for fewer or more months in the year?

(iv) What are the native manufactures and productions, needed by the greatest customer in the world, the Government of India, which could be had as good and cheap in India, as those to be had from England?

(v) Whether the native trades and handicrafts are being ruined or being encouraged under our rule?

(vi) What is the result of Sir C. Wood's (1853) Education Act in India?

These are only a very few of the Indian things which—I will not say are hotly contested, for few care either in the House of Commons or out, but—have their opposites asserted with equal positiveness.

I have no time to make my letter any shorter, although these are but a very few instances. What is wanted is that so high an authority as Mr Francis Galton should jot down other great branches upon which he would wish for statistics, and for *some teaching how to use these statistics in order to legislate for and to administer our national life* with more precision and experience.

One authority was consulted and he answered: "That we have statistics and that Government must do it." Surely the answering question is: The Government does not use the statistics which it has in administering and legislating—except indeed to "deal damnation" across the floor of the H. of C. at the Opposition and *vice versa*. Why? Because though the great majority

of Cabinet Ministers, of the Army, of the Executive, of both Houses of Parliament have received a university education, what has that university education taught them of the practical application of statistics? Many of the Government Offices have splendid statistics. What use do they make of them? One of the last words Dr Farr of the General Register Office said to me was: "Yes, you must get an Oxford Professorship; don't let it drop."

M. Quetelet gave me his *Physique Sociale* and his *Anthropometrie*. He said almost like Sir Isaac Newton: "These are only a few pebbles picked up on the vast seashore of the ocean to be explored. Let the explorations be carried out."

You know how Quetelet reduced the most apparently accidental carelessness to ever recurring facts, so that as long as the same conditions exist, the same "accidents" will recur with absolutely unfailing regularity¹.

You remember what Quetelet wrote—and Sir J. Herschel enforced the advice—"Put down what you expect from such and such legislation; after — years, see where it has given you what you expected, and where it has failed. But you change your laws and your administering of them so fast, and without inquiry after results past or present, that it is all experiment, see-saw, doctrinaire, a shuttlecock between two battledores."

Might I ask from your kindness—if not deterred by this long scrawl—for your answer in writing as to heads of subjects for the scheme? Then to give me some little time, and that you would then make an appointment some afternoon, as you kindly proposed, to talk it over, to teach, and to advise me? Pray believe me, Yours most faithfully, FLORENCE NIGHTINGALE.

I confess—but then I am a prejudiced person, for the prophetess was proclaiming my own creed—that this letter appears to me one of the finest that Florence Nightingale ever wrote. What is more it is almost as true to-day as it was thirty years ago. We are only just beginning to study social problems—medical, educational, commercial—by adequate statistical methods, and that study has at present done very little to influence legislation. What is more the requisite statistical teaching on which real knowledge must be based has hardly yet spread throughout our universities. The time has yet to come, when the want of a chair of statistical theory and practice in any great university will be considered as much an anomaly as the absence of a chair of mathematics. The logic of the former is as fundamental in all branches of scientific inquiry as the symbolic analysis of the latter.

To many it may seem as if we had here a proposal which Galton would welcome at once, none would doubt that he would give the closest consideration to it. But to those who knew Galton well three points of hesitation would suggest themselves: (i) He had no faith in a man simply because he was a professor; the men who in his day had made the most important contributions to science—Darwin, Wallace, Lubbock—and such personal friends as Spencer and Groves—were not professors. (ii) He did not till long after this lose his faith in working by committees, or in some form of co-operative work². (iii) He believed that small monetary prizes would produce excellent research work by able young men, and overlooked the fact that a stiff preliminary training is needful (then wholly lacking in statistics) if

¹ *Footnote.* I presume that no one now but understands, however vaguely, that if we change the conditions for the better, the evils will diminish accordingly.

² He had experienced the great value of the Meteorological Council and the Kew Observatory Committee, but these were largely homogeneous groups of highly trained men. Some of Galton's later committees, selected with the most catholic spirit, were such heterogeneous teams that he might as well have harnessed a thoroughbred, a mule, an ass and a camel to his wain, and have hoped for reasonable progress!

even the highest ability is to be productive of really scientific research. Without realising these characteristic elements of Galton's mind—phases of which I saw closely in after years—the reader cannot appreciate the trend of the present correspondence.

Galton at once thought most carefully over the matter. I have before me the rough notes he drafted before he replied. A few extracts from these notes emphasising my points may be made.

