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

IT would have been a pleasure to me in this address, given at the conclusion of my office as your President, to have cast a retrospect over the proceedings of our Institute during the four years that I have had the honour to hold it. But the subjects that have come before us are so varied that it seemed difficult to briefly summarize them in a manner that should not be too desultory.

On the whole, I thought it might be more useful if I kept to a branch of anthropometry with which many inquiries have made me familiar, and took the opportunity of urging certain views that seem to be worthy the attention of anthropologists.

Before entering upon these more solid topics, let me mention that the laboratory of which I spoke in my last address has been in work during the past year, and that about 1200 persons have been already measured at it in many ways, some more than once. I lay on the table a duplicate of one of the forms of application to be measured, and one of the filled-up schedules. It will be observed that I now have the impressions made in printers' ink of the two thumbs of each person who is measured, being desirous of investigating at leisure the possibilities of employing that method for the purpose of identification, not forgetting the success that attended Sir W. Herschel's use of it in India, but conscious at the same time of practical difficulties. There is no doubt that the imprints of the thumb or finger of different persons vary so much that a glance suffices to distinguish half a dozen varieties, while a minute investigation shows an extraordinary difference in small, though perfectly distinct, peculiarities. Neither is there any room for doubt that these pecularities are persistent throughout life; nor, again that so satisfactory a method of raising a very strong presumption of identity would be valuable in many cases. It will suffice to quote the following. A newspaper was lately sent me from the distant British settlement of North Borneo, where, owing to the wide and rapid spread of information nowadays, attention had been drawn to an account of a lecture I gave on one of the Friday evenings last spring, at the Royal Institution. It was on "Personal Description and Identification," and a writer in the *British North Borneo Herald* commented upon the remarks there made on finger imprints. He spoke of the great difficulty of identifying coolies either by their photographs or measurements, and added that the question how this could best be done would probably become important in the early future of that country. I also am assured that the difficulty of identifying pensioners and annuitants has led to frequent fraud from personation, involving in the aggregate a very large sum of money annually, as there is good reason to believe. If finger imprints could be practically brought into use, such frauds would be extremely difficult. I am still unable to speak positively as to the easiest and best way of making them, but the plan adopted at the laboratory is as follows. A copper plate is smoothly covered with a very thin layer of printers' ink by a printers' roller, the plate being cleaned every day. Either the plate, or the roller, but preferably the roller, is lightly touched by the thumb, which is afterwards pressed on paper. As the layer of ink is thin, none of it penetrates into the delicate furrows of the skin, but the ridges only are inked, and these leave clear impressions. In this way a permanent mark is registered. A little turpentine cleans the fingers effectually afterwards. But for purposes of identification a simpler process is necessary, one by which a person suspected of personation could furnish an imprint for comparison with the registered mark without having recourse to the troublesome paraphernalia of the printer. Such a process may perhaps be afforded by slightly smoking a piece of smooth metal or glass over the candle, pressing the finger on it, and then making the imprint on a bit of gummed paper that is slightly damped. The impression is particularly distinct, and is sufficiently durable for the purpose. As for the gummed paper, luggage labels can be used; even the fringe to sheets of postage stamps is broad enough to include as much of the impression as is especially wanted—namely, where the whorl of ridges takes its origin.

I hope at some future time to recur to this subject.

Correlation.—The various measurements made at the laboratory have already afforded data for determining the general form of the relation that connects the measures of the different bodily parts of the same person. We know in a general way that a long arm or a long foot implies on the whole a tall stature—*ex pede Herculem;* and conversely that a tall stature implies a long foot. But the question is whether their reciprocal relation, or correlation as it is commonly called, admits of being precisely expressed. Correlation is a very wide subject indeed. It exists wherever the variations of two objects are in part due to common causes; but on this occasion I must only speak of those correlations that are of anthropological interest. The particular problem I first had in view was to ascertain the practical limitations of the ingenious method of anthropometric identification due to M. A. Bertillon, and now in habitual use in the criminal administration of France. As the lengths of the various limbs in the same person are to some degree related together, it was of interest to ascertain the extent to which they also admit of being treated as independent. The first results of the inquiry, which is not yet completed, have been to myself a grateful surprise. Not only did it turn out that the expression and the measure of correlation between any two variables are exceedingly simple and definite, but it became evident almost from the first that I had unconsciously explored the very same ground before. No sooner had I begun to tabulate the data than I saw that they ran in just the same form as those that referred to family likeness in stature, which were submitted to you two years ago. A very little reflection made it clear that family likeness was nothing more than a particular case of the wide subject of correlation, and that the whole of the reasoning already bestowed upon the special case of family likeness was equally applicable to correlation in its most general aspect.[1]

