CHAPTER XIV

CORRELATION AND THE APPLICATION OF STATISTICS TO THE PROBLEMS OF HEREDITY

"It is full of interest of its own. It familiarises us with the measurement of variability, and with curious laws of chance that apply to a vast diversity of social subjects. This part of the inquiry may be said to run along a road on a high level, that affords wide views in unexpected directions, and from which easy descents may be made to totally different goals to those we have now to reach. I have a great subject to write upon, but feel keenly my literary incapacity to make it easily intelligible without sacrificing accuracy and thoroughness."

Natural Inheritance, p. 3.

A. *Introductory.* Thus wrote Francis Galton in 1889 when the significance of correlation and its measurement had impressed themselves upon him. Up to 1889 men of science had thought only in terms of causation, in future they were to admit another working category, that of correlation, and thus open to quantitative analysis wide fields of medical, psychological and sociological research. Turning to the writings of Turgot and Condorcet, who felt convinced that mathematics were applicable to social phenomena*, we realise to-day how little progress in that direction was possible because they lacked the key—correlation—to the treasure chamber. Condorcet often and Laplace† occasionally failed because this idea of correlation was not in their minds. Much of Quetelet's work and of that of the earlier (and many of the modern) anthropologists is sterile for like reasons.

Galton turning over two different problems in his mind reached the conception of correlation: A is not the sole cause of B, but it contributes to the production of B; there may be other, many or few, causes at work, some of which we do not know and may never know. Are we then to exclude from mathematical analysis all such cases of incomplete causation? Galton's answer was: "No, we must endeavour to find a quantitative measure of this degree of partial causation." This measure of partial causation was the germ of the broad category—that of correlation, which was to replace not only in the minds of many of us the old category of causation, but deeply to influence our outlook on the universe. The conception of causation—unlimitedly profitable to the physicist—began to crumble to pieces. In no case was B

* "Un grand homme [Turgot], dont je regretterai tousjours les leçons, les exemples, & sur-tout l'amitié, étoit persuadé que les vérités des Sciences morales & politiques, sont susceptibles de la même certitude que celles qui forment le système des Sciences physiques, & même que les branches de ces Sciences qui, comme l'Astronomie, paroissent approcher de la certitude mathématique." *Discours préliminaire, Essai sur l'application de l'analyse à la Probabilité des Décisions*, p. i, Paris, 1785.

† See for example Laplace's memoir in *Mémoires de l'Académie des Sciences* for 1783, pp. 693–702, where he entirely overlooks the correlation between the size of the population and the number of births in evaluating what is really the probable error of the birth-rate.

simply and wholly caused by A, nor indeed by C, D, E and F as well! It was really possible to go on increasing the number of contributory causes, until they might involve all the factors of the universe. The physicist was clearly picking out a few of the more important causes of A, and wisely concentrating on those. But no two physical experiments would—even if our instruments of measurement, men and machines, were perfect—ever lead to absolutely the same numerical result, because we could not include all the vast range of minor contributory causes. The physicist's method of describing phenomena was seen to be only fitting when a high degree of correlation existed. In other words he was assuming for his physical needs a purely theoretical limit—that of perfect correlation. Henceforward the philosophical view of the universe was to be that of a correlated system of variates, approaching but by no means reaching perfect correlation, i.e. absolute causality, even in the group of phenomena termed physical. Biological phenomena in their numerous phases, economic and social, were seen to be only differentiated from the physical by the intensity of their correlations. The idea Galton placed before himself was to represent by a single numerical quantity the degree of relationship, or of partial causality, between the different variables of our ever-changing universe. How far he was successful forms the subject-matter of this chapter.

I have said that Galton came to this fundamental conception from two aspects. The first problem was that of inheritance. To take an illustration: A character in the Father does not determine absolutely the like character in the Son; it is only one out of many contributory factors. The character is only a partial expression of the Father's germ-plasm; so it is with the Son's character—it is not at all a full expression of his germ-plasm. Again, the Son is not a product only of his Father's germ-plasm, but of his Mother's also, and those of both parents in their turn are products of innumerable ancestral stirps leading us back through long eons of evolution. Nor is the somatic or bodily character of the Son a product only of heredity, it is the integration of a number of factors acting throughout his prenatal and postnatal growths. From the physicist's standpoint of causation there was no way at all to attack this problem, the causes were too indefinite and elusive to be individually grasped and measured. They could only be dealt with one at a time—the measure of the resemblance of offspring to parent, a partial causation, led Galton to the idea of correlation.

The second problem which impressed itself on Galton's mind was that of correlation in the narrow biological sense. The word itself appears to have originated with Cuvier who denoted by it an association between two organs or characters of a family—thus the occurrence of a split hoof with a particular form of tooth, so that from the discovery of one organ a prediction could be made as to the nature of others. It has been said that Cuvier's conception did not involve causation*. I do not know that any correlationist of to-day would assert that the knowledge of the length of the femur, which would enable him to closely predict the length of the humerus, is an assertion of

* See C. Herbst, *Handwörterbuch der Naturwissenschaften*, Bd. III, S. 621, Jena, 1913.

causation in a sense different from that of Cuvier; he would merely think in terms of associations with differing grades of intensity. Be this as it may, Galton's second idea of measuring the degree of relationship arose from the fact that he had recognised that two characters measured on a human being are not independent, they vary with each other. The femur of man has its characters associated with those of the humerus.

Galton did not realise immediately that his two problems admitted of the same solution. His first actual attempts at solution of the inheritance problem were based on the weight of the seeds of mother and daughter plants. In the first place he used, about 1875, some seed like that of cress (see Vol. II, p. 392), and he started by endeavouring to correlate grades or ranks. This could not be very successful because the regression curve and the "isograms" (see Vol. II, p. 391) are not linear, but extremely complicated curves. Later in 1875 (*ibid.* p. 187) we find him experimenting with Darwin's assistance on the weight and diameter of sweet-pea seeds, and here he reached his first "regression line." I reproduce (p. 4) from Galton's data in a note-book the first "regression line" which I suppose ever to have been computed. I have recalculated the constants and redrawn the line. It is for sweet-pea diameters in mother and daughter plants. The correlation coefficient is ·33, almost exactly 1/3. Two points must here be noticed. First the parental mean is considerably higher than the offspring mean. If the offspring mean denotes that of the general population, this would indicate that Galton's parental population was not a random sample of the original general population. Secondly the means of the diameters of the daughter plant peas for each size of mother plant pea, give a series of points of rather irregular distribution, which conforms as well to a sloping straight line as to any other form of curve. Here we have the origin of Galton's "regression straight line." We see that as size of mother pea increases, so does size of daughter pea, but whether in excess or defect of mean the daughter pea does not reach the deviation of the mother's diameter from the mean value, the offspring is less a giant or a dwarf than the mother pea. This is Galton's phenomenon of regression. In this case the variabilities of mother and daughter peas were approximately equal, and Galton reached the idea that the slope of the regression line would measure the intensity of resemblance between mother and daughter. If there were no slope the diameter of daughter pea would be the same for all diameters of mother pea. If it sloped at 45°, i.e. a slope of unity, the daughter pea's diameter would be exactly that of the mother pea's, supposing their means were the same; if they were not, the deviations from their respective means would still be equal.

It is strange that both Galton and Mendel should have started from peas, the former from sweet and the latter from edible peas. Galton tells us distinctly why he chose the former, namely because he would not be troubled to the same extent by variation in size of peas within the same pod. We must leave it to the future to judge whether the correlational calculus, which has sprung from Galton's peas, is or is not likely to be of equal service with

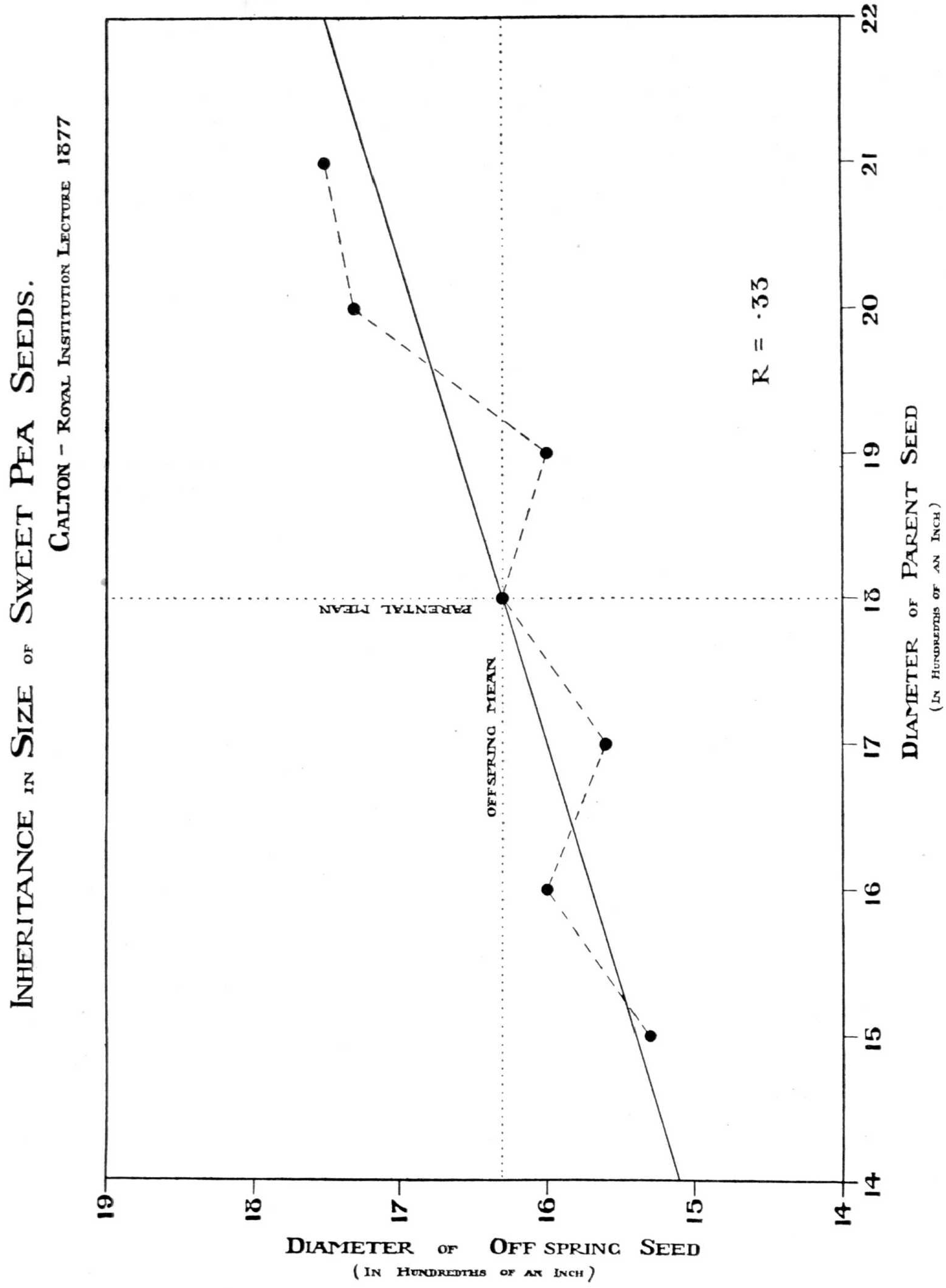

Fig. 1. The first "Regression Line."

the vast system of factorial genetics which has arisen from Mendel's peas—and this even in the theory of heredity. We see now what Galton might have done, he might have provided us with data to check Johansen's later bean-weight experiments, he might have thrown light on the "pure line." He might possibly have reached the correlation coefficient instead of the regression slope in his first attempt to get a measure of correlation. Whatever he might have done, he reached the idea of regression before he reached that of the coefficient of correlation. As long as he was dealing with heredity in the same sex, the approximate equality of variabilities in the two generations preserved him from any great error.

Galton was driven to his second problem by Bertillon's system for the identification of criminals. Bertillon claimed, as I remember Dr Garson did at a much later date, that the measurements chosen were practically independent. Galton needed a criterion to show whether such measurements as head length, foot length, stature, etc. were or were not associated. He saw that the problem closely resembled that of heredity, but he was troubled by the fact that the slope of his regression line depended on the units in which its two component variables were measured. It was not till more than 13 years* after his first attack on the subject that Galton realised, namely in 1889 during a walk in Naworth Park, that the two problems were identical, provided each character were measured in its own variability as unit (see our Vol. II, p. 393). With that provision the slope of the regression line becomes what we now term the coefficient of correlation. It is needful to realise this history of Galton's progress: namely that he reached regression and even the constancy of the array variabilities 12 to 14 years before he formulated his coefficient of correlation, in order to understand fully the sequence of his memoirs on this topic.

One further fact it is necessary to bear in mind in order to measure his achievements. He started like Quetelet from the normal curve as describing the deviations of a population or of any selected population, e.g. that of an array of offspring from a parent of given character. He did not start with a general definition of correlation and see whither that would lead him. His justification was that he was dealing with anthropometric characters or measurements on living forms whose deviations from type approximately followed this special law of distribution. Thus he naturally reached a straight regression line, and the constant variability for all arrays of one character for a given value of a second†. It was, perhaps, best for the progress of the correlational calculus that this simple special case should be promulgated first; it is so easily grasped by the beginner. But it has had the disadvantage that certain branches of science, as psychology for example, have rarely got further, and, without taking the trouble to apply tests, adopt linear

* In his *Natural Inheritance*, 1889, p. 79, Galton says his sweet-pea data were collected more than 10 years previously. His lecture at the Royal Institution, Feb. 1877, shows that he was then already in possession of sweet-pea data, and the first measurements seem to have been made in 1875.

† What we now term "homoscedasticity."

regression and homoscedasticity where it is quite inappropriate. It is interesting to note how the history of the spread of knowledge follows with halting steps the history of its discovery.

Again, if the reader anticipates that Galton was a faultless genius, who solved his problems straightaway without slip or doubtful procedure, he is bound to be disappointed. Some few creative minds may have done that, or appear to have done it, because, the building erected, they left no signs of the scaffolding; but the majority of able men stumble and grope in the twilight like their smaller brethren, only they have the persistency and insight which carries them on to the dawn.

B. *The First Idea of "Regression."* I think these conceptions will be well illustrated if we consider Galton's first paper dealing with the subject of regression, namely the lecture entitled: *Typical Laws of Heredity*, which he gave on February 9, 1877 at the Royal Institution. It is the next forward step he took after the memoir of 1875, in which he had propounded for the first time the continuity of the germ-plasm. See our Vol. II, pp. 184–8. The paper itself embraces three fundamental sections, which I will take in logical sequence if not that of the paper itself.

First: an account of the experimental data on sweet-peas. Galton assumes here that sweet-peas are invariably self-fertilised, a result which from my own observation I consider only partially true. There is also a further difficulty here: he does not take the *average* seed of the mother plant as representing the maternal character. He takes seeds of equal weight which may have been the ordinary produce of large-seeded plants, or the exceptional produce of small-seeded plants, and treats these as representing the parental character. This very fact would in itself involve regression in the offspring seeds, and leaves unsettled two important questions: (i) whether in the average result from all the seeds of a self-fertilising plant, there would be any regression at all, and (ii) whether there is any difference in the average seed weights of daughter plants grown from light and heavy seeds of the mother plant? Had Galton had these points in mind, he might have thrown light on controversies of a much later date. Again, does the size of the mother seed influence the daughter seed only by way of heredity? Galton's small seeds led to sickly and often sterile plants, and it is quite probable that this might affect the weight of their seeds (see our Vol. II, p. 181). Be this as it may, Galton found from his data* that there was a *linear* regression of daughter seed on maternal seed. He does not yet use the term "regression," but speaks of a "reverting" towards "what may be roughly and perhaps fairly described as the average ancestral type." But it is difficult to believe that this reversion was solely due to heredity; if the original seed had fully represented the maternal plant and that plant had been indefinitely self-fertilised, the Law of Ancestral Heredity would suggest no regression at

* He issued packets of seven sizes of seeds, each containing ten seeds, and nine friends grew the plants. Two crops failing, he had all the seed offspring of $7 \times 7 \times 10 = 490$ carefully weighed seeds.

all. It is not possible to say whether the observed "reversion" was due to the weight of a single seed not representing the true maternal character, to the hypothesis of self-fertilisation not being correct or to other causes. Theoretically the important point is that Galton reached linear regression as a first feature of his correlation table. The next point Galton reached was the homoscedasticity or equal variability of the arrays of daughter seeds corresponding to a given mother seed*. "I was certainly astonished to find the family variability of the produce of the little seeds to be equal to that of the big ones; but so it was, and I thankfully accept the fact; for if it had been otherwise, I cannot imagine, from theoretical considerations, how the typical problem could be solved" (p. 10).

The second logical stage in Galton's analysis is mathematical; he endeavours, assuming that the population is stable and is distributed normally, to find what relation must exist between the "reversion" coefficient and

* Thus far I have not been able to find Galton's data for the weights of sweet-peas in the *Galtoniana* here. It is not easy, however, to find a special topic in the mass of note-books and undated and unindexed papers. Quite possibly, however, he lent his measurements to somebody, as he lent many series of observations to myself. It would be interesting to see exactly the data from which he deduced the two fundamental principles of a normal bivariate distribution, i.e. the straight-line regression and the equivariability of the arrays. Galton gives the correlation table of filial and parental seeds in the Appendix, p. 226, of his *Natural Inheritance* for *lengths* not weights. This shows that the mean length and variability of the parent seeds were arbitrarily chosen, there being 70 of each. Further, in the table the offspring seeds are modified to show 100 in each array. We do not know therefore the true means or standard deviations of either parental or offspring populations. This does not, however, affect the determination of either means or standard deviations of arrays. I find in hundredths of an inch:

Diameter of Parent Seed	Mean Diameter of Array of Filial Seeds	Standard Deviation of the Array
21	17·26	1·988
20	17·07	1·938
19	16·37	1·896
18	16·40	2·037
17	16·13	1·654
16	16·17	1·594
15	15·98	1·763

My means do not agree with Galton's, possibly he found his before reducing his whole numbers to percentages. (It could not be by the distribution of the filial diameters "Under 15," as this would tend, I think, to reduce all his means below mine.) He does not give his array standard deviations nor the quartiles. However, on some such numbers as these Galton reached his results. The array means are not incompatible with a straight-line relation; the standard deviations suggest that the smaller parental seeds had offspring seeds of less variability than those of the larger seeds, rather than equivariability being the rule. This view might be modified if we knew the actual distribution of the filial seeds "Under 15." Many of these dwarf seeds I suspect were abortions, as their lumping up at the tail of the arrays really prevents the latter from being considered as "normal curves." Galton states (*loc. cit. supra*) that he had obtained confirmatory results for the foliage and length of pod; this indicates that his experiments must have been carried on for a second year, as he started only with the parental seed.

the variability constant of the equivariable arrays in order that the population may owing to the laws just stated repeat in the filial the parental distribution.

Now there are two points to be regarded here. Galton first states that he is going to suppose no sexual selection at work, and further he next supposes every female to be reduced to an equivalent adult male standard. It is true that he does this by the aid of percentiles, but what it really amounts to is this: If m_2 be the female mean character, σ_2 the standard deviation and Δ_2 the deviation of an individual female from type, m_1, σ_1 and Δ_1 corresponding quantities for the male, then Galton replaces the female $m_2+\Delta_2$ by a male $m_1+\Delta_1$, where Δ_1 has the same percentile value p for males as Δ_2 for females. This really amounts to taking $\Delta_1 = \frac{\sigma_1}{\sigma_2}\Delta_2$; it appears to me that this reduction of female to male value is more correct than that which he adopted later in his memoir of 1886 and in *Natural Inheritance* (see our p. 15). Having got his midparental value as the mean of the father's and mother's characters, the last reduced to male value, Galton correctly asserted that if there be no sexual selection and the original population followed a normal distribution, the midparental distribution also would be normal with a standard deviation $\frac{1}{\sqrt{2}}\sigma_1$. He next introduces an ingenious artifice; instead of supposing the offspring to "revert" he supposes the midparent to revert and then to have offspring whose type (i.e. mean value) is that of the original parentage. In other words, if X be the character in a midparentage, then $r'X$, where r' is the reversion coefficient, will be the same midparentage after reversion. This really signifies a uniform "squeeze" in the ratio of r' to 1 of the normal curve of midparentages, or the new curve of reverted midparentages will be a normal curve of standard deviation $\frac{1}{\sqrt{2}}\sigma_1 \times r'$. We have lastly to distribute the offspring of these midparentages about their mean values with a constant variability, which we will represent by Σ; thus the standard deviation σ' of the distribution of offspring will be given by

$$\sigma'^2 = \tfrac{1}{2}\sigma_1^2 r'^2 + \Sigma^2.$$

But, if this standard deviation of the final normal curve is to repeat the original population, σ' must equal σ_1, or we have

$$\Sigma^2 = \sigma_1^2\left(1 - \tfrac{1}{2}r'^2\right).$$

Here r' is the "reversion" of the midparent and is equal to $\sqrt{2}r$, if r be the reversion on a single parent*. In other words, if r be the reversion of offspring on parent then the constant standard deviation of the array of offspring for a given parent must be $\sigma_1\sqrt{1-r^2}$, if the population starts with a normal distribution and when reproduced is to have the same normal

* If the standard deviation of the "reverted" single parent be $r\sigma_1$, then $\sqrt{2}\,r\sigma_1$ will be the standard deviation of the reverted midparent, but if this be taken as $r'\sigma_1$ clearly $r' = \sqrt{2}\,r$.

distribution. This is the earliest appearance of the symbol r as a coefficient of "reversion"; the reasoning by which the result is obtained is only true, if parental and offspring generations have the same variability; in that case r is what we now term the coefficient of correlation, and Galton here deduces the relationship between the constant array variability and this coefficient.

In the course of his work he introduces the ideas of natural selection and of differential fertility. This section of the discussion is somewhat difficult to follow. Galton further supposes selection to take place symmetrically round the population mean or type. Finally to obtain the above result Galton supposes the selection and the fertility to be non-differential, or gives them mere percentage values for all parents alike*.

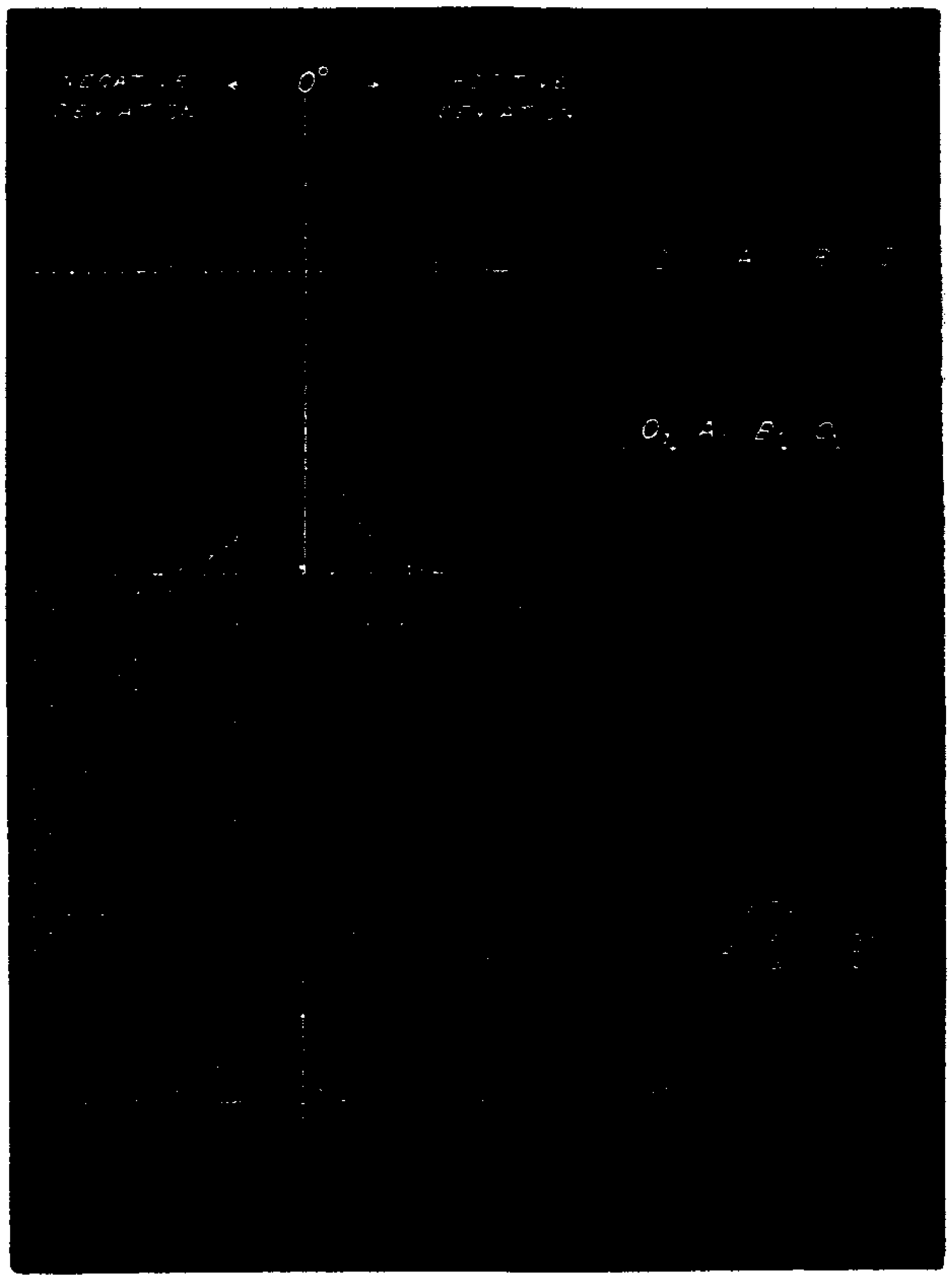

Fig. 2. Galton's Quincunx illustrating the nature of Regression.

The third point in this paper of Galton's is the ingenious "Quincunx" by which he illustrates the phenomenon of reversion and the continual maintenance by aid of inheritance of a stable population. Galton at first indicates how closely certain measured characters are given by a normal distribution and how such a normal distribution may be produced by a stream of pellets

* A paper in which this matter is more fully dealt with by the present writer will be found in *Biometrika*, Vol. VII, pp. 258–275, "On the Effect of a Differential Fertility on Degeneracy: A New Year's Greeting to Francis Galton, 1910."

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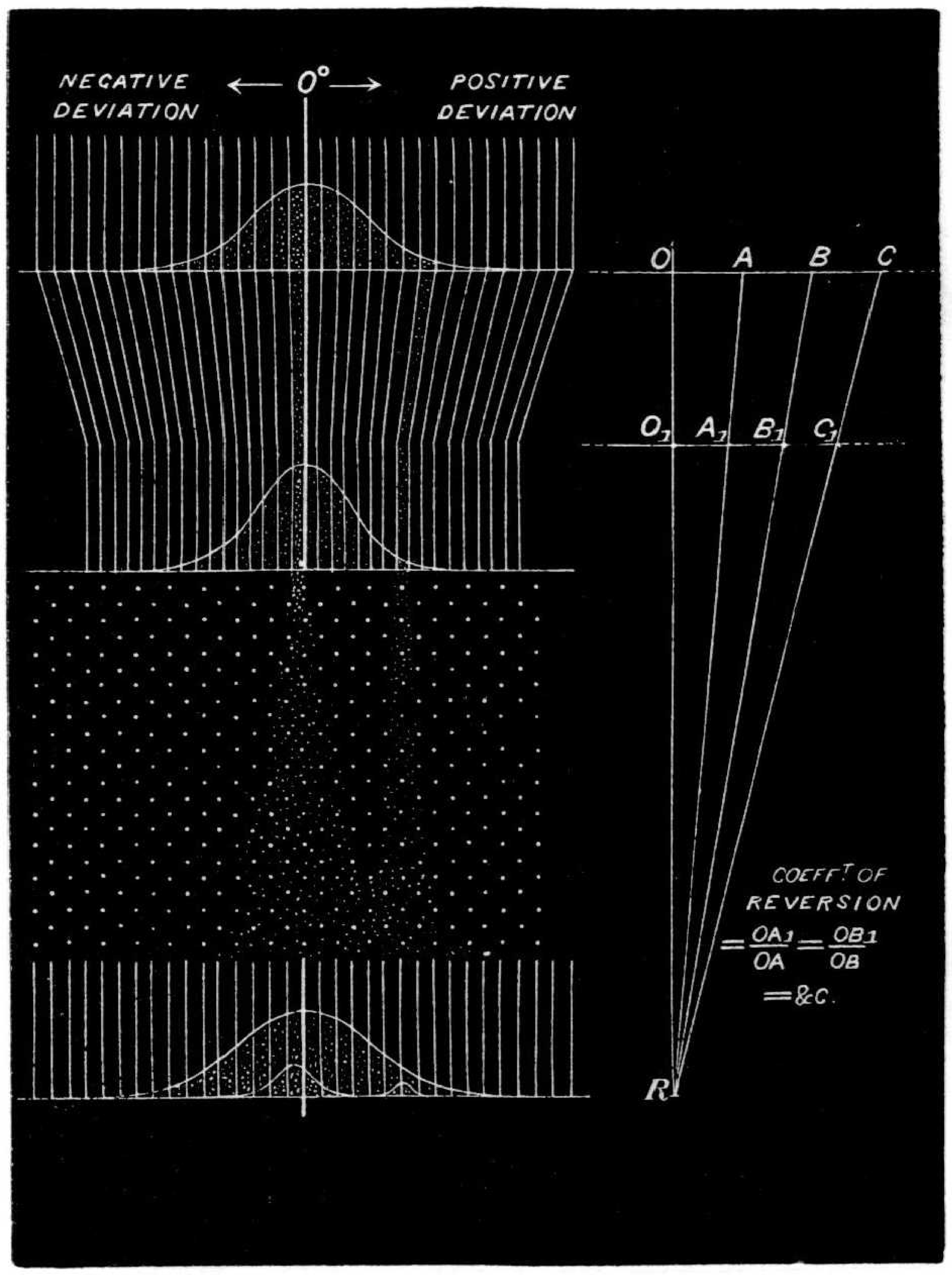

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falling vertically through a forest of horizontal pins. He next, starting with a normal distribution of variability σ_1, reduces the variability to $r\sigma_1$ by sloping his discharge tubes towards the type (see Fig. 2). This restriction of the tubes has the same effect as giving a uniform horizontal "squeeze" to his original distribution; he thus reaches his population of "reverted parents." If he now opens any single one of his tubes he will get a normal distribution, about the reverted parent character as type, which will have

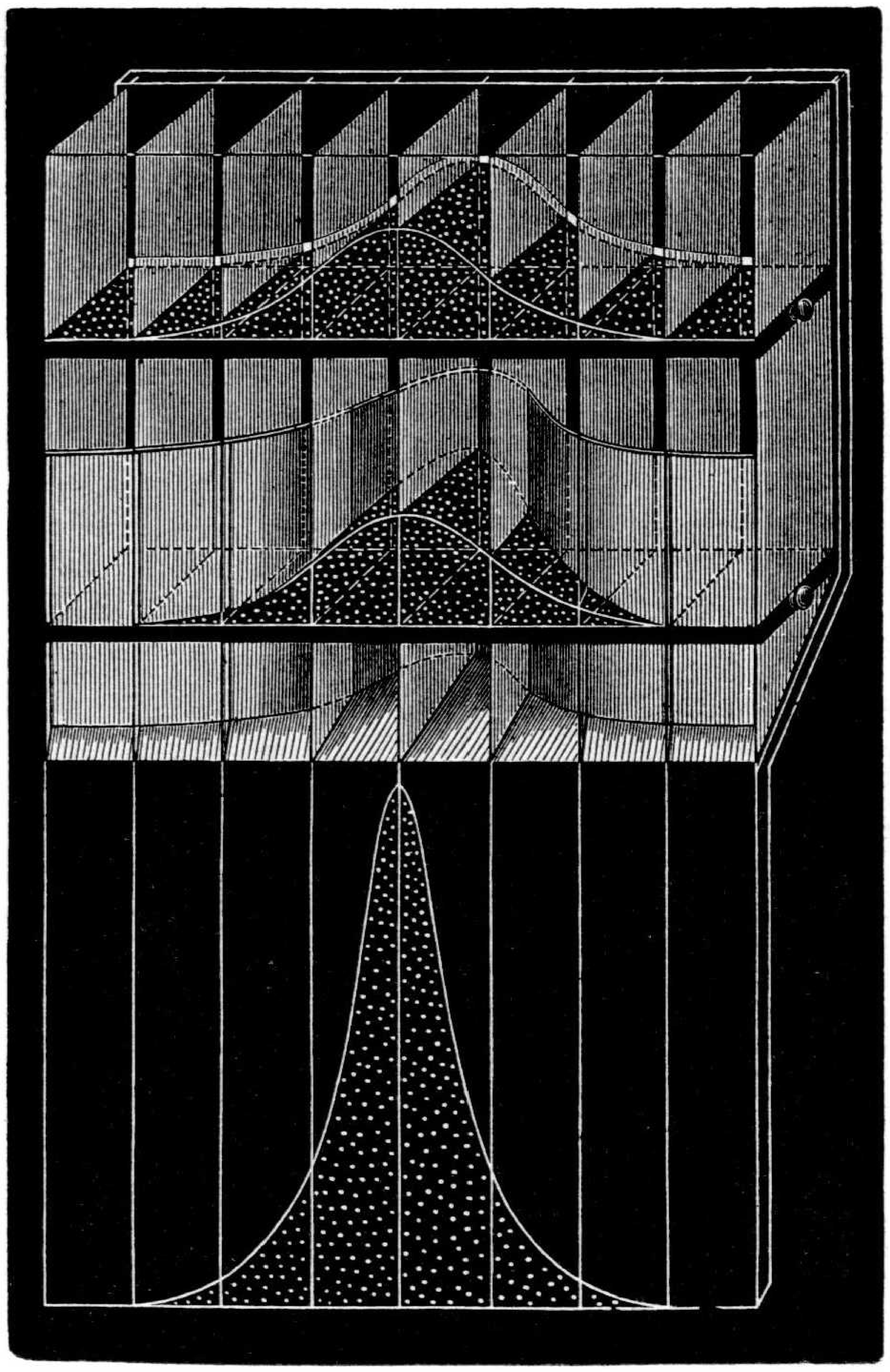


Fig. 3. Galton's Quincunx illustrating the effect of Natural Selection.

the proper variability $\sigma_1\sqrt{1-r^2}$ if a suitable choice be made of the extent of "pin-forest" through which the pellets fall. Since this variability is the same for all parentages, the extent is constant, and if all the tubes be opened, all the "reverted" parentages contribute their share to building up again the population from which we started.

Those who hold the hypothesis of the pure line to be true, apparently overlook the fact that while the gametic distribution might be stable, they must appeal to a stringent natural selection, or a differential fertility, to maintain stability for two successive generations in somatic characters. This stability Galton achieved by aid of reversion.

In dealing with the problem of Natural Selection, Galton takes only the case of selection round type and assumes that those selected to live, not those selected to die, will follow a normal distribution. This limits to some extent its general applicability, but he illustrates his idea by a second ingenious Quincunx (see Fig. 3), in which the middle stage is formed by a vertical normal-curve diaphragm which cuts off from the descending pellets, uniformly distributed over the horizontal bases of their compartments in the top stage, the "selected pellets," which again are on the removal of the sliding floor allowed to run down into the third stage compartments where they form a normal distribution of much reduced variability.

Speaking of the principles of "reversion" and reduced variability in the offspring of a given parentage, Galton says:

> "The typical laws are those which most nearly express what takes place in nature generally; they may never be exactly correct in any one case, but at the same time they will always be approximately true and always serviceable for explanation. We estimate through their means the effects of the laws of sexual selection, of productiveness and of survival, in aiding that of reversion in bridling the dispersive effect of family variability. They show us that natural selection does not act by carving out each new generation according to a definite pattern on a Procrustean bed, irrespective of waste. They also explain how small a contribution is made to future generations by those who deviate widely from the mean, either in excess or deficiency, and they enable us to discover the precise sources whence the deficiencies in the produce of exceptional types are supplied, and their relative contributions. We see by them that the ordinary genealogical course of a race consists in a constant outgrowth from its centre, a constant dying away at its margins, and a tendency of the scanty remnants of all exceptional stock to revert to that mediocrity, whence the majority of their ancestors originally sprang." (*loc. cit.* p. 17.)

Thus Galton stated his law of reversion originally; we see that it really covers the most marked features of bivariate normal correlation, we have even the now-familiar symbol r. Whether, however, he was at that time justified in asserting reversion as a typical law of heredity on the basis of his sweet-pea results may be open to question. Is the weight or diameter of a single seed a fair representation of a parental somatic character? Was Galton justified in considering the variability of his offspring constant? These are points which have much bearing on later work and on what correlation the r really signified in the case of Galton's actual experimental data.

C. *Heredity in Stature of Man. Development of the Conception of Regression.* That Galton had some doubts himself is, I think, clear from the fact that for eight years he published nothing further on the subject of regression, but started by aid of his family records to collect data bearing on inheritance in man: see Vol. II, pp. 363 *et seq.* As soon as he had obtained enough data to deal with the inheritance of stature in man he returned to the subject, and in 1885 and 1886 published a number of papers dealing with the topic. The first of these is his Presidential Address to the Section of Anthropology of the British Association, Aberdeen Meeting, 1885*. He next published a more detailed paper in the *Miscellanea* of the *Journal of the*

* *B. A. Transactions*, 1885, pp. 1206–1214; *Nature*, Vol. XXXII, pp. 507–510.

*Anthropological Institute**. He further took the subject as the topic of his Presidential Address at the Anniversary Meeting of that Institute in January, 1886†, having meanwhile again discussed it in a lecture at the Birmingham and Midland Institute entitled: "Chance and its Bearing on Heredity"‡. Finally we have the mathematical basis of Galton's work more fully provided in a paper on "Family Likeness in Stature" with an Appendix by J. D. Hamilton Dickson, presented to the Royal Society on January 1, 1886§. None of these papers is exclusive, each has something not in the others, but probably those in the *Miscellanea* of the *Journal of the Anthropological Institute* and in the *R. S. Proceedings* are the more important for those who have not time to read them all. We have throughout to remember that Galton was a pioneer, and could not see matters in the clearer light of to-day when we start from a knowledge of bivariate distribution with its two means, two variabilities and its coefficient of correlation; he did not yet clearly recognise the distinction between a coefficient of regression and a coefficient of correlation. It is difficult for the reader now-a-days to appreciate the paradox which Galton reached from his data and finds it needful to discuss at some length, namely: that the coefficient of regression for the offspring on a midparent is double what it is for the midparent on the offspring‖. A further difficulty is that Galton invariably thought in terms of grades, quartiles and the "ogive curve," and this I venture to think is by no means helpful for elucidating correlation, as the reader of the first ten pages of the Royal Society paper will find. It has always been a puzzle to me why Galton called in Mr Dickson and placed before him a somewhat artificial problem in probability the answer to which comes directly¶ from Galton's own two statements.

* Vol. xv, pp. 246–263. † Vol. xv, pp. 489–499.

‡ Reported in the *Birmingham Daily Post*, December 7, 1886.

§ *Roy. Soc. Proc.* Vol. XL, pp. 42–73, 1886.

‖ Since the midparental standard deviation is, when the female is reduced to male equivalent, $\sigma_1/\sqrt{2}$ in our previous notation, the two regression coefficients are respectively: $\frac{\sigma_1/\sqrt{2}}{\sigma_1}r$ and $\frac{\sigma_1}{\sigma_1/\sqrt{2}}r$, that is, $r/\sqrt{2}$ and $\sqrt{2}\,r$, or one twice the other. I think Galton was slightly puzzled here, because he had not yet fully realised that the two variabilities not being the same, he must measure each variate in its own unit of variability in order to make both regressions the same.

¶ Galton had discovered that the offspring of parents of character deviation x vary about $(r\sigma_2/\sigma_1)\,x$ with a standard deviation $\sigma_2\sqrt{(1-r^2)}$. Hence if y be the deviation of the n offspring of the n' parents of deviation x, and we assume, as Galton, that parental and offspring generations both follow the normal law, the number of offspring of deviation y will be

$$\frac{nn'}{\sqrt{2\pi}\,\sigma_2\sqrt{1-r^2}}\,e^{-\frac{1}{2\sigma_2^2(1-r^2)}\left(y-\frac{r\sigma_2}{\sigma_1}x\right)^2}.$$

But $n'=\frac{N}{\sqrt{2\pi}}\frac{1}{\sigma_1}e^{-\frac{1}{2}\frac{x^2}{\sigma_1^2}}$, where N is the total population of parents, thus substituting for n' we have

$$z=\frac{nN}{2\pi\sigma_1\sigma_2\sqrt{1-r^2}}\,e^{-\frac{1}{2(1-r^2)}\left(\frac{x^2}{\sigma_1^2}-\frac{2rxy}{\sigma_1\sigma_2}+\frac{y^2}{\sigma_2^2}\right)}$$

as the frequency distribution of offspring and parents, the well-known result, which was not even written down by Mr Dickson!

The most noteworthy point, however, is this, that Galton having the correlation table before him of the statures of 928 offspring and of their midparents proceeded after smoothing the frequencies to determine the contour lines and found them to be:

(i) a system of concentric and similar ellipses about the common mean of the filial and midparental statures.

Further:

(ii) the regression straight lines were conjugate diameters to the two axes of stature.

He also determined from his contours the ratio of the axes of this ellipse system, and the inclination of the major axis to the horizontal. The ellipse, which served as type, is given in the accompanying diagram (see Fig. 4, p. 14), and the observed values on this ellipse and the values computed from Mr Dickson's Formulae are*:

	Galton from Contours	From Dickson's Formulae
Regression Slope	1 in 3	6 in 17·5
Major to Minor Axis	10 to 5·1	$\sqrt{7}$ to $\sqrt{2}$ or 10 to 5·35
Inclination of Major Axis	25°	26° 36′

It is needless to say that Galton was delighted with this accordance. He wrote† as follows with regard to it:

"I may be permitted to say that I never felt such a glow of loyalty and respect towards the sovereignty and magnificent sway of mathematical analysis as when his [Mr Dickson's] answer reached me confirming, by purely mathematical reasoning, my various and laborious statistical conclusions with far more minuteness than I had dared to hope, for the original data ran somewhat roughly, and I had to smooth them with tender caution‡."

We ought on no account to overlook the fact that the theory of linear regression and the associated homoscedasticity were evolved by Galton from his sweet-pea experiments, confirmed by his stature measurements, and resulted practically in the form of the normal surface for two variates with its elliptic contours, before the mathematical theory of correlated errors was known to him. It is one of the most striking lessons in what may be achieved by a patient analysis of even crude observations. Yet without being discouraged in our own attempts at similar discoveries, we do well to remember that only an exceptional mind has the insight to discriminate between the essential and the non-essential in a mass of statistical data, and to select those two principles which illuminate the manner in which a population reproduces itself stably by aid of heredity—and what is more in so doing to pave the way to the solution of many other problems of a wholly different character. Fig. 5, p. 16, shows the regression line of offspring on midparent for the case of stature; it is, I think, the second regression line ever drawn, and Galton indicates by the line at 45° exactly how much the offspring fall behind the stature of their individual midparent. He added to this regression diagram, a picture of his "Forecaster of Stature"—which might equally well be used to

* *Journal of the Anthropological Institute*, Vol. xv, p. 263.

† *Ibid.* p. 255.

‡ *Ibid.* p. 255.

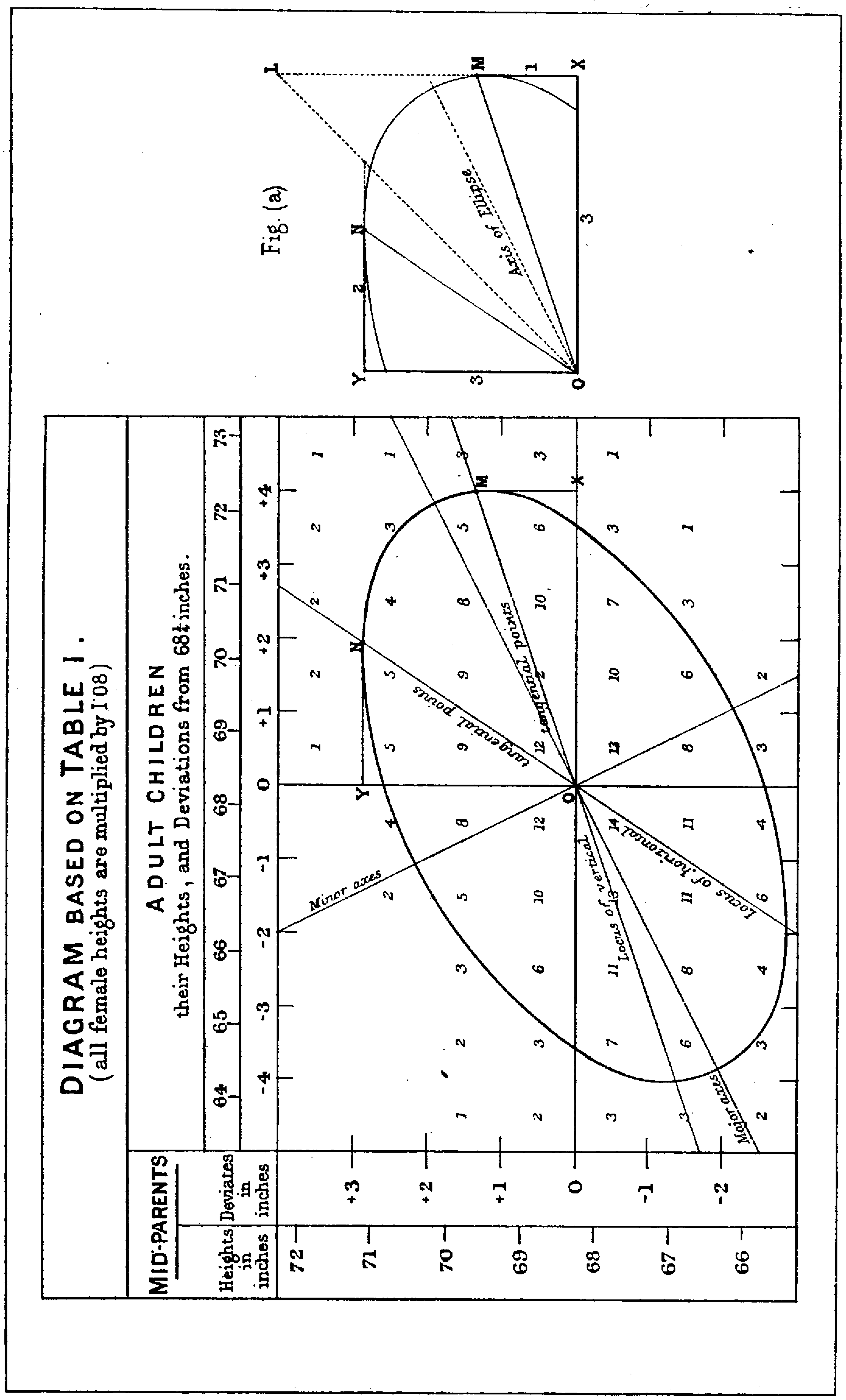


Fig. 4. Galton's Elliptic Contour drawn from his observations.

predict the probable value of any third variate from a knowledge of two others*. The working of the Forecaster is almost obvious on examination of the diagram, but for the benefit of those who come for the first time to the subject of regression I give Galton's own words:

"The weights *M* and *F* have to be set opposite to the heights of the mother and father on their respective scales; then the weight *sd* will show the most probable heights of a son and daughter on the corresponding scales. In every one of these cases it is the fiducial mark in the middle of each weight by which the reading is to be made. But, in addition to this, the length of the weight *sd* is so arranged that it is an equal chance (an even bet) that the height of each son or each daughter will lie within the range defined by the upper and lower edges of the weight on their respective scales. The length of *sd* is 3 inches $= 2f$†; that is, $2 \times 1{\cdot}50$ inch.

"*A*, *B* and *C* are three thin wheels with grooves round their edges. They are screwed together so as to form a single piece that turns easily on its axis. The weights *M* and *F* are attached to either end of a thread that passes over the movable pulley *D*. The pulley itself hangs from a thread which is wrapped two or three times round the grove of *B* and is then secured to the wheel. The weight *sd* hangs from a thread that is wrapped in the same direction two or three times round the groove of *A*, and is then secured to the wheel. The diameter of *A* is to that of *B* as 2 to 3. Lastly, a thread wrapped in the opposite direction round the wheel *C*, which may have any convenient diameter, is attached to a counterpoise.

"It is obvious that raising *M* will cause *F* to fall, and *vice versâ*, without affecting the wheels *A*, *B*, and therefore without affecting *sd*; that is to say, the parental differences may be varied indefinitely without affecting the stature of the children, so long as the mid-parental height is unchanged. But if the mid-parental height is changed, then that of *sd* will be changed to $\frac{2}{3}$ of the amount.

"The scale of female heights differs from that of the males, each female height being laid down in the position which would be occupied by its male equivalent. Thus 56 is written in the position of 60·48 inches, which is equal to $56 \times 1{\cdot}08$. Similarly, 60 is written in the position of 64·80, which is equal to $60 \times 1{\cdot}08$‡."

The last words indicate what is, I think, an important point: Galton obtains the female from the male stature by multiplying by the constant factor 1·08. This he obtained as the ratio of the male to the female mean value, and he practically assumes this ratio to be the same for all other statures.

In a certain sense I think this is, at least theoretically, a retrograde step from his suggestion of 1877. He then took the transmuted female mean to be the male mean plus the female deviation increased in the ratio of male to female variability. This appears to be theoretically a better process of transmutation. Practically the two methods will only agree, if the ratio of the two variabilities is equal to the ratio of the two means, i.e. if the so-called coefficients of variability of the two sexes are equal. This is approximately but not absolutely true for a number of human characters.

There are of course several other conditions which must be fulfilled to make Galton's definition of midparent valid, and some of these he discusses. In the first place the parents must mate at random with regard to the character dealt with, i.e. there must be no sexual selection in the form of assortative mating with regard to stature, tall must not tend to marry tall,

* It would only be needful to adopt scales in accordance with the constants of the bivariate regression formula.

† In this paper Galton uses the symbol *f* for the quartile deviate.

‡ *Journ. Anthrop. Institute*, Vol. xv, p. 262.