Professorship of Social Statistics.

Need of a professor of the theory of statistical methods and the application of them to definite social problems.

Higher laws of statistics, a mathematical head required... Professor will never get a class. Query as to Oxford at all? and query as to a professor? He will draw a big salary for certain but that is all you can be sure of....

Great difficulty of interrogating Nature etc. aright. One wants a committee for discussion. Great loss of time by false lines of quest.... A man like Moltke to plan campaign; not necessary that he himself should work out results... that might be done by special grants.... The hardest task is to frame questions. To obtain men who shall be masters not slaves of statistics and whose hearts shall be set on the solution of social problems.... Each problem is a separate and severe problem to be attacked in its own way by such facts as are available....

This is all true, but it is not the whole truth. I take it that the kernel of Florence Nightingale's proposal was the foundation of a school of higher statistics, and the production by it of minds keen on applying novel methods to social problems. It was not till much later that Galton fully realised this, giving up his faith in committees and in work to be done by small grants for essays. The chief factor in that change was, I believe, his friendship with a professor of the best type, W. F. R. Weldon, whose energy, idealism and enthusiasm, showed Galton how much could be achieved by the right academic spirit.

With these precursory remarks I give Galton's reply.

42 RUTLAND GATE. Feb. 10, '91.

DEAR MISS NIGHTINGALE, I think most progress may be made if I send the general ideas that your letter suggested to me, rather than by delaying to make a list of subjects suitable for inquiry; the reason why will be seen directly.

In the first place your object of obtaining a supply of men well versed in the appropriate methods of statistics, who shall apply them to the social problems of the day, seems to me a *most worthy* one, and well deserving a *great effort*.

In addition to the problems you specify, such may be mentioned as:

- (1) Number of hours' work, and corresponding amount and value of output in different occupations, whether purely mechanical, partly mental or aesthetic.
- (2) The effect of town life on the offspring, on their number and on their health.
- (3) What are the contributions of the several classes—as to social position and as to residence—to the population of the next generation? Who in short are the proletariat?

In pursuance of what I have said above I will not multiply instances.

The real difficulty in treating these and similar subjects is to specify exactly what is aimed at in a way free from all ambiguity, and again in a way to which the statistics that are available will give an answer also free from ambiguity. The difficulty of the physicist is to interrogate Nature by framing searching questions to her, and it is by this method, the applications of which seem so simple, after some philosopher has had the ingenuity to think of them, that all physical science is forwarded. But there are very few men capable of interrogating Nature aright; those

who are become the great men of science. Now the difficulty in social statistics is of exactly the same kind, but *greater*. Therefore by no straightforward and expeditious method can the problems in which you—and I may be permitted to add myself—are so much interested, be solved. Each is a separate and difficult undertaking requiring a vast deal of thought and planning, just like planning a campaign. Quetelet's own history is an example of this. His promises and hopes and his achievements in 1835–6 remained *in statu quo* up to the last edition of his work (*Physique Sociale*) in 1869. He achieved nothing hardly of real value in all those 33 years¹. So again Buckle, who started with a flourish of trumpets in the first chapter of his *History of Civilisation*, did next to nothing beyond a few flashy applications that have rarely stood after-criticism.

The way in which your object might best be attained requires, I think:

(1) A man (or men) conversant with the methods, and especially the *higher methods*, of statistics.

(2) Conversant with the existing statistical data.

(3) With his heart directed towards the solution, one by one, of such parts of such of your problems as he can, after much thought, see his way to attack successfully.

(4) Proportioning his labour so as to stop short when he has reached a fairly near approximate result, and not to waste himself in figures in order to procure a slightly closer approximation. In short he must be the master and not the slave of his statistics. The waste of effort by statisticians seems appalling. (I know it is so in meteorological statistics.)