It may be recollected that family likeness in any given degree of kinship—say that between father and son—was expressed by the fact that any peculiarity, that is to say, any difference from mediocrity in the father appears in the son, reduced on the average to just one-third of its amount. Conversely, however paradoxical it might at first sight appear, any pecularity in a son appears in the father, also reduced on the average to one-third of its amount. The "regression," as I called it, from the stature of the known father to the average son, or from the known son to the average father, was from 1 to $\frac{1}{3}$; from the known brother to the unknown brother it was $\frac{2}{3}$; from uncle to nephew, or from nephew to uncle, it was $\frac{2}{9}$; and in kinship so distant as to have no sensible influence, it was from 1 to 0. Whether the peculiarity was large or small, these ratios remained unaltered. The reason of all this was thoroughly explained, and need not be repeated here. Now the relation of head-length to head-breadth, whose variations are on much the same scale, or speaking in technical language, whose probable errors are the same, is identical in character to the relation between kinsmen. There is regression in both cases, though its value differs. The lengths of head-lengths and head-breadths are akin to each other in the same sense as kinsmen are. So it is in the closer relation between the lengths of symmetrical limbs, left arm to right arm, left leg to right leg. The regression would be strictly reciprocal in these cases. When, however, we compare limbs whose variations take place on different scales, the differences of scale have

[1] "Proc. Roy. Soc.," 1886, p. 42, and "Journ. Anthrop. Inst.," 1885, p. 252.

to be allowed for before the regression can assume a reciprocal form. The plan of making the requisite allowance is perfectly simple; it merely consists in dividing each result by the probable error of any one of the observations from which it was deduced. Unfortunately the method cannot be briefly explained except by using these technical terms. In some cases the scale of variation in the two correlated members is very different, and this divisor may be very large. Thus the length of the middle finger varies at so very different a rate from that of the stature that 1 inch of difference of middle finger length is associated on the average with 8·4 inches of stature. On the other hand, 10 inches of stature is associated on the average with 0·6 inch of middle finger length. There is no reciprocity in these numerals; yet, for all that, when the scale of their respective variations is taken into account by using the above-mentioned divisor, the values become strictly reciprocal. I shall be better able to enter more fully into this subject later on, towards the close of this address.

Variety.—The principal topic of my further remarks will be the claims of Variety to more consideration from anthropologists than it usually receives. Anthropologists commonly narrow their inquiries to the purpose of ascertaining the mean values of different groups, while the variety of the individuals who constitute them is too often passed over with contented neglect. It seems to me a great loss of opportunity when, after observations have been laboriously collected and subsequently discussed in order to obtain mean values, the very little extra trouble has not been taken that would determine such other values as would go far to express the variety of the individuals in those groups. Much experience some years back, and much new experience during the past year, has proved to me the ease with which variety may be adequately expressed, and the high importance of taking it into account. Numerous problems that ought to be of especial interest to anthropologists, deal solely with variety.

There can be little doubt that most persons fail to have an adequate conception of the orderliness of variability, and think it useless to pay scientific attention to variety, as being, in their view, a subject wholly beyond the powers of definition. They forget that what is confessedly undefined in the individual may be definite in the group, and that uncertainty as regards the one is in no way incompatible with statistical assurance as regards the other. Almost everybody is familiar nowadays with the constancy of the Average in different samples of the same large group, but they do not often realise the way in which a similar statistical constancy permeates the whole of the relations between the various members of the group. The Mean or the Average is practically nothing more than the middlemost value in a marshaled series. A constancy analogous to that of the Mean characterises each value that occupies the other fractional positions, such as the 10th per cent., or the 20th per cent. of the total length of the marshaled series. The condition of constancy is not a peculiar attribute of the 50th per cent., or middlemost.