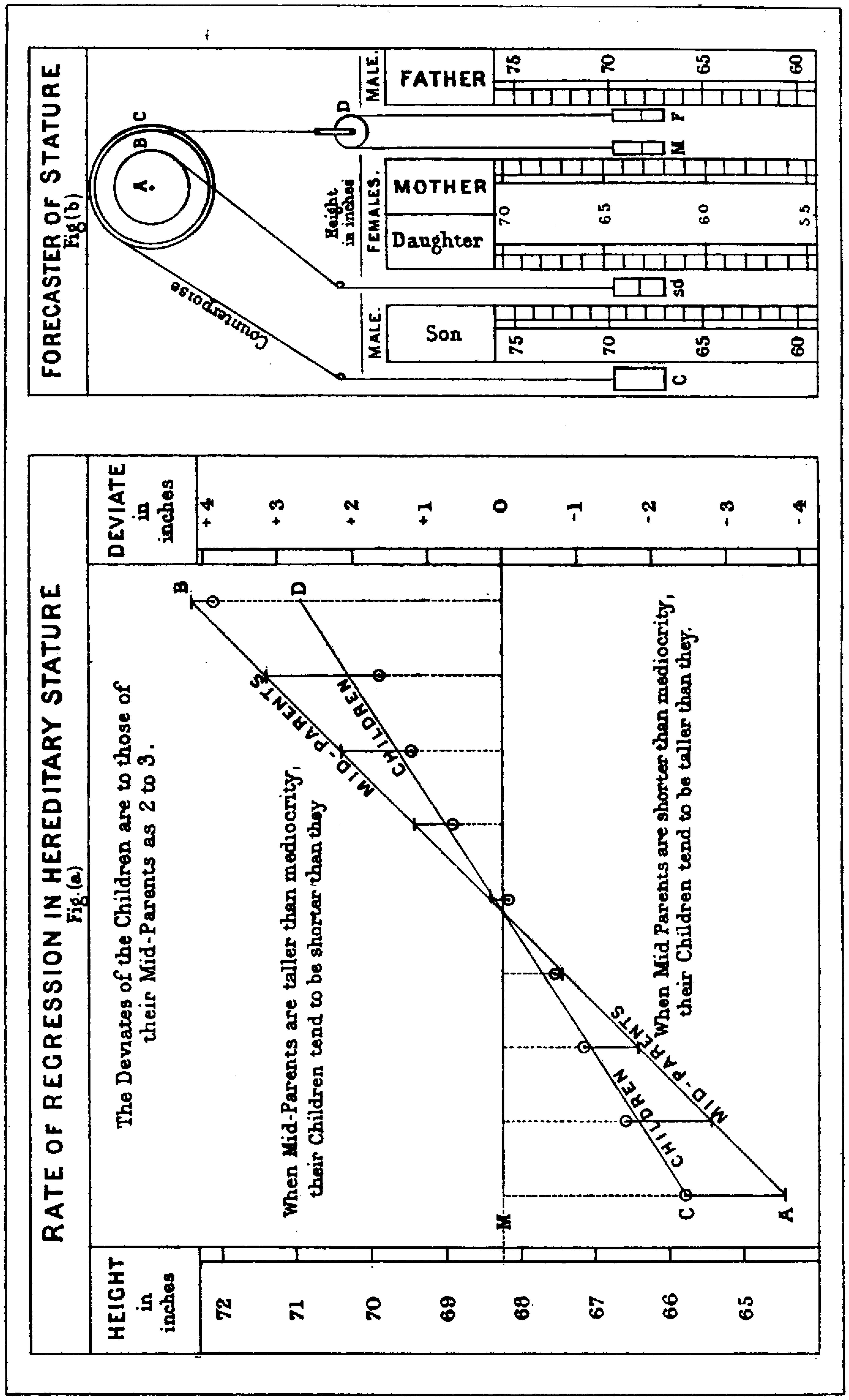


Fig. 5. Galton's Second Regression Line and his "Forecaster of Stature."

nor short, short. Galton discusses* the absence of assortative mating for stature and forms the following table, where the medium group embraces individuals of 67″ and up to 70″ stature for males or transmuted females:

Husband

Wife	Short	Medium	Tall	Totals
Short ...	9	28	14	51
Medium	25	51	28	104
Tall ...	12	20	18	50
	46	99	60	205

He notes that there are 27 like marriages short with short and tall with tall, and 26 contrasted marriages† short with tall, and argues that there is no assortative mating in stature. In a fuller treatment of the same data by the present writer the coefficient of resemblance between husband and wife was found to be ·093 ± ·047‡, which might just be significant. Later work has shown that there is sensible assortative mating not only in stature (·280), but in span (·199) and cubit (·198)§; in other words big men do tend to marry big women and small men small women. Galton's data show, however, so little assortative mating that his results were not sensibly influenced by disregarding it.

Galton now turns to another point, namely: Does the difference in stature of parents influence the stature of the offspring? He was clearly conscious that this was an important point, for on it depends whether his value for the midparental stature is or is not to be considered correct. As we should now express it, he was really asking whether the stature in the offspring was equally correlated with the statures of the two parents, or rather, that is the question he would have been asking had he transmuted his female deviations to male deviations by aid of the ratio of the two variabilities and not of the two means‖. If the two correlations be not equal, then Galton's "Forecaster," based on his conception of midparent, would give incorrect results. Galton indicates in a table (*Journ. Anthrop. Instit.* Vol. xv, p. 250) that the differential influence of the parents should not be very great, but he does not really

* *Journ. Anthrop. Instit.* Vol. xv, p. 251.

† Printed in *loc. cit.* 32 instead of 26.

‡ *Phil. Trans.* Vol. 187 A, p. 270, 1896.

§ *Biometrika*, Vol. II, p. 373.

‖ If r_{13} be the paternal, r_{23} the maternal coefficient of correlation and r_{12} that of assortative mating, the bivariate formula shows us that to give equal weight to father and mother we must have equality of the two expressions

$$\frac{r_{13} - r_{12}r_{23}}{1 - r_{12}^2} \text{ and } \frac{r_{23} - r_{12}r_{13}}{1 - r_{12}^2}$$

(*Roy. Soc. Proc.* Vol. VIII, p. 240, 1895), and this involves $r_{13} = r_{23}$, i.e. the equality of the parental influences.

determine it quantitatively. Actually for his data we have the following correlations*:

	Father	Mother
Son	·396 ± ·024	·302 ± ·027
Daughter	·360 ± ·026	·284 ± ·028

There was thus really quite a well-marked prepotency of the father in the case of stature. Later results on ampler and better material have failed to confirm this prepotency†; I think it may well have been due to amateur measuring of stature in women, when high heels and superincumbent chignons were in vogue; it will be noted that the intensity of heredity decreases as more female measurements are introduced. Daughters would be more ready to take off their boots and lower their hair knots, than grave Victorian matrons. As we have not since succeeded in demonstrating any sex prepotency in parentage, Galton's assumption that such did not exist justifies his theory. But this assumption was not justified by his actual data and affects seriously the values of the constants he reached, which are all too low in the light of more recent research. I think we should be inclined to say now that the regression of the offspring deviate‡ is on the average nearer to $\frac{4}{5}$ than to Galton's $\frac{2}{3}$ of the midparental deviate. Galton, however, recognised very fully that his numerical values were only first approximations. He writes:

"With respect to my numerical estimates, I wish emphatically to say that I offer them only as being serviceably approximate, though they are mutually consistent, and with the desire that they may be reinvestigated by the help of more abundant and much more accurate measurements than those I have at command. There are many simple and interesting relations to which I am still unable to assign numerical values for lack of adequate material, such as that to which I referred some time back, of the relative influence of the father and the mother on the stature of their sons and daughters.

"I do not now pursue the numerous branches that spring from the data I have given, as from a root. I do not speak of the continued domination of one type over others, nor of the persistency of unimportant characteristics, nor of the inheritance of disease, which is complicated in many cases by the requisite concurrence of two separate heritages, the one of a susceptible constitution, the other of the genus of the disease. Still less do I enter upon the subject of fraternal deviation and collateral descent§."

Galton's reasons for making a special study of stature are dealt with at considerable length and summarised as follows:

"The advantages of stature as a subject in which the simple laws of heredity may be studied will now be understood. It is a nearly constant value that is frequently measured and recorded, and its discussion is little entangled with consideration of nurture, of the survival of the fittest, or of marriage selection. We have only to consider the midparentage and not to

* *Phil. Trans.* Vol. 187 A, p. 270, 1896.

† See *Biometrika*, Vol. II, p. 378, 1902.

‡ Galton in this paper introduces the term "deviate": "I shall call any particular deviation a 'deviate,'" *Journ. Anthrop. Instit.* Vol. XV, p. 252. The term was perhaps unnecessary considering the existence of "deviation," but it has come into general use, and is perhaps more justifiable in Galton's sense than "variate," which is now so often used, not for a particular variation, but for the "variable" itself.

§ *Journ. Anthrop. Instit.* Vol. XV, p. 258.

trouble ourselves about the parents separately. The statistical variations of stature are extremely regular, so much so that their general conformity with the results of calculations based on the abstract law of frequency of error is an accepted fact by anthropologists. I have made much use of the properties of that law in cross-testing my various conclusions and always with success*."

Galton considers the fact that stature is not a simple element, but a compound of the accumulated lengths or thicknesses of more than a hundred parts, to be a distinct advantage and a source of the beautiful regularity of its frequency distributions†. He does not see that this may tend to screen some fundamental law which may be obeyed by the simple components. Thus we note that as a rule the parental correlations decrease as we take characters based on fewer elements, e.g. the parental correlations for span are less than those for stature, and those for forearm are less than those for span. There might be—I on my part do not assert that there is—an alternate inheritance in the simple components, which is screened in the complex compound‡. To this Galton might well have replied: Why should a single bone be looked upon as an ultimate element, if it develops from a number of centres of ossification, and pushing the matter further may we not be driven to find the simple component ultimately in a cell? The "simple components," which obey some equally simple law of inheritance, are still to find in the bony skeleton of man.

Two further terms defined by Galton may here be considered.

He recognises that the individuals in what we now term an array (a column, or row) of the correlation table are not in themselves blood kindred, they are not, for example, all sons of the same parents, or all brothers of the same individual. Their link is that they are all sons of a set of parents having the same small range of any character, or again all brothers who have a brother within the same small range of character. Thus these individuals probably differ in both ancestry and nurture. Galton proposes to call them "co-kinsmen" or more definitely according to the array type "co-filials" or "co-fraternals." By such terms he only means that their correlated variable (e.g. stature in parent, brother or collateral) has the same value, or limited range of values. Galton was thus fully aware that the variability within a family group of brethren, a fraternity, was not the same as the variability within such an array or co-fraternity, or co-kinship. Galton's terms have not come into general use, it is, perhaps, awkward to call individuals co-kinsmen who are not kinsmen at all. But the failure to distinguish between a fraternity in the true sense, and a co-fraternity in Galton's sense, has not been unfruitful of error§. It is, perhaps, best to stick to the words "filial array" or "fraternal

* *Journ. Anthrop. Instit.* Vol. xv, p. 251. † *Ibid.* p. 249.

‡ Those who assert that stature or cephalic index "mendelises," have not explained how the bones on the dimensions of which they are formed themselves react to inheritance. If these simpler elements "mendelise," how comes it that the compounds do, and what becomes of the correlations between these components?

§ If r be the correlation coefficient of offspring on midparent and R be the multiple correlation coefficient of offspring on the whole of its ancestry, then, σ being the standard deviation of offspring, $\sigma\sqrt{1-r^2}$ is the variability of a co-fraternity and $\sigma\sqrt{1-R^2}$ the variability of a fraternity, or group of blood brothers.

array," the word array suggesting that we are dealing with a wider group than a single family.

The next idea raised by Galton is very important for later researches. He goes to the root of his law of regression when he states that the somatic character of the parents does not fully define the somatic character of the offspring. The somatic character of the parents is not the full representative measure of the germ plasm of the stirp. This is represented by a long series of ancestors, who become so numerous as we go backward, that their mean value for a generation cannot differ from mediocrity. Regression in Galton's view is the result of the influence of parental heredity pulling the offspring so to speak towards the parental value and the mediocrity of the more distant ancestry pulling towards its own value of the character.

Now we may or we may not know something of the ancestry behind the first midparent. If we know nothing absolutely then the fact that the first midparent has a certain character value enables us to predict a certain probable value for the next midparent and so on. If we did know completely the ancestry, we might replace the whole ancestry by a single midancestor. To this midancestor, we may give the name "generant." Again we had better cite Galton's own words, because although the idea is suggestive he does not define it in a manner which enables us to determine mathematically its nature. From what we said above it is clear that we may have a true generant and a probable generant based on only a partial knowledge of the ancestry*.

Galton's Conception of the Generant.

"The explanation of it [Regression] is as follows: The child inherits partly from his parents, partly from his ancestry. Speaking generally the further his genealogy goes back, the more numerous and varied will his ancestry become, until they cease to differ from any equally numerous sample taken at haphazard from the race at large. Their mean stature will then be the same as that of the race; in other words, it will be mediocre. Or, to put the same fact into another form, the most probable value of the midancestral deviates in any remote generation is zero.

"For the moment let us confine our attention to the remote ancestry and the midparentages, and ignore the intermediate generations. The combination of the zero of the ancestry with the deviate of the midparentage is the combination of nothing with something, and the result resembles that of pouring a uniform proportion of pure water into a vessel of wine. It dilutes the wine to a constant fraction of its original alcoholic strength, whatever the strength may have been.

"The intermediate generations will each in their degree do the same. The middeviate in any one of them will have a value intermediate between that of the midparentage and the zero value of the ancestry†. Its combination with the midparental deviate will be as if, not pure water, but a mixture of wine and water in some definite proportion, had been poured into the wine. The process throughout is one of proportionate dilutions, and therefore the joint effect of all of them is to weaken the original wine in a constant ratio.

"We have no word to express the form of that ideal and composite progenitor, whom the offspring of similar midparentages most nearly resemble, and from whose stature their own respective heights diverge evenly above and below. If he, she or it, is styled the "generant" of the group, then the law of regression makes it clear that parents are not identical with the generants of their own offspring."

* *Journ. Anthrop. Instit.* Vol. xv, pp. 252–3.

† This sentence is not, I think, correct as it stands. A man might easily have four grandparents all taller than his parents. Galton probably meant to insert the words "on the average."

If U for any character be the deviate of the generant from the mean of the race, then the individual endowed with such U's for all characters would represent the stirp of any family. Unfortunately Galton does not give us any method for determining the U of the generant. I think, however, if we take the character U of the generant to be that linear function of the characters of all the ancestry which gives the highest correlation R with the character in the offspring, it throws light on Galton's idea. In this case U is simply proportional to the multiple regression expression. If we make the following hypotheses, which have considerable experimental evidence in their favour, namely:

(*a*) that the individual correlations of offspring with male and with female ancestors are equal,

(*b*) that such correlations with individual ancestors die out in a geometrical ratio, i.e. the correlations of the offspring with individual parents (father or mother), with individual grandparent (male or female), with individual great-grandparent, etc. form a series r_1, $r_1\alpha$, $r_1\alpha^2$, etc., where α is less than unity, then it can be demonstrated that the deviate U will be given by the formula*

$$U = \gamma\,(h_1 + \beta h_2 + \beta^2 h_3 + \ldots),$$

where h_1, h_2, h_3, etc. are the deviates of the midparental characters in the successive grades of ancestry and γ, β are constants, which can be found in terms of r_1 and α. Further, the fraternity of which U defines the stirp will vary round U with variability $\sigma\sqrt{1-R^2}$, where R (the "coefficient of multiple correlation") is known in terms of r_1 and α, or of γ and β.

The expression for U, or the deviate of the generant which defines the stirp, has been termed the *Law of Ancestral Inheritance*†. It is not a biological hypothesis, but the mathematical expression of statistical variates, which obey, as many measurable characters in man, certain forms of frequency distribution, these being maintained in successive generations. It can be applied with special values of γ and β to many biological hypotheses. We are, however, not concerned to discuss these matters here, but merely to point out that in the papers we are now dealing with Galton was feeling his way upwards towards this Law of Ancestral Inheritance, though I venture to think by a faulty stairway. The somewhat complicated mathematics of multiple correlation with its repeated appeals to the geometrical notions of hyperspace remained a closed chamber to him, necessary as multiple correlation now is for many practical problems of modern statistics. As I have said there is a true generant, i.e. one in which we insert the true values of the different ancestral midparental deviates, namely h_1, h_2, h_3, ... as above, and a probable generant for which we only know h_1 and put in probable values

* *Biometrika*, Vol. VIII, pp. 239–243.

† *Roy. Soc. Proc.* Vol. LXII, p. 386. For the fuller mathematical treatment see *Biometrika*, Vol. VIII, pp. 239–240 and Vol. XVII, pp. 129 *et seq.*

based on h_1 for h_2, h_3, ..., etc. Galton deals only with the latter. He writes as follows*:

"When we say that the midparent contributes two-thirds of his peculiarity of height to the offspring, it is supposed that nothing is known about the previous ancestor. But though nothing is known, something is implied, and this must be eliminated before we can learn what the parental bequest, pure and simple, may amount to. Let the deviate of the midparent be x (including the sign), then the implied deviate of the midgrandparent will be $\frac{1}{3}x$, of the mid-ancestor in the next generation $\frac{1}{9}x$ and so on. Hence the sum of the deviates of all the midgenerations that constitute the heritage of the offspring is $x(1+\frac{1}{3}+\frac{1}{9}+\text{etc.}) = x\frac{3}{2}$.

Now I think this result erroneous because it assumes that the quantities $\gamma, \gamma\alpha, \gamma\alpha^2, \ldots$ of the generant above can be found from the simple regression formula of parent on offspring. This we know to be very far from the fact, the multiple regression coefficients have no such simple relations to parental regression. The fallacy lies, I think, in this: we could imply that value of the grandparental from the parental deviate by means of the simple regression formula, but to do this is to assert that all the remaining h's, h_3, h_4, etc., are put zero, i.e. are given every conceivable value, with the mean value zero. But we are going to imply other than zero values for these h's, hence our system of implied ancestral values is not consistent and this, I think, is indicated by the total heritage coming out $x\frac{3}{2}$. To get over this difficulty Galton proceeds "to tax" each contribution to the heritage. He takes as two extreme cases (*a*) a uniform taxation of all ancestral contributions of $\frac{1}{n}$, and (*b*) a taxation geometrical in amount, supposing $\frac{1}{m}$ of the total only to be transmitted from one generation to a second. He thus reaches the following expressions:

$$x\left(\frac{1}{n}+\frac{1}{3n}+\frac{1}{9n}+\text{ etc.}\ldots\right)=\frac{1}{n}\,x\,\frac{3}{2},$$

$$x\left(\frac{1}{m}+\frac{1}{3m^2}+\frac{1}{9m^3}+\text{ etc.}\ldots\right)=x\,\frac{3}{3m-1}.$$

But x being the midparental character the heritage of the offspring is, Galton says, $\frac{2}{3}x$, thus $\frac{1}{n}=\frac{4}{9}$, and $\frac{1}{m}=\frac{6}{11}$. From this he draws the conclusion that both may be taken to be $\frac{1}{2}$ approximately. But here the reasoning seems at fault, for the offspring heritage of $\frac{2}{3}x$ is based on all the other midparental deviates h_2, h_3, ... taking their average or zero values. The regression coefficient would not be two-thirds, if they took the values $\frac{1}{3}h_1$, $\frac{1}{9}h_1$, etc.

Finally from what Galton has just said it would appear that we might have two series for determining ancestral contributions, the one in n, i.e. $\frac{1}{2}$, $\frac{1}{2}$, $\frac{1}{2}$, ..., or the one in m, i.e. $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$, ... But this is clearly not what he

* *Roy. Soc. Proc.* Vol. LXII, p. 61, and compare *Journ. Anthrop. Instit.* Vol. XV, pp. 260 *et seq.*

understands, for having determined the midparental contribution to be $\frac{1}{2}$ from either series, he now writes* of the values of $\frac{1}{n}$ and $\frac{1}{m}$:

"These values differ but slightly from $\frac{1}{2}$, and their mean is closely $\frac{1}{2}$, so that we may fairly accept that result. Hence the influence, pure and simple, of the midparent may be taken as $\frac{1}{2}$, of the midgrandparent $\frac{1}{4}$, of the midgreatgrandparent $\frac{1}{8}$ and so on. That of the individual parent would be $\frac{1}{4}$, of the individual grandparent $\frac{1}{16}$, of an individual in the next generation $\frac{1}{64}$ and so on."

Thus Galton reaches his *Separate Contribution of each Ancestor to the Heritage of the Child*, a principle which is often spoken of as his Law of Ancestral Heredity. In reaching it he apparently drops his $\frac{1}{n}$ series altogether and follows his $\frac{1}{m}$ series with its geometrical system of taxation. This is distinctly more in keeping with the expression for the generant deviate U above, which runs in a geometrical series. If we assume all the ancestors to have the same deviation h, we have $U=\frac{\gamma}{1-\beta}h$, and, if the offspring value might in such a uniform breed be also taken as h, it follows that $\gamma=1-\beta$. Hence if we take the first midparents' contribution to be $\frac{1}{2}$, i.e. $\gamma=\frac{1}{2}$, with Galton, it follows that $\beta=\frac{1}{2}$, and our series is Galton's geometrical series with his radix value, a half. But I venture to think it was inspiration rather than correct reasoning which led him to a geometrical series for U.

On the other hand his multiple regression coefficients $\frac{1}{2}, \frac{1}{4}, \frac{1}{8}, \ldots$ suffice to determine what the correlations between an individual ancestor in any generation and the offspring *ought* to be. They take the values for parents $\cdot 3$, for grandparents $\frac{1}{2}\times\cdot 3$, for great-grandparents $\frac{1}{2^2}\times\cdot 3$ and so on. Galton found his midparental regression $\frac{2}{3}$ and took his parental to be $\frac{1}{3}$†. This is not so far from $\cdot 3$, that we could say it confutes Galton's Ancestral Law. But we find Galton taking the grandparental regression and therefore the correlation $\frac{1}{9}$, the great-grandparental $\frac{1}{27}$ and so on. These values form a series $a, a^2, a^3, \ldots$ for the individual ancestral correlations and lead to $\gamma=1$, $\beta=0$, or to the generant U being solely determined by the parents, the higher ancestry contributing nothing to the generant‡. Hence it follows that Galton's Ancestral Law is not in keeping with the values he has taken for his individual ancestral correlations. The reasoning by which he has reached one or the other is defective. As I have said Galton's guess at a geometrical relation for the coefficients of U was an inspiration, but his idea that a grandson is the son of a son and so his regression (and with a stable population his correlation) must be $\frac{1}{3}\times\frac{1}{3}=\frac{1}{9}$ is fallacious. Regression coefficients cannot be obtained from each other in this manner.

* *Roy. Soc. Proc.* Vol. LXII, p. 62.

† This will be equal to the correlation, for the variabilities of both variates are taken to be the same.

‡ See *Phil. Trans.* Vol. 187, A, p. 306, 1896.

Galton, by means of seeking the slope of the regression line, found the regression of brother on brother to be $\frac{2}{3}$ and this accordingly would be the fraternal correlation; he then said: a nephew is the son of a brother, *therefore* his regression on his uncle $= \frac{1}{3} \times \frac{2}{3} = \frac{2}{9}$. Again I do not believe that regressions can be built up in this manner. It appears to be multiplying together probabilities that are not independent, but correlated; for all a regression provides is a probable deviation, and we cannot apply independent probabilities to a correlated triplet. Why may not a brother be considered as the son of a midparent and so have regression $\frac{2}{3} \times \frac{2}{3} = \frac{4}{9}$ instead of Galton's observed value $\frac{2}{3}$? Why might we not equally well argue that a nephew is the grandson of a midparentage, which gave rise to his uncle and thus the nephew-uncle regression be $\frac{1}{3} \times \frac{2}{3} \times \frac{2}{3} = \frac{4}{27}$ instead of $\frac{2}{9}$? Why should cousins* be considered the offspring of two brothers $\frac{1}{3} \times \frac{2}{3} \times \frac{1}{3}$ rather than as the grandsons of one midparentage $\frac{1}{3} \times \frac{2}{3} \times \frac{2}{3} \times \frac{1}{3}$? Even if we are always to take the "shortest way round," no argument is given in favour of it, and it could only be satisfactorily demonstrated by actual data.

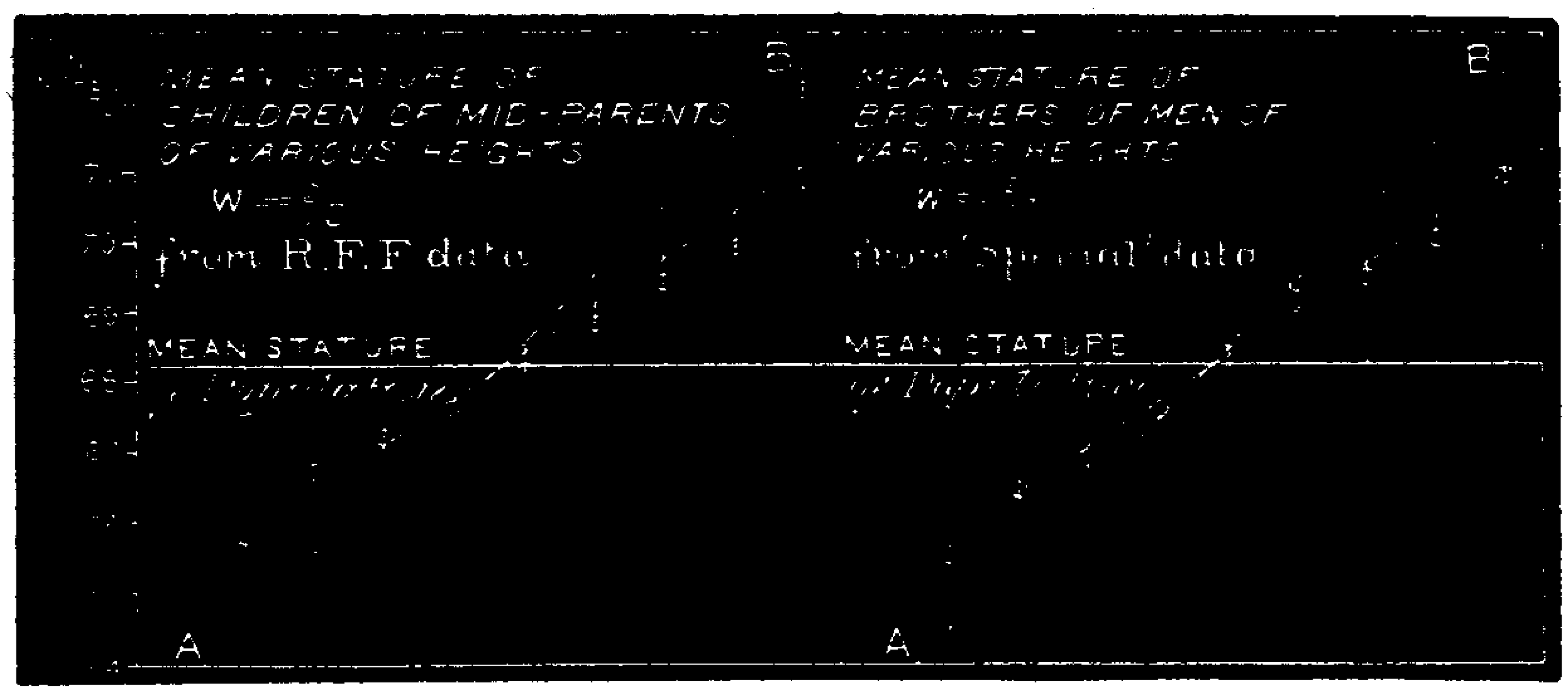


Fig. 6. Galton's Filial and Fraternal Regression Lines.

I do not think Galton's method of deducing the degrees of resemblance between kinsmen of various degrees of blood relationship from the single datum of the regression of a filial array on its midparent will pass muster; it is extraordinarily suggestive—no one had thought before of giving a quantitative measurement to the various types of kinship. Galton indicated how it could be done by aid of correlation tables and gave at this time two such tables†, those for midparent with offspring and for brother with brother. These are both from his *R. F. F.* (*Records of Family Faculties*), but he also provided another correlation table giving the distribution for a special series of pairs of brothers. In Fig. 6 will be found his regression lines for offspring on midparents, and for brother on brother. His method of reduction was, however, very different from any we should adopt to-day. When he wanted a mean he determined a median, and he did this by roughly proportioning (graphically) the total in the cell in which it lies, he worked not with the

* The value $\frac{1}{3} \times \frac{2}{3} \times \frac{1}{3} = \frac{2}{27}$ is given by Galton: *Natural Inheritance*, p. 133.

† If we include the earlier one for the seed-weights in mother and daughter plants for the case of sweet-peas (see our p. 4) we have here the four earliest correlation tables and regression lines ever published.

standard deviations, but the probable errors, and he determined these from the quartiles by rough proportioning as before. When he wanted a regression coefficient he plotted the medians of the arrays and fitted these with a straight line, presumably by testing with a straight edge. The slope of this straight line is Galton's regression coefficient. If we assume the standard deviation of the two marginal columns to be the same, then this regression coefficient is the coefficient of correlation, but that term was not used by Galton in the group of memoirs at present under discussion.

It will be remembered that Galton transmuted all his females to their male equivalent, and then found his regression for offspring on midparent to be $\frac{2}{3}$ and therefore on a single parent to be $\frac{1}{3} = \cdot 3333$. Reworking the whole material ten years later I found the mean of the four possible parental correlations to be $\cdot 3355$*, in singular accordance with his rougher methods, which, however, had largely screened the significant inequalities of the parental correlations in his case.

Turning to Galton's data for brothers I note that he nowhere tells us how he gets his regression coefficient of $\frac{2}{3}$. In the *R. S. Proc.* paper (*loc. cit.* p. 55) there is a small graph for the "Special" data for brothers, none for the *R. F. F.* data for brothers. The slope of the regression line Galton has run through the array medians is, as near as I can judge it, 34°, or the regression would be $\cdot 6745$, which Galton would call $\frac{2}{3}$. In the *Natural Inheritance*, p. 109, there are small charts for the regression lines of both the *R. F. F.* and the "Special" data, the former (which does not go truly through the mean) has an angle of 24° giving a correlation of $\cdot 4452$ and the latter an angle of about 33°, or a slope of $\cdot 6494$. Actually forming tables myself on Galton's data I found for the *R. F. F.* Regression of Brother on Brother $\cdot 4547$, and for the "Special" data $\cdot 5990$, not so violently diverse from Galton's results, when we consider the difference of methods, and personal equation in selecting pairs of brothers for tabular entry. But there is a point in which I find it needful to differ from Galton in the value of his material. I believe that the "Special" data were really heterogeneous; they contained pairs of brothers measured in an Essex volunteer regiment, who taken alone gave a regression of no less than $\cdot 7175$, while the remainder had only a value of $\cdot 5574$. I am inclined to think therefore that we need to throw out the volunteers, and if we do so the mean of Galton's two results, $\frac{1}{2}(\cdot 4452 + \cdot 5574) = \cdot 5013$, is very close to the mean value $\cdot 50$ which has since been found on more satisfactory and ampler data for a variety of characters in man. I doubt whether it is possible to accept Galton's original estimate of $\frac{2}{3}$ for fraternal regression and correlation, and believe that he may have been led to select the higher value of the two he had obtained by an idea that fraternal should equal midparental regression.

Anyhow in these numbers we find the first attempt to obtain a numerical measure of the degree of resemblance in brothers, just as in another part of the paper he has provided us with the first measure of filial resemblance. Galton knew quite well that his values were not final, but here, as so often, he blazed the track for others to build a highway.

* See our p. 18.

There is another suggestion in the Royal Society paper which has ultimately been followed up to great profit, namely that the variability within the family could be ascertained by considering the difference in the same character of pairs of brothers. Let R be the multiple correlation coefficient of an individual on all his ancestors or his correlation with his "generant," then since two brothers have the same ancestry the variability in a family of brothers is $\sigma\sqrt{1-R^2}$, where σ is the standard deviation of brothers. Now if x_1 and x_2 be the characters in a pair of brothers, for example their statures, we have $\frac{1}{2}(x_1+x_2)$ for their mean and $\frac{1}{4}(x_1-x_2)^2$ for their standard deviation squared, or so-called variance. If this be taken for a large number of pairs, then it may be shown that

$$\text{Mean variance for pairs of brothers} = \tfrac{1}{2}\sigma^2(1-r) = \tfrac{1}{2}\sigma^2(1-R^2),$$

where r is the simple correlation of brothers*.

These results have really been given as early as 1886 by Galton. He does not use R, and instead of standard deviations, speaks of quartile values, i.e. probable errors. He writes b for our $\cdot 67449\,\sigma\sqrt{1-R^2}$, p for our $\cdot 67449\,\sigma$, and our r is his regression of brother on brother or his w. Thus in his symbols:

$$\text{Mean (probable error)}^2 \text{ of pairs of brothers} = \tfrac{1}{2}p^2\,(1-w) = \tfrac{1}{2}b^2.$$

These results are given on pp. 58–59 of the *R. S. Proceedings*' memoir, and demonstrated by methods which appeal only to the most elementary conceptions. When we come to actual numerical values, Galton finds a series of values for b (the probable deviation in a group of brothers) which ranges from $0''\cdot 98$ to $1''\cdot 38$—a result which might be anticipated from the rather heterogeneous nature of his material. If for the reasons already stated we do not trust to the "Special" data only, but use also the *R. F. F.* results, the mean value (Table, p. 59) found by the various processes for b is $1''\cdot 179$. For p I find from Galton's table on his p. 69, $1''\cdot 684$, and thus deduce for R the value $\cdot 7140$, comparing not badly with the value $\cdot 7284$ obtained recently for brothers from probably better data†. Clearly with these values for p and b that for w, the regression of brother on brother or the correlation of brothers, is $\cdot 5096$ and not $\frac{2}{3} = \cdot 6667$ as Galton assumed it, trusting to his "Special" data; this is a result agreeing far better with later determinations of fraternal heredity‡.

The whole paper is a most remarkable one, not only for the wealth of new ideas it contains, but for the insight it shows Galton had into many problems which have only been recently, or are only at present, under

* *Biometrika*, Vol. XVII, pp. 130–1.

† *Ibid.* p. 138.

‡ A further point worth recording occurs on p. 58 of the *R. S. Proc.* memoir. Suppose samples of size n are taken from a normal distribution. Then the mean square standard deviation of these samples, $\bar{\mu}_2$, is given in terms of the standard deviation squared, Σ^2, of the sampled population, by $\bar{\mu}_2 = \dfrac{n-1}{n}\Sigma^2$. Galton puts this in probable deviation form as $d^2 = \dfrac{n-1}{n}b^2$ and putting $n = 4, 5, 6, 7$ applies it to find b^2 from mean square probable deviation (in his terminology the quartile) of brothers in families of different sizes. Thus anticipating more recent work on small samples.

discussion. He perceived for the first time that the problem of multiple correlation when solved would give the closest prediction possible to the probable value of the character in an individual from known characters in the kinsfolk, but he also recognised that long selection could not indefinitely reduce variability, that 30 % reduction in variability was about as much as could be hoped for (i.e. p to b in his notation).

"The possible problems are obviously very various and complicated, I do not propose to speak further about them now. It is some consolation to know that in the commoner questions of hereditary interest, the genealogy is fully known for two generations, and that the average influence of the preceding ones is small.

"In conclusion it must be borne in mind that I have spoken throughout of heredity in respect to a quality that blends freely in inheritance. I reserve for a future inquiry (as yet incomplete) the inheritance of a quality that refuses to blend freely, namely the colour of the eyes. These may be looked upon as extreme cases, between which all ordinary phenomena of heredity lie*."

These words show that Galton fully recognised that his theory applied only to continuously varying and blending characters.

The paper in the Anthropological Journal *Miscellanea,* while less replete with ideas requiring mathematical interpretation than that in the *R. S. Proceedings,* contains two matters which deserve notice. Over and over again we meet with the statement that more able men are born from undistinguished parents than from parents of marked ability. In the year 1927 it formed the subject of a series of controversial letters in *The Times* newspaper, in which neither side seemed to have any statistical ammunition, nor appeared to be aware that they were dealing with a forty year old paradox, which Galton had refuted in 1885:

"Let it not be supposed for a moment that any of these statements invalidate the general doctrine that the children of a gifted pair are much more likely to be gifted than the children of a mediocre pair. What they assert is that the ablest child of one gifted pair is not as likely to be as able as the ablest of all the children of very many mediocre pairs†."

In 1900‡ the biographer gave exact numbers for the production of ability on the assumption that one man in twenty may be treated as "able." It turned out that in 10,000 matings the 52 pairs of exceptional parents produced 26 exceptional sons, while the 9948 non-exceptional pairs produced 474 exceptional sons, thus the rate of production of exceptional sons by exceptional parents was 10 times greater than the rate by non-exceptional parents, but the latter produced more than 18 times as many exceptional sons as the former. The result flows merely from the fact that a rate of 10 times the production in the case of exceptional parents is counteracted in *total* output, by the fact that there are some 200 times more non-exceptional than exceptional pairs of parents. It is distressing to note how such distinguished scientists as Dr Leonard Hill are unable to grasp the interpretation of this simple statistical paradox first provided by Galton in 1885!

The second point is the publication of a diagram illustrating the variability of a stable population in the parental generation, for the midparentages, for the generants, and for the filial generation. The diagram (see our p. 28) is

* *R. S. Proc.* Vol. LXII, pp. 62–63.
† *Journ. Anthrop. Instit.* Vol. XV, p. 254.
‡ *Phil. Trans.* Vol. 195, A, p. 47.

Process through which the Distribution of Statures, in Successive Generations of the same People, remains unchanged.

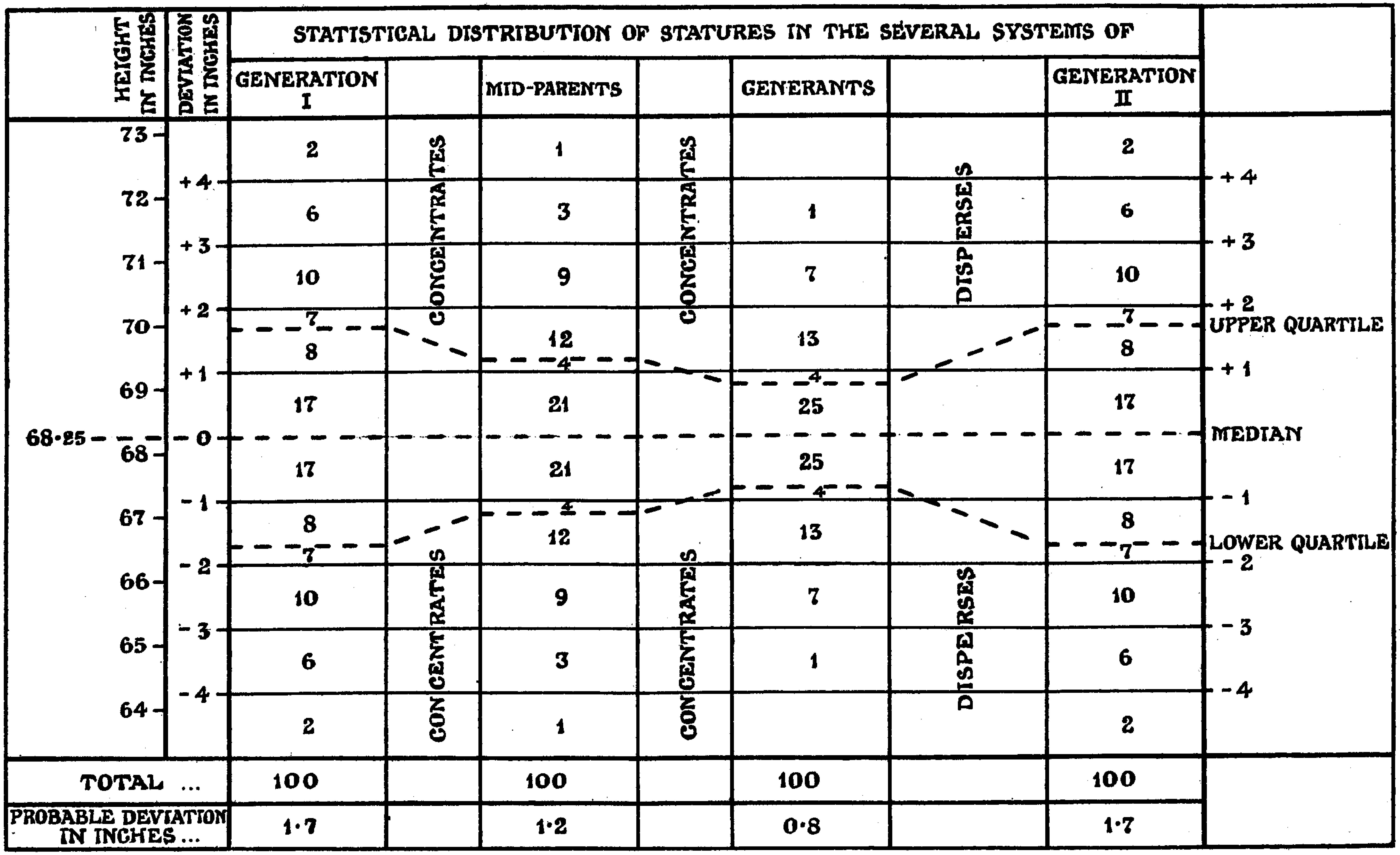


Fig. 7. Galton's Diagram showing how a stable Population reproduces itself.

not described at length*, and I have ventured to modify it in one or two directions, which I believe will make it somewhat clearer. The main difficulty I have is to interpret what Galton meant by the column headed "Generants." If he meant by "Generant" the hypothetical individual that I have represented by U above, a sort of "midancestor," replacing the whole stirp, then I think the variability of this midancestor should be given by $\sigma_1\sqrt{1-R^2}$, where σ_1 is the standard deviation of the population for the given character. For stature, using not σ_1 but the quartile, this would be Galton's b or $1''\cdot179$, or the value which Galton selects for b, i.e. $1''\cdot06$. In his diagram, we have under the "Generants" column "Probable deviation" $0''\cdot8$; this number does not occur, as far as I can see, anywhere else in the paper. One solution I can suggest is that Galton was thinking of the variability of pairs of his new population; in this the variability of these paired generants would be $b/\sqrt{2}$, and $\cdot8\times\sqrt{2}=1\cdot131$, almost the mean between the above values of b. Another explanation may be that Galton had not reached the comprehensive idea of the single midancestor, which I have defined by the "generant" above, but that his generant depended solely on the midparent and was to be taken as an individual with $\frac{2}{3}$ of the character of the midparentage. In this case the variability of the generant group would be $\frac{2}{3}$ of that of the midparental group, i.e. $\frac{2}{3}(1''\cdot2)=0''\cdot8$. If this be true the generant would be only a hypothetical individual who produced offspring varying about his own, and not about a regressed type. I trust this latter solution may be erroneous, as I should like Galton to have conceived the idea of a single individual—not one depending only on the parents—who would represent the whole stirp or ancestry. At any rate let us preserve in future the good word "generant" for the hypothetical individual who possesses, in the manner indicated by the function U, all the midaneestral characters which are capable of showing a blending inheritance. Such a generant is a sort of mean man for the stirp, who for statistical purposes represents the whole ancestry. If Galton had not this idea, he provided at least the origin from which it sprung! If his generants are the receded midparents, let us ourselves use generants for the midancestry, who will not of necessity involve regression at all.

Of the Birmingham Lecture on "Chance and its Bearing on Heredity" little need be said, it only adds to what we have already discussed, emphasis on the point that in a stable population the whole inheritance of any blending character depends on the knowledge of three constants: (i) the mean character in any generation, (ii) the corresponding variability, and (iii) a single hereditary correlation.

Galton gave a further account of his researches on regression in stature

* Galton is explaining how the new generation is a reproduction of the old and writes: "the process comprises two opposite sets of actions, one concentrative and the other dispersive, and of such a character that they neutralise one another and fall into a state of stable equilibrium (see Diagram [on our p. 28]). By the first set, a system of scattered elements is replaced by another system which is less scattered; by the second set, each of these new elements becomes a centre whence a third system of elements is dispersed" (*loc. cit.* p. 256). This is the only reference to the diagram or its interpretation I have noticed.

in his Presidential Address to the Anthropological Institute on January 26, 1886*. One or two points from this address may be noted. On pp. 491–3 he describes the working model which he exhibited to indicate how the *probable* stature of any man could be ascertained from that of a kinsman in any degree. Since the regression is constant all we have to do is to make use of the property of similar triangles. AB is a scale of stature, where M is the mean stature of the population. S' is any particular stature, O a point on the horizontal through M, so that $OM = 10$ units, then if $Om = 10r$, where r is the correlation of the particular grade of kinship, a string from O to S' will cut a vertical line through m in a point S, such that the point S gives the probable stature of the kinsman of the grade r of correlation. Galton put on a number of lines to determine probable stature in sons, nephews, grandsons, etc. He also constructed scales based on the standard deviation $(\sigma\sqrt{1-r^2})$ showing the percentile distribution for each grade of kinship. These scales could be shifted up and down on their respective lines ab, so that the probability could be measured of any deviation from the probable stature S. As Galton's numerical values for the regressions were somewhat doubtful, I constructed at his suggestion some ten years later a life-size "Geniometer" on this plan with the revised values we had then determined for the hereditary correlations. It is reproduced on Plate I. The original figures which are in brilliant colours† gave Galton and I hope my audience some amusement.

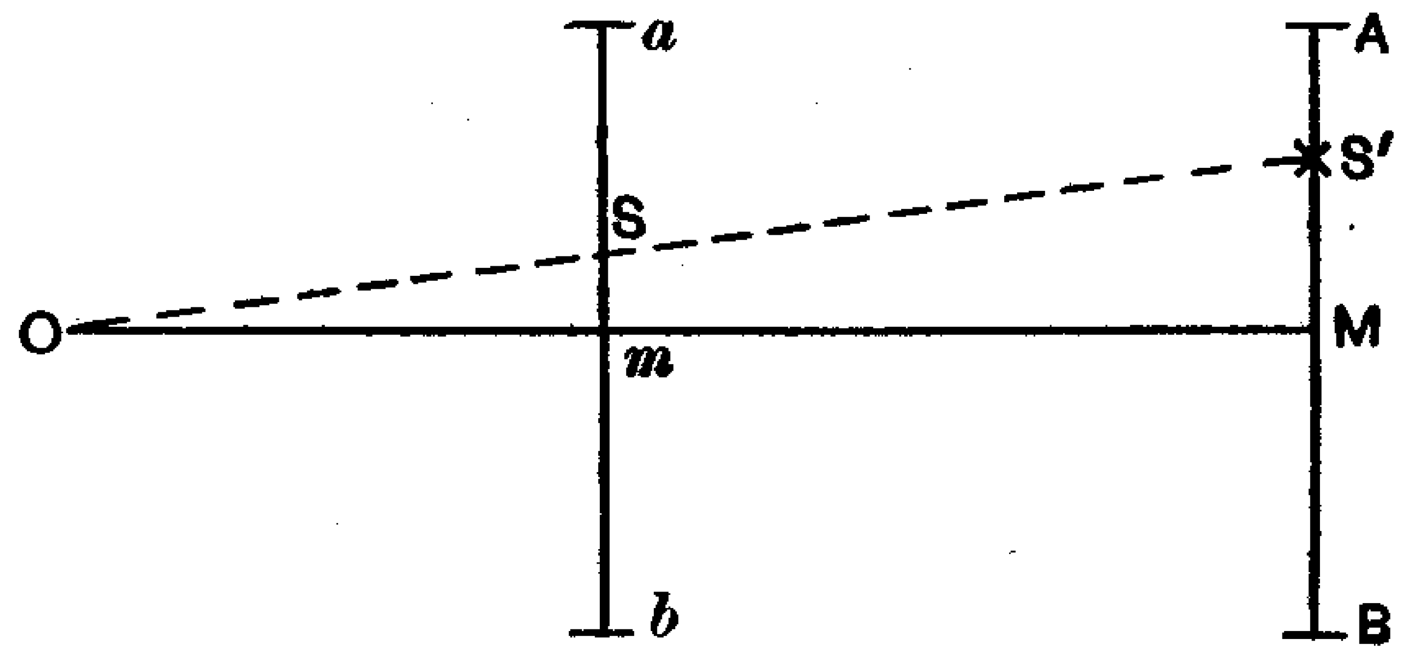


Fig. 8.

In a presidential address of this kind, it is legitimate to let one's thoughts run freely, there is no need sternly to demonstrate each step as may be thought fitting in a Royal Society paper. Accordingly Galton "let himself go." Some quotations will illustrate for the reader what opinions were forming in his mind, they are not demonstrated judgments—it is doubtful if some are demonstrable at all.

(i) *On the Normal Distribution or Law of Error* (pp. 494–5).

"I know of scarcely anything so apt to impress the imagination as the wonderful form of cosmic order expressed by the 'law of error.' A savage, if he could understand it, would worship it as a god. It reigns with severity in complete self-effacement amidst the wildest confusion. The huger the mob and the greater the anarchy the more perfect is its sway. Let a large sample of chaotic elements be taken and marshalled in order of their magnitudes, and then, however wildly irregular they appeared, an unexpected and most beautiful form of regularity proves to have been present all along. Arrange statures side by side in order of their magnitudes, and the tops of the marshalled row will form a beautifully flowing curve of invariable proportions; each man will find, as it were, a pre-ordained niche, just of the right height to fit him, and if the class-places and statures of any two men in the row are known, the stature that will be found at every other class-place, except toward the extreme ends, can be predicted with much precision."

* *Journ. Anthrop. Instit.* Vol. xv, pp. 487–499, 1886.

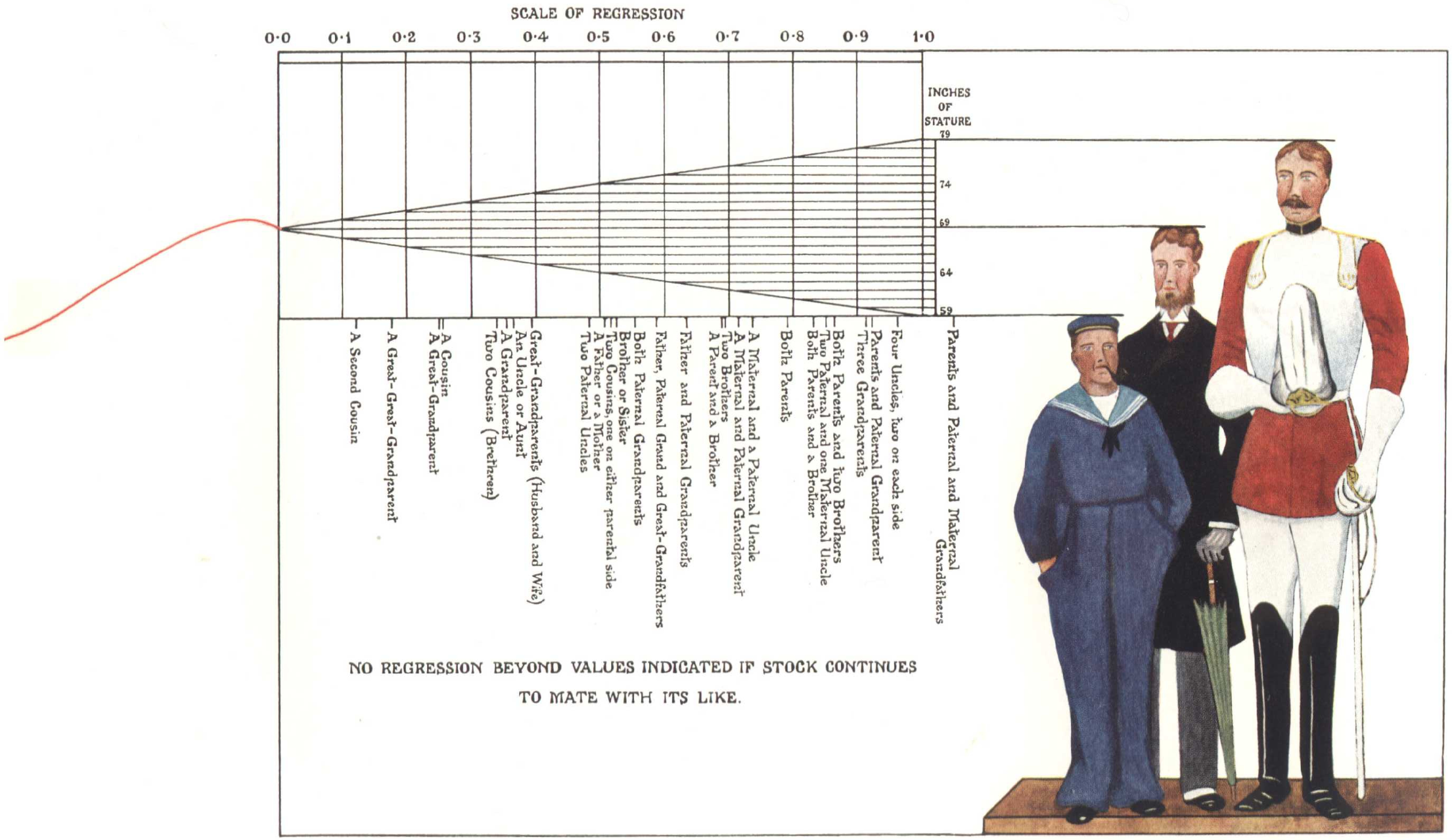
† The actual artist, who was then a member of my staff, is now a distinguished man of science, a grave and learned professor, and might not be too pleased if I gave his name away!