How to get all this? I gather that you have in view the establishment of a Professorship or Readership at Oxford. Before you *fix* your mind in that direction or in that of Cambridge, I should like to tell you by way of warning the experience our Geographical Society has had in doing the same for Geography in the two Universities. I happen to have been closely connected with the movement and am indeed going down to Cambridge next week to see if the dismal want of success of our Reader there can be obviated. The result of very much inquiry has been, that unless the subject on which a Professor lectures has a place in the examinations he will get *no class* at all. His position will be that of a salaried sinecurist, which is proverbially not conducive to activity. Still, he would have leisure and personally would have interest in his work, and if only a Reader, is removable after 5 years. A professor is permanent. He would live in much isolation at Oxford as far as his own subject is concerned, for all the main interests of the place are scholastic, and many of them are rather petty. It occurs to me that perhaps as good a way as any might be to found a professorship at the Royal Institution in London, and to require a yearly course of lectures. The Royal Institution audience is just the sort to stimulate on the one hand and to curb the vagaries of the inquirer on the other. It is a mixture of some of the ablest philosophers, of many persons of wide social interests and of the general public. The existing professors are all men of the highest ability in their several lines: Lord Rayleigh in Physics, Dewar in Chemistry, Victor Horsley in Biology. If a Professorship in Social Statistics could be established there on the same basis as those mentioned, it would have to be nominally renewed each year up to five years' (I think) tenure. Then the re-election is for another (practically) 5 years. The cost is, *I think*, about £300 to £500 a year, not more. Pray excuse my impertinence if you think it such, in venturing to suggest, but my only object is to show what seems to me to be the best *direction* of action. I think London would be by far the best residence for an inquirer into social statistics. Believe me, Very faithfully yours,
FRANCIS GALTON.

Looking back after thirty years one is compelled to think that Florence Nightingale's scheme, if it could have been carried out, was essentially better than Francis Galton's. How could a school of trained applied statisticians have been created by six lectures a year at the Royal Institution; that institute has a most valuable platform for announcing in a popular way the results of recent research, but it is not an academic centre for training enthusiastic young

¹ I venture to think that this is far too sweeping, it overlooks not only what Quetelet achieved in organising official statistics in Belgium, but his great work in unifying international statistics.

minds to a new departure in science. The very names mentioned by Galton are those of men who had become famous for research in their own lines before they became "professors" at the Royal Institution, of men, whose means of support did not depend on that institution. Looking round the possible field of candidates in 1891 what man was there who would have fulfilled the same conditions in statistical science as these men in their respective branches? There was only one man—Galton himself—and it is quite certain that he had not that man in view. And also that, if the endowment had been made, and others had suggested him, he would have refused the post. It could have contributed nothing to his influence or research activity, and would have curtailed his freedom in a way wholly distasteful to him. There is small doubt that Florence Nightingale's plan of a professorship round which a school of young enthusiasts might be developed was the wiser, if less showy policy¹. Between Galton's letters of February 10 and March 15, a brief note written by him on February 19—

"it would give me pleasure to call and talk over the scheme when you feel disposed. The more I think of it the more important it strikes me to be"—

indicates that the discussion had been continued by interview. During this or a later meeting Florence Nightingale must have emphasised the importance of a "school of youngish men." But Galton did not surrender his Royal Institution lectureship, or his advisory committee, or his essayists. Writing on March 15, 1891, he says:

With reference to your scheme, I have not been idle but have made some few inquiries; of course withholding your name. I think the net result is this:

(a) Lectureship or Professorship at the Royal Institution with the duty of giving at least six lectures a year and writing a paper.

(b) A studentship, prize or scholarship at Oxford or Cambridge.

(c) A regular Professorship somewhere. Query in London.

(d) Endowment of a Course of Annual Lectures—like the Hibbert Lectures—at some great centre. Query in London.

The selection between these would depend much on the funds disposable eventually.

There is no doubt that a small body of youngish men inspired with a common enthusiasm would do incomparably more than any endowment can ensure. One is often in despair at the thought of how little money can secure in the way of original work. The enthusiasm I mean is not that which is fed by public notice or high patronage, but by the intelligent kindly interest and prompt appreciation of a very few capable and honoured people like yourself of whatever really good work may be done. In short one wants a school of inquirers, having a nucleus of a few able and single-minded persons, not distracted by too many other interests, to originate and maintain the enthusiasm of their fellows and co-adjutors.

Then again some journal suitable for receiving such memoirs, long or short as the case may be, is a desideratum, as well as means of discussing them. This raises the question whether the Statistical Society might not appropriately be the body, in whose hands the endowment might be placed, in order to forward your object under the best attainable safeguards. Most statisticians belong to it, and a suitable committee of them might be trusted.

I will take *the chance* of finding you at home about 5 to-morrow (Monday) unless I receive a card to the contrary, Very faithfully yours, FRANCIS GALTON.