Greater interest is usually attached to individuals who occupy positions towards either of the ends of a marshaled series, than to those who stand about its middle. For example, an average man is morally and intellectually an uninteresting being. The class to which he belongs is bulky, and no doubt serves to keep the course of social life in action. It also affords, by its inertia, a regulator that, like the fly-wheel to the steam-engine, resists sudden and irregular changes. But the average man is of no direct help towards evolution, which appears to our dim vision to be the primary purpose, so to speak, of all living existence. Evolution is an unresting progression; the nature of the average individual is essentially unprogressive. His children tend to resemble him exactly, whereas the children of exceptional persons tend to regress to mediocrity. Consider the interest attached to variation in the moral and intellectual nature of man and the value of variability in those respects. For example, the average worth of the Hebrew race shows little that is worthy of note, but that race has been of peculiar interest on account of the great varieties of character that it has produced. Its variability in ancient and modern times seems to have been extraordinarily great. It has been able to supply men, time after time, who have towered high above their fellows, and have left enduring marks on the history of the world.

Some thorough-going democrats may look with complacency on a mob of mediocrities, but to most other persons they are the reverse of attractive. The absence of elevated and heroic natures in any group of men is a heavy set-off against the freedom from a corresponding number of very degraded forms. The general standard of thought and morals in a mob of mediocrities must be mediocre, and, what is worse, contentedly so. The lack of living men to afford lofty examples, and to educate the virtue of reverence, must leave an irremediable blank. All men would in that case find themselves at nearly the same dead average level, each as meanly endowed as his neighbour.

These remarks apply with obvious modifications to variety in the physical faculties. Peculiar gifts, moreover, afford an especial justification for division of labour, each man doing that which he can do best.

The method I have myself usually adopted for expressing and dealing with the variety of the individuals in a group, so as to treat a whole population in a compendious way, has been already explained on more than one occasion. I should not have again alluded to it had I not had much occasion of late to test and develop it, also to devise an unpretentious little table of figures that I call a "table of normal distribution," which has been of singular assistance to myself. I trust it may be equally useful to other anthropologists. It is appended to these remarks, and I should like after a short necessary preface to say something about it. The table and its origin, and several uses to which it has been applied, will be found in a book by myself, that is on the point of publication, called "Natural Inheritance" (Macmillan and Co.). All the data to which I shall refer will be found in that book also, except such as concern correlation.

These accompanied a memoir read by me only a month ago before the Royal Society.[1]

The first step in the problem of expressing variety among the individual members of any sample, is to marshal their measures in order, into a class. We begin with the smallest measure and end with the greatest. The object of the next step is to free ourselves from the embarrassment due to the different numbers of individuals in different classes. This is effected by dividing the class, whatever its size may be, into 100 equal portions, calling the lines that divide the portions by the name of grades. The first of these portions will therefore lie between grades 0° and 1°, and the hundredth and last portion between grades 99° and 100°. We have next to find by interpolation the values that correspond to as many of these grades as we care to deal with. It is of no consequence whether or no the number in the class is evenly divisible by 100, because we can interpolate and get the values we want, all the same. This having been done, the value that corresponds to the 50th grade will be the middlemost. It is the equivalent for all ordinary purposes to the mean or average value; but as it may not be strictly the same, it is right to call it by a distinctive name,