Take the red thread through any value on the scale of stature, say 74", then the average stature of persons having all the kinsfolk described below of that stature would be obtained by drawing a vertical line through the mark indicated by the kinsfolk till it meets the red thread, and carrying through this meeting point a horizontal line back to the scale of stature, which provides the average desired. For example the average nephew of *two* uncles, one paternal and one maternal, each 6 ft. 2 in. would be 6 ft. 0·6 in., but the average nephew of *four* uncles, two on each side, each 6 ft. 2 in. will be 6 ft. 1·8 in.* Again if both parents and paternal and maternal grandparents were of this stature, the grandsons would have progressed and be on the average 6 ft. 3·1 in. The statures are recorded for males, the corresponding female statures may be obtained by subtracting $\frac{8}{100}$ths from the male statures. In starting with females the male stature equivalent to that of the female must first be obtained by adding $\frac{2}{23}$rds of its value to the female stature. Thus a woman of 5 ft. 9 in. counts as a man of 6 ft. 3 in.

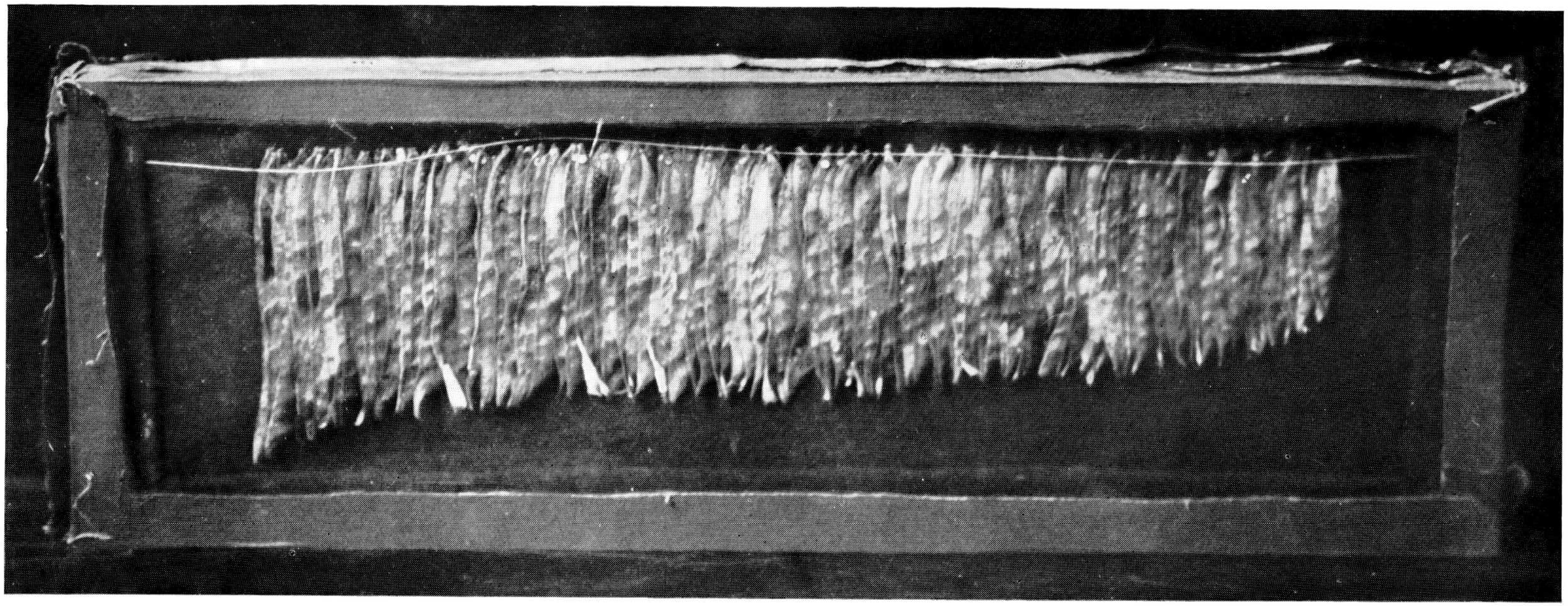
The reduction from my life-size diagram to the present small dimensions costs much in accuracy of reading, but serves to bring out the point that the regression ultimately changes to a progression.

* The regression coefficient used on the genometer was ·9614.

GENOMETER AFTER AN IDEA OF SIR FRANCIS GALTON



NO REGRESSION BEYOND VALUES INDICATED IF STOCK CONTINUES TO MATE WITH ITS LIKE.



Galton's "Ogive Curve" as exhibited by a marshalled series of Bean Pods. Unfortunately in the many years since Galton built up this illustration several tips have been broken off and in other cases some of the pods have burst open and the shell has curled round.

Galton was wont to illustrate the beauty of the "pre-ordained niche" on a marshalled series of bean pods which he had many years before prepared. This series is reproduced on Plate II. Unfortunately the tips of some of the pods have bent back, but the general scheme survives.

(ii) *The Phenomenon of Regression, a great Hindrance to the Establishment of Breeds* (pp. 495–6).

"It will be seen from the large values of the ratios of regression how speedily all peculiarities that are possessed by any single individual to an exceptional extent, and which blend freely together with those of his or her spouse, tend to disappear. A breed of exceptional animals, rigorously selected, and carefully isolated from admixture with others of the same race, would become shattered by even a brief period of opportunity to marry freely. It is only those breeds that blend imperfectly with others and especially such of these as are at the same time prepotent, in the sense of being more frequently transmitted than their competitors, that seem to have a chance of maintaining themselves when marriages are not rigorously controlled—as indeed they never are, except by professional breeders. It is on these grounds that I hail the appearance of any new and valuable type as a fortunate and most necessary occurrence in the forward progress of evolution."

Galton admits that the precise manner in which a new type comes into existence is unknown, but suggests that a multitude of petty causes may contribute to reshape the grouping of the germinal elements and so lead to a new and fairly stable position of equilibrium, which admits of hereditary transmission. In favour of this view he cites the frequent experience of "sports," useful, harmful and indifferent and therefore without teleological intent. These, he considers, have various degrees of heritable stability, and form fresh centres towards which some at least of the offspring have a tendency to revert. He considers that such sports, by refusing to blend freely, may be transmitted almost in their entirety.

"On the other hand, if the peculiarity blends easily, and if it was exceptional in magnitude, the chance of inheriting it to its full extent would be extremely small...*. I feel the greatest difficulty in accounting for the establishment of a new breed in a state of freedom by slight and uncertain selective influences, unless there has been one or more abrupt changes of type, many of them perhaps very small, but leading firmly step by step, though it may be along a devious track, to the new form."

* Galton gives in a footnote the percentage of sons who are as tall or taller than their fathers. I have recalculated this table on somewhat better data than Galton had available (*Biometrika*, Vol. II, p. 381). It now runs as follows:

Father's Stature	Probable Stature of Son	Percentage of Sons taller than Father	Father's Stature	Probable Stature of Son	Percentage of Sons taller than Father	Father's Stature	Probable Stature of Son	Percentage of Sons taller than Father
67″·5	68″·56	67·4 °/₀	72″·0	70″·88	31·6 °/₀	77″·0	73″·46	6·4 °/₀
68″·0	68″·82	63·7 °/₀	73″·0	71″·40	24·5 °/₀	78″·0	73″·98	4·2 °/₀
69″·0	69″·33	55·6 °/₀	74″·0	71″·91	18·4 °/₀	79″·0	74″·49	2·6 °/₀
70″·0	69″·85	44·0 °/₀	75″·0	72″·43	13·4 °/₀	80″·0	75″·01	1·6 °/₀
71″·0	70″·37	39·4 °/₀	76″·0	72″·95	9·5 °/₀	81″·0	75″·52	0·9 °/₀

The considerable changes from Galton's percentages arise from the facts: (i) that the sons in our data had a mean stature 1″ greater than their father's, (ii) that our regression was ·516 against Galton's ·333.

Whatever we may think of Galton's arguments, it is clear that in 1886 he did not believe in the influence of natural selection as producing new forms by acting on continuously varying small deviations. This may have been due to the influence which the idea of *perpetual* regression * had upon his mind. Whatever its source, Galton was in 1886 and later a firm believer, as the above passage indicates, in evolution by mutation. He was a mutationist before De Vries published his first paper on mutations (1900).

(iii) *On the Inheritance of Ability and its Application to the Upper House of Legislature* (pp. 497–9).

Galton inquires how far the results for heredity in stature may be applied to heredity in ability. He holds that considerable differences have to be taken into account, and he classifies them under three heads:

"*Firstly*, after making large allowances for the occasional glaring cases of inferiority on the part of the wife to her eminent husband, I adhere to the view I expressed long since as the result of much inquiry, historical and otherwise †, that able men select those women for their wives who are not mediocre women, and still less inferior women, but those who are decidedly above mediocrity. Therefore, so far as this point is concerned, the average regression in the son of an able man would be less than one-third."

On better data ‡ than Galton had at his command the regression of son's stature on father's stature is about ·52 instead of ·33, and, allowing for assertative mating, about ·82 on the midparent instead of Galton's ·67. When we introduce the grandparents the regression is not large. I think these points will explain Galton's difficulty as to ability without resort to the theory that extreme ability does not blend, which he suggests in his second statement:

"*Secondly*, very gifted men are usually of marked individuality, and consequently of a special type. Whenever this type is a stable one, it does not blend easily, but is transmitted almost unchanged, so that specimens of very distinct intellectual heredity frequently occur."

Unfortunately Galton gives no illustrations, and without statistical evidence it is difficult to interpret his meaning.

"*Thirdly*, there is the fact that men who leave their mark on the world are very often those who, being gifted and full of nervous power, are at the same time haunted and driven by a dominant idea, and are therefore within a measurable distance of insanity. This weakness will probably betray itself occasionally in disadvantageous forms among their descendants. Some of these will be eccentric, others feeble-minded, others nervous, and some may be downright lunatics."

The same point has been made frequently since Galton's day, but although isolated cases can of course be cited, the statement demands statistical demonstration. We require to know first whether the men "who leave their

* The theory of multiple regression shows us that if an individual mates with his like, he may regress on exceptional parents, but his offspring will not regress on him, nor further descendants either. A breed may be established if we select only parents and grandparents; the regression is thus of minor importance compared with the homogamy.

† See Vol. II, p. 105. ‡ *Biometrika*, Vol. II, p. 381.

mark on the world" are really always the able men, and if so, how many of them are "driven by a dominant idea." Again having defined this class, do statistics indicate that their offspring more often suffer from some form of nervous breakdown than the sons of men of lesser ability? Ryk ud med dine tal, bygmester! Talene på bordet!

I think Galton did not really believe that ability was inherited in a manner widely different from stature, for he now proceeds to suggest how a fitting House of Peers might be based on the knowledge gained by his inquiry. He supposes that in some new country it is desired to institute an Upper House of life-peers which shall be largely governed by the hereditary principle.

"The principle of insuring this being that (say) two-thirds of the members shall be elected out of a class who possess specified hereditary qualifications, the question is: What reasonable plan can be suggested of determining what those qualifications should be?

"In framing an answer we have to keep the following principles steadily in view: (1) The hereditary qualifications derived from a single ancestor should not be transmitted to an indefinite succession of generations, but should lapse after, say, the grandchildren. (2) All sons and daughters should be considered as standing on an equal footing as regards the transmission of hereditary qualifications. (3) It is not only the sons and grandsons of ennobled persons who should be deemed to have hereditary qualifications, but also their brothers and sisters, and the children of these. (4) Men who earn distinction of a high but subordinate rank to that of the nobility, and whose wives had hereditary qualifications, should transmit these qualifications to their children. I calculate roughly and very doubtfully, because many things have to be considered, that there would be about twelve times as many persons hereditarily qualified to be candidates for election as there would be seats to fill. A considerable proportion of these would be nephews, whom I should be very sorry to omit, as they are twice as near in kinship as grandsons*. One in twelve seems a reasonably severe election, quite enough to draft off the eccentric and incompetent, and not too severe to discourage the ambition of the rest. I have not the slightest doubt that such a selection out of a class of men who would be so rich in hereditary gifts of ability, would produce a senate at least as highly gifted by nature as could be derived by ordinary parliamentary election from the whole of the rest of the nation. They would be reared in family traditions of high public services. Their ambitions, shaped by the conditions under which hereditary qualifications could be secured, would be such as to encourage alliances with the gifted classes. They would be widely and closely connected with the people, and they would to all appearance—but who can speak with certainty of the effects of any paper constitution?—form a vigorous and effective aristocracy." (pp. 498–9.)

Galton does not state how he would start his Upper House *ab initio*, nor take into account the possible need of recruiting its stock from outside ability. His scheme would certainly introduce improved and better planned marriages among the peers, as they would be anxious to preserve the peerages within their own families. Here as elsewhere† he points out to our hereditary peers how little justification there is for their position, while at the same time he indicates that there is a basis in heredity for a really effective aristocracy. Such doctrines would scarcely appeal even now to either Tory or Democrat. Among the many proposals put forward for reforming the British House of Lords, none has endeavoured like Galton's to place it on a

* I think this is incorrect for reasons stated above (see pp. 22 and 24). The observed correlations between a man and his grandson and a man and his nephew are about equal.

† See our Vol. II, p. 93.

scientific basis by suggesting that the hereditary honour should follow ability in the stock and not be granted to a preordained individual.

D. *Attempt to demonstrate the Law of Ancestral Heredity on Eye-Colour.* In 1886 Galton published in the *Proceedings** of the Royal Society a paper on "Family Likeness in Eye-Colour." The only earlier paper I know which deals with this topic is that by Alphonse de Candolle†. That paper has no adequate statistical treatment, and suffers from two fundamental errors. The material was collected not only from Switzerland with its mixed races, but from Sweden, Germany and France, so that beyond the immediate parents, there must have been great differences in the eye-colours of the unrecorded earlier ancestry, and secondly the contributors were especially requested to leave out offspring of "doubtful" eye-colour, and also those of definite eye-colour whose parents had doubtful eye-colour. I do not think that in de Candolle's paper any results of real scientific value are reached. Galton's method of approaching the problem is entirely different. He starts from his Law of Ancestral Heredity, and endeavours to apply it to eye-colour, which he says does not usually blend. Accordingly he proportions the ancestral contributions not in the character of the individual but among the whole group of offspring. As Galton believed he had deduced from his mid-parental regression of $\frac{2}{3}$ the system $\frac{1}{2}+\frac{1}{4}+\frac{1}{8}+\ldots$ for contributions to the individual character in the case of stature, so he now supposes that an individual parent's eye-colour will determine on the average that of $\frac{1}{4}$ of the offspring, that of a grandparent $\frac{1}{16}$ of the offspring, and so on.

"Stature and eye-colour are not only different as qualities, but they are more contrasted in hereditary behaviour than perhaps any other simple qualities. Speaking broadly parents of different statures transmit a blended heritage to their children, but parents of different eye-colours transmit an alternative heritage. If one parent is as much taller than the average of his or her sex as the other parent is shorter, the statures of their children will be distributed in much the same way as those of parents who were both of medium height. But if one parent has a light eye-colour and the other a dark eye-colour, the children will be partly light and partly dark, and not medium eye-coloured like the children of medium eye-coloured parents. The blending of stature is due to its being the aggregate of the quasi-independent inheritances of many separate parts, while eye-colour appears to be much less various in its origin. If then it can be shown, as I shall be able to do, that notwithstanding this two-fold difference between the qualities of stature and eye-colour, the shares of hereditary contribution from the various ancestors are in each case alike, we may with some confidence expect that the law by which these hereditary contributions are governed will be widely, and perhaps universally applicable‡."

Galton starts his paper by considering whether there has been a secular change in eye-colour in the four generations to which his *Records of Family Faculties* extended. He started with those who ranked as "children" in the pedigree as Generation I; their parents, uncles and aunts were Generation II; the grandparents and their collaterals were Generation III, while the great grandparents and their collaterals were Generation IV. He gives the

* Vol. XL, pp. 402–416. Read May 27, 1886.

† "Hérédité de la couleur des yeux dans l'espèce humaine." *Archives des Sciences physiques et naturelles*, 3ième Période, T. XII, pp. 97–120, Geneva, 1884.

‡ *Roy. Soc. Proc.* pp. 402–3.

accompanying chart for the percentages of these eye-colours in the various generations, and concludes that there has been in these four generations

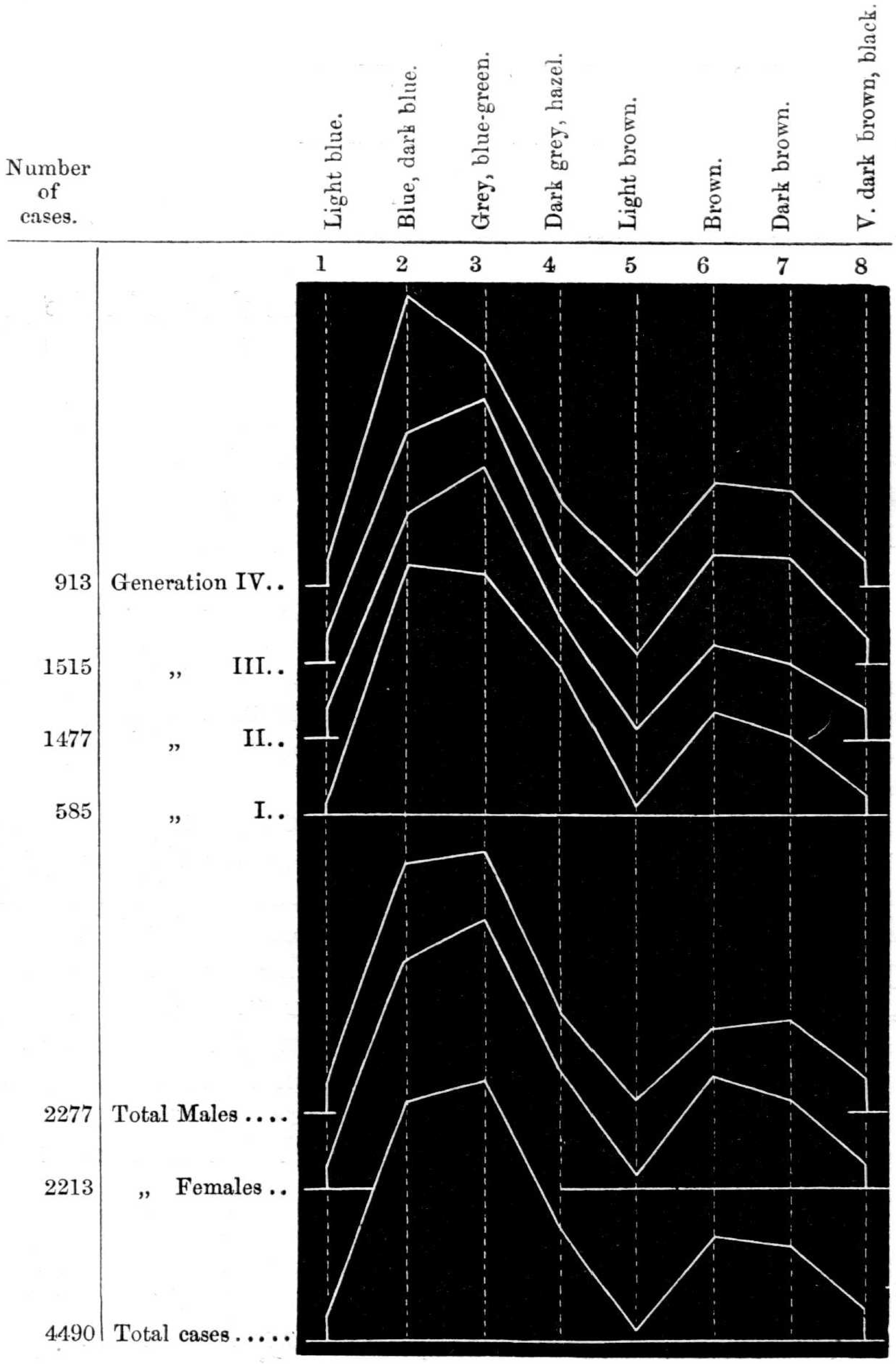


Fig. 9. Percentages of Eye-Colour in Successive Generations.

little secular change in eye-colour. It should, I think, be noted that the Generations III and IV are likely to be much older than Generations I and II when their eye-colours were recorded. Galton's data give the following percentage values on considerable numbers in the groups combined of Light and Dark Blue, Grey, Blue Green:

Generation	Male				Female			
	I	II	III	IV	I	II	III	IV
Percentages .. Probable Errors	58·2 ± 1·92	58·4 ± 1·22	62·9 ± 1·33	70·4 ± 1·43	58·0 ± 1·97	58·1 ± 1·23	58·6 ± 1·22	56·2 ± 1·58

It will be seen that, while there is no significant change in the percentage of light eyes in the women, there is really such a change in the light eyes of the men; the grandparental and great grandparental generations have more bluish eyes. Were it not for the fact that there is no change in the women, we might attribute this not to a racial change going on, but to men's eyes growing lighter with extreme age. I have no statistical data to produce, but my impression of the marked frequency of very light colour in old men's eyes is strong. At the same time I know no physiological reason why men's and not women's eyes should grow lighter with greater age.

On the basis of his diagrams Galton considers that he may disregard "a current popular belief in the existence of a gradual darkening of the population, and can treat the eye-colours of those classes of the English race who have contributed to the records, as statistically persistent during the period under discussion" (p. 406).

Galton next states that he considers that there are only two fundamental types of eye-colour, the light and the dark, but under this supposition the medium tints are troublesome. Such tints he has classified under "Dark Grey and Hazel." In these cases the outer portion of the iris is usually of a dark grey colour, and the inner of a hazel. The proportions of grey and hazel vary, and the eye is called "dark grey" or "hazel" according to the colour which happens most to arrest the attention of the observer. Galton's attempt to deal with these medium eyes, of which there are in the population about 12·7 °/₀, is to me unconvincing; yet the fact that he recognises their existence is more satisfactory than the Mendelian treatment which disregards them entirely!

Galton for conciseness terms all these eyes "hazel." He defines a hazel-eyed family to be one in which there is at least one hazel-eyed child, and he proceeds to inquire into the constitution and ancestry of such "hazel-eyed" families or sibships. He obtains the results tabulated on p. 37.

Now it is clear from the table that when there is a hazel-eyed child in a sibship, the percentage of dark eyes in the sibship is only very slightly reduced, but the number of light-eyed brothers and sisters is 16 °/ₒ below that of the general population. Again in the parental generation, there are 12 °/ₒ fewer light-eyed parents of hazel-eyed parents, and this 12 °/ₒ is transferred to the hazel-eyed group, the dark-eyed parents remaining at

Constitution and Ancestry of Hazel-Eyed Sibships.

Generations	Total cases observed in 168 families	Percentages		
		Light Eyed	Hazel Eyed	Dark Eyed
I. Siblings ...	948	45	32	23
II. Parents ...	336	49	25	26
III. Grandparents	449	60	13	27
General Population	4490	61·2	12·7	26·1

the general population percentage. The distribution of the grandparents of a hazel-eyed person is practically the same as that of the general population. From these data Galton concludes as follows:

"The total result in passing from Generation III to I, is that the percentage of the light eyes is diminished from 60 or 61 to 45, therefore by one quarter of its original amount, and that the percentage of the dark eyes is diminished from 26 or 27 to 23, that is to about [? by about] one-eighth of its original amount, the hazel element in either case absorbing the difference. It follows that the chance of a light-eyed parent having hazel offspring is about twice as great as that of a dark-eyed parent. Consequently since hazel is twice as likely to be met with in any given light-eyed family as in a given dark-eyed one, we may look upon two-thirds of the hazel eyes as being fundamentally light and one-third of them as fundamentally dark. I shall allot them rateably in that proportion between light and dark and so get rid of them. M. Alphonse de Candolle has also shown from his data that *yeux gris* (which I take to be equivalent to my *hazel*) are referable to a light ancestry rather than to a dark one, but his data are numerically insufficient to warrant a precise estimate of the relative frequency of their derivation from each of these two sources." (pp. 407–8.)

I find it very difficult to follow this reasoning, or to see from the table above its validity. It would seem to be essential to follow up the particular ancestry of each hazel-eyed individual, before we can draw the conclusions that Galton does from the *massed* numbers of children, parents and grandparents. Galton and de Candolle at least admit the difficulty of the hazel eyes; many Mendelian writers speak only of "brown" and "blue" eyes; others speak of hazel-eyed persons as heterozygotes*.

Galton having thus disposed of his *yeux gris*, now turns to the same multiple regression formula as he has used for stature, namely he makes the regression coefficient $\frac{1}{4}$ for a parent, $\frac{1}{16}$ for a grandparent and so on to higher ancestry. He also makes use of what is, I believe, an erroneous hypothesis, at any rate one inconsistent with his multiple regression coefficients,

* Sometimes a definition is given of pure blue eyes as being those without anterior pigment. According to one ardent Mendelian this can always and only be tested with a lens; another accepted relatives' statements, and came to the same conclusion without a lens. From twelve cases in which both eyes were carefully examined with a lens and thus found to be without anterior pigment, the excised eye when sectioned and examined microscopically showed quite clearly anterior pigment. Hitherto I have failed to come across any eye, however blue, which is without some anterior pigment when sectioned. At what degree of pigmentation does the recessive character cease?

namely, that if an individual has h of a certain character, the most probable value of the character in his parent will be $\frac{1}{3}h$, and in his grandparent $\frac{1}{3^2}h$ and his great grandparent $\frac{1}{3^3}h$ and so on.

Consequently, if we know nothing beyond the one parent of character h, the expected heritage is

$$h\left\{\frac{1}{4}+2\left(\frac{1}{3}\times\frac{1}{2^4}\right)+4\left(\frac{1}{9}\times\frac{1}{2^6}\right)+\dots\right\}=h\times 0{\cdot}30.$$

When one grandparent only is known to have h then the corresponding parent has $\frac{1}{3}h$, and the two great grandparents $\frac{1}{3}h$, the four great great grandparents $\frac{1}{3^2}h$ and so on. Thus the formula is

$$h\left\{\left(\frac{1}{3}\times\frac{1}{2^2}\right)+1\times\left(\frac{1}{2^4}\right)+2\left(\frac{1}{3}\times\frac{1}{2^6}\right)+4\left(\frac{1}{3^2}\times\frac{1}{2^8}\right)+\dots\right\}=h\times\left(\frac{1}{12}+\frac{3}{40}\right)=h\times 0{\cdot}16,$$

i.e. actually $0{\cdot}158\dot{3}h$.

If a parent and the corresponding two grandparents be known Galton says the parent will contribute $\frac{1}{4}$ of his character and the two grandparents and their ancestry $\frac{3}{40}$ as above. But I do not think this is correct, even on Galton's assumptions. In the previous case we predicted the great grandparents and higher ascendants from a knowledge of the grandparents *only*. But in this case we have not only these two grandparents, but also the knowledge of their offspring, the parent, to predict from, and accordingly Galton's $\frac{1}{80}$ for the rest of the ancestry is not satisfactory. As he is working in round numbers, Galton puts $\frac{3}{40}$ ($=\cdot075$) as equal to $\cdot08$.

Three cases are now dealt with: I, both parents only known; II, four grandparents only known; and III, both parents and four grandparents known. I gives $2\times\cdot30=\cdot60$ of heritage with a residue of $\cdot40$ undetermined. Galton distributes this residue in the general population proportions of light to dark eyes after distributing the hazel eyes $\frac{2}{3}$ to light and $\frac{1}{3}$ to dark eyes, which give 70 °/ₒ and 30 °/ₒ of those eyes. Thus the residue $\cdot40$ is to be given $\cdot28$ to light and $\cdot12$ to dark eyes. The corresponding residues for cases II and III are $\cdot36$ and $\cdot18$, which Galton distributes as $\cdot25$ and $\cdot11$, $\cdot12$ and $\cdot06$* respectively.

Galton now combines all these results in a table from which with knowledge of the ancestry as far as parents and grandparents are concerned he considers prediction of eye-colour in offspring can be ascertained (p. 39).

Let me illustrate the use of this table. A family of 12 given by Galton had both parents light-eyed, 3 grandparents light-eyed and 1 hazel-eyed. If we predict from parents only we should have

$$12\times(2\times\cdot30+\cdot28)=12\times\cdot88=10{\cdot}56 \text{ light-eyed.}$$

If we predict from grandparents only we should have

$$12\times(3\times\cdot16+1\times\cdot10+\cdot25)=9{\cdot}96 \text{ light-eyed.}$$

* More accurately the latter pair should be $\cdot13$ and $\cdot05$.

And if from all our information

$$12 \times (2 \times \cdot 25 + 3 \times \cdot 08 + 1 \times \cdot 05 + \cdot 12) = 10 \cdot 92 \text{ light-eyed.}$$

Thus the best prediction gives 11 out of 12 children light-eyed. Actually all 12 were light-eyed. Take again another family 2 parents hazel, 2 grandparents light, 1 hazel and 1 dark. Total family, 7 children. The prediction is $7\,(2 \times \cdot 16 + 2 \times \cdot 08 + 1 \times \cdot 05 + \cdot 12) = 4 \cdot 55$ light-eyed, the observed number was 4. Of course Galton only claims to give the average family, and some of the results he gives from his Table of 78 individual families are not good. But his Table III in which he deals with 16 groups of different ancestries is, considering what appears to me the doubtful character of his assumptions, really surprising. Out of 827, 629 were observed to be light-eyed. Predicted from parents only 623 were light-eyed, and from parents and grandparents 614. As a rule, however, III gives a better result than I; for example, out of 183 children, all of whose parents and grandparents were light-eyed (none hazel), 174 were observed to be light-eyed; here III predicts 172, and I only 161.

Prediction Table for Eye Colour in Offspring.

	Both Parents I		Four Grandparents II		Both Parents and Four Grandparents III	
	Light	Dark	Light	Dark	Light	Dark
Light-eyed Parent	0·30	—	—	—	0·25	—
Hazel-eyed Parent	0·20	0·10	—	—	0·16	0·09
Dark-eyed Parent	—	0·30	—	—	—	0·25
Light-eyed Grandparent ...	—	—	0·16	—	0·08	—
Hazel-eyed Grandparent ...	—	—	0·10	0·06	0·05	0·03
Dark-eyed Parent	—	—	—	0·16	—	0·08
Residue to be rateably assigned	0·28	0·12	0·25	0·11	0·12	0·06

It is certainly remarkable that the predictions should be even as accurate as they are—and they are indeed not perfect—considering the contradictory assumptions on which they are based*. Perhaps in the first glow of finding such an amount of accordance Galton was justified in writing:

"A mere glance at Tables III and IV will show how surprisingly accurate the predictions are, and therefore how true the basis of the calculations must be....My returns are insufficiently numerous and too subject to uncertainty of observation to make it worth while to submit them

* In particular Galton's assumption that the correlations of the offspring with the individual parent, grandparent, great grandparent, etc., form the series r, r^2, r^3, etc., is incompatible with his multiple regression coefficients $\frac{1}{4}$, $\frac{1}{16}$, $\frac{1}{64}$, etc. Any such series causes all those coefficients except the first or parental coefficient to vanish, and reduces the ancestral multiple regression to a simple biparental inheritance. Thus the parental characters determine completely those of the offspring, as in the well-known case of the Mendelian theory of gametic characters.

to a more rigorous analysis, but the broad conclusion to which the present results irresistibly lead, is that the same peculiar hereditary relation that was shown to subsist between a man and each of his ancestors in respect of the quality of stature, also subsists in respect to that of eye-colour." (pp. 415–6.)

The essential fact to be remembered here is that Galton supposes the ancestral contributions which blend in the case of the stature of the individual, will be found as alternative eye-colours in the same proportions as for stature in the total group of descendants. For example, if an ancestor contributes $1/p$th of his stature deviation to his descendant in the final generation, he will contribute his eye-colour to $1/p$th of his descendants in the same generation.

It would be of great interest to rework Galton's proportions with the actual correlations found from his data, and with the corresponding and consistent multiple regression coefficients, and ascertain whether accordance was not sensibly improved. His parental correlation $\frac{1}{3}$ is too small for his data, and his regression coefficients want considerable modification.

E. *Law of Ancestral Heredity applied to Basset Hounds.* Galton having applied his Law of Ancestral Heredity to Eye-Colour in Man sought for additional material to illustrate it. He found this eleven years later in Sir Everett Millais' large pedigree stock of Basset Hounds. This material reached him at the very time he was himself planning an extensive experiment with fast breeding small mammals*. One can but regret that that experiment was never undertaken. The Bassets are dwarf bloodhounds, and there are only two varieties of colour, they are either white with blotches from red to yellow technically termed "lemon and white," or they have in addition to this "lemon and white" black markings; in which case they are termed "tricolour." Galton had thus only two types to deal with, which he terms "tricolour" (T) and "non-tricolour" (N). A full report of his statistical reduction of Millais' data is given in a paper read before the Royal Society, June 3, 1897†.

Galton's material was contained in *The Basset Hound Club Rules and Studbook*, compiled by Everett Millais, 1874–1896, but with this valuable addition, that Sir Everett Millais had added the registered colours of nearly 1000 of the hounds (this copy is now in the Galton Laboratory). In this record are 817 hounds, the colour of whose parents are given, and 567 hounds in which the colours of the two parents and the four grandparents are known, and lastly in 188 cases in addition the colour of all the eight great grandparents.

Galton starts with the same idea as in the paper last dealt with, namely that each parent contributes $\frac{1}{4}$, each grandparent $\frac{1}{16}$ and so on, of the heritage taken as a whole to be unity. Here as in the case of eye-colour, the heritage is

* An extensive series on moth-breeding had been undertaken but had unfortunately failed to give any satisfactory results, partly owing to the diminishing fertility of successive broods, and partly to the disturbing effects of food differences and change of environment in different years.

† See *Roy. Soc. Proc.* Vol. LXI, pp. 401–413. An abstract appeared in *Nature*, July 8, 1897, Vol. LV, p. 235.

not taken to be that of an individual, but as represented by percentages of the total offspring, the coat colours being exclusive, i.e. there is no attempt to measure the degree of melanism. Galton gives some reasons for his law being a probable one:

"It should be noted that nothing in this statistical law contradicts the generally accepted view that the chief, if not the sole, line of descent runs from germ to germ and not from person to person. The person may be accepted on the whole as a fair representative of the germ, and, being so, the statistical laws which apply to the persons would apply to the germs also, although with less precision in individual cases. Now this law is strictly consonant with the observed binary subdivisions of the germ cells, and the concomitant extrusion and loss of one-half of the several contributions from each of the two parents to the germ cell of the offspring. The apparent artificiality of the law ceases on those grounds to afford cause for doubt; its close agreement with physiological phenomena ought to give a prejudice in *favour* of its truth rather than the contrary. Again, a wide though limited range of observation assures us that the occupier of each ancestral place *may* contribute something of his own personal peculiarity, apart from all others, to the heritage of the offspring. Therefore there is such a thing as an average contribution appropriate to each ancestral place, which admits of statistical valuation, however minute it may be. It is also well known that the more remote stages of ancestry contribute considerably less than the nearer ones. Further it is reasonable to believe that the contributions of parents to children are in the same proportion as those of the grandparents to the parents, of the great grandparents to the grandparents, and so on; in short, that their total amount is to be expressed by the sum of the terms in an infinite geometrical series diminishing to zero. Lastly, it is an essential condition that the total amount should be equal to 1, in order to account for the whole of the heritage. All these conditions are fulfilled by the series of $\frac{1}{2}+\frac{1}{2^2}+\frac{1}{2^3}+$ etc., and by no other*. These and the foregoing considerations were referred to when saying that the law might be inferred with considerable assurance *à priori*; consequently, being found true in the particular case about to be stated, there is good reason to accept the law in a general sense." (*loc. cit.* p. 403.)

Modern research shows that the "binary subdivisions of the germ cells, and the concomitant extrusion and loss of one-half of the several contributions from each of the two parents to the germ cell of the offspring" may have other interpretation than that put upon it by Galton. Objections may also be raised to Galton's proportioning of the "heritage" among the offspring, and to his allowance for ancestors whose characters are not known directly. But the criticisms of the "ancestral law," made chiefly by Mendelians, have failed to attack these weaknesses. They have been generally based on citing *individual* matings†, as if these had any application to a statistical law

* This seems incorrect: the conditions would appear to be equally well satisfied by

$$(1-a)(1+a+a^2+a^3+\dots),$$

which series leaves a constant a to be determined by observation of one kind or another. By putting $a=\frac{1}{2}$, Galton excluded his ancestral law from describing Mendelian gametic inheritance, which corresponds to $a=0$ or the parents' gametic constitutions *alone* determining the offspring.

† Occasionally hybridisations are cited. Galton in a letter to *Nature*, October 21, 1897, writes:

"Permit me to take this opportunity of removing a possible misapprehension concerning the scope of my theory. That theory is intended to apply only to the offspring of parents who, being of the *same variety*, differ in having a greater or less amount of such characteristics as any individual of that variety may normally possess. It does *not* relate to the offspring of parents of different varieties; in short it has nothing to do with hybridism, for in that case the offspring of two diverse parents do not necessarily assume an intermediate form."

Whether the limit to offspring assuming "an intermediate form" is needful is another question, and might raise a discussion as to whether the law could be applied to alternate describing what happens on the *average* in the case of a race or community mating at random. What Galton's critics have not seen is that the degree of accordance between his predictions and observed facts, if not perfect, is yet so considerable, in the cases of both eye-colour in Man and coat-colour in Basset Hounds, that it is not possible simply to put it for all characters on one side as of no importance. No entirely erroneous hypothesis could, I think, lead to such accordance as Galton shows in his Tables V and VI of this memoir!

I have already pointed out when dealing with Galton's views on eye-colour, that, because r is the regression coefficient of child on parent*, it does not follow that r^2 will be that of child on grandparent or of grandparent on child. Galton drops this manner of allowing for the unstated characters of the higher ascendants when he comes to the coat-colour of Bassets. He argues as follows: Out of 1060 parents of 530 offspring with tricolour coats 836 were tricolour (T) and 224 were lemon and white (N), i.e. 79 °/₀ and 21 °/₀; he accordingly says that the chance that a tricolour offspring has a tricolour parent is ·79. He concludes that if a dog has a tricolour parent, but nothing is known of the grandparents, these will be ·79 °/₀ tricolour, and the parents of these grandparents will be $(\cdot 79)^2$ °/₀ tricolour and so on. I am inclined to doubt the accuracy of this method of correction for the past ancestry of the tricolour for two reasons: (i) if both parent and grandparent were tricolour, then it seems to me there would be a greater probability of the great grandparent being tricolour than ·79, for we know that not merely one, but two generations of the offspring of these ancestors have been tricolour†; (ii) further, in each ascending generation besides the ·79 °/₀ tricolour of a tricolour animal there will be ·21 °/₀ non-tricolour, but these non-tricolour dogs will have also a percentage of tricolour ancestry, namely 56 °/₀ according to Galton's Table III, and I cannot see that he has allowed for the non-tricolour ancestors' contribution of additional ancestral tricolours in his method of reckoning his tricolour "coefficients" of tricolour grandparents. Noting that Galton calls A_s the ancestry of the sth generation and a_0 the offspring, we may cite his words from p. 406:

"Suppose all the four grandparents, A_2, to be tricolour, then only 0·79 of A_3 will be tricolour also, $(0{\cdot}79)^2$ of A_4, and so on. These several orders of ancestry will respectively contribute an average of tricolour to each a_0 of the amounts of $(0{\cdot}5)^3 \times 0{\cdot}79$, $(0{\cdot}5)^4 \times (0{\cdot}79)^2$, etc. Consequently the sum of their tricolour contributions is

$$(0{\cdot}5)^3 \times (0{\cdot}79)\,\{1 + (0{\cdot}5) \times (0{\cdot}79) + (0{\cdot}5)^2 \times (0{\cdot}79)^2 + \text{etc.}\}$$

which equals 0·1632. The average tricolour contributions from *each* of the four tricolour grandparents must be reckoned as the quarter of this, namely, 0·0408."

characters in either eye-colour or coat-colour; but Galton's disclaimer, made with regard to Professor Henslow's criticisms of the law (see *Gardeners' Chronicle*, September 25, 1897) based on plant hybridisations, has been overlooked by those who more recently have cited hybridisations as disproving the law.

* $r = 0{\cdot}3$ according to Galton.

† Thus from Galton's Table I we find that if parents and grandparents were all tricolour the percentage was 89, and not 79, tricolour offspring. Galton treats really correlated relationships as independent probabilities.

Now I think this does not involve all the tricolour ancestry of the four tricolour grandparents, for 0·21 of the great grandparents are non-tricolour, and there will be $(0{\cdot}21) \times (0{\cdot}56) \times (0{\cdot}5)^3 \times (0{\cdot}79) \times (0{\cdot}5)^2$ of the great great grandparents tricolour. At each stage a non-tricolour branch will split off, showing in the next ascending generation some tricolour. It appears to me that Galton has overlooked the sum of all these ancestral tricolour contributions in estimating the tricolour in a_0. They may be considerably less than those retained, but I do not think they can be disregarded without justification.

"By a similar process," Galton writes, "the average tricolour contribution from the ancestry of *each* non-tricolour grandparent is found to be 0·0243." (p. 406.)

It would seem that this is obtained from:

$$(0{\cdot}5)^3 \times (0{\cdot}56)\{1 + (0{\cdot}5) \times (0{\cdot}56) + (0{\cdot}5)^2 \times (0{\cdot}56)^2 + \text{etc.}\} = {\cdot}0972,$$

for one-fourth of this is 0·0243.

But the above expression is not, I think, correct, for after the great grandparental 0·56 of tricolour we must surely use not 0·56 but 0·79 to pass from tricolour to tricoloured ancestry. Thus the result should be

$$(0{\cdot}5)^3 \times (0{\cdot}56)\{1 + (0{\cdot}5) \times (0{\cdot}79) + (0{\cdot}5)^2 \times (0{\cdot}79)^2 + \text{etc.}\} = {\cdot}1157,$$

of which the fourth part is ·0289.

Here as before the non-tricoloured ancestors of earlier generations who would themselves have tricoloured parents, etc., are neglected.

Taking Galton's illustration (p. 406) of both parents tricolour, three grandparents tricolour, and one lemon and white, Galton's factor of ·8342 is only changed to ·8388 by the above correction, but this gives 100 tricolour hounds out of a total of 119 offspring in this category, while the observed tricolours were 101, a remarkably close accordance.

I illustrate the sort of accordance obtained in the following examples:

Both Parents Tricolour	Number of Tricolour Grandparents				
	4	3	2	1	Totals
Tricolour Offspring:					
Observed ...	106 (119)	101 (119)	24 (28)	8 (11)	239 (277)
Calculated ...	108	100	21	8	237

Both Parents and three Grandparents Tricolour	Number of Tricolour Great Grandparents					
	8	7	6	5	4	Totals
Tricolour Offspring:						
Observed ...	—	17 (18)	19 (21)	14 (16)	6 (6)	56 (61)
Calculated ...	—	16	18	13	5	52

The numbers in brackets denote total offspring.

6—2

Two cases give rather poor results, those for 1 parent and 3 grandparents tricolour, no great grandparents or higher ancestry known (92 calculated for 79 observed in 158) and 1 parent, 3 grandparents and 5 great grandparents tricolour with no higher ancestry known (18 calculated for 8 observed out of 31). In the latter case especially it is the observations which seem to me questionable, because for one parent tricolour and the other lemon and white, whatever be the more remote ancestry we get 139 tricolour to 122 non-tricolour, while with 3 grandparents and 5 great grandparents tricolour, the observations only give us 8 tricolour to 23 non-tricolour or a *drop* from 50 °/₀ to 26 °/₀ in tricolour, with an increase of tricolour ancestry. If we can trust the classification, then no simple Mendelian hypothesis will provide a formula to fit the data, because neither tricolour × tricolour nor non-tricolour × non-tricolour breeds true. I have said, if we can trust the classification, because as Galton points out there is a strange prepotency of sire over dam*, the ratio of sire colour to dam colour in offspring being of the order of 6 to 5. A more important fact bearing on the classificatory accuracy arises from an investigation by an entirely different method from Galton's†, where it appeared that the resemblance of the offspring to the sire was far less than to the dam. This suggested that the parentage was more certain in the case of the dam than in that of the sire, a difficulty not unlikely to arise from the carelessness of kennel attendants.

In the opinion of the present biographer the Law of Ancestral Heredity has been shown by Galton to be at least approximate in two very different cases, and this justifies further attempts to deal with it, either in Galton's or a more generalised form, on more satisfactory material and with possibly more accurate methods of computing the corrections for the unknown characters of the higher ancestors.

F. *Representations of the Ancestral Law.* Several graphical representations of Galton's form of the Ancestral Law have been provided. Perhaps the best is that devised by A. J. Meston of Pittsburg‡, which was modified by Galton himself in a communication to *Nature*, January 27, 1898.

The diagram (p. 45) is of the following nature.

It is based on a square of unit edge; 2 and 3 represent the parents; 4, 5, 6 and 7 the grandparents; 8, 9, 10, 11, 12, 13, 14, 15 the eight great grandparents, and so on. All even numbers represent males and uneven numbers females. $2n+1$ is the female mate of the male $2n$. The father and mother of n are always $2n$ and $2n+1$ respectively. Every ancestor in whatever line has now got a definite number, and every number denotes a definite ancestor. For example:

(i) What is the proper number to represent a child's mother's mother's

* In the *Roy. Soc. Proc.* paper, p. 404, Galton says the dam is prepotent. But on this page and in Table II, p. 410, sire and dam should be interchanged. This slip is acknowledged by Galton himself in a letter to *Nature*, October 21, 1897, on the *Hereditary Colour in Horses*, to which we shall refer later. It does not affect his work as he has made no use of this prepotency in his calculations.

† *Roy. Soc. Proc.* Vol. LXVI, p. 158. January, 1900.

‡ See *The Horseman*, December 28, 1897, Chicago.

father's father's mother's father's father's father's mother? The child's mother is 3, her mother $2\times 3+1=7$, her father $2\times 7=14$, his father 28, 28's mother $=2\times 28+1=57$, 57's father is 114, 114's father is 228, 228's father is 456 and lastly 456's mother is 913, which is the number signifying the required ancestor.

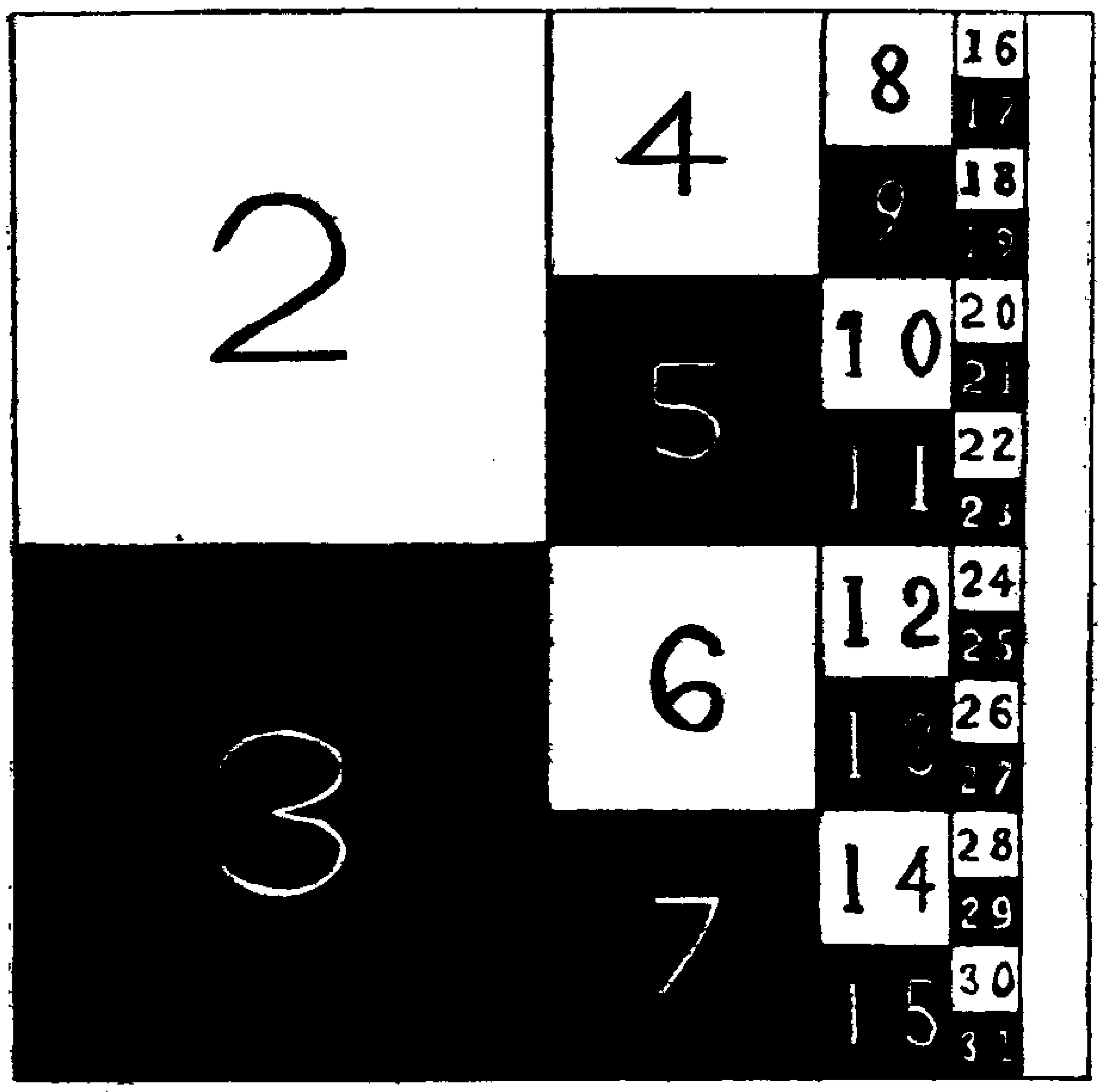


Fig. 10.

(ii) What ancestor does 253 represent? 253 is odd and therefore the mother of $\frac{1}{2}(253-1)=126$, who being even is the father of 63, who being odd is the mother of 31 who is the mother of 15 who is the mother of 7, who is mother of 3 the child's mother. Accordingly 253 is the child's mother's mother's mother's mother's mother's father's mother.

This numerical nomenclature is not due to Meston, but to Galton himself, appearing in his paper of 15 years' earlier date on "Arithmetic Notation of Kinship*."

We may, to use Galton's notations, say that:

$$m\,m\,f\,f\,m\,f\,f\,f\,m=913, \quad \text{and} \quad 253=m\,m\,m\,m\,m\,f\,m.$$

The ancestral lines of 913 and 253 separate off at the parents of the child's maternal grandmother.

G. *Experiments in Moth-Breeding.* Before we turn to a number of papers and projects directly arising from the "Law of Ancestral Heredity," it is desirable to say a few words on the abortive moth-breeding experiments (see our p. 49). At first sight the idea of breeding moths seems exceedingly hopeful. They breed rapidly and apparently could be fairly successfully reared and bred in captivity. Accordingly Galton in January, 1887, six months after the reading of his paper on Human Eye Colour, issued for

* *Nature*, September 6, 1883.

private circulation a circular entitled *Pedigree Moths*. He had already enlisted the assistance of an able entomologist, Mr F. Merrifield, of Brighton, who had suggested working with the Purple Thorn Moth (*Selenia illustraria*). The circular consisted of two parts, one by Galton stating the purpose of the experiments, and an Appendix by Merrifield asking his fellow entomologists for advice and help. After referring to the number of pupae he needed, Merrifield asks his colleagues for information as to the number of eggs, best means of mating and laying of fertile eggs, preservation of moths, cages, possible feeding, stupefying, etc., feeding of larvae, and preserving pupae. It will be seen that the experiments were not rashly entered upon by mere lay workers. Most careful inquiries were made and the plan of operations well thought out with no undue haste. I think it needful to emphasise this as I know of two later laborious experiments in moth-breeding which also failed to attain any satisfactory results, and of which we might possibly say that Galton's experience was not turned to profit by the undertakers. Too often such experience is overlooked, or the investigator trusts to a belief in his own greater skill, and the use of a different species*.