¹ An energetic professor would very soon have compelled even Oxford or Cambridge to grant degrees on the basis of "schools" or triposes in statistics, and I do not despair of such a future after the full admission of women to Oxford, and the extreme difficulty, even for a Cambridge don, to detect any feminist push in a proposal to graduate in statistics!

It is strange that Galton did not grasp that of his four alternatives (c), Florence Nightingale's own suggestion, was the sole one that could lead to a school of "enthusiastic youngish men," and that even such young men could not do their work in the spare moments of other employments; it was not only that they would need a leader, but they would need a livelihood. How strangely different the development of modern statistics might have been, had Galton confined himself solely to Florence Nightingale's proposal of a professorship and the creation of a school of social statistics or as she later headed some of her letters "Applied Statistics"! Boldly to have said we need £50,000 or £60,000 to carry out a real scheme would have been the wisest policy. Can we between us and with the aid of others who realise our standpoint induce the public to see the importance of the whole matter, and aid in such an endowment? Instead of appealing to the enthusiasm that a big scheme might have raised, Galton drew up a memorandum to be sent round to a number of prominent statisticians asking their advice as to the disposal of a sum of £4000 available to further the scientific study of social problems from a statistical point of view. He stated that a plan had provisionally commended itself for the distribution of three hundred pounds in honoraria of £50 each to a few selected writers, who should severally draw up a list of what seemed to them to be the most feasible problems in the branch of inquiry with which they were familiar.

"It would be their part to think out and to draw up reasonable plans of campaign, specifying the available data now in existence, and such other data as would be required, and which at the same time might be procured without serious difficulty."

The simultaneous direction of these six highly competent persons to different branches of the same scheme would, Galton thought, greatly assist in its inauguration and drawing public attention to its importance.

The fundamental suggestion then made for the remainder of the endowment was that of the Royal Institution lectureship. There is no evidence that this memorandum was ever issued, or received Florence Nightingale's approval. Indeed some of the sentences in later letters seem to suggest that it did not. She writes in a letter of April 19 (1891) with regard to the subjects of the essays:

"I would only suggest that the statistics on business which the Statistical Society so often and so wisely publishes are not quite the sort of thing, nor are Hygiene and Sanitation proper, for which also there is already much large machinery, official and unofficial. And I would ask: Would 'the matters that affect a large part of the community' include such subjects as so press on my mind, and to which you have so generously given a home?"

and then she reiterates the headings of the suggested topics of her first communication. Again, in a letter of a few days earlier evidently referring to the leaders of the Statistical Society whom Galton proposed to consult:

"Mr Giffen, I suppose, is a bright particular star, but not in my line of business—that of moral sanitation. Nor Sir J. Farrer. Also they are not your 'youngish men' whom you so wisely and so well propose to collect and educate."

It is not of importance for us now to know how far Galton's proposals failed to satisfy Florence Nightingale, or how far further examination of economic possibilities on her own side cooled her ardour.

In 1891 a Demographic Congress was to meet in London and Galton suggested that a memoir should be read before the Congress, if possible under Miss Nightingale's name, urging the more systematic collection and utilisation of demographic statistics, with a view to applying them to the solution of social problems. It is not clear from the remaining letters whether Florence Nightingale, while approving of this scheme, was unwilling that her name should be associated with it; still she was very desirous that her three or four problems should be especially mentioned, and remained willing to subscribe towards honoraria for the proposed essays. On April 21 Galton sent another letter, enclosing a memorandum, which was to be circulated to "half a dozen or so eminent authorities" asking about precise subjects and persons. This memorandum runs :

It is desired to promote Statistical Inquiry into the efficiency of legislative acts, intended to promote the well-being of large classes.

With this object in view it is proposed during the present year to offer £50 to £75 in remuneration for each of two or three essays, severally referring to selected branches of any of the following subjects: (i) Board School Education, (ii) Treatment of the Criminal Classes, especially of boy-offenders, (iii) Effect of Poor-Law and Workhouses, whether de-pauperising or not.

A statement or discussion is desired in each essay of the nature and value of the statistical information now accessible, and of such other information as exists in an unpublished form, and again of such as has not yet been collected but which might apparently be procured without serious difficulty. It is then expected that the writer would discuss the ways in which these data should be treated so as to lead to sound and practically important conclusions with the minimum of difficulty.

Should the results of this first attempt be encouraging, it is proposed to follow it up by further action in future years, perhaps of a wider character.