[1] "Proc. Roy. Soc.," Dec. 20, 1888, vol. 45. "Correlations and their Measurement, chiefly from Anthropometric Data." The general result of the inquiry was that, when two variables that are severally conformable to the law of frequency of error, are correlated together, the conditions and measure of their closeness of correlation admits of being easily expressed. Let x_1, x_2, x_3, &c., be the deviations in inches, or other absolute measure of the several "relatives" of a large number of "subjects," each of whom has a deviation, y, and let X be the mean of the values of x_1, x_2, x_3, &c. Then (1) $y = r\text{X}$, whatever may be the value of y. (2) If the deviations are measured, not in inches or other absolute standard, but in units, each equal to the Q (that is, to the probable error) of their respective systems, then r will be the same, whichever of the two correlated variables is taken for the subject. In other words, the relation between them becomes reciprocal; it is strictly a correlation. (3) r is always less than 1. (4) r (which, in the memoir on hereditary stature, was called the ratio of regression) is a measure of the closeness of correlation. (5) The probable error, or Q, of the distribution of x_1, x_2, y_3, &c., about X, is the same for all values of y, and is equal to $\sqrt{(1-r^2)}$ when the conditions specified in (2) are observed.

It should be noted that the use of the Q unit enables the variations of the and none simpler or more convenient occurs than the letter M. So I will henceforth use M to denote the middlemost or median value, or, in other words, that which corresponds to the 50th (centesimal) grade.

The difference between the extreme ends of a marshaled series is no proper measure of the variety of the men who compose it. However few may be the objects in the series, it is always possible that a giant or a dwarf, so to speak, may be included among them. The presence of either would mislead as to the range of variety likely to be found in another equally numerous sample taken from the same group. The values in a marshaled series run with regularity only about its broad and middle part; they never do so in the parts near to either of its extremities. In a series that consists of a few hundreds of individuals, the regularity is usually found to begin at about grade 5°, and to continue up to about grade 95°. Therefore it is out of the middle part, between 5° and 95°, or better out of a still more central portion of it, that points should be selected between which the rate of its variety may be measured. Such points are conveniently found at the 25th and the 75th grades. Just as the grade 50° divides the class into two equal parts, so the grades 25° and 75° subdivide it into quarters, and the difference between those values affords an irreproachable basis for the unit of variety. The actual unit is taken as the half of the value of that difference, because the value at 25° tends to be just as much below that at 50°, as the value at 75° is above it. Therefore the average of these two values is a better measure than their sum. Briefly, if we distinguish the measure at 25° by the letter Q_1, and that at 75° by Q_3, then the unit of variety is $\frac{1}{2}(Q_3 - Q_1)$, and this unit we will henceforth call Q. It is practically, but not strictly, identical with the "probable error" of a single observation, and is a useful symbol, as con-

most diverse qualities to be compared with as much precision as those of the same quality. Thus, variations in lung-capacity which are measured in volume can be compared with those of strength measured by weight lifted, or of swiftness measured in time and distance. It places all variables on a common footing.

sisting of a single syllable and a single letter instead of the 5 syllables and the 13 letters that form the very misleading phrase of "probable error." As M measures the average, so Q measures the variety, and they are independent of one another. In strength, for example, the relation of Q to M in the particular group of adult males on which I worked was as 1 to 10; in the statures of the same group it was as 1 to 40; in breathing capacity as 1 to 9; in weight as 1 to 14.

The mean or average is an arithmetical muddle of all the values in the series; it presents to the imagination by no means so clean an idea as the middlemost value M. Therefore, although the peculiarities of an individual are commonly considered in the light of deviations from the mean or average value, I prefer to reckon them as deviations from M. Practically the two methods are identical, but I find the latter more convenient to work with, and believe it to be the better of the two in every other way.

The causes and the laws of deviation, or of variation, are identical with those of error, and the well-known law of frequency of error gives data whence the *relative* values of the deviations at the several grades may be calculated for any normal series. If we know the actual deviation at any one specified grade, then the *absolute* values of those at every other grade can be calculated; consequently the variety of the whole series is expressed by only two data, a grade and the corresponding deviation.

The small table of distribution, of which I spoke, gives the values at each grade when Q is equal to 1. In this case the value at 25° is − 1, and that at 75° is + 1. If we desire to determine the Q of any such series, the only required datum, as has been just laid, is the value of the deviation at some one known grade; then, by dividing that deviation by the tabular value, we obtain Q at once. Or, conversely, if we know the Q of the series, and wish to calculate the deviation at any given grade we multiply the tabular deviation by Q. Thus, in the stature of men, which varies in an approximately normal manner, the value of Q is about 1·7 inch, therefore to find the deviation in stature at any grade among adult males, we multiply the tabular value by 1·7 inch.