Galton in his section of the circular, after referring to his work on regression in stature and to the Law of Ancestral Heredity as exhibited in eye-colour, states that he thinks it desirable to obtain data providing more than the three to four generations he has been able to deal with in these cases. He considers that moths would form suitable material, and that the time needful would be shortened by taking a species which bred twice a year. He proposed to measure the size of wing for six generations, and in order to measure the effect of selection to breed from large male and female, from small male and female, and from mediocre male and female. Thus he would establish three lines, and from the largest he would again pick the very large male and female, and from the smallest the very small male and female, and from the mediocre line the mediocre were again to be chosen; these latter were to act as a control series whereby to standardise the large and the small lines. After six generations Galton proposed to reverse the process, and return by selection to his original wild moth. The whole experiment would have taken at least six years. In his circular Galton makes two statements. One is that his Law of Regression leads to *his* Ancestral Law; this I believe to be incorrect. There is a relation between the ancestral correlations and a Law of Ancestral Heredity, but the numerical values given by Galton for his regression and his ancestral contributions are incompatible with each other (see p. 39 above). In the second place Galton makes the following statement:

"It is, however, highly probable from other considerations that though this simple formula may be closely true for the parents and nearly true for the grandparents, it may become sensibly and increasingly different for remoter progenitors. It is this fact that I want to investigate, because all theory concerning the nature of stability of type, and of much else, must be based on the facts of Regression, which such experiments as those proposed can alone, so far as I see, be likely to declare in a trustworthy way."

* It is possible that moths held in captivity for generations, like the silk-worm moth, would show less erratic results than wild moths reared under what must after all be very artificial conditions.

Now what is Galton's difficulty when he thus wishes to modify the contributions of the earlier progenitors?

I think his difficulty can be elucidated from passages in his other writings. In the first place, on February 2, 1887, Galton and Merrifield read papers to the Entomological Society. These are respectively entitled:

Pedigree Moth-breeding, as a means of verifying certain important Constants in the General Theory of Heredity, and

Practical Suggestions and Enquiries as to the Method of Breeding **Selenia illustraria** *for the purpose of obtaining Data for Mr Galton,*

and were published in the *Transactions**.

These papers consist of an enlargement of the proposals in the Circular of January and a fuller account of the methods to be adopted in obtaining, feeding, breeding, and measuring the moths. Between January and February apparently the measurement of length of wing had been definitely fixed upon. Galton in his paper says that the laws of simple heredity as he has propounded them involve only five constants.

"These admit of being separately determined, and they are at the same time connected by an equation that serves to verify their observed values. The equation depends on the fact alluded to, that successive generations of the same population yield identical biological statistics, although each family or brood is full of variations, and although the 'median' of each characteristic in each brood is on the average *always more mediocre* than the corresponding characteristic in the mean of the two parents. The first of these events, 'fraternal variability,' increases the variability of the population as a whole, and the latter event, which I call 'Regression,' decreases it; the two can be shown to counterbalance each other and give rise to a position of stable equilibrium. The five constants are (1), the Median of the race; (2), the Quartile of the race; (3), the Quartile of the broods of the same parents, i.e. brothers and sisters; (4), the Quartile of the broods of a large number of like parents, mixed together in a single group; (5), the coefficient of Regression." (p. 28.)

Before we go further let us endeavour to interpret this important passage in terms of more modern notation and more modern conceptions of multiple correlation. Corresponding to (1) we have the mean of the race M; to (2), the standard deviation of the race σ; to (3), the variability of the family, i.e. $\sigma_f\sqrt{1-R^2}$, where R is the multiple correlation coefficient of an individual on all his ancestry and σ_f is the standard deviation of the totality of offspring; to (4), $\sigma_f\sqrt{1-r^2}$, where r is the correlation coefficient between parent and offspring; and to (5), $\sigma_f r/\sigma_p$, where σ_p is the standard deviation of the parental generation in its totality. Now Galton, I think, throughout supposes (after reducing female to male values) that $\sigma_f=\sigma_p=\sigma$, or he supposes his parental variability to be the same as that of his general population and again equal to that of the total offspring population. Further he supposes M to be the same for every generation, and this is the most stringent limitation of all, for it hinders the possibility of a continuous (or discontinuous) change of type. We can illustrate this from a statement in a second paper of Galton's, that on the Coat Colour of Basset Hounds (see our p. 40 and p. 402 of the memoir itself). Therein he writes as follows:

"The law may be applied either to total values or to deviations, as will be gathered from the following equation. Let M be the mean value from which all deviations are reckoned, and

* *Trans. Entomological Soc.* London, 1887, Part I, pp. 19–34.

let D_1, D_2, etc. be the means of all the deviations, including their signs, of the ancestors in the 1*st*, 2*nd*, etc. degrees respectively; then

$$\tfrac{1}{2}(M + D_1) + \tfrac{1}{4}(M + D_2) + \text{etc.} = M + (\tfrac{1}{2}D_1 + \tfrac{1}{4}D_2 + \text{etc.}).\text{"}$$

This is sufficient evidence that Galton had not at the time under consideration reached the full meaning of multiple regression. The Ancestral Law is nothing but the principle of multiple regression applied to ancestral inheritance, but in this case the deviations must all be measured not from a general mean, but from the mean of the corresponding generation. The Law of Ancestral Heredity is therefore independent of the change of type, if such is taking place; it can tell us nothing of the laws ruling that change of type, which is something wholly independent of it. Galton's statements that the law may be applied either to total values or deviations is only true for a population stable through the whole ancestry, whereas the application to deviations (with the proper ancestral coefficients, i.e. the multiple regression coefficients) is generally true, and if Galton had recognised this, it would have saved him from doubts as to the compatibility of his law with evolutionary changes.

That Galton recognised the difference between the Quartile of the single brood and the Quartile of the clubbed broods of like parents shows that he fully appreciated the difference between R and r. I do not think, however, that he recognised that his Ancestral Law, i.e. the values he had chosen for his coefficients, actually enforced a definite relation between R and r. But he fully realised the relation between the regression coefficient and r, his "index of correlation*." We can now continue our citation from the Entomological Society paper, which brings out Galton's difficulty:

"The laws in which these constants play a part give calculated results that prove to be closely true to observation in the ordinary cases of simple heredity, where there has been no long-continued selection, but it does not at all follow that they will hold true for the descendants of a long succession of widely divergent parents. It is this that I want to test. The point towards which Regression tends cannot, as the history of Evolution shows, be really fixed. Then the vexed question arises whether it varies slowly or by abrupt changes, coincident with changes of organic equilibrium which may be transmitted hereditarily; in other words, with small or large changes of type. Moreover the values of the Quartile in (3) and (4) cannot be strictly constant and are probably connected in part with the value of the Median and require a modified treatment by using the geometrical law of error instead of the arithmetical one (*Proc. Royal Soc.* 1879). Again the diminution of fertility and of vitality that accompany wide divergence from racial mediocrity have yet to be measured, by comparing the A [selected large size] and Z [selected small size] broods with the M [mediocre size] broods. It was assumed not to vary in the approximate theory of which I spoke." (p. 28.)

These words bring out the difficulty which arose in Galton's mind from treating regression as taking place towards a *fixed* racial value, instead of supposing it to arise from measuring deviations from the means of their groups. In this way a rather mysterious entity "the racial centre of regression" was created, which was given biological significance, when it really was only a factor in the purely statistical description of mass phenomena. Once recognise that in each generation the deviation is measured from the

* He speaks of his five constants being connected by "an equation."

mean of its generation and we find no incompatibility of the Ancestral Law with any change of type. What we obviously must do is to study the change of type or of successive means; regression is a wholly independent matter, and "the racial centre of regression" something which has no essential existence, biologically or statistically.

The next point that Galton makes is that the variabilities $\sigma_f \sqrt{1-R^2}$ and $\sigma_f \sqrt{1-r^2}$ cannot be quite constant; it is not clear whether he only means by this that σ_f and therefore σ, the population variability, changes with the course of evolution. This is very possible, though there is small likelihood of its being discoverable in breeding only six generations of moths, *if kept under the same environment.* Selection might equally well change type and variability; but if the distributions of frequency were normal, the type and variability would be uncorrelated, and the selection of one would not necessarily affect the other. Hence I do not see why Galton says the change in the Quartiles is probably connected with the value of the Median; least of all do I grasp why he should refer at this point to Macalister's curve for the geometric mean. Whatever application that curve may have to variation in sensations, this is the only occasion on which I have seen it suggested that it has any claim to be used for bodily measurements. It might be as justifiably used for physical measurements on man as for those on moths, but I can hardly imagine profit coming from such an application.

The last point made by Galton, namely that the fertility and vitality of stocks widely divergent from the mediocre are likely to be affected, is a very important one and is probably the reason why it is not possible to carry size selection far, at any rate by rapid strides. This has been demonstrated not only on the moth material at present under discussion, but by more recent endeavours to modify small mammals by selecting for size.

The reader who is interested in this matter would do well to refer at least to Merrifield's first report* on the moth-breeding experiments. He will then quickly understand why they failed to satisfy Galton's thirst for data! The spring and autumnal broods were really dimorphous, the males appeared to be larger in one and the females in the other; the wing lengths were not the same in the two. Thus the fact of two broods a year would certainly not expedite matters. Further, the fertility of the largest and the smallest was reduced below that of the mediocre, and when Merrifield took steps to obtain by forcing under higher temperatures more frequent broods, not only did he increase the size of his moths' wings, but the "giant" line and the "dwarf" line became sterile and he had to start again from the mediocre. In fact artificial means had to be used to get the moths from the pupae near enough in time to breed with one another. Further, changes in environment or food had to be made to hasten the larvae to the pupal stage because food supplies were getting low. And all these changes appear to have been associated with variations in size so that finally the irregularities were too widespread for any statistical treatment of the data, or as Galton himself

* *Trans. Entomological Soc.* London (Dec. 7, 1887), 1888, pp. 123–136.

expressed it ten years later: "No statistical results of any consistence or value could be obtained from them*." Thus ended what had at first sight appeared to be a hopeful series of experiments, experiments upon which much thought and labour had been expended.

H. *Correlations and their Measurement.* As I have already pointed out the conception that the regression coefficient for inheritance could be applied to a measure of the relationship of associated variates, provided each was measured in terms of its own scale of variability, first occurred to Galton while he was taking a walk in the grounds of Naworth Castle in the year 1888 (see p. 393 of Vol. II). On December 5, 1888, Galton sent to the Royal Society a paper read fifteen days later and entitled: "Co-relations and their Measurement, chiefly from Anthropometric Data†." The twentieth of December is therefore the birthday of the conception of correlation in biometric data as apart from the idea of regression in heredity which Galton had reached some years earlier, without perceiving at once its capacity for wide generalisation in the treatment of associated variates in all living forms.

Like so much of Galton's work the present paper reaches results of singular importance by very simple methods; his methods are indeed so simple that we might almost believe they must lead to a fallacy had not Galton deduced thereby the correct answer. It is the old experience that a rude instrument in the hand of a master craftsman will achieve more than the finest tool wielded by the uninspired journeyman.

The first three paragraphs of this memoir define Galton's method of considering correlation, and indicate that in 1888 even the spelling of the word had not been fixed‡:

"'Co-relation or correlation of structure' is a phrase much used in biology, and not least in that branch of it which refers to heredity, and the idea is even more frequently present than the phrase; but I am not aware of any previous attempt to define it clearly, to trace its mode of action in detail, or to show how to measure its degree.

"Two variable organs are said to be co-related when the variation of the one is accompanied on the average by more or less variation of the other, and in the same direction. Thus the length of the arm is said to be co-related with that of the leg, because a person with a long arm has usually a long leg, and conversely. If the co-relation be close then a person with a very long arm would usually have a very long leg; if it be moderately close then the length of his leg would only be long, not very long; and if there were no co-relation at all then the length of his leg would on the average be mediocre. It is easy to see that co-relation must be the consequence of the variations of the two organs being partly due to common causes. If they were wholly due to common causes, the co-relation would be perfect, as is approximately the case with the symmetrically disposed parts of the body. If they were in no respect due to common causes, the co-relation would be *nil*. Between these two extremes are an endless number of intermediate cases, and it will be shown how the closeness of co-relation in any particular case admits of being expressed by a simple number.

"To avoid the possibility of misconception it is well to point out that the subject in hand has nothing whatever to do with the average proportions between the various limbs in different

* *Roy. Soc. Proc.* Vol. LXI, p. 402.

† *Ibid.* Vol. XLV, pp. 135–145.

‡ Five years later in 1893 when the volume containing the letter *C* of the *Oxford English Dictionary* was issued, the Galtonian or biometric sense of "correlation" was not given.

races*, which have been often discussed from early times up to the present day, both by artists and by anthropologists. The fact that the average ratio between the stature and the cubit is as 100 to 37† or thereabouts does not give the slightest information about the nearness with which they vary together. It would be an altogether erroneous inference to suppose their average proportion to be maintained so that where the cubit was, say, one-twentieth longer than the average cubit, the stature might be expected to be one-twentieth greater than the average stature, and conversely. Such a supposition is easily shown to be contradicted both by fact and theory." (*loc. cit.* pp. 135–6.)

Let us now describe Galton's procedure. In the first place Galton does not use means, he uses throughout medians, both for his marginal totals and his arrays. Further he does not use standard deviations, he makes use of the quartile measurements. Thus if Q_1, M and Q_3 be the measurements at first, second and third quartile divisions, he takes M as his median and $\frac{1}{2}(Q_3 - Q_1)$ as his measure of variation. Thus his results, unlike our modern treatment, depend essentially on assuming that all his data follow a normal (or "curve of errors") distribution‡. If M_c be the median of any character c and ${}_bM_c$ the median of an array of this character for a given value b of a second character c', then Galton plots:

$$\frac{{}_bM_c - M_c}{\frac{1}{2}(Q_3 - Q_1)_c} \text{ to } \frac{b - M_{c'}}{\frac{1}{2}(Q_3 - Q_1)_{c'}}.$$

In other words he reduces the deviation of an array median from the population median to its unit of variation obtained from the quartiles, and plots this to the deviation of the second character from its median reduced likewise to its own unit of variation. Then he plots:

$$\frac{{}_aM_{c'} - M_{c'}}{\frac{1}{2}(Q_3 - Q_1)_{c'}} \text{ to } \frac{a - M_c}{\frac{1}{2}(Q_3 - Q_1)_c},$$

where a is a value of the first character and ${}_aM_{c'}$ the median of the corresponding array of the second character, and thus gets a second series of points. He takes six or seven values of a and of b, plots two sets of six or seven points and notes that the first and second series of points are nearly on one and the same straight line§. He draws this straight line as closely as he can to the points and through the median, and reads off its slope. This slope is Galton's measure of co-relation. If we take the mean deviation of c' for a given value of c, Galton calls c the "Subject" and c' the "Relative," but perhaps it would be best to call the latter the "Co-relative." Galton's data consisted of about 350 males of 21 years and upwards, of whom the majority were young students, measured in his Laboratory in 1888. He deals with

* [The *variation* in the ratio of stature to cubit does, however, provide a means of determining the correlation. K.P.]

† [Rather 100 to 27 or thereabouts on Galton's numbers, i.e. 67·20″ for stature and 18·05″ for cubit. K.P.]

‡ In the table given on p. 52 for the correlation of Stature and Left Cubit it is very difficult to see any approximation to normality in the distribution of stature.

§ In order to get the *same* straight line, if c be the subject and c' the co-relative, and the "subject" axis horizontal, then it is needful when c' is subject and c co-relative to plot c' along the *same* axis as was used in the first case for c. In other words the character axes must be interchanged.

six characters: Head Length, Head Breadth, Stature, Length of Left Middle Finger, Left Cubit and Height of Right Knee. But he only provides as illustration one table such as we now term a correlation table, and one diagram illustrating how he found what we now term the correlation coefficient. The table and diagram dealing with the co-relation of stature and cubit are given below. Readings were made to one-tenth of an inch.

Correlation Table for Stature and Cubit.

Length of Left Cubit in inches, 348 adult Males.

Stature in inches	Under *16·45*	*16·45—16·95*	*16·95—17·45*	*17·45—17·95*	*17·95—18·45*	*18·45—18·95*	*18·95—19·45*	*19·45* and above	Totals
Above *70·45*	—	—	—	1	3	4	15	7	30
69·45—70·45	—	—	—	1	5	13	11	—	30
68·45—69·45	—	1	1	2	25	15	6	—	50
67·45—68·45	—	1	3	7	14	7	4	2	38*
66·45—67·45	—	1	7	15	28	8	2	—	61
65·45—66·45	—	1	7	18	15	6	—	—	47*
64·45—65·45	—	4	10	12	8	2	—	—	36
63·45—64·45	—	5	11	2	3	—	—	—	21
Below *63·45*	9	12	10	3	1	—	—	—	35*
Totals	9	25	49	61	102	55	38	9	348

* Printed as 48, 48, and 34 respectively in the *Roy. Soc. Proceedings.*

Diagram illustrating the Graphical Process of finding the Slope of the Regression Line, i.e. the Correlation Coefficient of to-day's terminology.

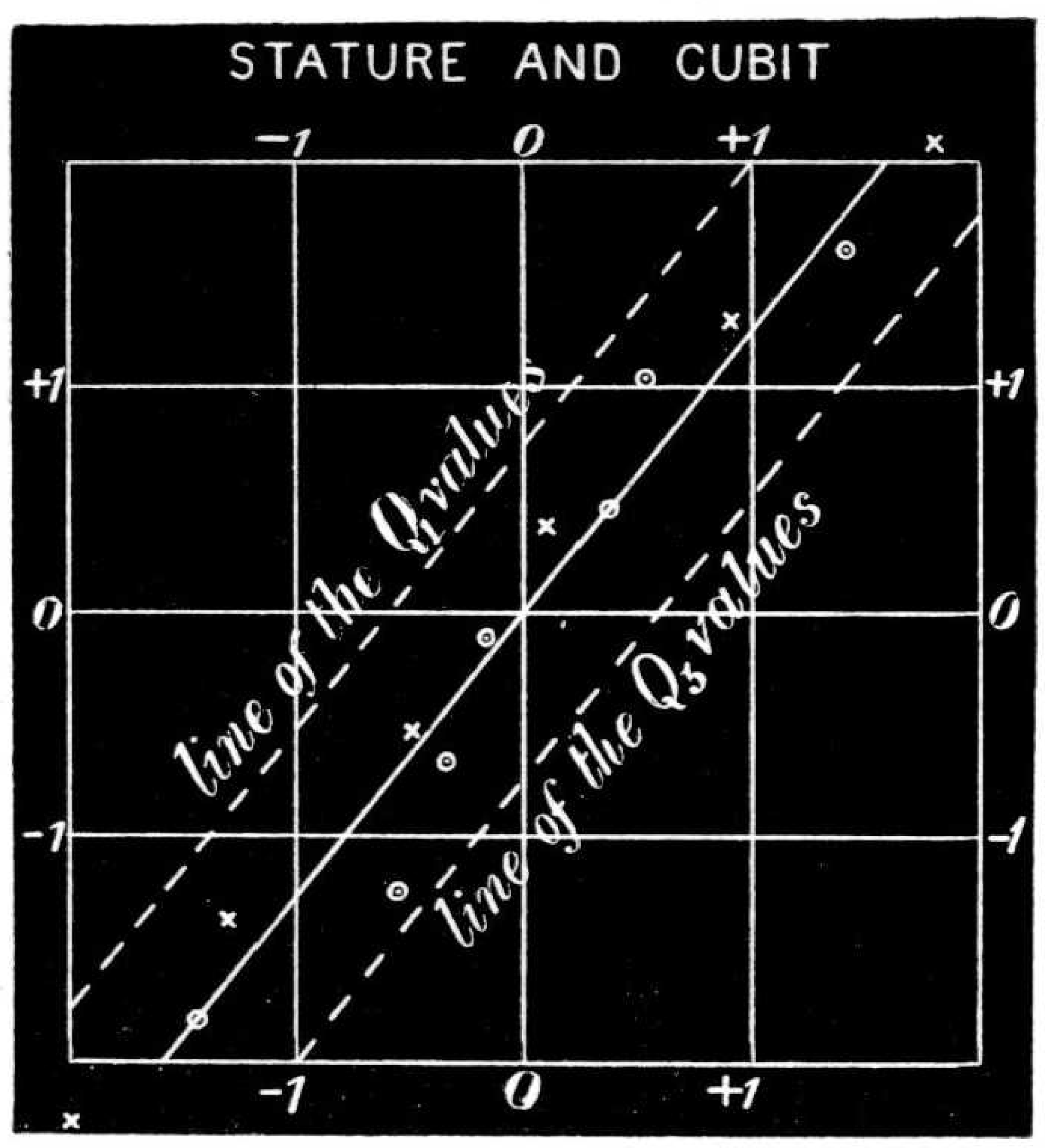


Fig. 11.

Galton says he constructed tables and diagrams like the above. "It will be understood that the Q value is a universal unit applicable to the most varied measurements, such as breathing capacity, strength, memory, keenness of eyesight, and enables them to be compared together on equal terms notwithstanding their intrinsic diversity. It does not only refer to measures of length, though partly for the sake of compactness, it is only those of length that will be here given as examples" (*loc. cit.* p. 137). Galton already saw clearly that his new method enabled comparison to be made on equal terms between variates with such intrinsic diversity as acuity of vision and head breadth*.

I have endeavoured to check Galton's work. I expect he found his medians and quartiles by plotting an "ogive curve" (see our p. 31 and Plate II) and smoothing it. The process of checking is rendered difficult by the following statements on p. 138:

"It is unnecessary to extend the limits of Table II [that of stature and cubit reproduced above] as it includes every line and column in my MS. table that contains not less than twenty entries. None of the entries lying within the flanking lines and columns of Table II were used."

The first statement seems to suggest that the whole table has not been printed, the second leaves one in doubt as to how to find the medians of the arrays, or indeed of the marginal totals, if none of the entries in the flanking lines and columns had been used. Unfortunately I have not succeeded in discovering the original work and manuscript tables for this memoir among Galton's papers†. Putting aside the possibility of re-examining Galton's own work by more modern methods, we can, I think, indicate how closely his semi-graphic median, quartile and regression slope methods accord with those obtained from much longer series by more accurate processes. First let us consider the correlation coefficients:

Character Pair	Correlation Coefficient	
	As found by Galton from 350 Male Adults	As found by Macdonell from 3000 Criminals
Stature and Cubit	0·80 {0·8290}	0.7999
Stature and Head Length ...	0·35	0·3399
Stature and Middle Finger ...	0·70	0·6608
Cubit and Middle Finger ...	0·85	0·8464
Head Length and Head Breadth	0·45	0·4016
Stature and Height of Knee ...	0·90 {0·8665}	—
Cubit and Height of Knee ...	0·80 {0·8028}	—

The values in the first column of this table were the first organic correlations ever published, and on that account are of great historical interest.

* It is not without interest to note that more than a quarter of a century later, Major Leonard Darwin could assert that the influences of environment and heredity could not be compared, because there was no common unit of measurement applicable to them both! He appeared still ignorant of Galton's use of Q. See *Eugenics Review*, Vol. v, p. 152.

† My colleague, Miss E. M. Elderton, has taken out the first 348 entries for male adults 21 years and upwards from Galton's Laboratory records, and the resulting values from her tables, computed by modern methods, are given in brackets in the above and the following tables. Our table for stature and cubit differs somewhat from Galton's but with a probable error of ·0113 the correlation is hardly significantly different from Galton's value. Both Knee Height and Cubit are measured in the Anthropometric Laboratory at University College, but the former is measured to the lowest point of the patella with the subject standing at rest, while Galton measured to the top of the knee with the subject sitting. Galton deducted the measured heel, we measure with boots off. Our correlation for male students of Knee Height and Cubit is only 0·66.

Macdonell's values, obtained by a far more refined and accurate method, indicate —especially when we remember that they are for a very different population—how successfully Galton solved his problem. Doubtless he was somewhat aided by the fact that anthropometric physical measurements are far more nearly normal than many other variates. Had his distributions been more skew, his median estimates would not have given as accurately the correlation coefficients. We can now compare the mean or median values and the standard deviations as found from the quartiles with later results:

Character	Means			Standard Deviations		
	Galton (Adults 21 and upwards)	Macdonell (Criminals)	Schuster (Oxford Students)	Galton (Adults 21, and upwards)	Macdonell (Criminals)	Schuster (Oxford Students)
Stature (cm.) ...	170·69*	166·46	176·50 {170·25}	6·58	6·45	6·61 {6·80}
Cubit (cm.)	45·70	45·06	— {45·85}	2·11	1·96	— {2·04}
Height of Knee (cm.)	52·00	—	— {52·15}	3·01	—	— {2·62}
Middle Finger (mm.)	115·32	115·24	—	5·63	5·48	—
Head Length (mm.) ...	193·55	191·66	196·05	7·11	6·05	6·23
Head Breadth (mm.)	152·40	150·04	152·84	6·82	5·01	4·92
Cephalic Index ...	78·74	78·28	78·02	—	—	2·92

Considering the difference of social class in the three series, Galton's results can hardly have exception taken to them, except in the case of the variabilities of Head Length and Head Breadth. These are excessive, but as we have not the original tables from which the quartiles were determined, it is not possible to investigate wherein they are anomalous†.

The degree of accordance reached by Galton's process may be illustrated by his tables for Stature and Knee Height:

Stature in inches	Mean of corresponding Knee Heights		Height of Knee in inches	Mean of corresponding Statures	
	Observed	Calculated		Observed	Calculated
70·0	21·7 (30)	21·7	22·2	70·5 (23)	70·6
69·0	21·1 (50)	21·3	21·7	69·8 (32)	69·6
68·0	20·7 (38)	20·9	21·2	68·7 (50)	68·6
67·0	20·5 (61)	20·5	20·7	67·3 (68)	67·7
66·0	20·2 (49)	20·1	20·2	66·2 (74)	66·7
65·0	19·7 (36)	19·7	19·7	65·5 (41)	65·7
—	—	—	19·2	64·3 (26)	64·7

The figures in brackets give the numbers of individuals upon whom the observed medians of the arrays were determined. It will be observed that the accordance between observation and theory is again very good.

* For a general hospital population: Stature = 170·59 (*Biometrika*, Vol. IV, p. 126).

† Galton says "The head length is the maximum length measured from the notch between and just below the eyebrows" (p. 137). Is this the glabella?

Table V on Galton's p. 143 is noteworthy. In Column 3 we have the coefficients of correlation tabled under the now familiar symbol r. In Column 4 we have the values of $\sqrt{1-r^2}$, to enable the Quartile of the arrays to be found. In Column 5 we have, placed one under the other, the two regression coefficients, and in Column 6 in the same manner the Quartiles of the arrays (i.e. $\cdot 67449\, \sigma_x \sqrt{1-r^2}$ and $\cdot 67449\, \sigma_y \sqrt{1-r^2}$)*. Throughout, without referring directly to the matter, Galton assumes linear regression and homoscedasticity, i.e. he is thinking in terms of the bivariate *normal* surface. Next he draws attention to the relation of his present work to his former work on heredity.

On the fifth line of p. 144, he has the words: "from $\frac{1}{1\cdot 7}$ to $\frac{1}{3} \times \frac{1}{1\cdot 2} = 1$ to $0\cdot 44$, which is practically the same." This should read "from $\frac{1}{1\cdot 7}$ to $\frac{1}{3} \times \frac{1}{1\cdot 2} = 1$ to $0\cdot 47$, which is identically the same," as it should be since it expresses the coefficient of correlation found from the second regression line. Galton emphasises the importance of the reduction in the variability of the array, as measured by $\sqrt{1-r^2}$, and points out how this affects the efficiency of Bertillon's system of identification by anthropometric measurements. Bertillon had asserted that his measurements were independent variates. A reference to Plate LII of our second volume will show that Galton had chosen several of Bertillon's "independent" measurements and determined their actual correlation.

Galton next outlines a method by which the influence of n variates on another might be determined. He suggests that after transmuting the variates we should sum them, when the probable error of the sum would "be $\sqrt{n}$, if the variates were perfectly independent, and n if they were rigidly and perfectly related. The observed value would be almost always somewhere intermediate between these extremes, and would give the information that is wanted" (p. 145).

This would not, I believe, be a feasible method of approaching multiple correlation; it neglects the possibility of negative correlations, and does not provide for the influence on one variate of all the remainder. It is an attempt to obtain a sort of average value of the interlinkage of a system of n variates†. I do not think that at this time Galton had realised the existence and importance of negative correlation.

* A large proportion of values in the 5th and 6th columns have rather serious numerical errors, corrected by Galton on a copy of the paper in my possession. He also states thereon that he wishes to change the symbol r to ρ, presumably because he was thinking of it as the "correlation coefficient," not as the regression coefficient, when units are reduced to respective variabilities. The regression coefficient without reduction he had termed r in his memoir on stature.

† Let $x_1, x_2, \dots x_s, \dots x_n$ be the n variates, and $\sigma_1, \sigma_2, \dots \sigma_s, \dots \sigma_n$ their standard deviations, $\bar{x}_1, \bar{x}_2, \dots \bar{x}_s, \dots \bar{x}_n$ their means. Then if $\chi = \overset{n}{\underset{1}{S}}(x_s - \bar{x}_s)/\sigma_s$, we have:

$$\begin{aligned}\sigma_\chi^2 &= n + 2\, S'(r_{ss'})\\ &= n, \text{ if all the correlations } r_{ss'} \text{ are zero,}\\ &= n + 2\tfrac{1}{2}\, n\,(n-1) = n^2, \text{ if all the correlations are } \textit{plus} \text{ one.}\end{aligned}$$

Hence $\sigma_\chi = \sqrt{n}$ and n in the two cases respectively, as Galton says. But the actual value of σ_χ

Galton sums up his results as follows*. Let x be the deviation of the subject, and y_1, y_2, y_3, etc. the corresponding deviations of the correlative, all deviations being reduced to their proper unit of variability, and also let the mean of the y deviations for the given x be $\bar{y}_x$, then we find:

(1) That $\bar{y}_x = rx$ for all values of x; (2) that r is the same, whichever of the two variables is taken for the subject; (3) that r is always less than 1; (4) that r measures the closeness of correlation.

It will be seen at once that we have here the first fundamental statement as to the correlation coefficient and its properties. Probably Galton did not recognise that $r=0$ does not signify independence of the two variates, only the independence of *means* of arrays. In addition to this, complete independence involves the arrays being similar and similarly placed curves. It was not till normal distributions were seen to be non-universal that the distinction between the vanishing of r and the absolute independence of variates was fully recognised. For the same reason the idea of non-linear regression did not cross Galton's mind. He got as far as an acceptance of the normal frequency distribution permitted. Only when we look at what has happened since 1888, do we realise the importance of that short paper on "Co-relations"! Thousands of correlation coefficients are now calculated annually, the memoirs and text-books on psychology abound in them; they form, it may be in a generalised manner, the basis of investigations in medical statistics, in sociology and anthropology. Shortly, Galton's very modest paper of ten pages from which a revolution in our scientific ideas has spread is in its permanent influence, perhaps, the most important of his writings. Formerly the quantitative scientist could only think in terms of causation, now he can

would not be proportional to the sum of the $r_{ss'}$ even if they were all positive. Perhaps a better measure of the same type would be to use σ_χ^2, where

$$\chi = \overset{n}{\underset{1}{S}}(x_s - \bar{x}_s)^2/\sigma_s^2 \text{ and } \bar{\chi} = n;$$

hence:

$$\begin{aligned}\sigma_\chi^2 &= \text{mean } (\chi - \bar{\chi})^2 \\ &= \text{mean } \left\{\overset{n}{\underset{1}{S}} (x_s - \bar{x}_s)^4/\sigma_s^4 + 2S' (x_s - \bar{x}_s)^2 (x_{s'} - \bar{x}_{s'})^2/\sigma_s^2 \sigma_{s'}^2 \right. \\ &\qquad \left. - 2nS(x_s - \bar{x}_s)^2/\sigma_s^2 + n^2\right\} \\ &= 3n + 2S'(1 + 2r_{ss'}^2) - 2n^2 + n^2 \\ &= 2n + 4S'(r^2_{ss'}),\end{aligned}$$

if the variates follow normal distributions, and thus σ_χ^2 lies between $2n$ and $2n^2$. This at any rate would present no difficulty arising from the existence of negative correlations. We see, however, from this result that possibly the best measure, u, of the total correlativity in a system would be simply to take

$$u = \frac{2S(r_{ss'}^2)}{n(n-1)},$$

for in this case u will always lie between 0 and 1, the former value corresponding to no association in the variates of the system, and the latter to perfect correlation of all of them.

* Galton has interchanged his x and y variates. The paper shows here as elsewhere signs of haste in preparation.

think also in terms of correlation. This has not only enormously widened the field to which quantitative and therefore mathematical methods can be applied, but it has at the same time modified our philosophy of science and even of life itself. The words which I have cited at the beginning of this chapter show that Galton, if he expressed himself modestly, still realised the importance of his work. The root idea at the bottom of correlation must not be treated as merely rebuilding on a securer mathematical basis statistical science. It is a much greater innovation which touches in its philosophical aspects the epistemology of all the sciences.

I have already referred (Vol. II, pp. 380–386) to Galton's attempt to introduce the conception of correlation* to anthropologists in 1889. It was a hopeless task! Most physical anthropologists in this country lack a thorough academic training, and statistical methods will only penetrate here after they have been adopted in Germany and France as they are being adopted in Russia, Scandinavia and America. English intelligence is distributed according to a very skew curve, with an extremely low modal value; we have produced great men, who have propounded novel ideas, but our mediocrity fails to grasp them or is too inert to turn them to profit. Years later these ideas come back to England, burnished and luring, through foreign channels, and mediocrity knows nothing of their ancestry!

In 1889 Galton read at the British Association (Newcastle-upon-Tyne) a note entitled: "Feasible Experiments on the Possibility of transmitting Acquired Habits by means of Inheritance"; it is published in the *B. A. Report*, p. 620, also in *Nature*, Vol. XL, p. 610, October, 1889. Galton considers that creatures reared from eggs would be most satisfactory and suggests fish, fowls and moths. He considers that fish may be taught to adopt habits not conformable to their nature (Möbius' experiment with pike and minnows). Fowls have an instinctive dread of certain insects, but might be taught to eat mimetic and harmless insects. Larvae are fastidious in their diet, but can be induced to take food which they naturally avoid, and which is found perfectly wholesome. Would acquired habits of this kind be in any case transmitted to their offspring?

I. *Natural Inheritance.* The ideas on heredity and correlation which had been working in Galton's mind during the decade of the 'eighties found final expression in his book entitled *Natural Inheritance*, published in 1889 when Galton was 67 years of age. It may be said that this publication created Galton's school; it induced Weldon, Edgeworth and the present biographer to study correlation and in doing so to see its immense importance for many fields of inquiry. It is idle to overlook the haste with which it was prepared and the many slips and positive errors to be found in its pages, but no one who studied it on its appearance and had a receptive and sufficiently trained mathematical mind could deny its great suggestiveness, or be other than grateful for all the new ideas and possible problems which it provided. The methods of

* Spelled thus in the Presidential Address of Jan. 2, 1889, and, I think, ever afterwards.

Natural Inheritance may be antiquated now, but in the history of science it will be ever memorable as marking a new epoch, and planting the seed from which sprang a new calculus, as powerful as any branch of the old analysis, and valuable in just as many fields of scientific research.

In its application to inheritance the work suffers from the same misinterpretation of "regression" that I have several times referred to, namely making the regression of offspring of given parentage a great biological law, when it really arises from the clubbing together of all offspring of given parentage *without regard to their earlier ancestry.* Given selected parentage and grandparentage alone, then with our present numerical values of the multiple correlation constants, it seems highly probable that the progeny of selected offspring would progress rather than regress on their parents and grandparents. In other words, given a line in which by chance or artificial selection there has been marked ancestry for two or three generations, and which is then isolated or inbred, there is reason to believe it would progress even beyond its ancestry rather than regress. Statistical investigations of heredity since 1889 seem to indicate a progressive evolution in selected lines, rather than a general regression to a population mean*. That would only arise from the far too frequent mating with mediocrity or worse than mediocrity. If Galton's misinterpretation of regression runs through *Natural Inheritance,* and makes him appeal to "sports" for evolutionary changes; if the reader is puzzled to know why Galton should study "variations proper," which according to him have no permanent value for evolution; still the book is a great book, for it applies a wholly new calculus—if one still in its infancy—to an important biological problem.

I think, however, that Galton fully grasped how much more important was his method than its special application. He writes that his conclusions

"depend on ideas that must first be well comprehended, which are now novel to the large majority of readers and unfamiliar to all. But those who care to brace themselves to a sustained effort, need not feel much regret that the road to be travelled over is indirect, and does not admit of being mapped beforehand in a way that they can clearly understand. It is full of interest of its own. It familiarises us with the measurement of variability, and with curious laws of chance that apply to a vast diversity of social subjects. This part of the inquiry may be said to run along a road on a high level, that affords wide views in unexpected directions, and from which easy descents may be made to totally different goals to those we have now to reach. I have a great subject to write upon, but feel keenly my literary incapacity to make it easily intelligible without sacrificing accuracy and thoroughness." (Chapter I, pp. 2–3.)

Galton in his Introductory Chapter states that there are three problems with which he will be principally concerned. The first problem is to determine how a population can, under the laws of heredity, keep stable from generation to generation. The second problem regards the *average* share contributed to the character in the offspring by each ancestor severally. The third problem is to measure numerically the nearness of kinship in

* There has always been this element of truth in Johansen's theory of "pure lines," that selected lines do not regress if they are isolated or inbred. The doubtful dogma of that theory is that exceptional members of a "pure line" are only "fluctuating variations," and so no further selection is of any value within a "pure line."

various degrees (pp. 1–2). Such are the three fundamentally novel problems which Galton set himself in *Natural Inheritance*; we shall endeavour to show the extent to which he has solved them, or at least has suggested methods of solving them, in the following discussion of that work.

Chapters II and III are general in character, expressing Galton's own views on heredity, and erring, if at all, in rather too much appeal to analogy. In the first of these chapters Galton states his opinion as to "natural" and "acquired" characters, indicating that he considers the inheritance of the latter extremely doubtful; he emphasises the importance of closely criticising the evidence offered in each case to prove the transmission of acquired faculties, citing especially the possibilities of intra-uterine influence*. He refers to the difficulty of combining male and female measures, and states that:

"Fortunately we are able to evade it altogether by using an artifice at the outset, else, looking back as I now can, from the stage which the reader will reach when he finishes this book, I hardly know how we should have succeeded in making a fair start. The artifice is never to deal with female measures as they are observed, but always to employ their male equivalents in place of them. I transmute all the observations of females before taking them in hand, and thenceforward am able to deal with them on equal terms with the observed male values." (p. 6.)

Galton for stature multiplied every female stature by 1·08 to reach its male equivalent, or added about one inch to every foot of female stature. He does not tell us how he demonstrated that equivalence, whether from the ratio of the mean values in men and women, or more adequately by finding it held (approximately) for all grades†. The true method is to reduce each deviation from the mean by dividing by its standard deviation, or other measure of variability, and it was an inspiration on Galton's part that led him to recognise that at any rate for the case of stature, the ratio of variabilities in male and female was close to the ratio of their mean values. See our p. 15 above.

On p. 7 Galton deals with what he terms *Particulate Inheritance*. He recognises that an individual may possess characters, which are known to have existed in an ancestor, but were not in the immediate parents. From this idea of latent characteristics Galton reaches the conception of inheritance in the individual as a "mosaic" of ancestral factors, and illustrates his views by two analogies, that of a builder's yard, with fragments of old buildings ready to be used again (p. 8), and the vegetations on two islands which spread to adjacent islets (pp. 10–12). I think he would have done better to have retained his earlier conception of the "stirp" (see our Vol. II,

* The complexity of this latter source must be borne in mind, if we can accept Galton's statements on pp. 15–16, that not a drop of blood passes from mother to child, and yet that a mother's system may be "drenched with alcohol and the unborn infant alcoholised" during all its intra-uterine existence.

† Probably in this latter way; see his p. 42, where he says we are to transmute female to male measures by comparing their respective "schemes," and devising a formula which will change one to the other. A "scheme," supposed normal, depends on two constants, the mean and the variability. Galton does not point this out, or state the inference which follows from his use of the factor 1·08.

p. 185), that is of the continuity of the germ-plasm. It is only in a figurative sense that we can look upon the inheritance of the individual as a mosaic, and speak of the contribution of an ancestor to the result. The individual is the product of the germ-plasms that go to his production, not of the individual ancestors. The study of the characters of the individual ancestors is only ancillary to a study of the possibilities of those germ-plasms. The correlation of a somatic character in a great grandparent, say, and great grandchild is not in any sense a real measure of what the former contributes to the latter, nor is the corresponding multiple regression coefficient such a measure. We are testing what *on the average* we can predict of the somatic characters of the offspring from a knowledge of what the germ-plasms of the "stirp" have produced in the past. In other words the term "contribution of an ancestor" should be interpreted as, or be replaced by, "contribution of the ancestor to the prediction formula." *It is in no sense a physical contribution to the germ-plasms on which the somatic characters of the offspring depend.* I do not think that anyone acquainted with the theory of multiple correlation would interpret the Law of Ancestral Heredity in any other sense; but Galton's use of the terms "particulate inheritance," "mosaic," "heritage from distant progenitors," must be admitted to be easily capable of misinterpretation.

Galton then deals with the "heritages that blend and those that are mutually exclusive," citing as an illustration of the former, skin-colour in crosses between white and negro, and of the latter eye-colour. He does not here, any more than in his fundamental paper on eye-colour (see our p. 34), explain for what reason he assumes the distribution of eye-colour in the array of offspring due to a definite ancestry will be in the same proportions as in the case of a blended character in an individual offspring. Galton concludes that:

"There are probably no heritages that perfectly blend, or that absolutely exclude one another, but all heritages have a tendency in one or the other direction, and the tendency is often a very strong one.... A peculiar interest attaches itself to mutually exclusive heritages, owing to the aid they must afford to the establishment of incipient races." (pp. 13–14.)

So far, however, as the struggle for existence and evolution are concerned, this last sentence must mean that a mosaic of the characters of two distinct races is for some environmental reasons more fitting than either pure race, and what is more, that the characters in the new mixed race will be stable and not segregate out again.

In the concluding paragraph we read:

"The incalculable number of petty accidents that concur to produce variability among brothers, makes it impossible to predict the exact qualities of any individual from hereditary data. But we may predict average results with great certainty, as will be seen further on, and we can also obtain precise information concerning the penumbra of uncertainty that attaches itself to single predictions. It would be premature to speak further of this at present; what has been said is enough to give a clue to the chief motive of this chapter. Its intention has been to show the large part that is always played by chance in the course of hereditary transmission, and to establish the importance of an intelligent use of the laws of chance and of the statistical methods that are based upon them, in expressing the conditions under which heredity acts." (pp. 16–17.)

Galton in his Chapter III deals with the theory of *Organic Stability*, illustrating it by the model of a polygonal slab, which has positions of stable equilibrium with various degrees of stability, i.e. which may require large or only small displacements to pass from one position of equilibrium to a second. He considers that his model (see Fig. 12) shows how the following conditions

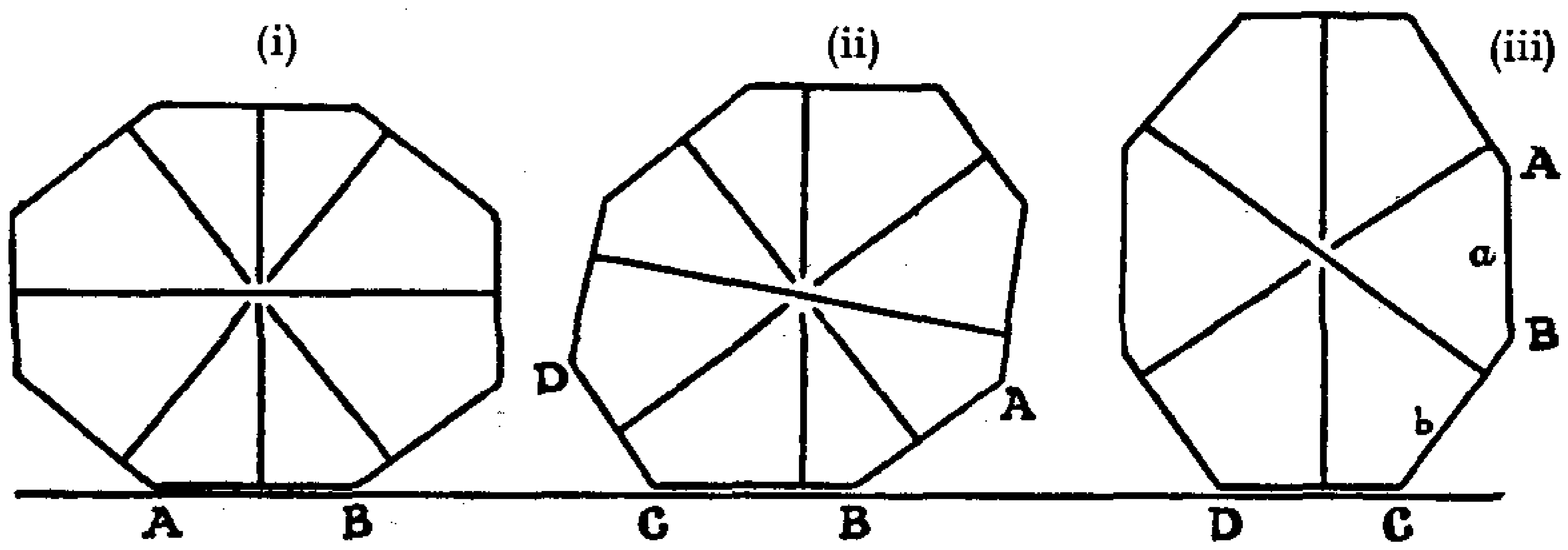


Fig. 12.

may co-exist: (1) Variability within narrow limits without prejudice to the purity of the breed (i); (2) Partly stable sub-types (ii); (3) Tendency, when much disturbed, to revert from a sub-type to an earlier form; (4) Occasional sports which may give rise to new types (iii) (pp. 27–30). Again the whole argument is one of analogy, and the reader may be pardoned a little vexation when he finds such important topics as the *Stability of Sports* and *Infertility of Mixed Types* only discussed (pp. 30–32) by reference to the analogy of hansom cabs and the impossibility of their useful blend with four-wheelers*!

The fact, I think, is that Galton's own ideas at this time were obscured by his belief that the ancestors actually did contribute to the heritage; he regarded the incipient structure of the new being to be the result of a clash of elements contributed from many ancestral sources, and the resulting building up out of more or less opposing elements of a particulate individual inheritance as the result of chance†. A further source of difficulty to Galton in his interpretation of hereditary phenomena lay in his mistake as to the nature of regression. This forced on him the conception of positions of stable equilibrium, each with its own centre of regression, and led him to the view that evolution must generally proceed by sports, and not by minute steps. It is true that on p. 32 he draws a distinction between the two views that the steps *may* be small and that they *must* be small, but as he has elsewhere applied his view of regression to indicate that small steps *cannot* be the source of evolution, the distinction is not really much of a concession (see our pp. 31–2). The following words of Galton deserve, however, to be quoted not only

* I find my copy of the *Natural Inheritance*, read and annotated forty years ago, defaced by many marginal notes expressing anger at Galton's analogies in this Chapter. But these notes were written before I had read and grasped the value of much of the later work in the book.

† Of course the Mendelian appeals to the same doctrine of chance to explain the variation in the members of an individual brood or litter, but he does so on the basis of homogeneous germ cells having a heterogeneous factor formula. I am inclined to believe that the germ cells of the same individual are not always and absolutely homogeneous, at any rate in the higher organisms, and that the clash of elements to be determined by chance need not lie in the factors of the formulae of the gametes, but in the fertilising germ cells themselves.

because they express his own strong convictions, but also because they may serve as a warning that we must appeal with caution to the continuity of the palaeontological record:

"An apparent ground for the common belief [that evolution proceeds by minute steps only*] is founded on the fact that whenever search is made for intermediate forms between widely divergent varieties, whether they be of plants or of animals, of weapons or utensils, of customs, religion or language, or of any other product of evolution, a long and orderly series can usually be made out, each member of which differs in an almost imperceptible degree from adjacent specimens. But it does not at all follow because these intermediate forms have been found to exist, that they are the very stages that were passed through in the course of evolution. Counter evidence exists in abundance, not only of the appearance of considerable sports, but of their remarkable stability in hereditary transmission. Many of the specimens of intermediate forms may have been unstable varieties, whose descendants had reverted; they might be looked upon as tentative and faltering steps taken along parallel courses of evolution and afterwards retraced. Affiliation from each generation to the next requires to be proved before any apparent line of descent can be accepted as the true one. The history of inventions fully illustrates this view. It is a most common experience that what an inventor knew to be original, and believed to be new, had been invented independently by others many times before, but had never become established. Even when it has new features, the inventor usually finds on consulting lists of patents, that other inventions closely border on his own. Yet we know that inventors often proceed by strides, their ideas originating in some sudden happy thought suggested by a chance occurrence, though their crude ideas may have to be laboriously worked out afterwards. If, however, all the varieties of any machine that had ever been invented, were collected and arranged in a museum in the apparent order of their evolution, each would differ so little from its neighbour as to suggest the fallacious inference that the successive inventors of that machine had progressed by means of a very large number of hardly discernible steps." (pp. 32–3.)

In concluding this chapter Galton apologises for largely using metaphor and analogy, on the ground that he wished to avoid any "entanglements with theory," as no complete theory of inheritance had yet been propounded that met with general acceptance (p. 34). This seems to me to show that Galton looked upon his statistical analysis of inheritance not as a theory of heredity, but as a description of hereditary facts, which it undoubtedly is.

Chapter IV deals with Galton's "ogive curve" (see our pp. 30–31) by which he represents a frequency distribution by aid of grades or percentiles. Galton had discussed this manner of representation in numerous earlier papers, and we may refer to Plate II for a graphic representation of his curve. The only novel point in Chapter IV is the suggestion, not very fully worked out, that the scheme of grades or percentiles might be applied to "inexact measures," i.e. to our present so-called "broad categories," and that these may be measures of a great variety of characters including relative professional success. He cites on this latter point Sir James Paget's analysis of the successes of 1000 of his pupils at St Bartholomew's Hospital. Sir James made five classes: (*a*) Distinguished, (*b*) Considerable, (*c*) Moderate, (*d*) Very limited success, and in the fifth class (*e*) he put Failures. Galton made the numbers in each 28, 80, 616, 151, and 125 respectively. Among the foremost were the three professors of anatomy in Cambridge, Edinburgh and

* It is a strange but widely spread notion that those who believe in continuous variation of a non-fluctuating character, must *ipso facto* suppose evolution to proceed by "minute steps." Given a race with mean cephalic index of 75 and a range in index from 65 to 85, there is nothing to prevent by isolation the establishment of a brachycephalic race of cephalic index 82—a spring as great as from Englishman to Jew—without transition through all the small intermediate steps from 75 to 82.

Oxford, and the three last were two men who committed suicide under circumstances of great disgrace and Palmer, the Rugeley murderer, who was hanged. There is possibly little knowledge to be obtained from the result for a single medical school, but comparative statistics for several would be of considerable value.