Galton's letter appears to have remained a month unanswered. The original proposal¹ had shrunk to comparative insignificance, and it is little wonder that there was no enthusiasm for it in its final form. On May 23, 1891, Florence Nightingale wrote apologising for her delay—"I can only sum up my apologies in: how good you have been and how bad I." She returned Galton's memorandum initialed and asked him to send it to the eminent authorities he might select. Five days later Galton replied that to his sorrow he must say that the season was too far advanced for him to attempt to carry through the preliminaries with hope of success:

"You would necessarily and naturally have to be consulted at each important stage, financial arrangements would have to be made and there is not now time for doing all this before the vacation begins and people, especially those of the Universities, scatter. I therefore am obliged to desist for the present at least....The more I think of it the more convinced I am that the assurance in some form of a continuation of these awards or other form of endowment would be an important element of success."

¹ As drawn up by Francis Galton and corrected by Florence Nightingale evidently at their first interview this ran:

_____ is desirous of founding a professorship of statistics, to be called by the name of the _____ Professorship of Statistics, for promoting by means of lectures or otherwise the cultivation and improvement of statistical science, and especially its practical application to social problems.

A last brief letter closes the correspondence:

10 SOUTH STREET, PARK LANE, W. *June 13, '91.*

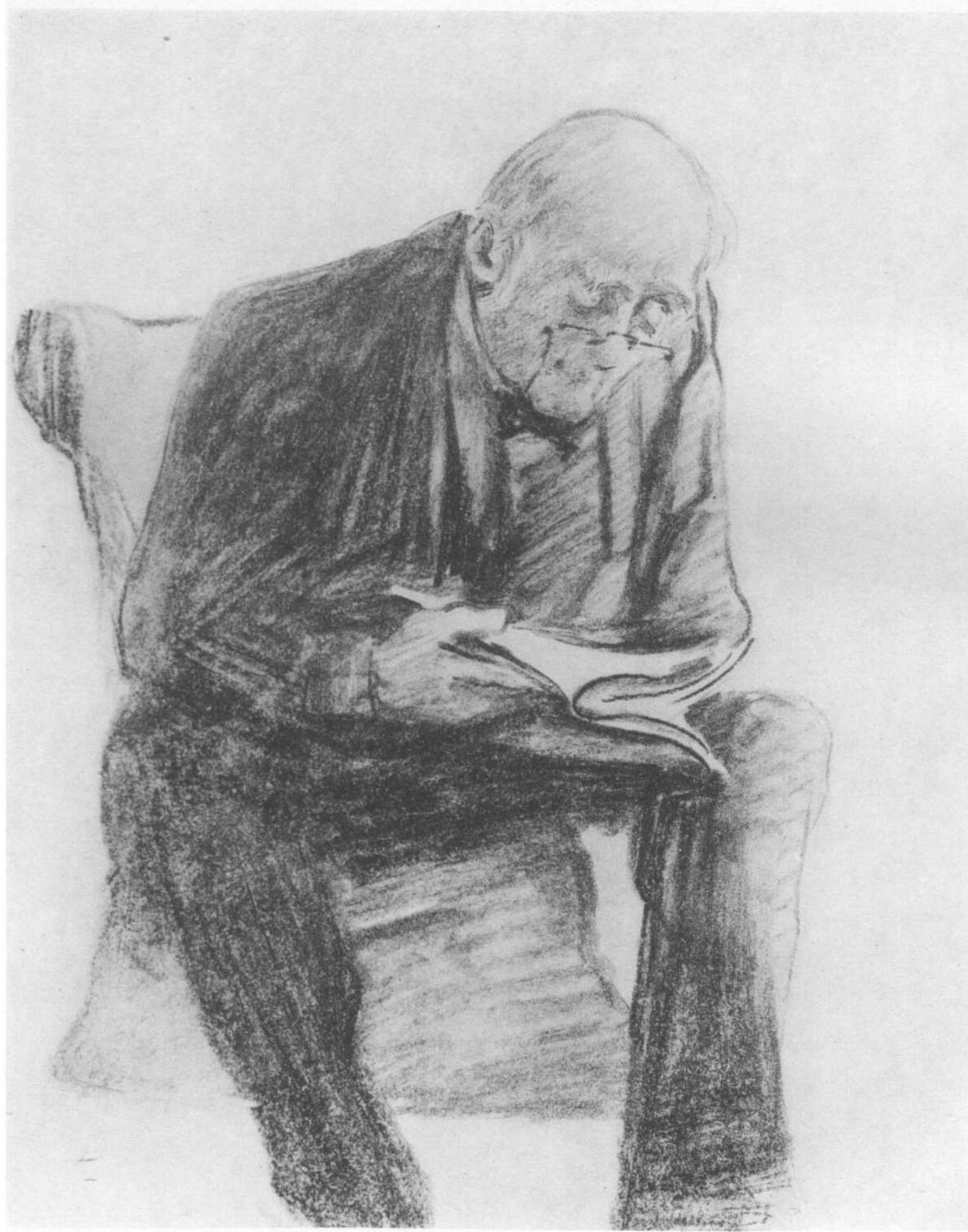
Statistical Inquiry Essays.