Chapter V deals with *Normal Variability*, and Galton shows how the distribution depends only on the two constants, the median and the quartile, and further that if two individuals whose grades are known be actually measured, then the median and quartile, and so the whole distribution of variation, can be discovered (p. 62, footnote, and cf. our Vol. II, p. 385). The origin of the normal distribution is illustrated mechanically by aid of the "quincunx" (see our pp. 9 and 10). Nor is Galton able to avoid becoming poetically enthusiastic in a paragraph headed *The Charms of Statistics*, for he writes:

"It is difficult to understand why statisticians commonly limit their inquiries to averages and do not revel in more comprehensive views. Their souls seem as dull to the charm of variety as that of the native of one of our flat English counties, whose retrospect of Switzerland was that, if its mountains could be thrown into its lakes, two nuisances would be got rid of at once. An average is but a solitary fact, whereas if a single other fact be added to it, an entire Normal Scheme, which nearly corresponds to the observed one, starts potentially into existence.

"Some people hate the very name of statistics, but I find them full of beauty and interest. Whenever they are not brutalised, but delicately handled by the higher methods, and are warily interpreted, their power of dealing with complicated phenomena is extraordinary. They are the only tools by which an opening can be cut through the formidable thicket of difficulties that bars the path of those who pursue the Science of Man." (pp. 62–63.)

Galton at the end of his Chapter V gives the two fundamental propositions on which his normal surface for the distribution of characters in two relatives depends. He envisages it in the following manner.

"(1) Bullets are fired by a man who aims at the centre of a target, which we will call its M, and we will suppose the marks that the bullets make to be painted red, for the sake of distinction. The system of lateral deviations of these red marks from the centre M will be approximately Normal, whose Q [Probable Error] we will call c. [This is the distribution of the first relative.] Then another man takes aim, not at the centre of the target, but at one or other of the red marks, selecting these at random. We will suppose his shots to be painted green. The lateral distance of any green shot from the red mark at which it was aimed will have a Probable Error, that we will call b. Now if the lateral distance of a particular green mark from M is given [a], what is the *most probable* distance from M of the red mark at which it was aimed? It is $\frac{c^2}{c^2+b^2}a$*.

"(2) What is the Probable Error of this determination? In other words, if estimates have been made for a great many distances founded upon the formula in (1), they would be correct on the average, though erroneous in particular cases. The errors thus made would form a normal system whose Q [Probable Error] it is desired to determine. Its value is $\frac{bc}{\sqrt{b^2+c^2}}$ †." (pp. 69–70.)

* Unfortunately Galton has the value $\sqrt{\frac{c^2}{c^2+b^2}}$, which is very liable to confuse the reader.

† In more modern notation, this may be looked upon as the variability of the array of the second relative $= c^2(1-r^2)$; therefore $r = \sqrt{c^2/(c^2+b^2)}$. Hence the regression of first relative on second relative $= rc/\sqrt{c^2+b^2} \times a = \frac{c^2}{c^2+b^2} \times a$. Again the variance of the difference in character between the two relatives $= c^2 + (c^2+b^2) - 2c\sqrt{c^2+b^2}\,r = b^2$, or b has for physical meaning the probable error of the distribution of the difference in character between the two relatives.

It was by the help of these propositions that Galton discussed the action of inheritance in stable populations. Assuming normal distribution of characters, as he did, then the above relations really involve the fundamental properties of bivariate regression, stated with a truly amazing minimum of algebra.

In Chapter VI Galton describes his data. After referring to the moth-breeding experiments then in progress, and to his much earlier experiments on the characters of sweet-peas, he passes to his *Records of Family Faculties* obtained by the offer of £500 in prizes. He obtained the records of 150 families, 70 by male and 80 by female recorders. The records contained data as to Stature, Eye-Colour, Temper, the Artistic Faculty, and some forms of Disease. As a measure of the amount of material thus obtained, we find 205 couples of parents and 930 adult children of both sexes. A further set of *Special Data* was obtained by circulars requesting measurements of the stature of pairs of brothers. The constants for this material differ considerably from those for the Family Records. I think Galton thought the former material more reliable, but in working through his data in 1895* I came to the conclusion that the Special Data, owing to the heterogeneity of their origin, were scarcely to be fully trusted.

The chapter on *Data* concludes with some account of Galton's work on the weight of sweet-pea seeds. He states that:

"The results were most satisfactory. They gave me two data, which were all that I wanted in order to understand, in its simplest form, the way in which one generation of a people is descended from a previous one; and thus I got at the heart of the problem at once." (p. 82.)

Galton had thus first learnt of the nature of regression in 1875 from his sweet-pea experiments. He gives in Appendix C, pp. 225–6, of the *Natural Inheritance*, the first correlation table for inheritance, that of the diameters of parental and filial plants. The regression is about $\frac{1}{3}$. I have drawn the regression line (see our p. 4). Galton also states that he had made confirmatory measurements on foliage and length of pod, but he does not enter into details.

Chapter VII contains the *Discussion of the Data of Stature*. This chapter covers the same ground as the papers dealt with in our pp. 11–20, but there is some amplification and some attempt to simplify the mathematical reasoning†. The table on p. 133 is, as I have indicated on our pp. 23–4, very doubtful as far as the numerical values are concerned. In particular Galton terms the mean *regression* w, and then says that the probable deviation of the regressed array is $p\sqrt{1-w^2}$, where p is the probable deviation of

* See *Phil. Trans.* Vol. 187, A, pp. 283–4.

† Certain corrections should be made. On p. 127, formula (2), there should be no radical before $c^2/(b^2+c^2)$. This is a relic of an error on p. 70, where $\frac{c^2}{c^2+b^2}a$ should be read for $\sqrt{\frac{c^2}{c^2+b^2}}$, see p. 224. The numerical value for b deduced from (2) is correct. On p. 128, the numerical value for b should be ·96 not ·98, and this value, ·96, should be inserted in the table on p. 129 instead of the 1·10 given under the (3) heading. The mean is then 1·03 instead of 1·06.

the population. This is not generally correct; Galton is confusing the regression coefficient with the correlation coefficient. As long as both relatives have equal variability, which we may suppose to be the case with father and son or uncle and nephew, the two coefficients are numerically equal; but when the two variates have not equal variability, this formula is of course incorrect. In the first entry in the table we have the regression of sons on midparent given as $\frac{2}{3}$, and Galton calculates from $p\sqrt{1-w^2}$ the probable deviation of the array of sons to be 1·27. The variability of midparents is, however, not equal to that of sons, but is in the ratio of 1 to $\sqrt{2}$; accordingly $r = w/\sqrt{2}$ must be used here instead of w, and the probable deviation of the array of sons is 1·50 and not 1·27.

Further the equality of the regressions of sons on midparents and of brothers on brothers is made by Galton to be $\frac{2}{3}$ in both cases. I think this value is too low in the case of midparents and too high in the case of brothers, the regressions being much more nearly in the ratio of 1·0 to 0·5 than in a ratio of equality. Other regressions entered in this table are very doubtful. We have to look upon the numerical values given as suggestions of the relative degrees of resemblance of various kinsmen, rather than conclusive values founded on observation of adequate numbers (see our pp. 23–4). The main result of Galton's work was to indicate the mechanism by which a population could remain stable notwithstanding variation and inheritance. It was a great direct achievement, and in the indirect light it cast on the general idea of correlation of still greater importance.

Chapter VIII contains the *Discussion of the Data of Eye-Colour.* This corresponds to the Royal Society paper, which I have already analysed on pp. 34–40 above. The same criticisms must be considered as still valid, and need not be repeated here.

Chapter IX deals with *The Artistic Faculty.* I do not think the contents of this chapter had been previously discussed by Galton. The data were deduced from the answers in *Records of Family Faculties* to the questions: "Favourite Pursuits and Interests?" and "Artistic Aptitudes?"

The object of this chapter is not to give a reply to the simple question, whether or no the Artistic Faculty tends to be inherited. A man must be very crotchety or very ignorant, who nowadays seriously doubts the inheritance either of this or of any other faculty*. The question is whether or no its inheritance follows a similar law to that which has been shown to govern Stature and Eye-Colour, and which has been worked out with some completeness in the foregoing chapters (p. 155). The conclusions

* It may be interesting with regard to these words to cite a few sentences from an obituary notice of Francis Galton which appeared in *Nature*, February 2, 1911 (Vol. LXXXV, p. 441). The writer says:

"Only once do I remember on a public occasion a slight severity in his usually gentle tone. A medical man of distinction [Dr Charles Mercier], speaking obviously without any knowledge of the literature of the subject, had asserted that the supposition that the children of parents with certain mental and moral peculiarities would reproduce these features, arose from a totally false conception of what the laws of heredity are. The mental and moral aptitudes were for the speaker outside the purview of hereditary investigation. Galton's reply was very simple: Much of what his critic had said 'might have been appropriately urged forty years ago, before accurate measurement of the statistical effects of heredity had been commenced, but it was quite obsolete now.'"

reached by Galton in this chapter are, I think, on the whole correct, but his handling of "broad categories" by means of percentages, in particular when no probable errors of the percentages are provided, is not to the modern statistician very conclusive. I think it would be labour well spent, should the opportunity arise, to work through his data afresh. Meanwhile we may arrange rather differently his tabulations and consider what flows from them. He tells us that he found it difficult to separate music from drawing, and finally classed both into a single group, the "artistic." Thinking also that parents were likely to overestimate the artistic capacity of young children, he excluded all but adults. Thus in the parental table the data chiefly refer to members of the second and third generations.

The first table I have deduced from Galton's data is that for Husband and Wife. It contains 894 couples and gives a percentage of 28 for males and of 32 for females with artistic temperament. The probable error of the difference 4 of these percentages is 1·46, or the difference is about 2·7 times its probable error, it may therefore be just significant. Galton concludes that:

> "Part of this female superiority is doubtless to be ascribed to the large share that music and drawing occupy in the education of women, and to the greater leisure that most girls have, or take, for amusing themselves. If the artistic gifts of men and women are naturally the same, as the experience of schools where music and drawing are taught apparently shows it to be, the small difference observed in favour of women in adult life would be a measure of the smallness of the effect of education compared with that of natural talent." (p. 156.)

I should not have thought the experience of art schools was in favour of the equality of artistic gifts in the two sexes. Galton's data really tell us nothing as to the grade of artistic faculty in the two sexes, as for this we require grouping in at least three categories. But my impression is that a larger proportion of the prizes and studentships for creative work still goes to the men, even in those schools where the women are in a majority.

Assortative Mating in Artistic Faculty.

		Husband		
		Artistic	Non-Artistic	Totals
Wife	Artistic ...	107 {80}	179 {206}	286
	Non-Artistic	143 {170}	465 {438}	608
	Totals	250	644	894

Assuming the artistic faculty to be a continuous normal variate, we find from the above table the coefficient of correlation between Husband and Wife to be no less than ·2418 ± ·0376. This value for the mating of like with like for a mental temperament is singularly in accord with the intensity of assortative mating for physical characters*. It denotes a resemblance between

* Stature, ·2804 ± ·0189; Span, ·1989 ± ·0204; Forearm, ·1977 ± ·0205. Health as measured by Duration of Life: Wensleydale and Wharfedale, ·2200 ± ·0244; Oxfordshire, ·2500 ± ·0211; Society of Friends, ·1999 ± ·0212. See *Biometrika*, Vol. II, pp. 373 and 487.

Husband and Wife as great as that between cousins. I have placed in brackets after the observed numbers those that would arise in each category if the mating were purely random. It will be seen at once that the tendency for like to marry like is increased at the expense of the unlike marriages. I fail to understand how Galton interpreted his percentages; naturally if like marries like above the random allotment, there must be a reduction in the marriages of unlike individuals, the random 42 °/ₒ of the latter being in fact reduced to 36 °/ₒ. Thus he writes:

"There is I think trustworthy evidence of the existence of some slight disinclination to marry within the same caste, for signs of it appear in each of the three sets of families with which the Table deals. The total result is that there are only 36 per cent. of such marriages observed, whereas if there had been no disinclination but perfect indifference, the number would have been raised to 42. The difference is small and the figures are few, but for the above reasons it is not likely to be fallacious. I believe the facts to be, that highly artistic people keep pretty much to themselves, but that the very much larger body of moderately artistic people do not. A man of highly artistic temperament must look upon those who are deficient in it, as barbarians; he would continually crave for a sympathy and response that such persons are incapable of giving. On the other hand, every quiet unmusical man must shrink a little from the idea of wedding himself to a grand piano in constant action, with its vocal and peculiar social accompaniments; but he might anticipate great pleasure in having a wife of a moderately artistic temperament who would give colour and variety to his prosaic life. On the other hand a sensitive and imaginative wife would be conscious of needing the aid of a husband who had enough plain common sense to restrain her too enthusiastic and frequently foolish projects*." (pp. 157–8.)

I have cited this passage, because, although it endeavours to explain a "slight disinclination to marry within the same caste," which Galton's data rightly interpreted show no evidence for, it yet throws light on some of his personal views of life. I can well picture what torture to him it would have been to be wedded to "a grand piano in constant action." While always exhibiting the best of old-fashioned courtesy to women, he had, when I first knew him, little belief in their intellectual strength; just as he held, that while women gifted with great physical strength existed, it was well for the repose of the other sex that they were rare (see our Vol. II, pp. 374–376). I think that later in life, when he came more in touch with academically trained women, and saw what work they could do on his own lines, his views suffered considerable modification. Again I am not content to pass without protest the rather sweeping statement that sensitive and imaginative persons, whether men or women, are apt to require restraining from "too enthusiastic and frequently foolish projects"; it denies that such persons often combine their sensitiveness and imagination with a rational power of control. It does not seem to me that the three factors, reason, sensitiveness and imagination, are incompatible, but that the success of truly great minds lies in the just combination of the three.

* Galton has written in pencil against this passage in his personal copy of *National Inheritance*, that it must be corrected, and I have also found some printed lists of *Errata*, in which the passage is stated to be incorrect. But none of the half-dozen copies I have examined of the work contains this *Errata* slip, and thus it is desirable to draw the attention of possible readers to a misinterpretation, which would certainly have been corrected in a second edition.

But let us return to less exciting questions. Galton does not, it is sad to record, classify his data in four fundamental parental tables, and till the material is reworked we must be content with the following arrangement.

Midparent and Child, Artistic Faculty.

		Both Parents Artistic	Only one Artistic	Neither Artistic	Totals
Child	Artistic ...	95	201	173	469
	Non-Artistic	53	319	666	1038
	Totals	148	520	839	1507

Unfortunately there is no distinction of sex in the offspring. Working out the correlation of this table in three different ways* I find the mean correlation coefficient to be ·4405 with a probable error of the order of ·024. There appears little doubt accordingly of the resemblance of offspring in artistic faculty to their parents, but the problem which Galton was investigating was not the existence of this resemblance, but whether its intensity might be taken as practically identical with those he had found for eye-colour and stature. The reader for whom the following remarks may be too technical is recommended to pass to the conclusions at the end of this paragraph. Galton assumes (i) equal inheritance from both parents, we will represent this by the correlation coefficient r; (ii) he does not correct by reducing female to male measure, we will suppose this done; (iii) he neglects the assortative mating, we will represent this by the correlation coefficient ϵ, in the present case this being equal to ·2418. The following results can be easily demonstrated:

$$(a)\quad \frac{\text{Variability of Midparent}}{\text{Variability of Offspring}} = \sqrt{\frac{1+\epsilon}{2}},$$

$$(b)\quad \text{Correlation of Offspring and Midparent} = \frac{r\sqrt{2}}{\sqrt{1+\epsilon}} = \cdot 4405,$$

$$(c)\quad \text{Regression of Offspring on Midparent} = \frac{2r}{1+\epsilon} = \frac{\sqrt{2}\times \cdot 4405}{\sqrt{1+\epsilon}},$$

or substituting the value of ϵ:

$$\text{Regression on Midparent} = \cdot 559 = \tfrac{2}{3} \times 0\cdot 84,$$

$$\text{Parental Correlation, } r = \cdot 3471 = \tfrac{1}{3} \times 1\cdot 04.$$

Now Galton deduced for regression of offspring on midparent for both stature and eye-colour the value $\frac{2}{3}$, and for parental correlation $\frac{1}{3}$. For the

* Treating the degrees of artistic faculty in the midparents as 1, 0·5, and 0, a biserial correlation coefficient after correction for class index gives ·4523 ± ·0138. The two possible divisions giving fourfold tables provide ·4655 ± ·0240 and ·4039 ± ·0298. The three results are thus in reasonable accord.

artistic faculty the former value is therefore 16 °/₀ in defect and for the latter value 4 °/₀ in excess. Galton, using what he admits to be a very crude method of percentages, shows that a regression of $\frac{2}{3}$ would give him:

Percentage of Artistic Offspring.

	Both Parents Artistic	One Parent Artistic	Neither Artistic
Theory ...	40 °/₀	38·5 °/₀	17 °/₀
Observation	36 °/₀	39 °/₀	21 °/₀

Observation differs by 10 °/₀ in the first case and 23·5 °/₀ in the last case from theory. Galton says that the first values are "in very happy agreement," that the second "agree excellently well" and that the third give "a very fair accordance," and concludes:

"that the same law of Regression, and all that depends upon it, which governs the inheritance both of Stature and Eye-colour, applies equally to the Artistic Faculty." (pp. 161–2.)

But if the best value we can find from Galton's data for the Regression differs 16 °/₀ from the value he assumes*, it is clear that we cannot assert that the accordance of percentages between theory and observation given in the above table justifies us in assuming on the present material that the Regression is the same for Artistic Faculty and Stature. Nevertheless while it may be impossible to accept on the basis of Francis Galton's data in *Natural Inheritance* that agreement between the constants for the inheritance of Stature and Artistic Faculty—that is between physical and psychical characters—which he thought he had found, we have yet to bear in mind two points: First that since 1889 more refined tools and better and more ample data have distinctly tended to confirm the equality of inheritance of mind and body; and secondly that Galton was foremost in the endeavour to obtain statistically a quantitative measure for the strength of resemblance in psychical characters.

Before we pass to the subject of Disease, it seems fitting here to note that Galton dealt with a second psychical characteristic, that of Temper. He refers to this on p. 85, where he deals with Marriage Selection, and also in Appendix D, pp. 226–238, which is a reprint with slight revision of a paper which first appeared in the *Fortnightly Review*, July, 1887†, under the title of "Good and Bad Temper in English Families." Galton found Temper in his Family Records described under 15 "Good" epithets and 46 "Bad" epithets, and he divided these into five classes, the first two corresponding to his "Good," and the remainder to his "Bad." These were

* If the midparental regression had been deduced from a correlation coefficient found in the ordinary way, its probable error would have been ·0096; the difference of ·667 and ·559 is ·108, more than 10 times that probable error. I think we must conclude that ·559 cannot be treated as $\frac{2}{3}$.

† Vol. XLII, N.S. pp. 21–30.

(i) Mild, (ii) Docile, (iii) Fretful, (iv) Violent, and (v) Masterful. I have already discussed this classification in Vol. II, p. 271. I would only add that if we follow Galton's classification of Good and Bad Temper, we find a slight *negative* correlation between Husband and Wife. If it be considered significant, the mild temper of one mate may be due to the experience that control is needful or at least advisable in the environment of a violent consort. On the other hand the fourfold table for siblings, i.e. offspring of the same parents, is:

Temper in Siblings.

		1st Sibling		
		Good Temper	Bad Temper	Totals
2nd Sibling	Good Temper	330 {264}	255 {321}	585
	Bad Temper	255 {321}	454 {388}	709
	Totals	585	709	1294

The numbers in curled brackets give the frequencies which would occur in each category on the basis of independent chance. It will be seen that observation shows a heaping up in the like categories at the expense of the unlike categories. The correlation coefficient is ·3167 for this fourfold table; there is thus a considerable degree of resemblance between the temper of siblings, but I believe this measure would be considerably increased if sullen and fiery tempers were not included in one group.

Chapter X deals with the subject of *Disease*. This is a most interesting and suggestive chapter, but the data were too sparse to provide definite conclusions of any kind.

Galton states (p. 165) first (by again appealing to an analogy!) his *Preliminary Problem*. We know, he tells us, the ages at death and the causes of death of the population as a whole. We know the proportions at each age of those who die of diseases *A*, *B*, *C*, etc. He would assume—which I think is somewhat doubtful—that the proportions of persons dying of these diseases at various ages in two successive generations are the same. If now we limit ourselves to persons dying at a certain age of disease *A*, how, if at all, does this affect the distribution of deaths from the diseases *A*, *B*, *C*, etc., in the offspring generation?

The problem is an exceedingly difficult, if an extraordinarily important one, for it requires an immense mass of data. In the first place the proportional death distribution is a function of social class, and of occupation; it is as we have seen a function of age; it influences fertility; and in more than one way is affected by sex*. Anyone who has seriously faced the problem, and seen the number of groups into which the various affecting factors compel him to sort the material, will recognise how hopelessly

* The male in many cases, as by foreign travel or by military or naval service, runs greater risks than the female.

inadequate must be the schedule-series which can be collected by any single investigator however energetic. Galton's deduced schedule extracted from his data was a good one, but I should like to see added two columns, one for occupations and one for domiciles, under which latter heading I understand such descriptions as "rural," "urban," "India," "Nigeria," etc., stating years of life in each domicile. I reproduce one of Galton's working schedules, in

Sample of one of Galton's Schedules for Heredity of Disease

Father's Name ....James Gladding
Mother's Maiden Name......Mary Claremont

Initials	Kin	Principal Illnesses and Ailments	Cause of Death	Age at Death
J.G.	Father	Bad rheum. fever; agonising headaches; diarrhœa; bronchitis; pleurisy	*Heart Disease*	54
R.G.	Brother	Rheum.; gout	*Apoplexy*	56
W.G.	Brother	Good health except gout; paralysis later	*Apoplexy*	83
F.L.	Sister	Rheum. fever; rheum. gout	*Apoplexy*	73
C.G.	Sister	Delicate (inoculated and died)	*Smallpox*	?
M.G.	Mother	Tendency to lung disease; biliousness; frequent heart attacks	*Heart Disease* and *Dropsy*	63
A.C.	Brother	Good health	*Accident*	46
W.C.	Brother	Led a wild life	*Premature Old Age*	62
E.C.	Brother	Always delicate	*Consumption*	19
F.R.	Sister	Smallpox three times	*General Failure*	85
R.N.	Sister	Bilious; weak health	*Cancer*	50
L.C.	Sister	?	*Fever*	21
	Offspring			
M.G.	Brother	Inflam. lungs; rheum. fever	*Heart Disease*	17
K.G.	Brother	Debility; heart disease; colds	*Consumption*	40
G.L.	Sister	Bad headaches; coughs; weak spine; hysteria; apoplexy	*Paralysis*	50
F.S.	Sister	Bad colds; inflam. lungs; hysteria	....	Living
R.F.	Sister	Infantile paralysis; colds; nervous depression	....	Living
L.G.	Sister	Inflam. brain, also lungs; neuralgia; nervous fever	....	Living
		Space left for remarks		

Suggested additions, columns for occupation and environment. Also transfer the word "Living" from "Age at Death" to "Cause of Death" column, and if living state age at time of record.

which he has changed the real names. It is of interest as showing a case in which inoculation was followed by smallpox and death, and a second case in which one person had smallpox three times, both being phenomena of which the possibility raised heated controversy in the 18th century.

Now if we remember that we can hardly form less than 15 principal disease groups nor fewer than 10 age groups, and that we have two parents, it will be seen that we require to divide our material to start with into 300 categories. It would be of little service, if we are to reach really definite conclusions, unless we had 50 to 100 parents in each of these groups, that is to say, records of 15,000 to 30,000 of the first generation, and it might be hoped five times as many in the second generation, a total say of 100,000 to 200,000 recorded deaths, and we must assume these cases to be in a fairly uniform social class and with a fairly like environment. Probably it would be best to work with one social class, and weed out cases having very differentiated environments. Galton had 160 usable family records, with an average of 75 individuals, so that he might hope to reach 12,000 records of disease and perhaps 6000 of deaths. Actually he had only about 2000 causes of death recorded, which might correspond to some 300 groups of two parents and five children. On the average this would give for each special age group and each special disease group about *two* parents of the same sex, mustering *ten* children of two sexes and all ages, from which to determine whether and to what extent a parent dying at a given age of a specific disease influences the offspring dying of that or other specific diseases at given ages*. The problem is one of probabilities and we shall not have data enough to answer it. Suppose a man to die of cancer between 65 and 75; then we may further be supposed to know the chance that a man of 35 to 45 will die of cancer, but how are we to determine, supposing the latter man is son of the former, whether the relationship in disease is one of chance or heredity? We can only do it, if we have enough pairs of fathers and sons like the above to calculate from the observed frequency the probability of sons dying round the given age of cancer, and to determine if it differs significantly from the probability of deaths in general from cancer of men between the ages of 35 to 45, whether they have or have not a cancerous parent. I have enlarged on these points, because the measurement of heredity in disease is a fundamental problem of eugenics, but its complexity in the general form is rarely recognised; the labour and great cost of such investigations are in most cases prohibitive. Galton spent £500 in getting his Family Records, but although inheritance of disease was to be an essential part of it, he obtained practically nothing of value. Thus he writes in this connection:

"I had hoped even to the last moment, that my collection of Family Records would have contributed in some small degree towards answering this question, but after many attempts I find them too fragmentary for the purpose. It was a necessary condition of success to have the complete life-histories of many Fraternities who were born some seventy or more years ago, that is during the earlier part of this century, as well as those of their parents and all their uncles and aunts. My

* It is most important to bear this age factor in mind, as the relative proportions of the diseases of which an individual may die vary in life from age to age.

records contain excellent material of a later date, that will be valuable in future years, but they must bide their time; they are insufficient for the period in question. By attempting to work with incomplete life-histories the risk of serious error is incurred." (pp. 166–7.)

And farther on Galton sees what is the kernel of the matter:

"*Data for Hereditary Diseases.* The knowledge of the officers of Insurance Companies as to the average value of unsound lives is by the confession of many of them far from being as exact as is desirable*....

"Considering the enormous money value concerned, it would seem well worth the while of the higher class of those offices to combine in order to obtain a collection of completed cases for at least two generations, or better still for three....They would have no perceptible effect on the future insurances of descendants of the families, even if these were identified, and they would lay the basis of a very much better knowledge of hereditary disease than we now possess, serving as a step for fresh departures. A main point is that the cases should not be picked and chosen to support any theory, but taken as they come to hand. There must be a vast amount of good material in existence at the command of the medical officers of Insurance Companies*. If it were combined and made freely accessible, it would give material for many years' work to competent statisticians, and would be certain, judging from all experience of a like kind, to lead to unexpected results." (pp. 185–6.)

Still from his "fragments" Galton drew certain suggestive conclusions. He tested the trustworthy character of his data by determining whether deaths due to cancer, consumption, drink and suicide appeared less frequently on his records than in ordinary tables of mortality and found that they did *not*. He concluded that his correspondents had entered with interest into what was asked for, and had freely trusted him with their family histories. Galton throws out a curious suggestion: Namely suppose that one parent has a disease A and the other a disease B; if the child inherits a tendency to both diseases, how far are they mutually exclusive, how far do they blend or how far does the blend change them into a third form of disease? I think, for example, there is evidence to show that such hereditary diseases as phthisis and rheumatism are largely antagonistic. What effect on offspring results from the marriage of mates from rheumatic and tuberculous stocks?

Galton considers that there was evidence in his records of two obvious hereditary tendencies in stocks, the one to disease and the other to the absence of

* It is worth noting that Mr W. P. Elderton (now Actuary and Manager of the Equitable Life Assurance Society) fifteen years later, speaking at a meeting of the Sociological Society where Galton had read a paper on Eugenics, made the following statement with regard to heredity of disease: "An important item in the study of heredity is the heredity of disease, and I think life assurance offices might be able to give useful statistics. When a person whose life is assured dies, a certificate of death is given to the office and is put away with the papers that were filled up when the assurance was taken out. These original papers state the causes of death of parents, brothers and sisters and their ages at death, or their ages if they were alive when the assurance was effected. These particulars give information for the study of heredity in relation to disease, and from the same source light might be thrown on a question of great importance —the correlation between specific disease and fertility. One point in conclusion. Dr Hutchinson spoke of the greater importance of environment, but in that he would hardly get actuaries to agree with him. Their observation, judged by life offices, experience and practice, would seem to show that environment operates merely as a modifying factor after heredity has done its work." *Sociological Papers*, 1904, Macmillan & Co., 1905, p. 62.

Fifteen years passed after Galton threw out the suggestion that material might be available in assurance offices, before an actuary told us it did actually exist. Twenty-five further years have rolled by and still nothing has been done!

disease. This seems to be confirmed by a strong inheritance of general physical health independent of any special disease, which has been established since Galton's inquiry. He purposely adopts in order to cover many popular expressions the term "consumption." But beside actual consumption he graded in three additional classes (for which he gives the rather vague descriptions used), the context of the record also being considered*. These are (i) Highly suspicious, (ii) Suspicious and (iii) Somewhat suspicious. He reckoned at the rate of 4 of (i) to three actually consumptive, 4 of (ii) to two actually consumptive and 4 of (iii) to one such. Dividing a total of consumptives thus formed by the total offspring he formed a ratio, which multiplied by 100 he termed the "consumptivity" of the fraternity. For example, in a fraternity of which one member was actually consumptive, two suspicious and four somewhat suspicious, Galton would reckon three consumptive members, and the taint, or consumptivity, would be 43 °/ₒ. To his surprise he found on making frequency distributions of consumptivity in fraternities, whether for one brother or one parent consumptive, that low and high degrees of consumptivity were both maxima, and moderate degrees gave a minimum or "anti-mode." Thus Galton, as far as I am aware, reached the first U-shaped distribution of frequency. He himself, notwithstanding his great belief in the normal curve, says it is not possible to torture the figures so as to make them yield the single-humped normal curve:

"They make a distinctly double-humped curve whose outline is no more like the normal curve than the back of a Bactrian camel is to that of an Arabian camel. Consumptive taints reckoned in this way are certainly not 'normally' distributed. They depend mainly on one or other of two groups of causes, one of which tends to cause complete immunity and the other to cause severe disease, and these two groups do not blend freely together. Consumption tends to be transmitted strongly or not at all, and in this respect it resembles the baleful influence ascribed to cousin marriages, which appears to be very small when statistically discussed, but of whose occasional severity most persons have observed examples." (pp. 175–6.)

Galton shows on pp. 177 and 179 by aid of very slender data, namely 14 fraternities with a "high" degree of consumption, which signified about 50 °/ₒ deaths from lung trouble, and nine fraternities severely affected as to the heart, that the parentages in the two cases were of a very different character. In the latter case there was practically no distinction between the diseases from which the father and mother died; in the former no more deaths than those of two fathers could be associated with lung trouble, while some nine mothers out of fourteen were consumptive. This led Galton to take the view that consumption, while partly due to the inheritance of a tuberculous diathesis, which may be transmitted equally by either parent, is also transmitted by infection, and that in this respect the mother is by her closer contact far more a source of infection than the father. Is this differential influence of parents for tuberculosis confirmed by later investigations? I have taken the unpublished results for some 400 phthisical patients in King Edward VII's Sanatorium, and classified their parents into definitely phthisical and "suspicious," where owing to mention of their ailments there was suspicion

* See his pp. 172–3. Something of the same kind is still undertaken by tuberculosis officers in grading the families of the admittedly tuberculous.

of lung trouble. In 413 cases where information as to the father was given, he was definitely phthisical in 7·02 °/₀ and there were suspicions of phthisis in 17·19 °/₀. In 420 cases of mother the corresponding numbers were 6·90 °/₀ and 13·81 °/₀. Thus Galton's view of the greater influence of the mother, whether by infection or by heredity, is not confirmed on large numbers*. Notwithstanding Galton's suggestion as to the fundamental part played by infecting mothers he proceeds on pp. 181–185 to discuss consumption on the basis of heredity. Although we may not feel this justifiable, his method is so suggestive and generally applicable that it must be discussed here. He starts by assuming that the distribution of resistance or immunity in the population may be supposed to have a normal distribution of mean M and—to use modern notation—a standard deviation σ. Now according to Galton's data 16 °/₀ of the deaths of his general population were from consumption, hence $M - \cdot 9945\sigma$ is the level of immunity at which consumption begins its ravages, and the mean immunity of those who die from consumption is $M - 1\cdot 5207\sigma$. But, if we accept Galton's figures for stature, the parental regression (and correlation) is $\frac{1}{3}$, or the marriage in which only one parent is consumptive gives rise to a "co-fraternity" (modern "array") of mean $M - \frac{1}{3} \times 1\cdot 5207\sigma = M - \cdot 5069\sigma$, with a variability or standard deviation of $\Sigma = \sigma\sqrt{1 - \frac{1}{9}} = \sigma \times \cdot 9428$. Accordingly the centre of this array is at a distance $\cdot 4876\sigma$ from the limit to immunity and the ratio of this to the standard deviation of the array $= \cdot 5172$. The table of the Probability Integral shows that this is only very slightly over 30 °/₀. Galton, disregarding the fact that by choosing his regression, he has *ipso facto* chosen the variability of his array, tries values for it which he thinks reasonable and which give him 31 °/₀, 29 °/₀ and 27 °/₀ of consumptives in the offspring of a consumptive parent. These are not far from the value 30 °/₀ we have obtained. Galton by his different methods obtained 26 °/₀ and 28 °/₀ of consumptive offspring of a consumptive parent, but this is only a minimum limit, as it does not appear that he confined himself to families all the members of which were already dead, or had passed practically through the age zone of really lethal tuberculosis. Of course the method supposes that within reasonable limits the degree of immunity of each individual remains constant, and that, within reasonable limits again, this degree of immunity is not affected by the size of the dose.

The importance of the method is greater than that of its application, which is rendered doubtful by the use of the special values, not confirmed, for stature, and by the fact that Galton had already attributed much of the result to infection. What, however, the method indicates is, that if we know the frequency of a particular type of disease in the community and its

* One curious result does seem to flow from my data. If we divide our patients into male and female, then of the 423 parents of the female subjects 8·75 °/₀ were definitely phthisical and 18·20 °/₀ were suspected; but of the 410 parents of male subjects only 5·12 °/₀ were definitely phthisical and 12·68 °/₀ suspected of phthisis. This suggests either that the parentage was more influential in the case of the female, or that women knew more or were less reserved than the men about the diseases of their parents.

frequency in the case of the offspring of a parent suffering from this disease, then by a series of approximations we can readily obtain the value of the correlation between offspring and parent, or the intensity of heredity in the case of that disease. Galton himself states:

"Too much stress must not be laid on this coincidence*, because many important points had to be slurred over, as already explained. Still, the *primâ facie* result is successful, and enables us to say that so far as this evidence goes, the statistical method we have employed in treating consumptivity seems correct, and that the law of heredity found to govern all the different faculties as yet examined, appears to govern that of consumptivity also, although the constants of the formula differ slightly." (p. 185.)

The penultimate chapter of *Natural Inheritance* is termed *Latent Elements*. The main point to which Galton appeals here is that the parents contributing on the average a definite amount to the heritage of a child, according to Galton each $\frac{1}{4}$, the residue of the stock of either parent can on the average only contribute a definite amount, i.e. $\frac{1}{4}$ on this view, to the child, or only $\frac{1}{4}$ of the characters of the ancestry can lie latent in the parent, and be contributed to the child. Galton argues that "either parent must contribute on the average only one quarter of the Latent Elements, the remainder of them dropping out and their breed becoming absolutely extinguished" (p. 188). He illustrates this by the selection of 13 out of a pack of 52 cards; any card may be chosen but actually 39 are rejected, yet if a great many sets of 13 are chosen, i.e. a great many individuals be taken, every card in the original pack will ultimately appear. "No given pair can possibly transmit the whole of their ancestral qualities; on the other hand there is probably no description of ancestor whose qualities have not been in some cases transmitted to a descendant" (p. 189). The throwing out of half the latent (as well as half the patent) elements at each crossing is really part of Galton's idea of all inheritance being particulate, a mosaic of ancestral characters. Even his idea of a blend is not a summation of continuous contributions, but a summation so to speak of quanta from individual ancestors.

In his next paragraph Galton deals with a *Pure Breed*, and again his error as to regression appears to come to the surface. He discovered regression simply as a statistical result, i.e. because he took parents of given characters, whose earlier ancestry might be anything whatever, he naturally found the offspring nearer than the parents to mediocrity. But unfortunately this idea of regression fixing itself in his mind became for him a biological fact, and he considered that he had discovered in stability of types, i.e. in groups each with their own focus or centre of regression, the source of evolution, or change from one type to a second. He now tells us that in the case of pure breed in which there has been long selection, the influence of a large quantity of mediocre ancestry would disappear, and so would the tendency to regression, except in so far as it is "connected with the stability of different types" (p. 189). In other words we have now *two* sources of regression, while Galton's original deduction of regression was purely statistical and depended

* That of the above percentages.

on the presence of the ancestral mediocrity. He does not state what experimental evidence there is for this other type of regression, and my impression is that it arose in his mind from a belief that regression was always in action and so evolution impossible by mere selection of continuous variations.

Under the same heading of *Pure Breed* Galton also considers the variation within a sibship or group of brethren. If, as he defines a pure breed, it be merely a line in which the ancestors have been given the same selection value for a large number of generations, then on Galton's theory of normal distribution of variates, the theory of multiple regression shows us that the variability will be the same within the sibship, whether the ancestry have been selected or not. Galton, to whom that theory was not familiar, deduces by a rough approximative method that the ratio of variability in the pure breed is to that in the mixed breed as ·98 to 1·00; but actually on his hypothesis they are equal; the variability of the sibship is independent of whether the characteristics of the progenitors are alike or unlike. Of course the reader must understand that by pure breed and mixed breed Galton is only referring to sibships which have their progenitors alike and their progenitors unlike in character respectively. All these progenitors are supposed of the same race, and he was not dealing with cross-breds, or mixture of races, in his "mixed" breeds.

In the final paragraph of this chapter Galton gives the results of his experience. He considers that for practical prediction you need to know not only the obvious somatic characters of the two parents, but the latent characters of their germ-plasms. These latter he considers can be respectively determined with a fair degree of approximation from the paternal and maternal uncles and aunts, if they exist in considerable numbers. Also what may be ascertainable of grandparents and their sibships will be of value. But he considers that if he were to start collecting family records again, he would limit himself to families having at least six adult children, and with as many members in both paternal and maternal sibships. There is much that is true in this view, yet at the same time, where a stirp occasionally throws a noteworthy individual, it may be doubted whether a sample of 12 in the first generation and six in the second is large enough to bring out all the latent possibilities which may be of importance. The desire of the Eugenist must always be for as complete a family pedigree as possible. It would not be feasible on a sample of 18 to say whether a single occurrence showed insanity to be a latent character of the stock or not.

Galton's final chapter contains a brief summary of the work, of which our present section is a more complete one. Only two points may be referred to. On p. 196 he writes:

> "There are no means of deducing the measure of fraternal variability [i.e. variability in the sibship from the same pair of parents] solely from that of the co-fraternal [i.e. the array of individuals who all have one parent of the same character value]. They differ by an element of which the value is thus far unknown."

We need no longer admit this ignorance. If R be the multiple correlation of an individual on all his ancestry, or on his "generant," then $\sigma\sqrt{1-R^2}$ is the variability of the sibship or fraternity proceeding from that generant, where σ is the standard deviation or variability of the general population. If r be the correlation of brothers in the ordinary sense, then $\sigma\sqrt{1-r^2}$ is the variability of an array or co-fraternity of brothers. The connecting link missed by Galton is: $R^2 = r$*.

The second topic is:

"the fundamental distinction that may exist between two couples whose personal faculties are naturally alike. If one of the couples consist of two gifted members of a poor stock, and the other of two ordinary members of a gifted stock, the difference between them will betray itself in their offspring. The children of the former will tend to regress; those of the latter will not. The value of a good stock to the well-being of future generations is therefore obvious, and it is well to recall attention to an early sign by which we may be assured that a new and gifted variety possesses the necessary stability to easily originate a new stock. It is the refusal to blend freely with other forms. Some among the members of the same fraternity might possess the characteristics in question with much completeness, and the remainder hardly or not at all." (pp. 197–8.)

It will be perceived from this paragraph that Galton does *not* hold the absence of regression in the "gifted" stock to be due to less mediocrity in the ancestry, but to the creation of a "new" stock by some trick of falling into a fresh position of stability, which enables the stock, at any rate in some of its members, to breed true. That is, he appeals to mutations for the source of "gifted" stocks.

Whether this be true or not, Galton I think reached his views owing to a misinterpretation of the statistical phenomena of regression. It was a misfortune that he really did not get beyond the idea of regression in two variates, because to be clear as to the true relation between his "mid-parental heredity" and his "Law of Ancestral Heredity" a knowledge of multiple regression is essential. But it was the greatest good fortune that he got as far as he did; he blazed the track, which many have followed since, and if he left unsolved or half-solved problems, his disciples ought to be grateful that the master has provided the problem as well as the tool, rather than be stern critics of his pioneer work†. *Natural Inheritance* is a great book even if it has its obvious blemishes.

The work concludes with the reproduction of tables from the memoirs on percentiles, on stature and on eye-colour, etc. Also with a series of Appendices. A gives particulars of Galton's own works and memoirs. B reprints Hamilton Dickson's paper (see our p. 12). C describes the experiments on sweet-peas, never fully dealt with. D reprints the *Fortnightly Review* paper on Temper (see our pp. 69–70 and Vol. II, p. 271). E reproduces Galton's paper on the Geometric Mean (see Vol. II, pp. 227–8). F reprints Galton and Watson on the Probable Extinction of Families (see Vol. II, pp. 341–343). G deals with the orderly arrangement of hereditary data, in particular with

* See *Biometrika*, Vol. XVII, p. 131.

† We have in a case in the *Galtoniana* of the Galton Laboratory the first map of Damaraland. Is it of less value because it is not an Ordnance map of what was once German South-West Africa?

the case of recording disease. Much of this the reader who wishes to go farther than our pages will find more easily here than in the original papers. Certain numerical misprints in the tables require that they should be carefully examined before use.

J. *Discontinuity in Evolution.* In 1894 appeared William Bateson's *Materials for the Study of Variation, treated with especial regard to Discontinuity in the Origin of Species.* One of the strange misconceptions with regard to Galton's views and to his work lies in the fact that he has been over and over again considered as the propounder of the view that evolution has taken place by the selection of slight or continuous variations. As a matter of fact Galton had for some years before the appearance of Bateson's book been preaching emphatically the doctrine of *discontinuity* in evolution. Indeed his opinions on the manner of evolution date back to 1872: see our Vol. II, pp. 84, 170–174 and 190. They are more clearly expressed in the preface to the 1892 reprint of *Hereditary Genius.* There we read:

> "Another topic would have been treated more at length if this book were rewritten—namely the distinction between variations and sports. It would even require a remodelling of much of the existing matter. The views I have been brought to entertain since it was written, are amplifications of those which are already put forward in pp. 354–5*, but insufficiently pushed there to their logical conclusion. They are that the word variation is used indiscriminately to express two fundamentally distinct conceptions: sports and variations properly so called. It has been shown in *Natural Inheritance* that the distribution of faculties in a population cannot possibly remain constant if on the average the children resemble their parents. If they do so the giants (in any mental or physical particular) would become more gigantic and the dwarfs more dwarfish, in each successive generation. The counteracting tendency is what I called 'regression.' The *filial* centre is not the same as the *parental* centre but it is nearer to mediocrity; it regresses towards the *racial* centre. In other words the filial centre (or the fraternal if we change the point of view) is always nearer, on the average, to the racial centre than the parental centre was. There must be an average 'regression' in passing from the parental to the filial centre." (pp. xvii–xviii.)

The flaw in Galton's argument is again one that we have had several times to notice, namely that he is overlooking the fact that he has clubbed together parents of all possible types of ancestry, and the "regression" of his sons is solely due to the large number of such parents who have sprung from an ancestry mediocre or below mediocrity. The amount of filial regression depends entirely on the amount of this mediocrity, and there will be no regression if two or three generations above the parents are of like deviation from mediocrity. Thus although it may still be a matter for experiment and discussion, whether evolution proceeds by variations proper or by sports, whether it be continuous or advance by jerks, the reason which made Galton the pioneer in advocating discontinuous evolution was a misinterpretation of his own discovery of "regression."

* These pages deal with Galton's idea of the stability of types: see our p. 61 and Vol. II, p. 113. It is quite reasonable to suppose that by successive selection of extreme variations proper we might reach a position of unstable equilibrium of the parts of an organism. But there does not exist experimental evidence at present to indicate that such instability would lead to a sport breeding truly rather than to non-viable forms of the organism. See our pp. 93–4.

This is expressed so definitely in the following paragraph that I must cite it:

"It is impossible briefly to give a full idea, in this place, either of the necessity or the proof of regression; they have been thoroughly discussed in the work in question*. Suffice it to say, that the result gives precision to the idea of a typical centre from which individual variations occur in accordance with the law of frequency, often to a small amount, more rarely to a larger one, very rarely indeed to one that is much larger, and practically never to one that is larger still†. The filial centre falls back further towards mediocrity in a constant proportion to the distance to which the parental centre has deviated from it whether the direction of the deviation be in excess or in deficiency. All true variations are (as I maintain) of this kind, and it is in consequence impossible that the natural qualities of a race may be permanently changed through the action of selection upon mere variations. The selection of the most serviceable *variations* cannot even produce any great degree of artificial and temporary improvement, because an equilibrium between deviation and regression will soon be reached, whereby the best of the offspring will cease to be better than their own sires and dams." (p. xviii.)

The flaw in the argument here is that Galton uses "filial centre" in *two* senses. In the first sense it refers to all the offspring of pairs of parents of the same character values, *whatever their parental ancestries may be.* Hence there must always be regression. In the second sense it is used of the offspring of an individual pair of parents, and interpreted to mean that the offspring of a given individual stock always regresses to the population mean, more and more in each generation. This is not true, the stock may with assortative mating even progress. The misfortune arose from Galton not having reached the formulae for multiple regression. Had he done so, he would have seen the contradiction between his "Law of Ancestral Heredity" and his interpretation of "Regression." Whether continuous or discontinuous evolution, or partly one and partly the other, expresses the truth, it is quite certain that Galton in 1892 supported evolution by mutations owing to an error of interpretation. His views on the subject undoubtedly contributed to directing attention to discontinuous evolution. He writes:

"The case is quite different in respect to what are technically known as 'sports.' In this a new character suddenly makes its appearance in a particular individual, causing him to differ distinctly from his parents and from others of his race. Such new characters are also found to be transmitted to descendants. Here there has been a change of typical centre, a new point of departure has somehow come into existence, towards which regression has henceforth to be measured and consequently a real step forward has been made in the course of evolution. When natural selection favours a particular sport, it works effectively towards the formation of a new species, but the favour that it simultaneously shows to mere variations seems to be thrown away, so far as that end is concerned. There may be entanglement between a sport and a variation which leads to a hybrid and unstable result, well exemplified in the imperfect character of the fusion of different human races. Here numerous pure specimens of their several ancestral types are apt to crop out, notwithstanding the intermixture by marriage that had been going on for many previous generations." (pp. xviii–xix.)

Unfortunately the only method of settling points of such fundamental importance—that of critical experiment—was not adopted‡. Some biologists

* [*Natural Inheritance*, 1889. See our pp. 57 and 65.]

† [This sentence is lacking in Galton's usual precision of statement.]

‡ Galton here first indicates what for years he believed to be the right experimental method for solving problems in heredity; his scheme, however, failed because he endeavoured to work

poured scorn on statistical methods even while they rejoiced in being ignorant of the mathematical processes, which would alone have enabled them to understand and criticise them effectively. Other biologists contented themselves with asserting that material collected by "non-biologists" could not possibly be of biological value. Many rash statements were made which would hardly now be maintained by the most ardent mutationist or Mendelian*. The controversy over Galton's method of dealing with heredity became a logomachy, or as some would say a tauromachy, and contributed little of permanent value to science. It was idle because the fundamental questions as to whether "variations proper" could serve as a basis for selection, and whether and to what extent sports bred true, were not investigated by agreed critical experiments. No one who has tried or even thought over such experimental work—bound to be of a secular nature—will be in the least likely to minimise the difficulty of devising and carrying through a crucial experiment. Nevertheless that was and remains the sole satisfactory method of settling a scientific dispute as to natural phenomena. The opinion that no real conclusion could be reached, except by direct experiment, was the actual reason why Galton's lieutenants ultimately retired from the controversy concerning the application of his methods to the measurement of heredity. Galton himself for another decade endeavoured to provide means for secular experimentation. What was the outcome of his attempts we shall see later on.

Again when Galton came to study finger prints, he was struck by the scarcity of transitional types; further his evidence indicated that there was little if any correlation between type and any bodily or mental characteristics, or that the types were peculiar to any human races.

"It would be absurd therefore to assert that in the struggle for existence, a person with, say, a loop on his right middle finger has a better chance of survival, or a better chance of early marriage, than one with an arch. Consequently genera and species are here seen to be formed without the slightest aid from either Natural or Sexual Selection, and these finger patterns are apparently the only peculiarity in which Panmixia, or the effect of promiscuous marriages, admits of being studied on a large scale. The result of Panmixia in finger markings corroborates the arguments I have used in *Natural Inheritance* and elsewhere, to show that 'organic stability' is the primary factor by which the distinctions between genera are maintained; consequently the progress of evolution is not a smooth and uniform progression, but one that proceeds by jerks, through successive 'sports' (as they are called), some of them implying considerable organic changes; and each in its turn being favoured by Natural Selection.

"The same word 'variation' has been indiscriminately applied to two very different conceptions, which ought to be clearly distinguished; the one is that of 'sports' just alluded to,

by a committee of incompatibles. I shall return to his attempts later, but their first foreshadowing appears in the 1892 preface to *Hereditary Genius*:

"It has occurred to others as well as myself, as to Mr Wallace and to Professor Romanes, that the time may have arrived when an institute for experiments on heredity might be established with advantage. A farm and garden of a very few acres, with varied exposure, and well supplied with water, placed under the charge of intelligent caretakers, supervised by a biologist, would afford the necessary basis for a great variety of research upon inexpensive animals and plants. The difficulty lies in the smallness of the number of competent persons who are actually engaged in hereditary inquiry, who could be depended upon to use it properly." (p. xix.)

* For example, that two-factor dominant and recessive Mendelian hypotheses would account for the heredity of coat-colour or eye-colour. Or that albinotic eyes were those without any granular pigment, and individuals possessing them would breed true.

which are changes in the position of organic stability, and may through the aid of Natural Selection, become fresh steps in the onward course of evolution; the other is that of the variations proper, which are merely strained conditions of a stable form of organisation, and not in any way an overthrow of them. Sports do not blend freely together; variations proper do so. Natural Selection acts upon variations proper, just as it does upon sports, by preserving the best to become parents, and eliminating the worst, but its action upon mere variations can, as I conceive, be of no permanent value to evolution, because there is a constant tendency to 'regress' towards the parental type. The amount and results of this tendency have been fully established in *Natural Inheritance.* It is there shown, that after a certain departure from the central typical form has been reached in any race, a further departure becomes impossible without the aid of these sports. In the successive generations of such a population, the average tendency of filial regression towards the racial centre must at length counterbalance the effects of filial dispersion; consequently the best of the produce cannot advance beyond the level already attained by the parents, the rest falling short of it in various degrees*."