MY DEAR SIR, I sorrowfully acknowledge your just award that the season is now too far advanced for you to attempt to carry out the preliminaries. I can only hope that when the vacations are over I may still appeal to your wisdom. You have been more than kind. And no one could do for the matter what you would. I trust your Demography is making favourable progress. I am ever yours gratefully, FLORENCE NIGHTINGALE.

One can but regret this conclusion to what might have been a great success, the realisation of an ideal common to two of the most remarkable minds of the nineteenth century. They were both "passionate statisticians," both saw a great need—a need which still largely exists—and both had shown themselves capable of carrying great enterprises to successful conclusions. Yet somehow Francis Galton seemed to overlook the very kernel of Florence Nightingale's scheme, and the whole vanished in a trivial essay project. Yet the correspondence was, I believe, not without influence on Galton himself, and probably contributed not a little to guide him consciously or unconsciously when he came to make his own foundation in linking it up with a school of statistical training. An additional twenty years demonstrated to him not only the futility of advisory committees, but how little in the way of research could be achieved by the offer of small monetary prizes.

Something would certainly have failed in this chapter, if we had been unable to show even this slender link between the master builder of the modern theory of statistics and the "Passionate Statistician" whose mind had been so deeply stirred by his greatest forerunner, Quetelet:

"I might have done it for you. So it seems:
Perhaps not. All is as God over-rules.
Besides, incentives come from the soul's self,
The rest avail not."



SIR FRANCIS GALTON
1910
From a sketch by his niece, Mrs. Ellis

Note illustrating Francis Galton's Views on Religion.

I found the following remarks in Francis Galton's handwriting among material collected for a new edition of *Inquiries into Human Faculty*. Its bearing on what has been said on pp. 257, 282, and in the footnote, p. 102 will be obvious. The date of the manuscript must be about 1892.

Probably every one has at some time had the feeling that if a dearly loved parent were taken from him, the grief and loneliness after the loss would be insupportable; yet parents die, and their children, after a burst of poignant grief, recover themselves and survive, and most persons of middle age are orphans, leading happy lives full of interests, and mellowed rather than saddened by recollections of the past. The early loves of men and women are intense; they are wholly bound up in one another and the words 'for ever' and the like are the stock expressions of their phraseology, but how transient in many cases are these dispositions. The mind is not wholly dependent on its anchorage to any one given sentiment; if it be cut adrift, at least in early life, after a short while new interests will arise, to which it will moor itself as securely as before. The sense of necessary dependence on any given sentiment may be very strong, but its reality is belied by the experience of what daily occurs around us. Thus if a suspicion were lodged in the mind of a fervent Roman Catholic that the Virgin Mary exercised no protective power over him, the dread lest that suspicion should grow into a conviction would be a far worse terror to him than the anticipation of any earthly orphanage; yet Protestants holding that view lead lives as calm as those of the Catholics. Similarly, the thought to the Christian of being orphaned of Christ is no less horrible; but Jews and Unitarians, some of high position in society, and others, philosophers and men of letters, having no belief in the incarnation and intercessory powers of Christ, live and die as contentedly as Christians. So again, the thought of being orphaned of the paternal guidance of a being having the peculiar attributes of the Jewish Jehovah, would give a terrible shock to many, yet it is notorious that the majority of thoughtful Germans and numerous English Agnostics, whose views on other subjects are treated with general respect and who lead well balanced and contented lives, do not entertain that belief. It is astonishing how devoid of sympathetic intelligence most men are. They are afraid to face the fact that good and able men disagree fundamentally on the elements of religious doctrine, and that therefore no certainty can be claimed for any one of these doctrines. At the best they are only persuasions.