The views of Galton here summarised show that the view he took in the *Natural Inheritance* of 1889†, that evolution was largely carried out by "sports" or in jerks, i.e. was chiefly discontinuous, was not the outcome of reading Bateson's work, although in that work he found support for his ideas. It will be seen at once also that he had divided, years before later controversies, "variations" into "sports"—now termed "mutations"—and "variations proper," which Galton held (and had indeed demonstrated) were inherited, but believed could not be of permanent value, because of what he termed the "constant tendency to regress." The fact that they are inherited distinguishes Galton's "variations proper," and very definitely distinguishes them, from the "fluctuating variations" of Mendelian writers, which are asserted by them to be non-inheritable. How far the theory of discontinuous variation—with all its contradiction in many cases of the palaeontological record‡—was really forced on the attention of biologists by Galton's writings it is not possible to say. We do know that both De Vries and Bateson were at one time enthusiastic students of Galton's works. However this may be, what is now clear is that there is no "unexpected law of universal regression" as Galton supposed, it is merely a misinterpretation of his own data and the constants based upon them.

It is important to examine this point, not only with regard to Galton's views on Discontinuity in Evolution, but also owing to the many biological misinterpretations of the statistical conception of regression. Galton found that the *average* value of the stature of sons of fathers having an excess h in stature above the population mean had only an excess of $\frac{1}{3}h$ above that same mean. Practically all his conclusions are based on this single fact and the statement that the array of such sons varies about this regressed mean with a variation about 6 °/。 less than the variation of the general population. The reduction of variation is so small, that it is possible practically to select sons of the same character deviation as their parents possessed. In order to

* *Finger Prints*, 1892, pp. 19–21.

† See Chapter III, "Organic Stability," and compare our pp. 60–62 above.

‡ "The distinctive feature of palaeontological evidence is that it covers the entire pedigree of variations, the rise of useful structures not only from their minute, apparently useless, condition, but from the period before they occur." HENRY F. OSBORN, 1889.

illustrate what Galton overlooked let us take his Ancestral Law coefficients as if they represented the absolute truth and investigate what would be the mean stature of sons if their parents and grandparents were by natural or artificial selection raised to a deviation h above the population value.

The mean of the sons would now be

$$\tfrac{1}{4}(h+h)+\tfrac{1}{16}(h+h+h+h)=\tfrac{3}{4}h,$$

the offspring have accordingly regressed $\tfrac{1}{4}h$ from the parental deviation. Now suppose selection to cease, and owing to isolation or other cause the offspring to interbreed; then their offspring will have the average value

$$\tfrac{1}{4}(\tfrac{3}{4}h+\tfrac{3}{4}h)+\tfrac{1}{16}(h+h+h+h)+\tfrac{1}{64}(h+h+h+h+h+h+h+h)$$
$$=\tfrac{3}{8}h+\tfrac{1}{4}h+\tfrac{1}{8}h=(\tfrac{3}{8}+\tfrac{1}{8})h+\tfrac{1}{4}h=\tfrac{3}{4}h.$$

In other words there is *no further regression*, or what these offspring lose in the regression of their parents *is compensated by the exceptionality of their grandparents*. Applying the formula once more we have for the offspring average

$$\tfrac{1}{4}(\tfrac{3}{4}h+\tfrac{3}{4}h)+\tfrac{1}{16}(\tfrac{3}{4}h+\tfrac{3}{4}h+\tfrac{3}{4}h+\tfrac{3}{4}h)+\tfrac{1}{64}(h+h+h+h+h+h+h+h)$$
$$+\tfrac{1}{256}(h+h+h+h+\text{to sixteen times})$$
$$=\tfrac{3}{8}h+\tfrac{3}{16}h+\tfrac{1}{8}h+\tfrac{1}{16}h=(\tfrac{3}{8}+\tfrac{1}{8})h+(\tfrac{3}{16}+\tfrac{1}{16})h=\tfrac{3}{4}h,$$

or the exceptional great grandparents make up for the loss of the regressed parents and the exceptional great great grandparents for the loss of the regressed grandparents; and so on. In other words there is no "unexpected law of universal regression." Regression in Galton's sense arises solely from the fact that by clubbing into a single array the offspring of all fathers of a given character deviation he has given them not only mothers whose average stature will be mediocre, but also a mediocre ancestry. But if there be isolation and inbreeding what Galton treated as a regression is a permanently progressed value for the offspring. Indeed if we continue to select, not with increased deviation, but with the same deviation (h), there is, so far from a regression, a continuous progression towards the selection value. For example if we select for

	1 generation	2 generations	3 generations	4 generations	5 generations
we progress:	$\tfrac{16}{32}h$,	$\tfrac{24}{32}h$,	$\tfrac{28}{32}h$,	$\tfrac{30}{32}h$,	$\tfrac{31}{32}h$,

and then inbreeding due to isolation or other cause after any one of these generations maintains the group at the progressed value.

Shortly there is no law of "universal regression," and we can deduce from Galton's own theory that his "variations proper," if selected and inbred, would establish a breed with a "new centre of regression." It is of course more than probable that our new centre of regression, i.e. the type of our new breed, may be unsuited to survive, that is to say in Galton's sense may be unstable. One cannot alter one character in an organism without modifying all the correlated characters, and some of those altered are likely to have survival value. But Galton's own data and Galton's own theory rightly interpreted lead to no "universal regression," still less to an argument that "variations proper" cannot be the subject of selection and the formation of new breeds.

This does not prove that "variations proper" have been the basis of evolution, but it removes Galton's chief reason for belief in evolution by discontinuity, that is by sports or mutations. The law of "universal regression"—over which Galton undoubtedly stumbled—is only true when we neglect ancestry beyond the parents and suppose mating at random, but these are not the conditions which exist when intense selection is taking place and the selected interbreed.

Having prefaced Galton's views on Discontinuity with some criticism, which I think is needful, of his theory of regression, we may turn to his paper on "Discontinuity in Evolution," which was published in *Mind*, Vol. III, N.S. pp. 362–372, July, 1894. Galton begins by saying that students of the laws of variation need not be disheartened by the impossibility of learning what is the cause of variation. Galton, who, as we have seen, believed in individuality in the numerous germ cells of an organism, and that germ cells were subject to selection, found no difficulty in attributing variation to the effect of interacting germinal elements*. He considered that the actual cause of any particular variation might be put on one side by those who study the degree and character of variation generally.

We are next provided with a definition of race based upon the idea of a typical centre of regression. As I understand him A and B are two different races, if the offspring of the members of A and the offspring of the members of B regress to two different centres of regression. But how can we practically demonstrate this? If we take the offspring of a pair of individuals of race A, the degree in which they differ from their parental mean will depend upon the long line of ancestry of those parents (to adopt Galton's own views); if the parents were relatively small in stature, say, for their ancestry, the offspring average may exceed the parental stature; if they were relatively tall, the offspring average may fall short of the parental. If we choose such a large number of parents of given statures, that we may assume the ancestors of the parents have for average value that of the general population, then the offspring average will regress to the population mean, and should we know the regression coefficient accurately, this will provide the population mean or "typical centre of regression." Similarly we might determine the typical centre of the race B, and ascertain whether the two centres were or were not significantly different. But I cannot see that this is any more than inquiring whether the populations A and B have different

* I must confess to feeling it extremely difficult to accept the view that the population of germ cells belonging to an individual organism are like atoms, identical in character, and have a germinal capacity defined by absolutely the same formula. Such a population of germ cells is, if parasitical, still an organic population, and one continually in a state of reproduction and change. No other organic population that we know of is without variation among its members, and I find it extraordinarily hard to believe that it is a matter of complete indifference which individual spermatogonium of an organism is the ultimate source for fertilisation of an individual ovum of a second organism.

means, and we might proceed at once to do this without introducing at all the ideas of inheritance and regression. Galton's definition might be of service if we could determine from the regression of the offspring of a single pair of parents, or a few pairs, the typical centre, but this is no more feasible than to determine from a few individuals the population mean; the very backbone of Galton's conception of parental regression is that the ancestors of the parents cover all the possible pairs in the community, or are on the average mediocre.

Having defined his races A and B to be those having different centres of regression, which if the races are stable simply connotes different population means, Galton concludes that if A and B are stable then intermediate types are less stable. I think this is only a theory, not necessarily a demonstrable fact. It may be that races A and B have not diverged from a common ancestral race C by continuous variation, but there is nothing in Galton's theory of regression to prevent A and B arising from C or even A from B by continuous variation. The idea of "stability" as a source of organic evolution is one that Galton was very fond of; when a race has been largely selected, it topples over, so to speak, into a new form of organic equilibrium with a new centre of regression. In this way Galton would account for "sports" and the prepotency and permanency of certain sports, and he considers that *most* breeds have arisen from sports. He then refers to various kinds of sports as in peacocks, peaches, and the appearance of remarkable intellectual gifts in man. Under the latter category he cites Sebastian Bach. "Can anybody believe that the modern appearance in a family of a great musician is other than a sport?" (p. 368). He also refers to Inaudi the mental arithmetician, who started as an illiterate Piedmontese boy. In the latter case, however, the Inaudi stock may well have possessed similar, if less intense powers which were never called into activity, while in the former case we now possess the pedigree of the Bach family, and their remarkable musical power is certified for five or six generations. All variation is discontinuous when examined in small groups such as families, and the extreme deviations in such small groups may be easily interpreted as sports. Newton again may well have been a sport, but till we know more than we do at present of his mother's ancestry, it is hardly wise to hold that he was such. Nor again if some of these men are to be considered "sports," can we dogmatically assert that they might, like the "japanned" or black-shouldered peacocks, have produced offspring regressing to a new typical centre.

"The phrase organic stability must not as yet be taken to connote more than it actually denotes. Thus far it has been merely used to express the well-substantiated fact that a race does sometimes abruptly produce individuals who have a distinctly different typical centre, in the sense in which those words were defined. The inference or connotation is that no variation can establish itself unless it be of the character of a sport, that is, by a leap from one position of organic stability to another, or as we may phrase it, through '*transilient*' variation. If there be no such leap the variation is, so to speak, a mere bend or divergence from the parent form, towards which the offspring in the next generation will tend to regress; it may therefore be called a '*divergent*' variation. Thus the unqualified word variation comprises and confuses what I maintain to be two fundamentally different processes, that of transilience and that of divergence, and its use destroys the possibility of reasoning correctly in not a few important matters. The interval leapt over in a transilience may be at least as large as it has been in any hitherto observed instance, and it may be smaller in any less degree. Still whether it has been large or small, a leap has taken place into a new position of stability. I am unable to conceive the possibility of evolutionary progress except by transilience, for if they were merely divergences, each subsequent generation would tend to regress backwards towards the typical centre, and the advance that had been made would be temporary and could not be maintained." (p. 368.)

We see that Galton only differed from the mutationists by supposing that their not-inherited "fluctuating variations" were really inherited, although they were of no permanent account, being rendered nugatory by his principle of regression. In view of the inheritance he had found for grades of stature, he could hardly hold otherwise than to suppose them inherited, but he coupled this inheritance with a misinterpretation as I have shown of his own statistical theory of regression, which left him practically in the ranks of the mutationists—a strangely inconsistent position for one who has been looked upon as the founder of the Biometric School! A little farther on Galton writes:

"These briefly are the views that I have put forward in various publications during recent years, but all along I seemed to have spoken to empty air. I never heard nor have I read any criticism of them, and I believed they had passed unheeded and that my opinion was in a minority of one. It was, therefore, with the utmost pleasure that I read Mr Bateson's work bearing the happy phrase in its title of 'discontinuous variation,' and rich with many original remarks and not a few trenchant expressions....It does not seem to me by any means so certain as is commonly supposed by the scientific men of the present time, that our evolution from a brute ancestry was through a series of severally imperceptible advances. Neither does it seem by any means certain that humanity must linger for an extremely long time at or about its present unsatisfactory level. As a matter of fact, the Greek race of the classical times has surpassed in natural faculty all other races before or since*, and some future race may be at least the equal of the Greek, while it is reasonable to hope that when the power of heredity and the importance of preserving valuable 'transiliences' shall have been generally recognised, effective efforts will be made to preserve them." (pp. 369 and 372.)

What direction those "effective efforts" should take Galton does not indicate. He tells us that human sports of considerable magnitude in both the moral and intellectual fields assuredly occur. But when we face the question of increasing the number of their offspring we soon recognise that endowment of parenthood will achieve little in the case of a rare mutation; we find ourselves led into the thorny field of speculating on the eugenic as distinguished from the social value of monogamy and on the possible utility of endogamy in the perpetuation of human sports. The elimination of animal passions still strong in man would have to be carried much farther than it has yet been, before the tribal customs as to marriage and family life could be safely called into question even in the case of individuals of surpassing intellectual or moral eminence, were it feasible, indeed, to determine such individuals with anything like unanimity. The difficulty of the problem should not discourage all consideration of it, for it is clearly fundamental if we are consciously to use heredity to elevate mankind; on the other hand the very difficulty of the problem forbids hasty solutions being adopted and proclaimed as essentials of the eugenic programme†.

* [See, however, my remarks, Vol. II, pp. 107–109.]

† See on this point, *The Times* (December 31st) report of a discussion under the auspices of the Eugenics Society at the Educational Congress on December 30, 1927.

Galton's interest in Discontinuous Evolution was further manifested in the same year by a circular which will be found in the *Transactions of the Entomological Society of London*, 1895 (April 3rd). It consists of three questions addressed to breeders and others, not only entomologists but to those who pursue any branch of natural history. The questions are for information on the following topics:

"(i) Instances of such strongly-marked peculiarities, whether in form, in colour, or in habit, as have occasionally appeared in a single or in a few individuals among a brood; but no record is wanted of monstrosities, or of such other characteristics as are clearly inconsistent with health and vigour.

"(ii) Instances in which any one of the above peculiarities has appeared in the broods of different parents. [In replying to this question, it will be hardly worth while to record the sudden appearance of either albinism or melanism, as both are well known to be of frequent occurrence.]

"(iii) Instances in which any of these peculiarly characterised individuals have transmitted their peculiarities, hereditarily, to one or more generations. Especial mention should be made whether the peculiarity was in any case transmitted in all its original intensity, and numerical data would be particularly acceptable that showed the frequency of transmission: (*a*) in an undiluted form, (*b*) in one that was more or less diluted, and (*c*) of its non-transmission in any perceptible degree."

The context attached to the questions shows that Galton was still troubled by the question of regression: "Regressiveness and stability are contrasted conditions and neither of them can be fully understood apart from the other." As I have endeavoured to indicate regression is merely a statistical result, which holds for a population, not for an individual, when we table the former with a knowledge of only a limited number of the kinsfolk of individuals and *assume the mean of each generation to remain the same**. The biological problem is to determine how this mean changes and is quite independent of the statistical idea of regression. As I have indicated above (p. 83) the offspring of selected ancestry on Galton's own theory do *not* regress to the population mean, and in this respect the only contrast that could be drawn between the offspring of a "sport" and of such selected ancestry is the question of the extent to which a sport breeds true without having even a limited amount of selected ancestry. This is really the point which Galton's third question would tend to answer†.

K. *Eugenics as a Religious Faith.* I have already pointed out that a very fundamental characteristic of Galton's mind was his desire that our progressive knowledge of natural law should at once be turned to practical service in attempts to elevate the race of man. He could not think of the doctrine of

* This assumption is made by Galton, but it is not in the least needful to the statistical theory of regression, which measures each generation from its own mean.

† I am not certain whether it was in reply to this circular that Galton received information about a singular family of lunatic cats. He described the family in a letter to *The Spectator* (April 11, 1896), entitled: *Three Generations of Lunatic Cats.* The sires of the kittens were unknown, but may be assumed to have been normal. Nevertheless the lunacy, which may be considered as a sport, was transmitted by the mother to all her offspring and grandchildren with undiluted strength. The only doubt that can be raised is whether the sire of "Phyllis," who was brought from Ewart Park, Northumberland, might possibly have been a wild cat. It is a pity the family could not have been preserved for the study of hereditary lunacy.

evolution merely as a contribution to academic biology; for him the type of "sport" of greatest interest and value was that embracing the human moral or intellectual "sports," and he desired at once to know how we might perpetuate for the service of mankind such supermen as might appear. Evolution according to him was providing for the survival of the physically and mentally more vigorous members of the race, and he desired to see this achieved with greater rapidity and less pain to the individual. In 1894 a book entitled *Social Evolution*, written by Benjamin Kidd, was published, and created for a time some stir as dealing with the relation of supernatural, or at least ultra-rational religion to the social evolution of man. It was not written from the scientific standpoint and contained little of permanent value. It led Galton, however, to publish a rather remarkable article on "The Part of Religion in Human Evolution*." Kidd's thesis may be briefly summarised as follows: Intra-group struggle for existence is the *sine quâ non* of social progress; this beneficent working of the struggle for existence is so painful to existing men that they would not, if they were rationalists, pay the price for it; to check the anti-social and anti-evolutionary force of reason religion has been evolved to provide an ultra-rational sanction for moral conduct.

Galton starts his paper by suggesting that superstitions in barbaric times, such illusions as totems, tutelar deities, and we may add tribal and national gods, gave cohesive force and compactness to a group and tended to render it successful against other groups, which on rational grounds had begun to question such illusions. Galton recognises the important part religion may play in determining national stability. Even after men of education have realised the irrationality of a national creed, it may be unwise precipitately to destroy it.

> "The social system of every nation, including its religion, whatever that may be, has adjusted itself into a position of stability which is dangerous to disturb. Deep sentiments and prejudices, habits and customs, all more or less entwined with the established religion of each nation, are elements of primary importance to its social fabric. It is true that vast changes become obvious in the social system of every progressive people, whenever its habits and customs at one period are compared with those of another long after, but, as a rule, the changes are piecemeal. Each change is primarily confined to a single part, the remainder adapting itself to the new condition with a comparatively small shift of the position of the centre. Commonsense teaches how much can be thus done with safety at any given time. Great and sudden changes in religion are hardly to be attempted except when the stability of the existing system is tottering and on the point of falling." (p. 758.)

Whatever views we may take about religion, whether we regard it as a supernatural revelation or not, we can agree that one of its chief functions is to curb selfishness in the individual, to inculcate altruism, and by restraining human passions to help the stabilisation of society. With this end in view religion from the earliest times has been the guardian of tribal custom in regard to marriage, birth and death. It has therefore concerned itself with matters which from our present knowledge of the laws of natural selection and heredity we recognise as bearing on human evolution. It is impossible—

* *The National Review*, August 1894, pp. 755–765.

and this the Church is now beginning to recognise—to place the scientific doctrine of evolution and the moral conduct of man as inspired by religious belief in separate idea-tight compartments. Slowly, but nevertheless surely, this aspect of religion is taking possession of the minds of the more thoughtful clergy. It has long been seen by many men of science that it formed the most hopeful field for co-operation between the old supernaturalism and the new scientific knowledge. It is from this conception that Galton, as an agnostic, starts to bring religion into touch as a living force with our belief in human evolution.

Galton cites three definitions of religion. (*A*) that of John Stuart Mill: The essence of Religion is the direction of the emotions and desires towards an ideal object, recognised as rightly paramount over all selfish objects of desire. (*B*) that of Kant: Religion consists in our recognising all our duties as Divine commands. And (*C*) that of Gruppe: A belief in a State or Being which, properly speaking, lies outside the sphere of human striving or attainment, but which can be brought into this sphere in a particular way, namely by sacrifices, ceremonies, prayers, penances and self-denial.

Gruppe's definition is historical, indicating religion only by its past outward and nigh outworn forms. Kant's definition of religion as a recognition of supernatural sanction for *all* duties is too narrow in its sanction and too wide in its duties*; it demands also a continuous revelation as duties continuously change with human progress. It was not unnatural therefore that Galton selected Mill's definition of religion. He points out that any guiding idea that takes passionate possession of the mind of a person or a people is an adequate adversary to purely selfish considerations, and that such would be religious in Mill's sense but not in that of Kant or Gruppe.

> "Many of the ordinary emotions which influence conduct admit of being excited to so high a pitch that the merely self-regarding feelings do not attempt to withstand them, but yield themselves unresistingly to be sacrificed to the furtherance of a cause. That the emotions can be so excited, whether in a party or in a nation, easily and often irrationally, is one of the common teachings of history." (p. 757.)

No supernatural command or sentiment is needful. Religious enthusiasms in the sense of Kant or Gruppe may give great help, but they are not indispensable.

> "The ambitions, loves, jealousies, and hates of nations, families, and persons, seem fully strong enough to force men who are under their influence, to disregard what is commonly understood by the phrase selfish desires." (p. 758.)

Galton, under a conviction of its truth, then makes the following affirmation:

> "The direction of the emotions and desires towards the furtherance of human evolution, recognized as rightly paramount over all objects of selfish desire, justly merits the name of a religion." (p. 758.)

* To render unto Caesar what is Caesar's may be the dictate of a great religious teacher, but is scarcely a *religious* duty—even if Caesar be not a foreign war-lord!

Thus, I think, he sympathises with the Victorian scientific criticism of religion as defined by Kant and Gruppe, but he desires to see a religion in Mill's sense built up to replace the formal religions. He holds that:

"the destructive task is a necessary though painful preliminary, because until obstructions are thoroughly cleared away, and the view is quite open, the character and exigencies of the vacant space cannot be rightly understood, nor can a judgment be formed as to how far and in what way rebuilding is needful. It is also pardonable enough that the work of destruction should be over zealously indulged in by some who have long chafed under what they consider to be the irrationality of one or other of the many conflicting creeds.

"All earnest inquirers recognize the awful mysteries that surround human life, but they are angered by theosophies that attempt to solve part of the problems by means of hypotheses that are improbable in themselves, while they introduce gratuitous complications. For instance if we strip from Milton's fable and from the *dramatis personae* of *Paradise Lost* all the glamour thrown over them by his superb diction, a grotesquely absurd framework remains behind. His high undertaking to justify the ways of God to man becomes ludicrously inadequate. The same spirit under another guise that moved our ancestors in the days of the Reformation to shatter the authority of Rome, is abroad again but is now directed against the dogmas of the time. The spirit is that of a determination to face and view the grand and terrible problem of life in the clear light of day, and not through artificial mediums that partly hide, partly colour and partly refract it." (pp. 758–9.)

Galton, while desiring a reformation of religion in the sense of Erasmus, was perfectly conscious that the bulk of our people, who may be weary of the old superstitions, are in such a backward state that they will be more ready to accept new superstitions* than to seek a rational basis for a national religion. Granted the discredit of the long accepted ultra-rational faith, granted that a nation "be suffering in a still more acute form than our own from poverty, toil, and an unduly large contingent of the weakly, the inefficient, and the born-criminal classes, and that the existing social arrangements are acknowledged to be failures," what will follow? Galton held that socialistic experiments on various scales and in various ways will be largely tried and will be admitted to be ineffective *owing to the moral and intellectual incompetence of the average citizen*†.

"There would then be a widely-felt sense of despair; there would be ominous signs of approaching anarchy and of ruin impending over the nation, while a bitter cry would arise for light and leading. A state of things like this is by no means impossible in the near future, even here in England, and therefore, it deserves some consideration as being something more than a merely academic question. In the imagined event, preachers of all sorts of nostrums would abound, mostly fanatics who could see only one side of a question, and on that account they would be all the more earnest in their opinions and persuasive to the multitude." (p. 759.)

Thus Galton uses the probable ineffectiveness of socialistic experiments as an argument in favour of the acceptance of eugenics as a social and at

* Salvationism, Spiritualism, Theosophy, Christian Science, to say nothing of the resurrection of the urge, dating from the Neolithic period, to the sacrifice of the people's deity or its totem, and a communal feast on the remains.

† I do not remember any other reference of Galton's to Socialism. The present passage indicates that it was not in its ideas antipathetic to him, but that he conceived it would fail owing to the moral and mental feebleness of the average citizen. The present experiments in Russia and China will serve to test his opinions in the eyes of sympathetic onlookers. Their failure would convince men according to Galton that racial progress in the eugenic direction must precede or accompany social reconstruction.

the same time a religious programme for the nation. He puts into the mouth of a supposed agnostic and "somewhat fanatic" preacher opinions which were undoubtedly his own—even though he states that, with much sympathy for them, he would not commit himself to them without serious reservations the statement of which "would merely distract the argument*." It would indeed be a loss, if Galton's views thus boldly expressed should perish in an ephemeral review article. He himself has added in a list of his papers in my possession a note to the effect that this article suggests Eugenics—although that name is not mentioned—as a religion in accordance with our modern views on human evolution. We note here the beginning of Galton's last period in which he devoted his activities to eugenic propaganda. I cite the following characteristic passages:

"The mystery is unfathomed as to whence the life of each man came, whether it pre-existed in any form or not. The mystery is equally great as to what will become of his life after the death of the body; whether it will be perpetuated in a detached form as some creeds say, whether it will be absorbed into an unlimited sea of existence, as other creeds assert, or whether it will cease entirely. As regards this life, there are also mysteries. Every act may or may not have been determined by previous conditions, but man has the sense of being free and responsible: he is accustomed to do and to be done by as if he were so, therefore we may provisionally believe that he is free and should act on that supposition. There is a further mystery as regards the cosmic conditions under which we live, for no assurance can as yet be obtained of any supernatural guidance, the facts alleged in evidence of its existence being more than counterbalanced by those that point the other way. We cannot, in consequence, tell with certainty whether human life is subject to an autocracy, or whether, at least for practical purposes, it exists as an isolated republic; but the latter appears at present to be the more probable, and should, therefore, guide our conduct. Each man's destiny during his life may then be viewed with propriety as depending entirely on his own physiological peculiarities and on his surroundings. He has, consequently, to conduct himself as a member of a free executive committee during his brief life, guiding his actions by whatever he can learn of the tendencies of the cosmos, in order to co-operate intelligently with what he cannot in the long-run resist. The sense of responsibility that is imposed by this view would sober, brace, and strengthen the character, just as that of dependence on an autocratic power effeminates and enfeebles it....

"On the foregoing basis our agnostic might say: 'Let us consider what is peculiarly profitable and proper for man to attempt. One of the most prominent conditions to which life has been hitherto subject, is the newly discovered law of the survival of the fittest, whose blind action results in the progressive production of more and more vigorous animals. Any action that causes the breed or nature of man to become more vigorous than it was in former generations is therefore accordant with the *process* of the cosmos, or, if we cling to teleological ideas, we should say with its *purpose*.

"It has now become a serious necessity to better the breed of the human race. The average citizen is too base for the every day work of modern civilization. Civilized man has become possessed of vaster powers than in old times for good or ill, but has made no corresponding advance in wits and goodness to enable him to direct his conduct rightly. It would not require much to raise the natural qualities of the nation high enough to render some few Utopian schemes feasible that are necessarily failures now. Conceive, for the sake of argument, the nation to be divided in the imagination into three equal groups L, M, N, in order of their natural civic capacities. At present the production of the forthcoming generation is chiefly effected by L and M, the lowest and the middle; if it were hereafter effected by M and N, the middle and the

* What these reservations may be we do not know, they probably related to evolutionary, as opposed to revolutionary change. The opinions of the "supposed agnostic" are so akin to those which Galton has himself expressed in other passages, but never more briefly or forcibly, that we may well be certain they are really his own.

highest, a distinct gain would be achieved in the lifetime of many of those who initiated the reform, for it is probable that the inefficient multitude of weaklings in brain, character, and physique would be sensibly diminished in thirty years.

"Our agnostic preacher might go on to say that this terrible question of over-population and of the birth of children who will necessarily (in a statistical sense) grow into feeble and worse than useless citizens must be summarily stopped, cost what it may. The nation is starved and crowded out of the conditions needed for healthy life by the pressure of a huge contingent of born weaklings and criminals. We of the living generation are dispensers of the natural gifts of our successors, and we should rise to the level of our high opportunities. The course of nature is exceedingly wasteful in every way. It is careless of germs, tens of thousands of pollen grains perishing of which none have had the chance of effecting fertilization, by being transported to the proper spot at the proper moment, by the blind agency of an insect ferreting among the flowers for food. It is equally careless of the microbes whose part in the animal world is analogous to the pollen of the flowers; they are produced in myriads, though only one is needed for fertilization. It is no exaggeration to say that the number of them which is produced each year by an average male of any of the larger animals, would suffice to fertilize a million of females, if every one of them were utilized. The course of nature is also indifferent and ruthless towards our own lives, but reason can teach us to effect with pity, intelligence, and speed many objects that nature would otherwise effect remorselessly, unintelligently and tediously. By its action, suffering may be minimized and waste diminished. Wherever intelligence chooses to intervene, the struggle for existence ceases, that struggle being by no means so absolute a necessity in evolution as Mr Kidd assumes it to be.... Horses are bred in the number and of the stamp required, within the limits of excellence that experience has taught to be possible. A general high level of the qualities that make a good horse has been attained without any aid from natural selection, artificial selection having superseded it.

"Before, however, as even a fanatic must allow, any form of artificial selection could be applied to the human race, other than such moderate, yet not ineffective, reforms as might produce the results mentioned a little way back, much is needed. Accurate knowledge has to be obtained on numerous details connected with productiveness, of which we are now curiously ignorant and careless to study, while national customs would have to be profoundly modified. The fanatic might, however, fairly urge that in considering what is feasible, and what not, the three following canons ought to be freely accepted:

"1. The customs of every nation are liable to change to an extent that is barely credible to those who do not bear history in mind; therefore the existing customs of any nation may be lightly regarded while discussing future possibilities.

"2. No custom can be considered seriously repugnant to human feelings that has ever prevailed extensively in a contented nation, whether barbarous or civilized.

"3. Any custom established by a powerful authority soon becomes looked upon as a duty, and, before long, as an axiom of conduct which is rarely questioned.

"Fortified by these three canons, an anthropologist who is necessarily familiar with the customs of many nations will find abundant elbow-room for his wildest speculations. There is hardly any proposition, however monstrous it may seem to us now, that is thereby precluded from consideration....

"It is quite credible that a nation whose old religious notions and social practices, whatever they were, have avowedly failed, who have been aroused to the knowledge that man possesses vast and hitherto unused powers over the very nature of unborn generations, who have learnt to realize the dilatoriness, ruthlessness, and pain that accompany the evolution of man, when it is left as now to cosmic influences, and who have satisfied themselves that the present low state of their race might be materially improved by concerted national action, should seize with irresistible ardour upon the idea of utilizing their power.

"That is to say, the nation might devote its best energies to the self-imposed duty of carrying out, in its manifold details, the following general programme: (1) Of steadily raising the natural level of successive generations, morally, physically, and intellectually, by every reasonable means that could be suggested; (2) of keeping its numbers within appropriate limits; (3) of developing the health and vigour of the people. In short, to make every individual efficient, both through nature and by nurture.

"A passionate aspiration to improve the heritable powers of man to their utmost, seems to have all the requirements needed for the furtherance of human evolution, and to suffice as the basis of a national religion, in the sense of that word as defined by J. S. Mill, for, though it be without any ultra-rational sanction, it would serve to 'direct the emotions and desires of a nation towards an ideal object, recognized as rightly paramount over all selfish objects of desire.'" (pp. 761–3.)

I trust this long citation will not have wearied the reader; for his biographer it contains some of the most important lines Galton ever wrote. There is no reason to be afraid of plain words. Man has learnt how to breed plants and most inferior forms of life that are of service to him. He has yet to learn how to breed himself. When he has studied heredity and environment in their influences on man, the application of the laws thus found to the progressive evolution of the race will become the religion of each nation. Such is the goal of Galtonian teaching, the conversion of the Darwinian doctrine of evolution into a religious precept, a practical philosophy of life. Is this more than saying that it must be the goal of every true patriot* ?

L. *Miscellaneous Papers on Evolution, Heredity, etc.* We may now turn to a series of short papers by Galton, chiefly published in *Nature*, and dealing with hereditary and evolutionary topics from 1897 onwards.

The first paper we note is entitled: "Rate of Racial Change that accompanies Different Degrees of Severity in Selection," and will be found in *Nature*, April 29, 1897 (Vol. LV, p. 605). This is an important paper, because it deals with the effect of continued selection in modifying a variate continuously distributed in a population. Galton starts with his two-thirds regression of the offspring on the midparent for stature and the reduction of the variability of the offspring of such midparents in the ratio of 1·5 to 1·7 inches. He then continues to select both parents at the 99th, 95th, 90th, 80th and 70th grades for 1, 2, 3, 4, 5 and an infinite number of generations in order to determine the progression there would be in stature by such continuous selection. Galton, unfortunately ignorant of the formulae of multiple regression, makes three erroneous assumptions, namely: (i) that the regression between each generation is the same, namely $\frac{2}{3}$, notwithstanding the earlier ancestry being as we advance more and more selected; (ii) that the variability of the array of selected ancestry remains for the later generations the same as for the first selected generation; this is of course incorrect, the variability steadily diminishing towards a finite limit; (iii) that if the selected race be now left to itself, it will regress indefinitely to the old general population mean:

"It must be borne in mind, that there is no stability in a breed improved under the supposed conditions; but that as soon as selection ceases it will regress to the level of the rest of the population through stages in which the deviation, at starting, sinks successively to $w, w^2 \ldots w^n$ of its value†. It may, however, happen that a stable form will arise during the process of high

* Some may question whether we have more here than in Comte's *Religion of Humanity*. I think so, because it is freed of the ceremonialism which Comte and Gruppe demanded as a factor of religion, and it is essentially based on the acquirement of knowledge in a field of science, which had little if any existence in Comte's day.

† w is Galton's regression coefficient in the case of selected midparent, *with no selected previous ancestry*.

breeding, that shall afford a secondary focus of regression, and become the dominant one, if the ancestral qualities that interfere with it be eliminated by sustained isolation and selection. Then a new variety would, as I conceive, arise; but into this disputable topic there is no need to enter now." [See, however, my footnote, p. 79.]

We now know that on the theory of multiple regression, this indefinite regression has no existence; there is a slight regression in the first generation of breeding from the selected stock, but it ceases with this generation. We have again in the cited passage evidence that Galton was obliged to appeal to "sports" to account for evolutionary progress, because he had misinterpreted the theory of regression. If w be the regression of the offspring of the first generation of selected midparentage, the regression of the offspring of parents of the first generation, *who have also selected midgrandparents*, is not to be taken w again. Thus the formulae, the numerical table and the conclusions drawn from it in this paper are I think in error. But the idea at the back of it that the more intense the selection, the more rapid is the relative progress, is true; as also the idea that there may in each case be a limiting value. Probably no such continued selection is really feasible; too many characters in the organism are highly correlated, so that if it were possible to carry under conditions of viability an individual character to a height much above the population mean, some one or other of the correlated characters would be almost certain to be incompatible with the continued efficiency of the organism in relation to its environment or its functions*.

I have not recalculated Galton's table, because with the data at present available, I am inclined to believe that selection for two or three generations and then inbreeding would be followed, at any rate in some characters, by a progression rather than a regression. In other words the strength of inheritance is such that with a very brief period of selection followed by isolation a continuous differentiation will proceed—so far as it is not checked by a counter natural selection. This suggests that we may have to seek in heredity itself for the basis of progressive evolution; a variation maintained for a couple of generations, followed by an isolation of the offspring, will continue to progress. If this be true we surmount the difficulty of why variations to which the environment is not hostile, or indeed may be favourable when they are sufficiently developed, can reach the stage of development at which they become important as a new factor of efficiency in the individual. We see that it is not natural selection, but the mere force of heredity, which leads in isolated groups to the genesis of variations of sufficient importance to have survival value to the individual. We may term this theory of the genesis of remunerative variations the "Heredity Theory of Progressive Evolution." It seems at first sight in flat contradiction to Galton's views on continuous regression when selection ceases, unless the selection has led to the creation of a sport. Yet it really flows

* Nor is this confined only to the functions of the individual, but may concern the functions of other members of its race. Thus breeders of bull-dogs have gone on continuously selecting the size of the head until the mortality of puppies and bitches at littering has become so serious as to threaten even the survival of the breed.

from a more complete view of multiple regression, and the more accurate values we now have for the heredity constants.

The second paper to be referred to was published in *The Gardeners' Chronicle* for May 15, 1897, and is entitled "Retrograde Selection." In this Galton asks from horticulturists advice as to the cultivation of a plant or plants existing in an original stock R and a stable variety V. For example V might be a dwarf variety. Galton proposes to endeavour by selection to pass back from V to R. If the plants in progress of selection be X, then Galton proposes to pass towards R by selecting the plants above the quartile of X on the R side of the character. He describes very fully how the experiments could be made in an orderly fashion and the needs as to soil and methods of growth; he refers to his paper of April (see our p. 93) for a measure of the rate at which changes might be supposed to take place. No doubt much might be learnt from such experiments—if only, that "retrograde selection" is impossible. I am not certain that Galton had not this in mind, for if in his view stable varieties could only originate in sports, we could not select back to the original stock, unless selection itself conduces to sporting. I do not think the paper led to any actual experiments on Galton's part, although the Editor of the *Chronicle* wrote strongly in favour of such experiments in a leader in the following week, and there were some suggestions on May 29, 1897*.

In *Nature*, November 4, 1897 (Vol. LVII, p. 16), Galton gave a brief account of E. T. Brewster's paper on "A Measure of Variability and the Relation of Individual Variation to Specific Differences†." Brewster is really comparing what we now term Interracial with Intraracial Variability, measuring his variability by what is practically the coefficient of variation V. His thesis is that if for any two interracial characters A and B, V_A is $> V_B$, then for the corresponding intraracial characters in the "allied races" v_a will be $> v_b$. There does not seem any obvious theoretical reason for this, but Galton holds that Brewster "has provisionally established his thesis that whenever any special character varies much in individuals of the same race, it is probable that it will be found to vary much in 'allied races' and conversely."

The next paper to be considered is entitled: "Hereditary Colour in Horses," and appeared in *Nature*, October 21, 1897 (Vol. LVI, pp. 598–9). Galton tabulated his data from material collected and published by "Tron Kirk" in the Chicago journal, the *Horseman* (Christmas Number, 1896). In the fundamental table which Galton gives there are 3025 matings of bay sires, but as "Tron Kirk" informs us that 3100 foals were born to no more than 46 different bay sires‡, or an average of 67 foals to the sire, it is clear that

* The interpretation put by the practical gardeners was that Galton wanted to go back from an *improved* form to a *poor* original. But I do not think this by any means the chief purport of his paper; he wanted if possible to go back from a specialised variety to the form, not necessarily inferior, from which it had been obtained.

† *Proceedings, American Academy of Arts and Sciences*, May, 1897.

‡ It is not said that the matings of bay sires cover these 3100 foals, but presumably they do. The difference in numbers may be due to the omission of grey foals or to twinning.

Reproduction in close colour facsimile of Dr Sorby's painting of a tree with the pigment from black human hair. [Colouring matter largely from melanin pigment granules?]

Reproduction in close colour facsimile of Dr Sorby's painting of a tree with the pigment from dark red human hair. [Colouring matter largely from the diffused pigment of the fibrillae?]

there will be a great bias in the returns owing to the limited number of gametic constitutions in the sires. The following table gives the results as

Table of Colour Inheritance in Horses.

	No. of Observations	Colour of Dam	Colour of Sire	Percentages of Colour in Offspring			
				Chestnut	Bay	Brown	Black
A (i)	68	Chestnut	Chestnut	100	—	—	—
(ii)	1900	Bay	Bay	10	81	6	3
(iii)	19	Brown	Brown	—	42	52	5
(iv)	25	Black	Black	—	4	28	68
B	407	Chestnut	Bay	33	61	4	2
	366	Bay	Chestnut	30	63	3	4
C	52	Chestnut	Brown	—	86	11	2
	69	Brown	Chestnut	16	65	10	9
D	72	Chestnut	Black	6	76	15	3
	57	Black	Chestnut	30	40	—	30
E	221	Bay	Brown	1	79	14	6
	450	Brown	Bay	6	66	18	10
F	156	Bay	Black	3	60	30	7
	268	Black	Bay	7	53	16	24
G	55	Brown	Black	—	22	38	40
	6	Black	Brown	—	16	50	33

Percentages taken only to whole numbers.

published by Galton. In the first line of the series of rows, *A* (i), we see that for 68 cases of chestnut mated with chestnut all the offspring were chestnut. Galton does not comment on this, but it was the source of considerable later controversy. A certain number of matings of chestnut with chestnut taken from Wetherby's *Thoroughbred Studbook* gave the same result as the first row of Galton's matings; but a longer series, wherein it was pointed out that the *Studbook* did contain some instances of chestnut mated with chestnut not producing chestnut, was rejected on the ground that these instances must be due to error of record, a most circular process of reasoning.

It is clear from *B* where we are dealing with a fairly adequate number of crossings both ways that (i) there is not a predominance of sire or dam for chestnut with bay matings, and (ii) bay may contain a factor of chestnut. If we work out from *B* the number of bays with a factor for chestnut we find them to be $31{\cdot}5\,{}^{\circ}\!/_{\circ}$, while $68{\cdot}5\,{}^{\circ}\!/_{\circ}$ lack that factor. Applying these percentages to the 1900 bay and bay matings in *A* (ii) we should anticipate 9·9 °/。 chestnut and 90·1 °/。 non-chestnut foals, a result almost exactly that observed.

If we judged by A (iii), A (iv), and G, we should conclude that black and brown had no factor for chestnut. In that case chestnut and bay crossed with black and brown would produce no chestnut foals. This is flatly contradicted by C, D, E and F, which indicate that browns and blacks can contain a factor for chestnut. The only explanation is, perhaps, a rather forced one, namely that the matings in A (iii), A (iv) and G were few in number and possibly made from a very few sires of brown and black colour without factors for chestnut, while C, D, E and F, providing far more numerous matings, contained blacks and browns with such factors. C, D, E and F, indeed, tend to confirm this, for when the sire is black or brown there are far fewer chestnuts produced than when the dam is black or brown; in the latter case the larger number of dams used would give a greater chance of their carrying factors for chestnut.

Galton himself by *averaging* up the likenesses in coat-colour of foal to dam and to sire concluded that as some 32·83 °/。 of foals followed the colour of their dam and 31·75 °/。 that of their sire, there was no prepotency, but such an averaging method misses the possibility of discovering a prepotency due to the presence or absence of "factors." The equality of male and female hereditary influence is borne out by the long series B, but hardly by the other and shorter series such as C and D*. Galton was very fully aware of what he terms the "rudeness" of the data. He had been troubled with much the same problems in considering hair colour; but he had then obtained an analysis of the pigments in human hair from Professor Sorby and the latter investigator had shown the existence of two distinct pigments, one red and one black. Sorby painted two trees in these two pigments extracted from human hair, pictures which used to hang in Galton's dressing room and are now in the Galton Laboratory. More recent microscopic investigation seems to show that the same two pigments occur in the hair of horses and dogs, but that the red pigment is diffused in the hair, i.e. "the whole ground substance of the fibrillae is impregnated with it." On the other hand the black or melanine pigment occurs in the form of pigment granules†. On examination of a number of specimens of horse hair in samples from ribs and mane of chestnut horses, ranging from the golden chestnut of the Trakehnen stud to the black chestnut, the diffused red pigment was found in all, but the pigment granules varied from scarcely any in the golden and light chestnuts to close packing in the dark and black chestnuts. This corresponds very closely to the range of granular pigmentation found in passing from light red to dark auburn hair in man. Such results suggest that it is very desirable to study microscopically the distribution of the two pigments in the hair of horses'

* For example in 124 matings of chestnut dam by brown or black sire there were only 6 chestnut foals, but in 126 matings of chestnut sire with black or brown dam there were 46 chestnut foals.

† Pearson, Nettleship and Usher: *Monograph on Albinism*, Part II, pp. 319–345, Cambridge University Press.

coats, and to remember that granular pigmentation varies enormously within the range of coat-colours described as chestnut by hackney breeders. Galton assumes that full red pigmentation counts for 1·0 and takes chestnut to be 0·8; bay, 0·7; brown, 0·4; and black, 0·1. Then by using the results of the several lines in *A*, he concludes that each chestnut parent contributes 40 units to the offspring, each bay 33·7 units, each brown 25·3, and each black 10·4. He is now able to deal with the crosses in *B*, *C*, *D*, *E*, *F* and *G*. He finds that there should be in the offspring of the matings:

	B	*C*	*D*	*E*	*F*	*G*	
Theoretically	74	65	50	59	41	36	units of red
While observations give	70	64	60	61	54	35	units of red

But is this really conclusive? It is possible that there are almost the same amounts of diffused pigment in the different coats and that the visible colours arise from the relative amounts of granular pigmentation. Had Galton put the amount of red pigment = 0·8 for all coat-colours, he would have got his theoretical and observational numbers in *perfect* agreement. But I do not think this would justify the assumption that the amount of red pigment is the same for all coats. I think then that we cannot assume his far rougher agreement is any proof of the numbers he has selected, or indeed of his theory of average parental contributions*. After the experience we now have had in the coat-colour of mammals, I feel fairly convinced that it is necessary to supplement the macroscopic classification by microscopic examination, for the categories formed in the former manner contain a great range of both diffuse and granular pigmentation.

Prepotency in Trotting Horses. Still another paper of the same year occurs in *Nature*, July 14, 1898 (Vol. LVIII, pp. 246–7). It is entitled "The Distribution of Prepotency," and deals with data for American Trotting Horses. Galton was much occupied at this time with the idea of the prepotency of individuals. He believed that some favoured individuals had a power of impressing their exceptional characters on their offspring, and that this prepotency was of the nature of a "sport." The American Trotting Horse data provided, he considered, a method of testing this belief. Wallace's *Year Books* give lists (i) of the sires of offspring any one of which has succeeded in trotting one mile in 2 minutes and 30 seconds or less, or who has "paced" (ambled) the same distance in 2 minutes and 25 seconds or less; (ii) of the dams of two such offspring, or else of one such offspring and one such grandchild. Galton selected from these lists of sires and dams those foaled before 1870 and therefore who would be at least 25 years of age in the *Year Book* for 1896, which he was using. He considered that this would give at least 20 years of breeding age to the parents and 5 years of attempted

* I took the relative proportions of red to be r_1, r_2, r_3, r_4, and determined their values to fit *B*, *C*, *D*, *E*, *F*, *G* by least squares instead of guessing their values; the ratio of the r's was 1·00 : 1·04 : 1·14 : 1·06, almost a ratio of equality!

record making at least to the foals. In this way Galton obtained 716 sires and 494 dams who had produced offspring satisfying the above conditions, and he classified them by the number of times they had produced such marked foals. Reduced to percentages of sires and dams the following table resulted:

Percentage numbers of Standard Performers produced by a single Sire or Dam.

	1	2	3	4	5	6—10	11—20	21—30	31—40	41—50	51 and over
Sire	46	17	10	7	3	9	4	1	1	1	1
Dam	50	35	10	3	1	1	—	—	—	—	—

Galton explains the difference between sire and dam by remarking that while the sire produces some 30 foals annually, the dam produces only one, and therefore the chance of a large number of standard performers is much less for her. He even allows that some of the exceptionally noteworthy performances of the sires (Blue Bull, 60; Strathmore, 71; George Wilkes, 83; Happy Medium, 92; and Electioneer, 154 standard performers) may be due in part to the best mares being sent to famous sires. But he concludes that the extraordinary "tail" of high-class offspring of the sires must be due to some prepotency in some of the sires which enables them to impress their character on their offspring, and he remarks:

"My conclusion is that high prepotency does not arise through normal variation, but must rank as a highly heritable sport, or aberrant variation; in other words its causes must partly be of a different order, or else of a highly different intensity, to those concerned in producing the normal variations of the race. In a sport the position of maximum stability seems to be slightly changed. I have frequently insisted that these sports or "aberrances" (if I may coin the word*) are probably notable factors in the evolution of races. Certainly the successive improvements of breeds of domestic animals generally, as in those of horses in particular, usually make fresh starts from decided sports or aberrances, and are by no means always developed slowly through the accumulation of minute and favourable variations during a long succession of generations."

Here, I think, Galton has forgotten two things:

(i) The average difference between the first and second individuals in a group of 100 tabled to any character is no less than ·36 of the variability of the group, and in a random sample may be still higher, but this is no adequate reason for treating the first individual (or the last) as a sport because he is not, like mediocre individuals, practically continuous with his neighbours.

(ii) That the number of distinguished offspring any individual gives rise to must be considered in relation to his total output. Galton merely says that a sire produces "some 30 foals annually." I do not think this is adequate.

Many years ago I saw a good deal of the working of a large thoroughbred stud; the stud contained a number of stallions, some famous for their racing

* The word is quite good English, if Joseph Glanvill and Sir Thomas Browne are authorities.

achievements or for those of their progeny, others less famous. The service lists of the former were always full up with external and home mares; this could not be said of the latter. I think it would be safe to say that the former stallions served annually at least double the number of mares the latter did. I hold therefore that to really demonstrate even a relative superiority in producing standard performers Galton ought to have taken the number of standard performers per total foals produced and this for both sire and dam. Owing to one cause or another one mare may fail more frequently than another to produce her annual foal. Out of four viable foals she might produce three standard performers. Galton's method would make her a less exceptional mare than one that produced four standard performers out of ten foals. Thus his conclusions may be correct, but they cannot be said to be proven until we know the relation of exceptional to total offspring. The distribution of standard performers to sires looks like a "*J*"-shaped frequency curve, and I do not understand why it is not as justifiable as a *J*-shaped curve for cricket scores, nor do I believe that anything is deducible from the deviation of its tail from normality*.

Foundation of "Biometrika." In October 1900 the present biographer sent in a paper to the Royal Society; that paper was printed in the *Philosophical Transactions* and was published in November of the *following* year. Meanwhile William Bateson, who had read the paper as one of the referees, wrote a sharp criticism of it, which the then Secretary of the Royal Society printed and issued in slip to the Fellows, before the latter had any opportunity of studying the criticised paper itself†. Michael Foster, notwithstanding the remonstrances of the biometricians, failed to see any objection to a referee criticising a paper before its publication, and as a result of his attitude, it was determined early in 1901 to found a Journal for the publication of biometric papers. Weldon and the biographer were to be Acting Editors with Galton as Consulting Editor. It is all past history now, and with twenty volumes issued of *Biometrika*, one can afford to smile, when one thinks of Bateson and Michael Foster as unwitting parents of what they would have considered an unviable hybrid! *Biometrika* appeared in October 1901, and Galton contributed an introductory notice entitled "Biometry" (Vol. I, pp. 7–10). A good deal of that paper would now be unintelligible without the light

* There is a long review in the same number of *Nature* (Vol. LVIII, pp. 241–2) by Galton of Alexander Sutherland's *The Origin and Growth of the Moral Instinct.* Galton praises the book highly, as extremely original and extending and confirming the masterly sketch by Darwin in Chapters IV and V of his *Descent of Man* of the evolution of the moral instinct. Galton does not, however, contribute any special views of his own, except the remark that "it would be very interesting to trace and describe the origin and purport of superstitious fears in human nature and their bearing on moral instinct." Galton, it must be remembered, was always appreciative and generous in reviewing; there is, even allowing for this, much information collected in Sutherland's book, which should give it a permanent place in the evolutionist's library.

† Shortly afterwards a resolution of the Council was conveyed to me, requesting that in future papers mathematics should be kept apart from biological applications. βίος was an admissible topic, μέτρον also, but their combination was anathema, and that at a time when statistical theory had to be worked out step by step as the biological applications demanded.

that the above historical facts throw upon it. Galton's tale of Sir Joseph Banks and the young geologists was the parable which he provided in order that he who runs might read.

"Now that nearly a century has slipped past since the event, there can be no harm in digging up and bringing to light a buried but amusing historical fact."

Then follows the inner story of the foundation of the Geological Society. "But," continues Galton,

"it is not in the least my intention to insinuate that Biometry might be served by any modern authority in so rough a fashion, but I offer the anecdote as forcible evidence that a new science cannot depend on a welcome from the followers of old ones, and to confirm the former conclusion that it is advisable to establish a special Journal for Biometry."

Speaking of those early difficulties of Biometry, Galton writes:

"The new methods occupy an altogether higher plane than that in which ordinary statistics and simple averages move and have their being. Unfortunately the ideas of which they treat, and still more the technical phrases employed in them, are as yet unfamiliar. The arithmetic they require is laborious, and the mathematical investigations on which the arithmetic rests are difficult reading even for experts; moreover they are voluminous in amount and still growing in bulk. Consequently this new departure in science makes its appearance under conditions that are unfavourable to its speedy recognition, and those who labour in it must abide for some time in patience before they can receive much sympathy from the outside world. It is astonishing to witness how long a time may elapse before new ideas are correctly established in the popular mind, however simple they may be in themselves. The slowness with which Darwin's fundamental idea of natural selection became assimilated by scientists generally is a striking example of the density of human wits. Now that it has grown to be a familiar phrase, it seems impossible that difficulty should ever have been felt in taking in its meaning. But it was far otherwise, for misunderstandings and misrepresentations among writers of all classes abounded during many years and even at the present day occasional survivals of the early stage of non-comprehension make an unexpected appearance. It is therefore important that the workers in this new field who are scattered widely though many countries, should close their ranks for the sake of mutual encouragement and support. They want an up-to-date knowledge of what has been done and is doing in it....

"This Journal, it is hoped, will justify its existence by supplying these requirements either directly or indirectly. I hope moreover that some means may be found, through its efforts, of forming a manuscript library of original data. Experience has shown the advantage of occasionally rediscussing statistical conclusions, by starting from the same documents as their author. I have begun to think that no one ought to publish biometric results without lodging a well arranged and well bound manuscript copy of all his data, in some place, where it should be accessible under reasonable restrictions, to those who desire to verify his work. But this by the way*.

"There remains another urgent reason of a very practical kind for the establishment of this Journal, namely that no periodical exists in which space could be allowed for the many biometric memoirs that call for publication. Biometry has indeed many points in common with Mathematics, Anthropology, Botany and Economic Statistics, but it falls only partially into each of these. An editor of any special journal may well shrink from the idea of displacing matter which he knows would interest his readers, in order to make room for communications that could only interest or be even understood by a very few of them." (pp. 7–8.)

Thus Galton in his eightieth year heartened his young lieutenants for their task, and his words have been through some 28 years a guide to the

* It is noteworthy that Galton's suggestion of a store of data (which has been provided in the archives of the Galton Laboratory for all papers worked out there) has recently been revived by Professor Julian Huxley, and suggestions made for storing measurements in the British Museum (Natural History).

surviving editor of *Biometrika*, never in the first place to expect recognition too quickly, and always if possible to give opportunities for publication to the younger men, whose work and enthusiasm might elsewhere meet with a cool reception.

Gifted Sons of Gifted Fathers. On November 28, 1901 (*Nature*, Vol. LXV, p. 79) Galton published a paper entitled: "On the Probability that the Son of a very highly-gifted Father will be no less gifted."

"Here we meet again with the specious objection which is likely to be adduced, as it has already been urged with wearisome iteration, namely, that the sons of those intellectual giants whom history records, have rarely equalled or surpassed their fathers*. In reply I will confine myself to a single consideration and, ignoring what Lombroso and his school might urge in explanation, will now show what would be expected if these great men were as fertile and as healthy as the rest of mankind.

"The objectors fail to appreciate the magnitude of the drop in the scale of intelligence, from the position occupied by the highly exceptional father down to the level of his *genetic* focus (as I have called it), that is the point from which his offspring deviate, some upwards, some downwards. They do not seem to understand that only those sons whose upward deviation exceeds the downward drop can attain to or surpass the paternal level of intelligence, and how rare these wide deviations must be."

Galton points out that besides the exceptional quality of the father there are three other factors influencing the position of the offspring's genetic focus: (i) the quality of the mother, (ii) the quality of the father's ancestry and (iii) that of the maternal ancestry. The problem is—if we do not discuss it from an individual case—what weight to give to these three additional factors. Now it is a well-recognised fact that while exceptional parents produce exceptional sons at a much higher rate than non-exceptional parents do, the pairs of the latter are so much more numerous than those of the former that it is far more probable that an exceptional man is the son of non-exceptional than of exceptional parents. Hence when we are dealing with average results (ii) and (iii) will not be highly contributory. On the other hand many exceptional men have wives much above the average, and we ought to reckon something for the influence of the mother. Let us take her influence to be measured by an exceptionality one-fifth that of her husband and suppose him to be one man in a thousand†. If we have somewhat over-estimated the average exceptionality of the wife, as one woman in two hundred, we have done so purposely partly to account for possibly neglected paternal and maternal ancestry, and partly to give the son a better chance of reaching to his father's exceptionality. On these assumptions we may treat the problem on rather more modern lines than Galton has done. The "genetic centre" or mean of the array of offspring of our exceptional man and his wife will be at a distance $2{\cdot}086 \times \sigma$, where σ is the standard deviation or variability of the population for the given character. This supposes the parental correlations to be equal and of intensity ·46, and the coefficient of assortative mating to be ·25, both reasonable average values. The *average* son of our

* See what has already been said regarding this point on our p. 27 above.

† Assuming that the coefficient of assortative mating to be ·25 then the average wife of an exceptional husband (1 in 1000) would only be 1 in 40.

exceptional parents is only one man in fifty-four. We have now to determine what is the variability of this array of sons about the mean and how many sons in that array will equal or exceed the father in exceptionality. The variability of such sons is $\cdot 8133\sigma$, and the deviation from the filial mean of the father is $(3\cdot 100 - 2\cdot 086)\,\sigma = 1\cdot 014\sigma$, or the deviation is $1\cdot 014/\cdot 8133 = 1\cdot 25$ nearly in terms of the sons' variability. From which we ascertain that only 11 times in 100 occasions would the son equal or exceed his father's exceptionality, i.e. one in nine sons. Granting an average of three sons to each father we have to examine the cases of three exceptional fathers before we come across a son equal to his father in ability.

But Galton was considering a much higher degree of exceptional ability; he suggests seven or eight times the quartile for his excess above mediocrity. Let us take one man in 100,000, and suppose a nearer approach in the mother to Galton's view, say she was one woman in fifty, then the deviations of the parents would be $4\cdot 27\sigma$ and $2\cdot 05\sigma$ respectively. The genetic centre would be $2\cdot 32\sigma$, and the deviation of the filial mean from the father in terms of filial variability would be $(4\cdot 27 - 2\cdot 32)/\cdot 8133 = 2\cdot 40$ nearly, or 8 sons out of 1000 would reach or exceed their fathers' level or 1 son in 125, or allowing three sons to a father only 1 son would arise in the case of 40 fathers of distinction who would be at least his father's equal.

In a population of 10,000,000 adult men there would be 100 of this exceptional ability each producing able sons at the rate of ·025 apiece. The remaining 9,999,900 produce 97·5 or nearly at the rate of 1 per 1000, or ·001 apiece. That is to say 39 exceptional men are produced by non-exceptional fathers to one produced by exceptional fathers, but the latter produce exceptional sons at 25 times the rate of the former. This is the paradox which Galton tried in vain to make people understand. It has quite recently been again confusing the minds of Professors Raymond Pearl and Leonard Hill, who cannot grasp how great ability is inherited, because the majority of distinguished men have not distinguished fathers.

Pedigrees. Few of those who have had the task of making pedigree charts have not been worried by the unmanageable size to which they are apt to grow, but still more by the difficulty of indexing in a connecting form the material on which pedigrees are ultimately to be based. Galton in a paper entitled "Pedigrees," published in *Nature*, April 23, 1903 (Vol. LXVII, pp. 586–7), suggests a method of what may be termed an "index pedigree"—or as he himself termed it a "pedigree based on fraternal units." This consists in giving a numbered page to each family group. The family group consists of: (i) Father and (ii) Mother with reference to their family group numbers; (iii) their offspring, with any facts the purpose of the pedigree is to illustrate stated about them; thus the main information is to be found on the page, where an individual is one of the offspring, i.e. under his family group number; (iv) the wife or husband of each child with their family group numbers; and (v) the family group number which gives the offspring of each marriage in the first family group. The birthdays of the *parents* in every family group are given, in each case for the purpose of distinguishing couples with the same names. The following is a slightly modified reproduction of Galton's illustration. The whole "index pedigree" will of course have an index, the main family group of any individual and the family group founded by him being recorded. For example we look up Frank Gore in this index and find against his name 205, 340. The latter entry will give his birthday and confirm that

*Family Group**.

John Gore Amy Myers		29 October 1822 4 May 1826		31 *d* 43 *c*	101
		Characterisation			
Fred. Gore	101 *a*		Mary Drew	144 *a*	205
George Gore	101 *b*		Jane Boyle	136 *e*	211
Ellen Gore	101 *c*		John Piers	105 *b*	207
Susan Gore	101 *d*		*Unmarried*	—	—
Steph. Gore	101 *e*		*Unmarried*	—	—
Fanny Gore	101 *f*		Harry Pitt	163 *f*	223
George Drew Eliz. Patten		27 March 1827 9 May 1830		51 *d* 62 *a*	144
Harry Drew	144 *a*		Rose Spry	123 *e*	315
Mary Drew	144 *b*		1. Fred. Gore	101 *a*	205
" "	144 *b*		2. George Lewis	165 *c*	340
Fred Gore Mary Drew		26 November 1858 4 October 1862		101 *a* 144 *b*	205
Frank Gore	205 *a*		Anne Fox	218 *a*	340
Amy Gore	205 *b*		James More	265 *e*	344
Anne Gore	205 *c*		*Unmarried*	—	—
Alex. Gore	205 *d*		Eva Sully	241 *d*	370
Rose Gore	205 *e*		Stephen Bell	270 *b*	315

* Slightly modified from Galton's form. Letters to individual numbers need only be attached when there are no names.

he is the man we are seeking. It will give us information as to all his children and by reference to the number in the last column on the right we can find out the characteristics of his grandchildren and so on to lower descendants. By reference to the other number 205, we find the full particulars of all his brothers and sisters, and can trace by the numbers 344, 370, 315 all his nephews and nieces, and then upwards to their ancestors or downwards to their descendants. The family numbers 101 and 144 lead us to the particulars of his father and mother, and to those of all his paternal and maternal uncles and aunts respectively. 101 and 144 also give us the family group numbers of his paternal grandparents. The numbers 211, 207 and 223 will enable us to find all his paternal, while 315 will give us his maternal cousins. 340 will lead us to his half-brothers and sisters.

It is fairly clear that if the General Registry were indexed in this way, or even special registries like those of the Society of Friends, pedigree making would be easy work. The Family Group system becomes somewhat more cumbersome in the case of rapidly breeding mammals, for example, dogs. In this case it is needful to replace the family group by the dam, sire and single litter, even if the mating be repeated, as the material becomes too unwieldy. For very small mammals—guinea-pigs, rats or mice—where names are not given, it is the index number of the individual which needs careful thought, especially if it is desired to provide in that index number some indication of the generation to which an individual belongs. A small letter may be given to each individual in the litter attached to the family group index number, and F_s may be added to denote the sth generation from foundation stock, but it is difficult if, say, an F_2 sire has been mated with an F_3 dam to indicate this relation briefly. The difficulty is greater when such a mating is some distance back in the ancestry. If such matings have occurred in considerable numbers the use of generation marks in the index number of animals becomes a doubtful blessing, and we may well fall back on Galton's Family Group numbers plus a small letter.

Nomenclature of Kinship. Galton, still thinking over various methods of expressing kinship, turned from the numerical expression of it to seek a brief nomenclature, and published in *Nature*, January 28, 1904 (Vol. LXIX, pp. 294–5) a paper entitled: "Nomenclature and Tables of Kinship." In this he endeavours to give a self-explanatory, brief and euphonious name to each grade of kinship, which he had in earlier papers provided with an appropriate literal or numerical symbol (see our Vol. II, pp. 354–5, and the present volume, pp. 44–5). He does it in the form of a schedule, here reproduced (see p. 106), for recording in all his known relatives some character X known to exist in A.B. This schedule is practically what he used in the same year to obtain the distribution of successes in the kinsfolk of Fellows of the Royal Society, a topic to which we shall return shortly. The schedule is republished here because it may form a starting point for those desirous of making similar inquiries. One of the most important points in it is the insistence (by the presence of a separate column) on the importance of enumerating the *total* number of relations of each class. Even up to the present year I have seen disease schedules drafted in which the question is asked: How many brothers (sisters, cousins, uncles, aunts, etc.) have been subject to the disease? without the slightest consciousness that such information is idle unless accompanied by the statement of the total number of relatives, affected and not affected, in each class.

There is only one way and that a rather incomplete one in which such imperfect data can be somewhat inadequately utilised. That is by ascertaining the *average* number of relatives of each class in the population at large. Galton often pressed the present writer for data on this point, but there arose considerable difficulties in the way of obtaining them, perhaps the chief of which were the secular changes in the size of families and the infant death-rate*.

* There are also difficulties with regard to the "weighting" of the large families both in the collecting of the data, and in the actual use of them when obtained.

Distribution of the Peculiarity X in the Family of A.B.

fa = Father, or father's, according to its place; similarly, *me* = Mother; *bro* = Brother; *si* = Sister; *so* (or *son* where more euphonious) = Son. The links in the chain of kinship are to be read as leading outwards from A.B. Thus, *me da* signifies "A.B.'s mother's daughter is." *fa bro son* means "A.B.'s father's brother's son is."

Ordinary names for generalised kinships	Titles showing the precise chain of kinships	Adults alone		Titles showing the precise chain of kinships	Adults alone		Names in full of those whose initials appear in the preceding column
		Total No. of sons and daughters	Initials of those whose X deserves record		Total No. of sons and daughters	Initials of those whose X deserves record	
Grandfather Grandmother	*fa fa* *fa me*	1 1		*me fa* *me me*	1 1		
Uncles ... Aunts ...	*fa bro* *fa si*			*me bro* *me si*			
Father ... Mother ...	*father* *mother*	1 1		— — — —	— —	— —	
Brothers ... Sisters ...	*brother* *sister*			— — — —	— —	— —	
Half-brothers Half-sisters	*fa son* *fa da*			*me son* *me da*			
Nephews ... Nieces ...	*bro son* *bro da*			*si son* *si da*			
First cousins Male ...	*fa bro son* *fa si son*			*me bro son* *me si son*			
First cousins Female ...	*fa bro da* *fa si da*			*me bro da* *me si da*			

Maiden name of the wife	Year of marriage	Number who survived infancy		Initials of those whose X deserves record
		Sons	Daughters	

Number of Kinsfolk. This question of the "Average Number of Kinsfolk in each Degree" was raised by a paper with this title published in *Nature*, September 29, 1904 (Vol. LXX, p. 529). Galton tells us that the simplest conditions for a general theory are those which suppose (i) the population to be stable, i.e. its numbers statistically constant in successive generations; (ii) that the generations do not overlap; (iii) that they are completed by passing wholly into history; and lastly (iv) that any individual is taken into account at whatever age he or she may have died. Galton further supposes that the numbers of the two sexes may be taken as roughly equal. Thus he considers it only needful to work out the results for a single sex. Suppose the average number of females born to a woman who is a mother to be d, then he says that on the average only one of her female children will be fertile of female children, or the chance that any one of these females will be fertile of females is $1/d$. Any mother has d female and d male children and therefore any one of these children will have $d-\frac{1}{2}$ brothers and $d-\frac{1}{2}$ sisters *on the average*. Galton uses a dash to denote a female relative who is fertile of females. Thus the number of sisters (*si*) is $d-\frac{1}{2}$, but the number of fertile sisters (*si′*) is only $(d-\frac{1}{2})/d$, and each of these produces d daughters (*da*). Accordingly the number of sisters' daughters (sororal nieces) of a woman will be $(d-\frac{1}{2})/d \times d = d-\frac{1}{2}$. In this way the following table is reached:

	Specific Kinships	Average Number in each	
Ancestry:	*me′* (mother)	1	1
	me′ me′ (mother's mother)	1×1	1
	me′ me′ me′ (mother's mother's mother) ...	$1 \times 1 \times 1$	1
Collaterals:	*si* (sisters)	$d-\frac{1}{2}$	$d-\frac{1}{2}$
	me′ si (maternal aunts)	$1 \times (d-\frac{1}{2})$	$d-\frac{1}{2}$
	me′ me′ si (maternal grandmother's sisters)	$1 \times 1 \times (d-\frac{1}{2})$	$d-\frac{1}{2}$
	si′ da (sisters' daughters)	$(d-\frac{1}{2})/d \times d$	$d-\frac{1}{2}$
	me′ si′ da (mother's sisters' daughters) ...	$1 \times (d-\frac{1}{2})/d \times d$	$d-\frac{1}{2}$
	si′ da′ da (sisters' daughters' daughters) ...	$(d-\frac{1}{2})/d \times 1 \times d$	$d-\frac{1}{2}$
Descendants:	*da* (daughters)	d	d
	da′ da (daughters' daughters)	$\left(d \times \frac{1}{d}\right) \times d$	d
	da′ da′ da (daughters' daughters' daughters)	$\left(d \times \frac{1}{d}\right) \times \left(d \times \frac{1}{d}\right) \times d$	d

Further explanatory letters were published by Galton, October 27, 1904, November 10, 1904, and January 12, 1905. These note one or two misprints and also reply to an objection raised by Professor G. H. Bryan. The reader will find an interesting paradox to solve, if he asks why his wife's sisters' daughters are on the average slightly less numerous than those of his own wife!

Kinsfolk of Fellows of the Royal Society. The main purpose of several of the notes by Galton discussed above becomes clear when we read the paper he communicated to *Nature* on August 11, 1904 (Vol. LXX, pp. 354–6) entitled: "Distribution of Successes and of Natural Ability among the Kinsfolk of Fellows of the Royal Society." Galton received more than 200 replies to a circular with a blank schedule (see our pp. 105–6) which he had sent to the Fellows. In this paper he deals with the 110 which arrived up to a certain date, and contained one or more noteworthy kinsfolk of the Fellow. Galton introduces a slightly arbitrary system of marking, namely 3, 2, 1 or 0 marks to measure more or less noteworthiness, but gives lists of what sort of positions and honours he paid attention to. All Fellows of the Royal Society were given the highest or starred class with 3 marks. In many cases the judgment as to noteworthiness depended on the opinion of the F.R.S. who filled in the schedule, more especially when it concerned the women of his family. Those who will take the trouble to examine the book later published by Galton and Schuster (see our pp. 113–121) will see how differently various Fellows rated "noteworthiness" in their own families; some consider success as merchant or solicitor, or even the becoming an advocate, as a noteworthy achievement, while others would probably never for a moment suppose such occurrences in their family as more than the ordinary routine of middle-class professional life. Galton for obvious reasons does not provide the marks he allotted to such noteworthiness, and he probably marked it low, but the fact that he gave the highest number of marks to *every* Fellow of the Royal Society makes his present biographer somewhat sceptical as to the value of his system in grading ability; at the one end you may have a born scientific genius who revolutionises men's ways of thinking of nature, at the other the professional scientist, not known outside his own country, scarcely beyond his own university, and in no way more able than the normal man in any profession who makes a living by his calling. Admittedly Galton's task was a very difficult one and probably his method may have been, if rough, sufficiently accurate to demonstrate the results he considers to flow from it. Let us consider some of these results:

In the first place he gives a Table, we may notice, for the successes of *male* kin of Fellows of the Royal Society through *A* (Male) and *B* (Female) lines. In this Table the columns headed "Index of Success" are the *total*

Successes of Kinsmen of Fellows of the Royal Society.

A. Through Male Lines		*B*. Through Female Lines	
Kinship	Index of Success	Kinship	Index of Success
fa fa bro	26	*me me bro*	5
fa bro son	45	*me si son*	31
fa fa	67	*me fa*	58
fa bro	66	*me bro*	64
Total	204	Total	158

marks for "noteworthiness" obtained by the *total* corresponding grade of kinsmen of the 110 Royal Society Fellows. It is important that the reader should bear this in mind as it is not an index in the usual sense of a ratio or percentage.

On this Galton comments:

"A popular notion that ability is mainly transmitted through female lines is more than contradicted by these figures."

A first impression might be that this result is due to overlooking ability in the women, but Galton had on his schedule a list referring only to women*. Even if the Fellows did overlook the capacity of their mothers (which is not usual with sons of ability) this does not account for their overlooking the achievements of their mothers' relatives. I think the explanation is to be sought in other directions. We find that our 110 Fellows had 57 fathers of distinction, but only 16 mothers. The fathers are credited with 136 marks or 2·4 marks apiece and the mothers with 24 or 1·5 marks apiece. It is clear that more than half the fathers of the Fellows were "noteworthy" in a fairly high degree and not more than 16 of these noteworthy fathers, possibly none of them, married a woman of special ability. That is to say they handicapped their sons by not marrying women of marked *ancestral* distinction. Had they chosen wives with equal *ancestral* distinction to that of their own line these 57 fathers would probably have had a still larger number of noteworthy sons. I particularly emphasise the word ancestral because an examination of the above table shows that our 57 fathers did not simply marry mediocrity. The *me bro* group is sensibly equal to the *fa bro* group in noteworthiness, and the *me fa* group (i.e. that corresponding to the fathers-in-law of the fathers) is not so far behind the *fa fa* group. In other words it would appear that our fathers of distinction were thrown by circumstance or inclination into the society of women (from whom they chose their wives) who were the sisters or daughters of men of distinction, but that in making their choice they paid no attention to their wives' earlier ancestry. The point is a somewhat subtle one and wants testing on more ample data, but it seems to me the real explanation of the results in Galton's table. We cannot conclude from it that ability in men is mainly transmitted through the male line. If the above interpretation be correct then the eugenist must ever bear in mind that it is not enough from the standpoint of offspring to marry a woman of ability, he marries so to speak also her stock†.

The next point Galton makes is that the families of the Fellows must be fertile, because the number of brothers, whether of selves or of fathers, comes out 2·43. This would correspond, since $d-\frac{1}{2}=2{\cdot}43$, to $2d=5{\cdot}86$ or practically nearly 6 members in the family. But although this is a measure of the observed fertility in the 110 Fellows, I think a word of warning is needful.

* Suggested categories: Social leader, Great force of character, Reputed very clever, Artistic (in any way) to an exceptional degree, Successful worker in educational, civic and philanthropic matters, Brilliant prize winner at school or college, etc.

† This is of course only repeating the biological fact, that the genetic potentialities of an individual are only very partially determined by his or her somatic characters, and the only way to obtain a wider appreciation of them (in the case of man where experimental breeding is impossible) is to examine the whole stirp as fully as possible.

Fifty-seven fathers of these Fellows were themselves distinguished men. Now let us start from distinguished fathers; we have seen (see our pp. 102–3) that they are likely to have a higher percentage than mediocre fathers of distinguished sons, but the probability of a distinguished son occurring to a distinguished father depends on the number of his male offspring. Hence if we start by selecting distinguished men, we are likely to find that their fathers had families above the average, especially if those fathers were themselves distinguished. I do not think therefore that we can reach a measure of the fertility of distinguished men from the number of their brothers, nor indeed from the number of their fathers' brothers, if a large number (upwards of 50 °/₀) of those fathers were themselves distinguished. We require the number of children of the Fellows themselves, and this has not been provided.

The next point raised by Galton is of very considerable interest, namely the relative intensity of heredity in the direct line and in the collaterals of this line. I am a little puzzled to follow Galton here. In the direct line of male ancestors there is only one representative in each generation, and there is no necessity to divide by 110 the total marks obtained by each grade of ancestry if we are dealing only with relative measures of noteworthiness. In the one case of *fa fa* and *me fa*, the grandparents, Galton does divide by two. Yet when he comes to the collateral kinsmen, he puts down the *total* marks gained by brothers, and these number not 110 but $110 \times 2{\cdot}43$ brothers, and therefore it is not legitimate to compare the *total* marks obtained by brothers with those obtained by "selves" or fathers. In the same way Galton does divide by two the sum of the total marks obtained by paternal and maternal uncles, but forgets that uncles are more numerous than fathers or selves! I have therefore ventured to recompute Galton's Table III, adding to it one or two additional items, but giving in each case the average number of marks obtained by a kinsman of the given grade instead of Galton's total marks of the class. The following will illustrate the method by which my average has been obtained. There are four kinds of male cousins: (i) *fa bro son*; their average number is $1 \times \{(d-\frac{1}{2})/d\} \times d$, for there is only one father; his average number of brothers $= d-\frac{1}{2}$, and $1/d$ (see our p. 107) is the probability that a brother will have sons and d is the number of his sons. Accordingly (on Galton's theory) $d-\frac{1}{2}$ is the number of male cousins that a man will have of the class *fa bro son*; (ii) *fa si son* will also provide $d-\frac{1}{2}$ male cousins; (iii) *me bro son* and (iv) *me si son* will give the same number, or the average number of total male cousins is $4\,(d-\frac{1}{2})$. Galton gives $2{\cdot}43$ as the average number of brothers in the self generation and the father generation. Hence $d-\frac{1}{2}=2{\cdot}43$ and $d=2{\cdot}93$, and therefore on Galton's theory $9{\cdot}72$ is the average number of male cousins or $19{\cdot}44$ the average number of cousins of both sexes combined*. Now the following are the total marks obtained by the cousins of 110 men, i.e. $2138{\cdot}4$ cousins: *fa bro son*, 45; *fa si son*, 25; *me bro son*, 46; and *me si son*, 31; total marks, 147. Average marks of a cousin: $\cdot 07$.

* This seems to me rather a low average number of cousins for the individual, but I think it is the number which results from supposing the population stable; probably no population ought to be considered as such.

Proceeding in this manner we obtain the table below. I am inclined to think the average marks for Sons and Nephews too low, as it is possible that many of them would not have had time to reach full noteworthiness. As I have noted there is something defective in the earlier generations through the female line, and I have contented myself with using *fa fa bros* and *fa fa fa* as representatives of their grade. Galton does not even give *fa me fa* so that we cannot tell whether they got zero marks or he omitted to classify them. It is quite probable that many men know more of their father's paternal than of his maternal grandfather, a result of the old habit of tracing descent only through the male line.

Average Noteworthiness of Kinsmen in Direct and Collateral Lines of 110 *Fellows of the Royal Society.*

Generation	Kinship	Numbers	Total Marks	Average Marks	Kinship	Numbers	Total Marks	Average Marks
I	Self	110	330	3·00	Brothers	267·3	170	0·64
II	*fa*	110	136	1·24	*fa bros* } *me bros* }	534·6	130	0·24
III	*fa fa* } *me fa* }	220	125	0·57	*fa fa bros*	267·3	26	0·10
IV	*fa fa fa*	110	11	0·10	—*	—	—	—
Additional	Sons	322·3	49	0·15	—	—	—	—
	—	—	—	—	Nephews } = *bro sons* + *si sons* }	534·6	48	0·09
	—	—	—	—	Cousins	2138·4	147	0·07

* No entries have been made by Galton for the father's great uncles.

From this revised table Galton's main conclusion flows as definitely as, perhaps more definitely than, from his own Table. The ancestor in the direct line is far more noteworthy than the average collateral in the same grade. To be in the direct line from distinguished ancestry amounts to much, but to be merely the collateral of a great man means very little. Examining the numbers in the first three lines of the table we see that the collaterals of a man of distinction have on the average only $\frac{1}{5}$ of the noteworthiness of his direct ancestor in each generation. As Galton puts it elsewhere, to be the cousin of a man of ability means little if the kinsman gets only the cousin's average share; it might mean a good deal if the character did not blend and the kinsman ran the chance, if a small one, of getting the whole of his cousin's exceptionality.

Galton next discusses the "Relation of Success to Natural Ability." He proceeds by stating that success is due to the combined effect of Natural Gifts and Circumstances. His method is to record success in terms of 1, 0, and -1 marks to each division of a third of the frequency distribution for ability† and he marks each grade of circumstance ("healthy rearing, family

† The mean values of the thirds, 1·09, 0, and $-$ 1·09, would be more legitimate.

and social influences, education, money, leisure, and surroundings that encourage work or idleness") in the same way. Galton then assumes that if S be the measure of success, A of ability and C of circumstance,*

$$S = \tfrac{1}{2}(A + C),$$

and he then points out that the regression of success on ability will be just one half, if ability and circumstance be uncorrelated. But I see absolutely no reason for assuming the above form of relation between Success, Ability and Circumstance†.

Galton considers the intensity of the relationship of Ability to Environment at some length. He suggests that "a bright attractive boy receives more favour, and thereby has more opportunities of getting on in life, than a dull and unpleasing one, but these advantages are not without drawbacks; attractiveness leads to social distractions, such as have ruined many promising careers." Then he cites Henry Taylor's couplet:

> "Me, God's mercy spared from social snares with ease,
> Saved by the gracious gift, ineptitude to please."

But I fear that no generalities, only numerical observations, can lead us to a true appreciation of the value of r_{AC}. Researches since Galton's day show how small is the correlation of Ability and Environment. Galton suspected this and wrote that he believed home influences were much less potent than might be supposed. Galton states that the result of his inquiry was "to prove the existence of a small number of more or less isolated hereditary centres, round which a large part of the total ability of the nation is clustered, with a closeness that rapidly diminishes as the distance of kinship from its centre increases."

He further held that these exceptionally gifted families were an asset to the nation. "It must suffice for the present to mention the existence of at least nine gifted families connected with Fellows of the Royal Society, two or three of whom are exceptionally gifted." He concludes (as he has done elsewhere: see Vol. II, pp. 120–2) that it would be both feasible and advantageous to make a register of gifted families. Such a register Galton started for other fields of noteworthiness than the scientific, and fragments of this boldly outlined scheme still lie in the archives of the Galton Laboratory.

I have given considerable space to this paper of Galton, partly because it forms the basis of the later book on Fellows of the Royal Society, but

* The regression of success on ability would be $\frac{1}{2}(\sigma_A + \sigma_C r_{AC})/\sigma_A$, where σ_A and σ_C are the variabilities and r_{AC} the correlation of ability and circumstance. Clearly the regression of success on both ability and circumstance $= \frac{1}{2}$, if $r_{AC} = 0$.

† Preserving the type of symbols used in the last footnote the better form of relationship would be

$$\frac{S - \bar{S}}{\sigma_S} = \frac{r_{SA} - r_{SC}\, r_{AC}}{1 - r^2_{AC}} \frac{A - \bar{A}}{\sigma_A} + \frac{r_{SC} - r_{SA}\, r_{AC}}{1 - r^2_{AC}} \frac{C - \bar{C}}{\sigma_C},$$

where a bar denotes a mean value. Short of determining from actual observation the three correlations r_{SA}, r_{SC}, and r_{AC}, I do not see that we can profitably guess at values (such as $\frac{1}{2}$) for the multiple regression coefficients.

chiefly because, although I doubt the accuracy of some of the processes adopted, it is highly suggestive for kindred researches, and appears to have attracted little of the attention it deserved at the time of its publication in *Nature*.

Closely associated with the material on which the above memoir was based is a letter Galton published in *The Times*, November 17, 1904, with regard to the character and ancestry of Lord Northbrook, who had died on the 15th of the same month. Galton was in a position to comment on the character of Lord Northbrook, for he had served on a council* with him for two years and noted his "rare combination of thoroughness and quickness," which were reported family characteristics of the Barings. Galton was also well acquainted with the family history of the Barings for Lord Northbrook as a Fellow of the Royal Society had replied to Galton's schedule very amply and sympathetically. A full pedigree of the Barings as a noteworthy family would be well worth working up. Like many families of distinction in Great Britain, the Barings in the direct male line show foreign blood.

Noteworthy Families.

We now turn to the work which embraces the data on which the preceding two communications were based. The material was collected by schedules issued by Galton which were filled in by about half the Fellows and returned to him. From these Mr Edgar Schuster† selected the families in which there were at least three noteworthy kinsmen, and formed lists of their achievements on Galton's model. He thus compiled the brief biographical notices of sixty-six noteworthy families which fill about two-thirds of the volume. The book is entitled:

Noteworthy Families (Modern Science). *An Index to Kinships in Near Degrees between Persons whose Achievements are honourable and have been publicly recorded.* By Francis Galton, D.C.L., F.R.S., Hon. D.Sc. (Camb.) and Edgar Schuster, Galton Research Fellow in National Eugenics. Vol. I of the *Publications of the Eugenics Record Office of the University of London*. John Murray: London.

The intention was to collect similar material in other fields and publish corresponding volumes for Literature, Art, Politics, etc. Some of this material was actually collected‡.

If we consider briefly the material compiled by Schuster one is bound to confess that it is disappointing. As only about half the Fellows replied, and the families of only 63 are discussed, it is clear that we cannot look upon the results as representative of the Royal Society, much less of British

* Probably that of the Royal Geographical Society of which Lord Northbrook was at one time President.

† Mr Schuster had, in October 1904, been elected to the first Research Fellowship in National Eugenics founded by Francis Galton in connection with the University of London: see Chapter XVI below.

‡ "This volume is the first instalment of a work that admits of wide extension." Galton's *Preface*, p. ix.

Science. We cannot assume that the bulk of those who did not reply omitted to do so because their families presented no noteworthy members. We thus obtain no wholly trustworthy general picture of the frequency with which noteworthy men of science arise from noteworthy or commonplace families. Further in the 63 families dealt with as noteworthy we feel the definition is too arbitrary, several scarcely reach real distinction, and for those that do and are well worthy of record a trained genealogist could have given a truer picture and more interesting account of the family (with a pedigree chart!) from fairly accessible sources. We have indeed no certainty that our sample is a "random" one. Galton in his *Preface* of xliii pages, which forms the more valuable part of the book, admits that the facts given are "avowedly bald and imperfect," but considers that they lead to certain important conclusions, for example he considers they show "that a considerable proportion of the noteworthy members in a population spring from comparatively few families" (p. ix). This is very likely true, but it is difficult to accept it on evidence which does not indicate how many noteworthy persons there are in the population or how many we are to expect in a family, and deals only with what is probably not a truly random sample of even the men of science in the population, i.e. 63 out of a total which in 1914 was fixed at 1729 for the British Empire*.

Galton notes several important points, which may be of value as cautions to future circularisers. I cite some of them :

"The questions were not unreasonably numerous, nor were they inquisitorial; nevertheless, it proved that not one-half of those addressed cared to answer them. It was, of course, desirable to know a great deal more than could have been asked for or published with propriety, such as the proneness of particular families to grave constitutional disease. Indeed the secret history of a family is quite as important in its eugenic aspect as its public history; but one cannot expect persons to freely unlock their dark closets and drag forth family skeletons into the light of day." (pp. ix–x.)

Galton accordingly only asked for information on points which "could be stated openly without the smallest offence to any of the persons concerned."

One matter astonished Galton; he found it extraordinarily difficult to obtain even for near kin the number of kinsfolk of each person in each specific degree of kinship. Sometimes the omission was no doubt due to oversight or inertia, but Galton was surprised to find in how many cases the number of near kin was avowedly unknown.

"Emigration, foreign service, feuds between near connections, differences of social position, faintness of family interest, each produced their several effects, with the result as I have reason to believe, that hardly one-half of the persons addressed were able, without first making inquiries of others, to reckon the number of their uncles, adult nephews, and first cousins. The isolation of some few from even their nearest relatives was occasionally so complete that the number of their brothers was unknown." (pp. x–xi.)

Galton (p. xiii) states that he uses the epithet "noteworthy" to correspond in all branches of effort to that which would rank with an F.R.S. among scientific men. He considers that the term covers all those who appear in the *Dictionary of National Biography*, and about half those who appear in

* *Who's Who in Science*, 1914.

Who's Who. No attempt, he tells us, is made in *Noteworthy Families* to deal with the transmission of ability of the highest order. Galton here repeats what he has suggested elsewhere, namely that genius is akin to insanity: "the highest order of mind results from a fortunate mixture of incongruous constituents, and not such as naturally harmonise. Those constituents are negatively correlated, and therefore the compound is unstable in heredity" (p. xv). I do feel it impossible to accept this view; it is quite easy to cite the names of men, to whom the world accords the title of genius, who have had a strain of madness. But one is apt to exaggerate their number and possibly their greatness. Galton states that "the highest imaginative power is dangerously near lunacy." He tells us that he once heard Bonamy Price narrating how as a young man he had asked Wordsworth what was the exact meaning of the lines in the famous *Ode to Immortality**:

"Not for these I raise
The song of thanks and praise;
But for those obstinate questionings
Of sense and outward things,
Fallings from us, vanishings;
Blank misgivings of a Creature
Moving about in worlds not realised," etc.

Wordsworth had replied that he had had not unfrequently to exert strength, as by shaking a gatepost, to gain assurance that the world around him was a reality. Galton concludes that at such times the mind of Wordsworth could not have been wholly sane; indeed he goes further and considers that such conduct suggests temporary insanity. Yet it seems to me that to every contemplative man, or at least to every contemplative child, such slipping away from their momentary environment, even in a crowded gathering, will not be unfamiliar, and that they can remember instances when they have experienced a distinct effort to recall themselves to their space and time relations; even if they do not need to shake a gatepost, they may require to shake themselves. It is very curious that Galton, who was so essentially a psychologist and attributes much to the subconscious mind†, should have been unfamiliar with the states in which the mind seems switched off from external reality, although conscious that it is still continuing. It is, I think, unreasonable on this ground to associate Wordsworth with insanity. I cannot

* This is not Wordsworth's title, which is: *Ode, Intimations of Immortality from Recollections of Early Childhood*—a title which suggests when the "obstinate questionings" arose, and with them the remedy.

† Galton indeed held genius to be something akin to inspiration, and supposed that the powers of unconscious work possessed by the brain are abnormally developed in those who exhibit it. "The heredity of these powers has not, I believe, been as yet especially studied. It is strange that more attention has not been given until recently to unconscious brainwork, because it is by far the most potent factor in mental operations. Few people, when in rapid conversation, have the slightest idea of the particular form which a sentence will assume into which they have hurriedly plunged, yet through the guidance of unconscious cerebration it develops itself grammatically and harmoniously. I write on good authority in asserting that the best speaking and writing is that which seems to flow automatically shaped out of a full mind." (See pp. xvii–xviii of the work under discussion.) Lagrange when listening to music or at social gatherings would sink into deep reveries and lose all consciousness of his environment.

help regretting that the greater authority of Galton was thrown into the scale which was already weighted with Lombroso and Ellis.

In the following chapter Galton discusses the proportion of noteworthies to the generality, but his final conclusion that "the proportion of one noteworthy person to one hundred of the generality who were equally well circumstanced as himself does not seem to be an over-estimate" requires perhaps more evidence than is provided.

Chapter V deals with "noteworthiness as a measure of ability," and discusses on the lines of his paper in *Nature* (see our pp. 108 *et seq.*) the interrelation of Success, Ability and Environment. I have already commented on Galton's treatment of this topic. It seems to me that his discussion is based solely on classification and nothing can be predicted of the correlation between these three factors until their relative frequencies in the several classes have been determined by observation.

Chapter VI deals with Galton's convenient nomenclature for kinship (see our p. 106). In Chapter VII we have the vital question investigated of the number of kinsfolk to be expected in each degree. I say this is vital, for without it we cannot possibly obtain any measure of the strength of heredity. Galton does not here adopt the method of his paper described on our p. 107, where he worked with a stable population, but he makes his returns for each class of kinship on the basis of the F.R.S.'s returns, the schedule containing an inquiry as to the number of kinsfolk in each degree, who *survived childhood.* Hence Galton's previous results do not strictly apply as they were based on all children born, as well as on a theoretically stable population. His Table V (p. xxx) gives only the data for 100 Fellows. I looked at the schedules and found a rather larger number available as schedules appear to have come in later after his Table V was completed. But as the averages were not essentially altered, I will reproduce Galton's numbers, citing them in a different form. The problem wants answering on far more extensive material, but I do not know where else to find even a rough approximation to the average number of relatives a man may expect.

Average Number of Kinsfolk in each Degree.

Class	Kin ♂	Average Number	Kin ♀	Average Number	Size of Family
Brothers and Sisters	*bro*	2·06	*si*	2·07	5·13
Uncles and Aunts	*fa bro*	2·28	*fa si*	2·07	5·35
	me bro	2·19	*me si*	2·38	5·57
Totals	Uncles	4·47	Aunts	4·45	—
First Cousins	*fa bro son*	2·65	*fa bro da*	3·02	Total First Cousins 19·44
	fa si son	1·84	*fa si da*	2·08	
	me bro son	2·36	*me bro da*	2·66	
	me si son	2·37	*me si da*	2·46	
Totals	♂ Cousins	9·22	♀ Cousins	10·22	

Clearly the number of nephews and nieces is also contained in the table. A man may expect on the average 4·49 nephews and 5·10 nieces, while a woman would have on the average 4·73 nephews and 5·12 nieces. The later generation seems to give a slightly smaller family than the earlier generation. Since the families include only those who have reached adult age, and the infant death-rate was certainly greater in the older generation, the decrease in size of family is probably larger than appears. The calculations show that an individual has on the average about one fertile relative in each specific type of kinship. Galton now says that he proposes to make "the reasonable and approximate assumption" that "the number of fertile individuals is not grossly different to that of those who live long enough to have an opportunity of distinguishing themselves".... "Thus if a group of 100 men had between them 20 noteworthy paternal uncles it will be assumed that the total number of their paternal uncles who reached mature age was about 100, making the intensity of success as 20 to 100 or as 1 to 5. This method of roughly evading the serious difficulty arising from ignorance of the true values in the individual cases is quite legitimate, and close enough for present purposes" (p. xxxiii).

The argument is not easy to follow. Galton, for example, has (p. xxx) shown that the number of paternal uncles who *survived childhood* in the case of 100 F.R.S.'s is 228, and he now says we must consider this as only 100, and so we see the above number reduced to less than one half. But I think he is contrasting those who survived childhood with those who lived long enough to have an opportunity of distinguishing themselves. He considers that only one individual in each grade of kinship can on the average be fertile in a stable community, and such an individual would probably live to an age at which he would have had an opportunity of distinguishing himself. But it is difficult to see why those who have an opportunity of distinguishing themselves are limited to the fertile. The unmarried uncle may equally with the married have a chance of distinguishing himself. Assuming that "survived childhood" meant to the Royal Society Fellows the surviving 15 years of age —and Galton refers to competitive success at school—and that by 40 years any man has had an opportunity of distinguishing himself, then only some $\frac{1}{5}$ of those alive at 15 are dead before 40. Thus our 228 paternal uncles would scarcely be reduced to 182 if they had died at the rate of the total *average* community. Probably their lives were considerably better than the average, and it would be safe to suppose nearly 200 lived to forty years. This is 100 °/ₒ more than Galton proposes to take. I should therefore be prepared to double Galton's number of candidates for distinction in each collateral grade of kinship* (but this will not affect his conclusions, if we are discussing only relative, not absolute degrees of noteworthiness) and to suppose the same number of relatives in each grade, which is approximately true (see our p. 107).

In Chapter VIII Galton limits his inquiry to males. He says that:

> "Women have sometimes been accredited in these returns by a member of their own family circle, as being gifted with powers at least equal to those of their distinguished brothers, but definite facts in corroboration of such estimates were rarely supplied." (p. xxxiv.)

* This does not apply to the direct line, in which the number who lived to bear offspring is known exactly. Of course any direct ancestor may have died without reaching the age when he could obtain noteworthiness, but Galton does not consider the effect of this.

It may be difficult to get adequate appreciation of women's noteworthiness, but it is still more difficult to measure heredity in ability, unless we have some direct measure of whether ability can be transmitted through the mother with strength equal to that of transmission through the father. We know whether the father was or was not noteworthy, but if we have no measure of the ability of the mother, we cannot determine whether an able maternal stock transmits its ability equally through an able and through a mediocre woman member. Further Galton does not discuss the sons of Fellows as many might not have reached maturity; 467 persons were addressed, 207 of these sent serviceable replies, of which only 65 are treated in Schuster's list of noteworthy families of F.R.S.'s (pp. 1–79). Galton's data are numerically based on the 207 cases. He states that the real crux of the problem lies in what the remaining 260 were like. Abstention might be due to dislike of publicity, to inertia, or to pure ignorance; such causes would hardly affect the randomness of the sample, but if the 260 did not reply because they had no noteworthy kinsfolk this would influence the sample, and badly influence it. The two extremes are that (*a*) we suppose the 260 to share the richness of the 207 in noteworthy kinsfolk, (*b*) we consider that the 260 had no noteworthy kinsfolk. Galton says he cannot guess which of these hypotheses is the more remote from the truth, but considers that actuality cannot be very far removed from their mean value. I cannot find, however, that this is what Galton has really used. For example the F.R.S.'s had 81 noteworthy fathers. The percentage of noteworthy fathers on the first hypothesis is $81 \times \frac{100}{207} = 39{\cdot}13$ and on the second hypothesis is $81 \times \frac{100}{467} = 17{\cdot}34$; thus the mean of the two is 28·24. Galton, however, does not take the mean of the two hypotheses, but of the numbers 207 and 467, and gets 337; then he finds $\frac{81 \times 100}{337} = 24{\cdot}04$, and this is the percentage he actually uses. Taking 1 man in 100 as noteworthy—a somewhat arbitrary assumption—he states (p. xl) that F.R.S.'s have 24 times as many noteworthy fathers as the generality of men. Before we pass to Galton's final table we may cite one or two points he makes which are of distinct interest and importance for similar investigations. In Chapter IX he gives the result of marking individual degrees of noteworthiness; he made three categories and gave to them in degree of noteworthiness marks 3, 2, 1. He then reduced the total of marks for each degree of kinship (657) to the total number of cases of noteworthiness (329). As a first appreciation the two results differed very little; thus (p. xxxvii):

Comparison of Results with and without Marks in 65 Families.

	First Degree	Second Degree	Third Degree	First Cousins	Total
Number of marks assigned ...	225	208	102	122	657
Marks reduced by factor $\frac{329}{657}$ ...	113	104	51	61	329
Number of Cases of Noteworthiness	110	112	46	61	329

The reason for this approximate concordance lies in the distribution of triple, double and single marks being much the same in the different kinship groups. Galton concluded that marking for different degrees of noteworthiness would be a waste of energy in such a rough inquiry as that he was undertaking. But I think it would have been of great interest had Galton divided his material in another way, i.e. classified his F.R.S.'s into the three categories of noteworthiness, and tested whether their kinsmen had the same or different totals of marks. In other words he would have answered the question of whether ability leading to noteworthiness is inherited in quality as well as quantity.

The next point is very important. Most men know beside their own name that of their mother, i.e. her maiden name. Hence both the numbers and achievements of the uncles and aunts in both paternal and maternal lives are known and there is no difference of a sensible kind in Galton's totals. This holds also for the achievements of the grandparental generation. But when we come to the great grandparents and great uncles, there have been further changes of name in *fa me fa*, *me me fa*, *fa me bro* and *me me bro*, and Galton attributes the ridiculously low number of cases of noteworthiness compared with those for *fa fa fa* and *fa fa bro* with a loss of record owing to change of name. This probably has a good deal to do with it, but it does not account for the successes of *me fa fa* and *me fa bro*, who of course bear the mother's maiden name, being only half those of *fa fa fa* and *fa fa bro*, who bear the father's name. I am inclined to think that the factor of assortative mating to which I have referred on p. 109 is at least a contributory cause.

I now reproduce Galton's final table of results, to which I have added percentages*:

Numbers and Percentages of Noteworthy Kinsmen recorded in 207 Returns of F.R.S.'s.

Kinship	Numbers Recorded	Percentages	Kinship	Numbers Recorded	Percentages
fa	81	28·24	—	—	—
bro	104	32·26	—	—	—
fa fa	40	13·94	*fa fa fa*	11	3·83
me fa	42	14·64	*fa me fa*	2	0·70
fa bro	45	15·69	*me fa fa*	5	1·74
me bro	52	18·13	*me me fa*	1	0·35
fa bro son	30	10·46	*fa fa bro*	12	4·18
me bro son	19	6·62	*fa me bro*	2	0·70
fa si son	28	9·76	*me fa bro*	6	2·09
me si son	22	7·67	*me me bro*	2	0·70
Total Cousins	99	34·51	—	—	—
Male Cousins, each type	24·75	8·63	—	—	—

* Obtained from Galton's assumption that we may take the mean of the extreme cases, i.e., we multiply by $\frac{1}{2}\left(\frac{100}{207}+\frac{100}{467}\right)=\cdot 3486$. I prefer this to his actual method.

From this table we see how degree of noteworthiness diminishes as we pass from the near relatives of the noteworthy to more distant kinsmen. If we accept Galton's two hypotheses: (i) that only one relative in each class can on the average be considered as having lived and been mature enough to have had the opportunity of reaching noteworthiness (see our p. 116) and (ii) that one person in a hundred of the generality is noteworthy, then the above percentages express the numbers of times the F.R.S.'s have more noteworthy kinsmen than the generality of men*. It will be seen that the kinsmen with surnames different from those of the F.R.S.'s fathers and mothers have even a lesser percentage of distinction than the generality of men! Allowing that this may be to some extent due to ignorance of the names, and so of the achievements of these relatives, are we justified in holding that the percentage of noteworthiness in the generality is as high as 1 °/₀? Galton himself says:

"The reader may work out results for himself on other hypotheses as to the percentage of noteworthiness among the generality. A considerably larger proportion would be noteworthy in the higher classes of society, but a far smaller one in the lower; it is to the bulk, say three-quarters of them, that the 1 per cent. estimate applies, the extreme variations from it tending to balance one another.

"The figures on which the above calculations depend may each or all of them be changed to any reasonable amount, without shaking the truth of the great fact upon which Eugenics is based, that able fathers produce able children in a much larger proportion than the generality." (p. xli.)

Finally Galton refers to the fact that while there was a general high level of ability in the families of F.R.S.'s, some parents were in no way remarkable, so that the "Fellow" was simply a "sport," in respect of his taste and ability. "It is," he remarks, "to be remembered that 'sports' are transmissible by heredity, and have been, through careful selection, the origin of most of the valuable varieties of domesticated plants and animals. Sports have been conspicuous in the human race, especially in some individuals of the highest eminence in music, painting and in art generally, but this is not the place to enter further into so large a subject." Galton cited Bateson, De Vries and his own earlier writings (see our pp. 79 *et seq.*) for the treatment of this topic.

I find it very difficult to accept the view that a Fellow of the Royal Society, whose parents or even the whole of whose known kindred fail to be remarkable, or rather to have been recorded as remarkable, is a sport. In the first place when a pedigree like that of the musician Bach is fully worked out, he is seen to be very far from a sport; he is only the ablest member of a very able musical stirp. And in the next place, if we take a family every member of which for indefinite generations has been mediocre for any given character, we find the variability of an array of offspring is some 70 °/₀ of the variability of the population at large, which contains among its members the specially able. Hence although the specially able will not

* We might divide these numbers by two, if we assume that in collateral kinship, there will be two on the average who will reach an age when to be noteworthy is possible.

arise as frequently from the mediocre stirp as from the able stirp, they will occur albeit in smaller numbers. I see no reason for terming such occurrences "sports" (see our pp. 78–9, 102–3 above).

Galton's *Preface* was written when he was 84 years of age; it was written at a time when he was feeling keenly that he could no longer undertake the lengthy accumulation of data and their reduction. Nevertheless it is remarkable in its discovery of new problems to be solved and in the suggestions of how they may be solved. The rest of the book is somewhat ephemeral in character, and its judgments of noteworthiness open to criticism, but I think Galton's contribution deserves to be preserved, and I have therefore abstracted it at length here.

Miscellanea. Closely allied to the endeavour Galton made to obtain a register of noteworthy scientific families was a schedule he prepared entitled: "Register of Able Families," with a view to collecting material on a broader basis than that of the Royal Society. The object of the inquiry was "to collect information concerning a large number of exceptionally able families in *all ranks* of society." Ability and exceptionality are therein defined as follows:

"Ability refers to the powers of mind or body, to character, and to every quality which makes a person valuable to his country or to the society in which he lives. It is shown by an artisan who becomes a foreman or an employer, by a clerk who rises to a position of trust, by a private soldier who gains a commission, by a student who wins scholarships and university honours, by those who educate themselves in the absence of other opportunities of instruction, and by all who have fairly achieved honourable distinctions."

Exceptionality, we are told, refers to the middle classes:

"The same amount of ability that is exceptional among them would be very much more exceptional among the lower classes, but not very uncommon in the most distinguished circles of society. The interpretation of the word in each particular case is left to the judgment of the correspondent."

Then comes a characteristically Galtonian paragraph:

"The merit of a family as a whole falls under three distinct heads: (1) Its number, large families being more valuable than small ones when the individuals are of equal merit. (2) The average merit of the individuals. (3) The absence of serious drawbacks in respect to character or physique. *Civilised man being at present the worst bred of all animals, it is extremely rare to find families who are unstained by any moral or physical blemish**. Correspondents should, therefore, not err on the side of diffidence in proposing names; it will be the business of the office to examine the returns that are received and to select the best."

This circular was issued, but probably not in large quantities. What returns Galton obtained I do not know. At any rate no filled-in copies were among the papers that reached the Laboratory named after him. He may have destroyed what he received as worthless, or recognised before its issue that the circular must fail of its object. Exceptional ability is the last to recognise itself under that name, and if you ask mediocrity to register ability you will find that even if it can recognise its existence, it cannot appreciate its degrees, and will almost certainly underestimate its national importance.

* Italics the biographer's.

Galton, as I have often informed the reader, was ever young, ever believed that his fellow mortals had the same enthusiasm for the acquisition of knowledge that he himself had, and was always trustful that they would act as dispassionately in assessing their fellow mortals as he himself acted. Thus he launched his schedules and seemed never discouraged even when they brought little or no harvest!

In the January number of the *Monthly Review* for 1903 Sir Edward Fry published a paper entitled: "The Age of the Inhabited World." In this paper he endeavoured to show that Natural Selection is incapable of doing much that has been accredited to its agency, especially citing the case of mimetic insects. He wrote:

"...useful deception will not take place until the protected form is nearly approached. Thus during the whole interval occupied in passing from the normal form of group *A* to near the normal form of group *B*, natural selection will have been entirely inoperative....Either birds are deceived by a small amount of imitation or they are not. If they are, natural selection cannot have produced perfect imitation; if they are not so deceived, then group *A* has passed over from its original form to something close upon the form *B* without any guidance from this principle."

Galton criticised this statement in *Nature*, February 12, 1903 (Vol. LXVII, p. 343) in a letter entitled: "Sir Edward Fry and Natural Selection." He writes:

"I deny this sharp dilemma and assert the existence of many intermediate stages. Two objects that are somewhat alike will be occasionally mistaken for one another when the conditions under which they are viewed are unfavourable to distinction. The light may be faint, only a glimpse of them may have been obtained, the surroundings may confuse their outlines*. While these conditions remain unchanged, the frequency of mistake serves as a delicate measure of even the faintest similarity....If one edible group *A* has individual peculiarities within the limits of variation, that give it a resemblance, however slight, to one of the noxious group *B*, it will occasionally be mistaken by a bird for a *B* and allowed to live unharmed. The similarity may be due to a characteristic attitude, to a blotch of colour, to a preference for resting on a part of the foliage to which its own form bears some likeness, or to other causes. In any case, it may well prove to be the salvation of 1, 2 or more per cent. of those who would otherwise have been seen and eaten. If so the thin edge of natural selection will have found an entrance, and its well-known effects must follow."

It will be noted that Galton says "within the limits of variation." That point is so often overlooked that I must again emphasise it. Few biologists have ever measured the blotch or spot on a butterfly's wing in the case of 400 or 500 members of the same species. They think in terms of a type specimen and suppose the type of one species has to be *gradually* shifted by small stages to the type of another. But the absolute *range* of variation may possibly be 25 °/。 of the type value†. Stringent selection for one or two generations may easily raise the type 10 °/。 or 15 °/。. Such selection is not the same thing as proceeding by minute stages.

* I think Galton is here thinking of his own experimental work on degrees of resemblance and the use of blurrers: see our Vol. II, pp. 329–333.

† The mean length of thigh bone in the type Englishman is say 447 mm., but the *range* of English thigh bones runs from 381 to 513, a range practically covering the type of all existing races. If existence for man depended on the length of his thigh bone there is nothing to prevent severe selection—say the destruction of all individuals with thigh bones over 400 mm.—lowering the English thigh bone to the value, 411 mm., of the Fuegian even in a couple of generations.

Sir Edward Fry replied in *Nature*, March 5, 1903 (Vol. LXVII, p. 414), and falls at once into the fallacy of supposing that because variation in group A is continuous, it can only approach group B by converting *minute* points of likeness in the midst of unlikeness into such a preponderance of likeness as to produce deception. He holds, as so many others have held, that the theory of the accumulation of *minute* variations fails to account for the facts of mimetism. The error lies in supposing that because the organ varies "continuously," therefore evolution by natural selection involves a gradual accumulation of minute variations in a given direction. Let us suppose the edible group A to enter a new environment, where the protected group B exists, and that a small percentage of A differing *widely* from type has a sufficient resemblance to B to escape destruction at any rate to some *smaller degree than its brethren. The bulk of A will be rapidly destroyed*, but the widely divergent section will be, as it were, isolated by the destruction of their fellows, they will inbreed, and the tendency will be, according to the heredity theory of progressive evolution (see our p. 58), for the protecting character to continually increase in intensity, until in a larger and larger percentage it succeeds in deceiving its foes. Sir Edward Fry's appeal to the interspace that separates "the first minute change that deceives no one to the point of first deception," in which interspace he holds natural selection cannot operate, is clear evidence to my mind that he did not know how wide is the range of variation in nearly all organs of all organisms. Natural selection is not forced to choose an individual differing by a minute amount from the type. To hold this view is to think only in terms of the type, and not in terms of the whole population.

Some further communications very typical of Galton may be noted here.

He was far too human not to appreciate what the mass of men found of interest, and among other gatherings, he enjoyed great race meetings. Speaking of the Derby he writes in his *Memories* (p. 179):

> "For my own part, I especially enjoy the start of the horses, for their coats shine so brightly in the sunshine, the jockeys are so sharp and ready, and the delays due to false starts give opportunities of seeing them well. I don't care much for its conclusion; but I used often after seeing the start to run to the top of the rising ground between the starting point and the stand, and sometimes got a good opera-glass view of much of the finish."

That Galton frequently went to the Derby is clear, and two instances deserve notice as characteristic of the man. On one of these occasions he persuaded Herbert Spencer and an Oxford clerical don to accompany him. We can imagine how Galton would enjoy this incongruous party who, however, he tells us, enjoyed each other's society. "All went off quite well, except that Spencer would not be roused to enthusiasm by the races. He said that the crowd of men on the grass looked disagreeable, like flies on a plate; also that the whole event was just what he had imagined the Derby to be."

Nevertheless Spencer was sufficiently fascinated to join Galton's Derby party again. We have unfortunately not the don's impressions of the philosopher, the statistician or the races! On another occasion Galton found it too hot to run to the hill, and facing the distant stand he watched the massed faces on the grand stand before the race and just as the horses approached the winning post. The result of his observations was communicated to *Nature**, and runs thus:

The Average Flush of Excitement.

"I witnessed a curious instance of this on a large scale, which others may look out for on similar occasions. It was at Epsom, on the Derby Day last week. I had taken my position not far from the starting-point, on the further side of the course, and facing the stands, which were about half a mile off, and showed a broad area of white faces. In the idle moments preceding the start I happened to scrutinise the general effect of this sheet of faces, both with the naked eye and through the opera-glass, thinking what a capital idea it afforded of the average tint of the complexion of the British upper classes. Then the start took place; the magnificent group of horses thundered past in their fresh vigour and were soon out of sight, and there was nothing particular for me to see or do until they reappeared in the distance in front of the stands. So I again looked at the distant sheet of faces, and to my surprise found it was changed in appearance, being uniformly suffused with a strong pink tint, just as though a sun-set glow had fallen upon it. The faces being closely packed together and distant, each of them formed a mere point in the general effect. Consequently that effect was an averaged one, and owing to the consistency of all average results, it was distributed with remarkable uniformity. It faded away steadily but slowly after the race was finished. F. G."

There is a notion still very current that gouty constitutions should avoid stoneless fruits, in particular strawberries. Galton's creed was that: "General Impressions are never to be trusted. Unfortunately when they are of long standing they become fixed rules of life, and assume a prescriptive right not to be questioned." What about gout and that noble fruit the strawberry? Galton (as well as his biographer) had come across instances, wherein belief dominating desire, enforced asceticism, and so deprived the believer of much harmless pleasure, by dogmatically asserting harmful consequences. Judge of Galton's joy while reading the biography of Linnæus, at discovering that the great naturalist, when the doctors failed to cure his gout, had got quit of his disease by large doses of strawberries! Galton wrote in 1899 a letter to *Nature*† on Linnæus' strawberry cure for gout. One can see the twinkle in his eye as he looked from his writing table towards Harley Street.

"The season of strawberries is at hand, but doctors are full of fads, and for the most part forbid them to the gouty. Let me put heart into those unfortunate persons to withstand a cruel medical tyranny by quoting the experience of the great Linnæus....Why should gouty persons drink nasty waters at stuffy foreign spas, when strawberry gardens abound in England?"

A further characteristic letter appeared in *Nature*, December 20, 1906 (Vol. LXXV, p. 173) regarding the "Cutting a Round Cake on Scientific Principles." The problem to be solved was clearly a personal one for Sir Francis and his niece, who averaged a small cake every three days. "Given a round tea-cake of some 5 inches across and two persons of moderate appetite to eat it, in what way should it be cut so as to leave a minimum of exposed surface to become dry?" The accompanying diagram shows

* June 5, 1879 (Vol. XX, p. 121).

† June 8 (Vol. LX, p. 125). See D. H. Stoever, *Life of Sir Charles Linnæus*, 1794 (Eng. Trans.), p. 416.

Sir Francis' solution. Broken lines show intended cuts; ordinary straight lines the cuts that have been made. The segments are kept together by an elastic band.

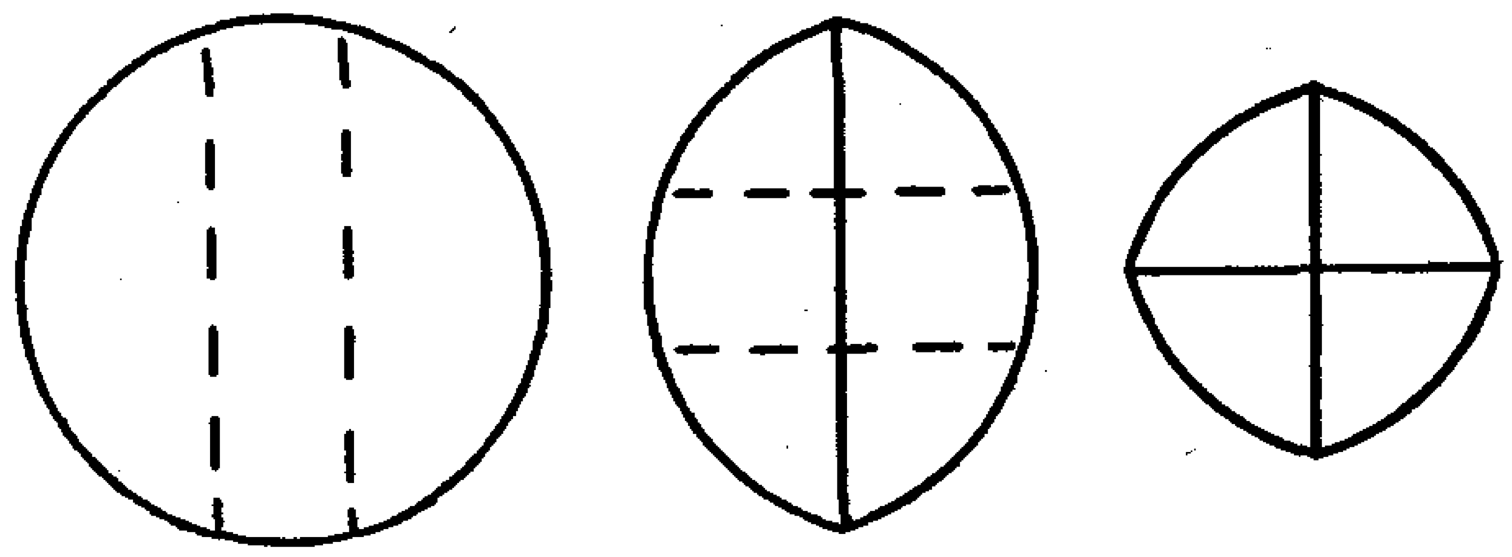


Fig. 13.

Always assuming, which I feel some doubt about, that both consumers of this cake ate their daily allotment of the circular rind, the method leaves an unconscionable amount of dry rind (some $\frac{4}{7}$th) for the third day's consumption! I rather suspect that the cook would have been instructed by the lady of the house to bake in rectangular tins in future.

Another amusing contribution: "Number of Strokes of the Brush in a Picture," was made to *Nature*, June 29, 1905 (Vol. LXXII, p. 198). Galton as I have already noted* sat in 1882 for his portrait (not a very successful one) to Graef. The source of the failure is, perhaps, revealed, for Galton finding it tedious to sit doing nothing counted the painter's slow methodical strokes per minute and then averaged them up. As he knew only too well the number of hours spent in the sittings, he obtained the total he desired to ascertain, some 20,000 strokes to the portrait. About 22 years later he was painted by Charles Furse†, whose method was totally different from that of Graef. He looked hard at Galton while mixing his colours, then he made dabs so fast that Galton found difficulty in keeping up his count. The difference of the two artists' work will be recognised, if the reader compares the Graef picture (Vol. II, Plate XI) with the Furse picture (Frontispiece to Vol. I). It may, however, destroy his pleasure in both, if he thinks of the two artists both having caught the aspect of Galton when silently counting! However to Galton's great surprise Furse's dabs came out about 20,000 to Graef's 20,000 strokes! Only we must remember that Furse did not fully complete his portrait. For comparative purposes Galton computed the number of stitches in an ordinary knitted pair of socks and found 102 stitches in the widest part to each row and 100 rows to 7 inches, whence he computed that the leg parts of a pair of socks would contain over 20,000 separate movements, or rather more than required for a portrait. Galton concludes:

"Graef had a humorous phrase for the very last stage of his portrait, which was 'painting the buttons.' Thus, he said, 'in five days' time I shall come to the buttons.' Four days passed, and the hours and minutes of the last day, when he suddenly and joyfully exclaimed, 'I am come to the buttons.' I watched at first with amused surprise, followed by an admiration not far from awe. He poised his brush for a moment, made three rapid twists with it, and three

* Vol. II, p. 99 and Plate XI.

† Furse died October 16, 1904, of phthisis. His unfinished portrait of Galton must have been one of his last works.

well painted buttons were thereby created. The rule of three seemed to show that if so much could be done with three strokes what an enormous amount of skilled work must go to the painting of a portrait which required 20,000 of them. At the same time, it made me wonder whether painters had mastered the art of getting the maximum result from their labour. I make this remark as a confessed Philistine. Anyhow I hope that future sitters will beguile their tedium in the same way that I did, and tell the results*."

Committee for the Measurement of Plants and Animals. It is impossible to pass over in Galton's *Life* the last decade of the nineteenth century without some reference to this Committee; it took up too much of Galton's energies and consumed too much of his valuable time to remain without some notice in his biography. But the time has hardly yet arrived, when it is possible to write fully about it, and cite at length the voluminous letters and other documents which indicate the parts played by various individuals in first hindering and then entirely perverting the original purposes of the Committee.

The Committee was appointed at Galton's suggestion by the Royal Society Council on January 18, 1894, and consisted of Francis Galton (Chairman), Francis Darwin and Professors Macalister, Meldola, Poulton and Weldon (Secretary), with the very definite purpose of "conducting Statistical Inquiries into the Measurable Characteristics of Plants and Animals." The first report was made in 1896, and consisted of a detailed account of Weldon's measurements on *Carcinus mœnas*, and also his "Remarks on Variation in Animals and Plants†." In the latter paper Weldon emphasised his own view that while "sports" in certain exceptional cases may contribute to evolution, ordinary "continuous" variations were a more probable source of change and further stated, what is almost self-evident, that "the questions raised by the Darwinian hypothesis are purely statistical, and the statistical method is the only one at present obvious by which that hypothesis can be experimentally checked." In asserting this he was only saying that heredity and selection in Nature are mass phenomena and must be treated as such. To those who have read the earlier pages of this chapter, it will be clear that Weldon's view as to the relatively small importance for evolution of "sports" was opposed to Galton's, but this divergence of opinion by no means caused friction between the Chairman and the Secretary of the Committee. It did, however, call forth reams of criticism and numerous letters of protest from William Bateson to the Chairman. The only addition to the Committee in 1896 was, however, that of the present biographer. That Weldon's paper admitted of criticism not only from the biological, but from the statistical side must be allowed, but the fatal mistake was the old one, the evil of attempting to work through a Committee. Had Weldon's paper been published

* Would the result be that many subjects would have the strained look of those practising mental arithmetic? The late Mr Hope Pinker told me that he was once modelling a bust of Jowett. The Master remained stolidly silent; Pinker found his task hopeless, and told Jowett that he must throw up his commission, unless the Master consented to talk. "I will try to be good, I will try," replied Jowett, and the portrait was completed. It is not always the artist's fault, if sittings end in a failure.

† See *Roy. Soc. Proc.* Vol. LVII, pp. 360–382.

as his independent contribution, it could have been criticised in the usual way; he could have defended it, and its merits as well as the difficulties of its subject would have been amply recognised. As it was the reason given for the criticisms (which came from more than one quarter) was that of saving the Committee from making serious blunders*. The Chairman became the centre to which attack and rejoinder were directed, and in despair he wrote to Weldon on November 17, 1896:

"Herewith is another paper from Bateson, and I enclose with this his accompanying letter to myself. We must talk over what is the fairest course to adopt when we meet (as we probably shall) before the meeting of the R. Soc. on Thursday.

"You see that he offers to print his four letters for circulation among members of the Committee. My greatest difficulty in thinking what should be done arises from the lengthiness of these papers. I wish the issue could be stated in much more condensed language.

"It would in many ways be helpful, if Bateson were made a member of our Committee, but I know you feel that in other ways it might not be advisable†. The other members besides yourself hardly do enough."

In 1897 the Committee was enlarged by the addition of zoologists and breeders, some of whom had small desire to assist quantitative methods of research—*Sir E. Clarke, F. D. Godman, W. Heape, E. Ray Lankester, E. J. Lowe*, M. T. Masters, O. Salvin, W. T. Thiselton-Dyer and W. Bateson. It was further rechristened "Evolution (Plants and Animals) Committee of the Royal Society." For several years there was no dominant personality, who could effectively guide this very mixed assembly. Personally I ceased to attend its meetings, resigning in 1900, and was followed in that year by Weldon and later by Galton. Mr Godman then became Chairman and the Reports of the Committee were devoted entirely to the publications of Bateson and his school. The capture of the Committee was skilful and entirely successful‡. I think the feeling of the young biometricians towards Galton's enlarged Committee was more or less expressed by the letter to Galton I now quote, the date is February 12, 1897:

"I wanted to write a few words to you about yesterday's meeting, but have hardly had, nor indeed hardly now have time to do so. I felt sadly out of place in such a gathering of biologists, and little capable of expressing opinions, which would only have hurt their feelings

* A paraphrase of some of these criticisms will indicate the spirit in which they were written. Vast labour, it was said, had been put into the work and its author no doubt thought himself justified in the conclusions put forward. Perhaps the Committee had thought too little of the responsibility it undertook in publishing such work. The author must know that many would accept his conclusions though few would be able to follow the paper or judge the matter for themselves. Nevertheless the critic found the evidence so inadequate and superficial that he could not understand how responsible people could entertain the question of accepting it. He very truly regretted the countenance given to such a production, etc. etc. Poor Galton! There are some people, whose unfortunate temperaments compel them to believe that as a matter of conscience they are born to be their brothers' keepers.

† Bateson had absolutely no sympathy with the statistical treatment of biological problems, the very work for which the Committee had been appointed.

‡ Perhaps the small understanding shown by the ruling spirits of the Royal Society of what had taken place, was evidenced in 1906, when inquiries were made as to whether the Society would accept the Weldon Memorial Medal and Premium, and the President wrote suggesting that the Evolution Committee would be an appropriate selecting body!

and not have been productive of real good. I always succeed in creating hostility without getting others to see my views; infelicity of expression is I expect to blame. To you I mean to speak them out, even at the risk of vexing you.

"All the problems laid down by you in your printed paper seem to me capable of solution, and nearly all of them *in one way only*, by statistical methods and calculations of a more or less delicate mathematical kind. The older school of biologists cannot be expected to appreciate these methods, *e.g.* Ray Lankester, Thiselton-Dyer, etc. A younger generation is only just beginning its training in them.

"I believe that your problems could be answered by direct and well devised experiments at a 'farm' or institute under the supervision of some two or three men who appreciate the new methods. I think you were entirely right in the idea of a committee to carry out such experiments. But I venture to think that the Committee you have got together is entirely unsuited to direct such experiments. It is far too large, contains far too many of the old biological type, and is far too unconscious of the fact that the solutions to these problems are in the first place statistical, and in the second place statistical, and only in the third place biological. It was the character of the Committee as now constituted which led me to support Michael Foster's motion that the Committee should not experiment, but assist experiment, and further to object to his words 'under the Committee.' Fancy the attempt to make real experiments on variation, correlation, or coefficients of heredity 'under a Committee' of which, I shrewdly suspect, only the Chairman and Secretary know the significance of these terms!

"Hence to sum up, your method seems to me a right one—a Committee to undertake experiments of a definite statistical character*. But your actual Committee is quite a wrong one. It might be a good Committee to press the public with subscription lists; but it is, I believe, a hopeless one to devise experiments which will solve in the only effective way these problems."

Meanwhile besides the criticisms already referred to, there were factors, other than the hope of peace, inducing Galton to enlarge his Committee and widen its programme. As early as February 3, 1891, Alfred Russel Wallace had written to Galton urging that the time was ripe for an experimental farm or institute to undertake researches which might decide disputed points in organic evolution.

Copy of Letter from Alfred Russel Wallace to Francis Galton†.

PARKSTONE, DORSET. *February* 3, 1891.

MY DEAR MR GALTON, Don't you think the time has come for some combined and systematic effort to carry out experiments for the purpose of deciding the two great fundamental but disputed points in organic evolution,—

(1) Whether individually acquired external characters are inherited, and thus form an important factor in the evolution of species,—or whether as you & Weismann argue, and as many of us now believe, they are not so, & we are thus left to depend almost wholly on variation & natural selection.

(2) What is the amount and character of the *sterility* that arises when closely allied but permanently distinct species are crossed, and then "hybrid" offspring bred together. Whether the amount of infertility differs between the hybrids of species that have presumably arisen in the *same area*, & those which seem to have arisen in very *distinct* or *distant areas*—as oceanic or other islands.

* The Royal Society had on Dec. 11, 1896, decided to retain the old name of the Committee, which contained the word "measurement." It was not till the following year, that with enlarged numbers and a wider programme, the Council acceded to Galton's request that the Committee be called "The Evolution (Plants and Animals) Committee."

† I have to thank Alfred Russel Wallace's son, Mr W. G. Wallace, for kindly permitting me to publish the following letters of his father.

Both these questions can be settled by experiments systematically carried on for ten or twenty years. The question is how is it to be done. Talking over the matter with Mr Theo. D. A. Cockerell, a very acute & thoughtful young naturalist, we came to the conclusion that a Committee of the British Association would probably be the best mode of carrying out the experiments, by the aid of a B. Ass^{n}. grant & a Royal Society grant, aided perhaps by subscriptions from wealthy naturalists.* It seems to me that *one* paid observer giving his whole time to the work could carry out a number of distinct series of experiments at the same time,—and if the Zool. Soc. would allow some of the experiments to be made with their animals in their gardens much expense would be saved. To be really good however the hybridity experiments (and the others too) would have to be carried out with large numbers of animals, and thus some sort of small experimental farm would be required. Surely some wealthy landlord may be found to give a small tenantless farm for such a purpose. Then, using small animals such as *Lepus* and *Mus* among mammalia, some gallinaceous birds and ducks, and also insects, a good deal could be done even on a large scale, at a small cost. On the same farm a corresponding set of plant-experiments could be carried out; and an intelligent well educated gardener or bailiff, with a couple of men, or even one, under him, could superintend the whole operations under the written directions and constant supervision of the Committee.

Would you move for such a Committee at the next B. Ass^{n}. Meeting? *You* are the man to do it both as the original starter of the theory of non-inheritance of acquired variations, the only experimenter on pangenesis, & the man who has done most in experiment and resulting theory on allied subjects.

We thought first of a separate Society, but I doubt if a new society could be established & supported, whereas a Committee either of the B. Ass^{n}. or of the Royal Society could do the work quite as effectively & would probably receive as much support from persons interested in these problems. It seems to me a sad thing that years should pass away & nothing of this kind be systematically done. I feel sure you would meet with general support if you would propose the enquiry. Believe me, Yours very faithfully, ALFRED R. WALLACE.

FRANCIS GALTON, F.R.S.

P.S. It would of course be better still if a fund could be raised sufficient to establish an *Institute* for *experimental Enquiry into the fundamental Data of Biology*. This is surely of far higher importance than the anatomical, embryological, & other work for which the Plymouth Biological Station was founded. A. R. W.

42, RUTLAND GATE, S.W. *Feb.* 5/91.

MY DEAR MR WALLACE, The views you express so clearly & forcibly, agree with those I have often considered—ranging between a modest cottage with hutches & a bit of ground, up to an Heredity Institute. There was also a half move in this direction made last spring by Ray Lankester, Romanes & others. The difficulties I fear and which I hope you can remove, are as follows. Let us suppose that funds have been collected, a small farm procured and a sensible manager installed in it and that operations are ready to begin. Also I would suppose that the cost of conducting experiments would be met by those who devised them, who themselves had obtained a grant for the purpose from the R. Soc., Brit. Assoc. or otherwise.

Now (1) I doubt if it would be easy to devise a sufficiency of experiments to occupy the establishment of a sort that wd. generally be recognised as crucial. In the two groups of desiderata you mention, no one that I know of, has yet suggested an experiment, much less several experiments, that those who believe in and those who don't believe in the hereditary transmission of acquired characters would accept as fair. If a few such could be devised all my fears as to the utility of the establishment would vanish. If it could settle this one question pains and cost would be amply repaid.

(2) Similarly as regards the sterility question though in a much less degree. The uncertain and often large effects of confinement on fecundity would be a serious disturbing cause.

It then seems to be the first desideratum before making any move that a fairly long list of definite problems, that such an establishment might be set to work upon, ought to be drawn up. Would you put your views as to these on paper?

The number of experimenters is sadly small.

(3) Another difficulty is that the experiments are not likely to be so carefully tended & guarded in an establishment as they would be by oneself or by personal friends. I have had some very marked evidence of this in my own experience, which I don't like to put on paper for fear of causing annoyance.

If the difficulties I have mentioned can be shown to be small, all the rest would be plain sailing. The farm would bear a similar relation to Heredity both plant and animal that the Kew Observatory does to experimenters in Physical Science.

It might grow into a repository of stud books and all about domestic animal breeding, and pay its way well in this department. Also it might become a repository of family genealogies & facts about human heredity, and also pay its way here; the people love to have their genealogies put on record, photos of family portraits preserved, &c. & would pay for the trouble it might cost to keep them.

But the first thing is the experimental farm—in connection with Kew or Chiswick—the Zool. Society & Marine biological laboratories. It could be started moderately under the same roof, so to speak, as one of these, so as to avoid many expenses of a separate establishment, while an independent home was being prepared for it to be entered into if it succeeded.

I have much that would be helpful to say, if you can remove these initial difficulties of prospect. Very sincerely yours, FRANCIS GALTON.

Pray give our united kind remembrances to Mrs Wallace, & accept them yourself.

Copy of Letter from Alfred R. Wallace to Francis Galton.

PARKSTONE, DORSET. *Feby. 7th,* 1891.

MY DEAR MR GALTON, On receipt of your interesting letter I sat down & jotted the enclosed notes of the *kind* of experiments that it seems to me *would* test the theory of heredity or non-heredity of individually-acquired characters. Also a few as to fertility or sterility of hybrids, & as to the real nature of *some* of the supposed *instincts* of the higher animals. I do not myself see *much* difficulty in carrying out any of these, but then I am not an experimenter as you are. Still, I shall be glad to know exactly where the difficulty or insufficiency lies. If these, or any modifications of them, would be valuable & to the point, it seems to me that the mere keeping the plants and animals in health & properly isolated would fully occupy the keeper or keepers of the farm,—while the actual experiments—the deciding on the *separation without selection* of the various lots to experiment with,—which should be crossed & when, and other such matters, would recur only at considerable intervals & could be supervised by the members of the Committee, or some of them, by means of, say, a weekly inspection.

I have limited my notes to three points in which I feel most interest, but of course experiments in *variation* such as Mr Merrifield is carrying on for you, could be added to any extent if there were any danger of the keepers having too little to do!

All the experiments I suggest would require considerable numbers of individuals to be kept healthy and to be largely increased by breeding,—and they would all have to be continued during several years depending on the duration of life of the various species experimented with.

My wife and I are in pretty good health & beg to be kindly remembered to Mrs Galton. As everybody seems to come to Bournemouth we shall hope some day to have a call from you.

Yours very faithfully, ALFRED R. WALLACE.

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

This letter was accompanied by a detailed list of possible experiments.

42, RUTLAND GATE, S.W. *Feb.* 12/91.

MY DEAR MR WALLACE, I have thought much & repeatedly over your letter & have talked with Herbert Spencer & with Thiselton-Dyer, but cannot yet see my way. I hate destructive criticism,—for it is so easy to raise objections,—& want to offer constructive criticism & to help progress but have every point in view & in all the details I see serious difficulty without any considerable gain.

As an example of many others of the suggested experiments, take the first, viz. that of plants in windy & in still localities. Suppose (1) there was a difference in the seedlings from them, then the advocates of non-inheritance of acquired faculties would protest against its applicability saying that there *had* been selection, the lofty plants & the wide spreading ones would have been preferentially blown down and the weakly ones would have been killed by the rigour of conditions, therefore there had been selection in favour of the small & hardy. Now suppose (2) that there was no difference,—then the same people would say "I told you so." The expt would be for them a case of "heads you lose, tails I win."

Next, to produce any notable effect the expt must, as agreed by all, be protracted for many generations.

Lastly, nature affords an abundance of excellent examples, far superior to artificial ones. Thus take an (elevated) region swept with winds but with hollows in it which are sheltered and all of which is forest clad. The trees in the sheltered hollows will have been from time immemorial finer than those of the same kinds of the exposed places; collect the seeds and plant them under like conditions elsewhere.

During a (Swiss) tour a man might collect an abundance of such seeds of contrasted origin of many species of trees. Even a morning's walk would afford more data than a century of artificial experiment.

So again the seeds of plants originally of English stock but reared for some generations in various parts of the world might be collected and planted side by side.

[The last is Thiselton-Dyer's proposal.]

The only certain employment in the plant department of your proposed farm is to make experiments such as these, or rather to verify in a regular methodical way much that is known already, including expts on the opposite side such as graft-hybridism.

Dyer says that no *experimental* work is likely to succeed at such places as Kew in the ordinary course of work, where careful oversight is required. The men have much other work to do. It would require a man to be specially devoted to its oversight.

The animal experiments seem to be enormously costly.

The case you mention of hybrids & sterility would require many hundreds of animals at the lowest of the computations you give data for. Where the effects of disuse are concerned the animals should be, as a rule, underfed as regards their appetites and only eat just enough to keep them in health; then as there is a deficiency of material for growth, economy of structure would be effective. This would be *very* difficult to ensure. Some of the most interesting experiments are those of the Brown-Séquard type, but these must be put out of court in the present mood of the public & of the law.

Is not the bird nesting experiment continually the unconscious subject of experiment in those fowls who have been hatched from eggs in incubators?

Did you happen to see some remarks I made at Newcastle British Assoc/n, which are printed in the last Journal but one?

I suggested expts on those creatures which are reared from eggs apart from parents. Chickens in incubators, fish, & insects. The incubator industry is large in France & so is the silk-worm. But the naturalists present seemed not inclined to dwell on those views*.

Could anything be made of the following:

A farm for the verification of easy experiments, within easy reach of London.

Cordial relations between it and

(1) The Zoo., the Horticult., Kew, & Royal Agricult. Society.

(2) Private persons of various ranks who would agree to help in expts.

Library of reference on heredity got mostly by begging.

Log-book of daily work preserved (? in duplicate).

Publication of results in some one of the existing Scientific periodicals.

Superintendent (qualifications & Salary to be considered).

All under a c/ttee (? of the Royal Society).

In all this I am keeping the Kew Observatory in view as a somewhat analogous institution.

But before anything could be done, even before asking for its serious consideration, a few *carefully* and *fully* worked out proposals of experiment ought I think to be drawn up. I mean just as much as would have been done if the proposer handed them in to the Gov/t Grant or other committee, for a grant of money. Very sincerely yours, FRANCIS GALTON.

* See our p. 57 above.

Copy of Letter from Alfred R. Wallace to Francis Galton.

PARKSTONE, DORSET. *Feby.* 13*th*, 1891.

MY DEAR MR GALTON, It will be I am afraid impossible to discuss the difficulties of experiment you urge by correspondence, and I will therefore confine myself to a short reply to the objections you have actually made, which seems to me very easily done.

Plants in windy and still air.

You say, "it might be said" there had been selection. But this is very easily obviated, & is the very point on which experiment is superior to observation of nature. In an ordinary open garden or field plants properly cultivated are *not* killed or prevented from flowering & seeding by wind. They grow healthily under it, and I feel sure that not *one* in a *hundred* plants would so suffer. The contrast wd. be produced not by the *violence* of the wind in the one case but by its absence in the other set, they having grown in a glass-covered (or glass-sided) garden. If a common perennial plant was grown—a mallow or a wallflower—for example—a set of 50 or 100 plants might be grown on for 3 or 4 years so as fully to establish whatever change could be produced in the *individuals* by the diverse conditions. Then at the end of that time take the *whole* of the seed produced by each lot,—take two samples of say the 100 smallest or lightest or better perhaps 100 of the average of each, and cultivate them side by side under *identical conditions.* It would not matter to *me*, or I think to *you*, what anybody *said*, but if there were—(*a*) a decided & measurable difference in the two lots of plants from which the seeds were taken, and—(*b*) there was *no* measurable or decided difference between the plants grown from these seeds under identical conditions, this would be *one definite fact against inheritance**,—while if there was a difference of the same nature & fairly comparable in amount it would be a decided *fact in favour of inheritance.* No doubt it might be urged that the effect would be minute but cumulative, & that might be admitted, & the experiment continued under exactly the same conditions for say ten generations. If then no differential effect were produced in the offspring the evidence would be strong against inheritance. Of course the fairest way would be for the advocates of inheritance to formulate the experiments they would admit to have weight, and the opponents of inheritance to do the same.

Then you say "nature affords an abundance of excellent examples, far superior to artificial ones." This I altogether demur to. In nature we *always & inevitably* have selection of various kinds, due to soil, aspect, winds, enemies, overcrowding, &c. &c. &c. & we cannot *possibly separate* the effects of these from any possible *inherited effects* due to diversity of conditions. But this is what we *can & do* do in cultivation.—We save plants from overcrowding & therefore from the struggle with other plants, we can give all the same soil & aspect, protect all alike from enemies, give both the same selection or the same absence of selection of seeds. In nature you cannot possibly tell whether any peculiarity in individuals is due to *conditions* or to *genetic variation*, while if you take those cases where the difference is clearly in *adaptation* to *conditions*—as the dwarfer plants at higher altitudes—you have the probability, *almost certainty*, of a considerable amount of nat. selectn. By experiment you are able to avoid all these uncertainties & determine the effects of certain definite modifications of environment on *individuals*,—& then ascertain whether the modifications thus produced are inherited.

In nature too, you have the uncertainty introduced by double parentage; each parent in all cross-fertilised plants, may have had *different characters &* have grown under *different conditions.* In experiment you eliminate this cause of uncertainty.

Of course the experiments with animals would involve expense, but with the smaller animals not very much,—& I understood you to say that *this* would not be an obstacle.

If you or any one else will point out the difficulties or uncertainties in the other experiments I suggested I will be glad to answer them, as I think I have done in the *one case* you have referred to.

It is only in this way that we can arrive at a satisfactory mode of procedure, & I regret that I cannot have the advantage of discussing the question with yourself & others who are well acquainted with the subject and with the special difficulties of experimentation.

Believe me, yours very truly, ALFRED R. WALLACE.

* [i.e. of acquired characters.]

P.S. Pray do not trouble to reply to this unless you think anything further from me may be of any use. A. R. W.

Of course I have referred to the one experiment of *wind & no wind* as an example, not by any means considering it one of the best experiments. A. R. W.

It will be seen that Wallace had a due appreciation of the necessity for "large numbers"; he recognised that the true method of approaching these problems was *statistical*. If the time was ripe for such experimental work forty years ago, what must we consider it now?

Apparently it was not till 1895 that Galton having got his Committee on the Measurement of Plants and Animals recurred to Wallace's idea of an experimental farm, which Wallace in 1896 termed a "Biological Farm." But a new possibility had arisen, that of acquiring the Darwin house at Down as a station for experimental evolution. Everything was favourable to such a desirable project. The Darwin family were prepared to part with the house for a national purpose on terms which meant a very large contribution from themselves. Galton named a large sum which an anonymous donor was willing to contribute towards the work of experimentation. There can be little doubt that had the scheme been pushed with energy, Down might thirty years ago have been obtained for a purpose urgently necessary and thoroughly in keeping with the spirit of Charles Darwin's work. But a bold scheme only appeals to the bolder minds, and these seemed to be entirely wanting among the men to whom Galton wrote with the hope of engaging their support for the proposed Biological Farm*, as it was termed in the circular issued by Galton on November 30, 1896. I reprint that document here:

*To*________________

The Committee appointed by the Royal Society, for the Measurement of Plants and Animals, proposes to hold an informal meeting at the Royal Society, on Friday, December 4th, at 4 p.m., which they hope you will favour with your presence.

The purpose of the meeting is to discuss the propriety of asking aid from the Council of the Royal Society in establishing and maintaining a Biological Farm, to supply materials (mostly zoological) appropriate to the investigations on which the Committee is occupied, and for undertaking experiments in breeding during many successive generations for the use of those who study the causes and conditions of Evolution.

The general idea that such a Farm would fulfil, somewhat resembles that which was present to the founders of the physical Institute known as the "Kew Observatory," which has been for many years under a Committee of Management appointed by the Council of the Royal Society. It was to procure a place where investigators could have experiments carried on at their own cost, subject, of course, to the permission of the Committee of Management, the cost being, in most cases, defrayed out of grants in aid to the investigators, made by the Royal Society or by the British Association.

It is likely that a farm-house with 20 acres of suitably varied land, and some running water, would amply suffice, so long as the experiments were chiefly confined to small animals. The farm would be in the charge of a resident caretaker under the direct authority of a scientific superior, who might hold the office of Secretary to the Committee of Management. It would be his duty to see that their instructions were duly carried out.

Independently of the farm, and perhaps preliminary to the attempt to raise money for its maintenance, the suggested Committee could accomplish a very important service in a similar direction, for the performance of which it is believed that funds would be immediately

* Meldola, who was throughout warmly in favour of such an institution, actually termed it a "Biometric Station" in December, 1896.

available. That is, they might communicate with persons, many of high social position, who are breeders on a large scale in their own grounds, thereby initiating a widely spread system of co-operation in carrying out experiments desired by the Committee. It is not to be expected that the several results would be equally trustworthy with those made under specially trained management as in the proposed farm. On the other hand, whenever it was found that similar experiments made simultaneously at many different places led to the same results, those results would eminently deserve confidence. The incidental advantage of interesting influential persons in the work of the Committee would be great.

The cost of the complete scheme does not seem likely to be very formidable. It would be chiefly made up of the rental of the farm, the erection of enclosures, hutches, etc., the small initial cost of the animals, their feed, and the wages of the caretaker and assistants. The salary of the Secretary need not at first be large, since the duties of the office would not then be so onerous as to prevent his holding other appointments.

The meeting will be asked to consider this scheme, amending and altering it as desirable, to discuss its cost, and the ways of meeting that cost. If, after this, the prevalent feeling should be in favour of further proceedings, the meeting might appoint an Executive Committee, not consisting exclusively of Fellows of the Royal Society, to examine the subject closely in its various details, to consider the precise experiments that might be first undertaken, and to report to an adjourned meeting.

FRANCIS GALTON

(*Chairman of the Committee of the Royal Society for the Measurement of Plants and Animals*).

42, RUTLAND GATE, S.W.
November 30*th*, 1896.

The response was most heartrending. Even such warm friends of Galton as Sir J. D. Hooker and Herbert Spencer were not helpful. The former thought that experiments on plants could be undertaken at Kew, and no new station was needful; the latter thought the course suggested impolitic, the proposed purchase of the Darwin house was no doubt appropriate as a matter of sentiment, but most inappropriate as a matter of business. He would be disinclined to cooperate if any such imprudent step were taken*. Great matters must spring from small germs, which would only justify themselves by their success. Real encouragement came only from Adam Sedgwick, from Meldola, and from Weldon ("Surely £4000 can be raised somehow!"). The Darwin brothers it is needless to say wrote most generously and helpfully, but the scheme fell dead even among the biologists who thought it worth while to come to the meeting with the view of discussing it. There was among them no broad conception of what a station for experimental evolution might achieve for their science, and there was not the slightest chance of enthusiasm and energy being put into the project so that it might be carried to a successful issue. The money for the acquisition of Down was still to be found, but there was the sum of £2000 assured by the anonymous donor†, and one distinguished biologist, thinking a bird in the hand was worth two in the bush, asked, if they had not come to allot that sum for their experimental work, what had they come for? I never left a meeting with a greater feeling of despair, and this was shared by Weldon, and to a lesser extent by Galton, who was consoled to some extent by Francis Darwin's writing that, however much he regretted the Down project could not be worked, he was not going to

* As a matter of fact Spencer had not been consulted, but had heard of the matter indirectly through Adam Sedgwick, and had then written to Galton to know what it was all about!

† "There is assurance that a sum of £2000 would be available to start the undertaking, if a thoroughly satisfactory programme could be agreed to."

consider that scheme as finally dead. Now after thirty years it looks as if Down would be retained as a national possession. One may hope that it will be put to as good and fitting a purpose as Galton proposed for it. He has left a lengthy paper dealing with the work he considered the Biological Farm should undertake; it is based on the suggestions he received from many quarters, modified by his own ideas. It is a scheme for "Further accurate observations on Variation, Heredity, Hybridism, and other phenomena that would elucidate the Evolution of Plants and Animals." The matter is arranged under 16 headings, and it is sad to consider that, although more than thirty years have passed since the scheme was drafted, but little has been done to solve the problems therein suggested. It is impossible to print the full manuscript here, but some idea of what it deals with may be judged from its table of *Contents*:

"A. *Preparatory.* (1) Procedure (especially emphasising the need for continuity in observation and for secular experiments). (2) Cooperation (Institutions and Individuals). (3) Breeds suitable for Experiments (necessity for stores of pure stocks of small animals). (4) Place for Station (Down, and existing establishments). B. *Heredity as affected by and related to:* (5) Close interbreeding, Panmixia, Prepotency. (6) Hybridism. (7) Telegony. (8) Acquired modifications in parent. (9) Mental influence on Mother ("Jacobise" in a variety of ways). (10) Instinct (nest building by birds, who have never seen the nest of their species; directive instinct in dogs, taken to unknown place and watched from a distance by a stranger). (11) Variations, "Sports" and their intensity of inheritance. (12) Natural and Physiological Selection. (13) Parthenogenesis. (14) Fertility (many problems stated). (15) Sex and its causes. (16) Gestation."

The bundle of papers in which this and other schemes and letters from innumerable correspondents are included is labelled by Galton: "Old Papers concerning the Evolution Committee of the R. Soc. of probably no present value. Might be useful if a Darwinian Institute were ever founded." "Of probably no *present* value"—what a criticism of the biologists of 1890–1900!

Here, as in Experimental Psychology, Galton was ahead of his age, and few have recognised how much even by raising these questions, he stimulated that movement for experimental biology, which the present generation of biologists believes was unthought of by their Victorian predecessors. Thus came to an end Galton's plan for an experimental station for evolution; it was another illustration of the futility of working through ill-assorted committees. I say came to an end, but hardly in Galton's mind. It must I think have been in 1903, when in the summer vacation the biometricians were employed on their summer tasks at Peppard and Galton was of the company, that the matter again arose. One evening he asked his two lieutenants to prepare a draft scheme for a biological farm, to state its size, staff, equipment, its probable cost and annual expenditure for maintenance and experimentation. Weldon and I talked the matter over, and felt that although Galton was well-to-do, he was not so wealthy, that to run a biological farm might not deprive him of some of the easements necessary to his age. We therefore determined to estimate the cost of the farm on the scale of maximum effectiveness. It was a pious fraud, but the suggestion of a biological farm was never again referred to, and Galton's thoughts of increasing human knowledge soon turned to less expensive projects.

*Appendix to Chapter XIV**.

"The Weights of British Noblemen during the last Three Generations," *Nature*, January 17, 1884 (Vol. XXIX, pp. 266–268).

This rather amusing paper is not included in any list of Galton's memoirs known to me, nor were any offprints of it to be found in the *Galtoniana*. It seems to have been forgotten by Galton himself and would have certainly been overlooked by me had I not stumbled across it in reading Romanes' review of Galton's *Record of Family Faculties* and *Life History Album* in the same number of the Journal. Galton—whom the Goddess of Chance certainly favoured—became acquainted with the fact that an old established firm of wine and coffee merchants had been since about 1750 in the habit of weighing their customers, and that upwards of 20,000 persons, many of whom were famous in English history of the eighteenth century, had for their use or amusement sought the firm's huge

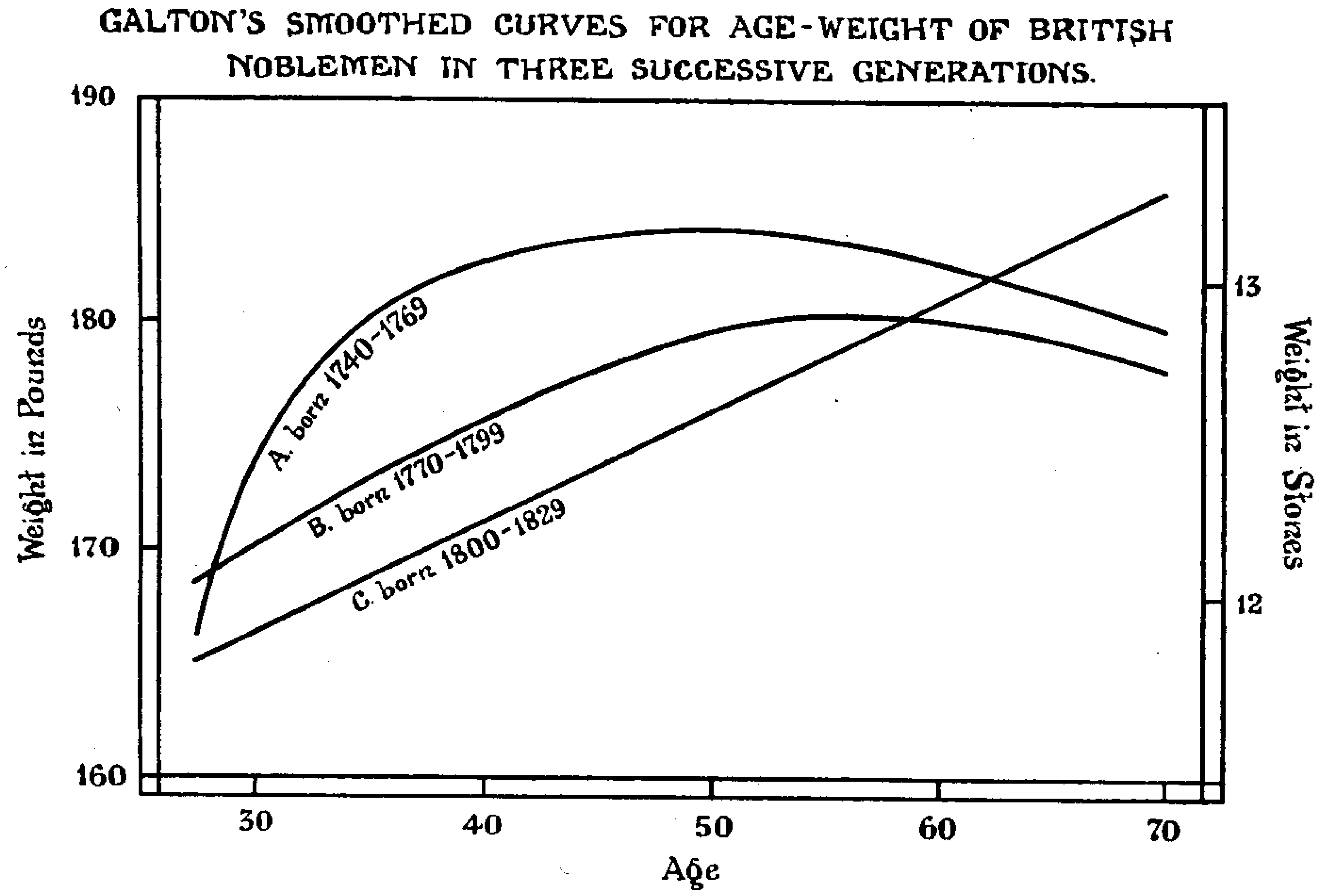


Fig. 14.

beam scales. Galton confined his attention almost entirely to noblemen as a well-rounded class, whose ages were easily ascertainable, and to their data in respect only of two characteristics, namely the degree of fluctuation in weight as exhibited by the age-weight curves of individual noblemen, and the difference in the average age-weight curves of noblemen born in the three periods 1740–1769, 1770–1799, 1800–1829. He found that the average annual fluctuation in the earlier group was about 7 lbs. and that in the latest group it was only 5 lbs. He concluded that this pointed to an

* Some notice of the following paper should have appeared in Section H of Chapter XIII (Vol. II), but its existence was then unknown to me.

irregularity in the mode of life that was greater two or three generations back than now. Further he found that the "prime" for weight was also earlier in age for the older generations, being hardly discoverable at all in those born in the first third of the nineteenth century or in the professional classes of the 'eighties. His three smoothed curves reproduced on p. 136, with the table of mean weights at each central age, indicate that noblemen of the generation which flourished about the beginning of last century attained their meridian and declined much earlier than those of the generation sixty years their juniors, or indeed than the mid-Victorian professional classes, where the culminating point was difficult to ascertain.

Galton's data were somewhat scanty as the following table will indicate, but his general conclusions appear to be justified:

Actual Mean Weights in pounds at Various Ages.

Class	Years of Age					
	27	30	40	50	60	70
Born 1740–1769	166 (13)	176 (18)	184 (24)	181 (21)	181 (18)	180 (12)
Born 1770–1799	168 (24)	171 (23)	172 (24)	184 (26)	178 (26)	178 (15)
Born 1800–1829	165 (35)	165 (44)	171 (43)	175 (37)	181 (22)	188 (7)
Mid-Victorian Professional Class	161	167	173	174	174	?

"There can be no doubt," he writes, "that the dissolute life led by the upper classes about the beginning of this century, which is so graphically described by Mr Trevelyan in his *Life of Fox*, has left its mark on their age-weight traces. It would be most interesting to collate these violent fluctuations with events in their medical histories; but, failing such information, we can only speculate on them, much as Elaine did on the dints in the shield of Launcelot, and on looking at some huge notch in the trace [for the individual], may hazard the guess, 'Ah, what a stroke of gout was there!'"

Although no great importance can be attached to Galton's results for this particular class of subject, yet the problems his paper suggests might be profitably studied on more ample material now extant. I am therefore glad to have brought to light once more this long forgotten paper.