If we know the *measures* at any two specified grades of a normal series, we are easily able to calculate both Q and M, and can thence derive the measures at any other desired grades. I have long since pointed out the possibility of a traveller availing himself of this method of anthropological investigation; but, for the want of the annexed table of distribution, he would probably be puzzled in making the necessary calculation. With the aid of this table the calculation is most readily performed. Let us suppose that the traveller is among savages who use the bow, and that he desires to learn as much as he can about their strengths. He selects two bows; the one somewhat easy to draw, and the other somewhat difficult, and at his leisure, either before or after the experiment he ascertains exactly how many pounds weight is required to draw them severally to the full Then by exciting emulation and by the offer of small prizes, he induces a great many of the natives to try their strengths upon them. He notes how many make the attempt, and how many of them fail in either test. This is all the observation requisite, though common sense would suggest the use of three and not two bows, in order that the data from the third bow might correct or confirm the results derived from the other two. Let us work out a case, not an imaginary one, but derived from tables I have already published, and of which I will speak directly. Let the problem be as follows:—

30 per cent. of the men fail to exert a pulling strength of 68 pounds; 60 per cent. fail to pull 77 pounds. What is the Q and the M of the group?

Consider this 30 per cent. to be the exact equivalent of grade 30°, and the 60 per cent. of grade 60°. The reason why the percentage of failure, and the number of the grade are always to be taken as identical will be found in a footnote to the table, and I need not stop to speak of it. Now, the tabular value at grade 30° is $-0·78$; that at 60° is $+0·38$; the difference between them being 1·16. On the other hand, the difference between the two test values of 68 pounds and 77 pounds is 9 pounds. Therefore Q is equal to 9 pounds divided by 1·16; that is, to 7·8 pounds. M may be obtained by either of two ways, which will always give the same answer. We may subtract 0·38 × 7·8 pounds from 77 pounds, or we may add 0·78 × 7·8 pounds to 68 pounds. Each gives 74 pounds. Observation gave precisely these values both for Q and for M. The data were published in the Journal of this Institute in 1884 as a table of "percentiles," and were derived from measures made at the International Health Exhibition. The value of M is given directly in the table, but that of Q happens not to be given there; it may easily be found by interpolation between those that are.

That table of percentiles affords excellent material for experimental calculations on the principle of this test, and for estimating its trustworthiness in practice. It contains a variety of measures referring to eighteen different series, all corresponding to the same grades—namely, to 5°, 10°, 20°. and onwards for every tenth grade up to 90° and ending with 95°. The measures refer to stature, height sitting above seat of chair, span, weight, breathing capacity, strength of pull, strength of squeeze, swiftness of blow, keenness of eyesight, and in each case the values are given for adult males and adult females separately. I have since found ("Natural Inheritance," pages 56, 201), that when the deviations are all reduced in terms of their respective Q values, by dividing each of them by its Q, that the average value of all the deviations at each of the grades in the eighteen series closely corresponds to the normal series, though individually they differ more or less from it, some in one way, some in another. On the whole, the error of treating an unknown series as if it were a normal one can rarely be very large, always supposing that we do not meddle with grades lower than 5° or higher than 95°.

It will be of interest to put the comparison on record. It is as follows:—

Grades	5°	10°	20°	30°	40°	50°
Observed	− 2·44	− 1·87	− 1·24	− 0·77	− 0·40	0
Normal } − below 50° + above 50°	2·44	1·90	1·25	0·78	0·38	0
Observed	+ 2·47	+ 1·92	+ 1·21	+ 0·75	+ 0·38	0
Grades	95°	90°	80°	70°	60°	50°

The "observed" are the mean values, made as above described, of the eighteen series; the "normal" are taken from the table of distribution given further on.

An ingenious traveller might obtain a great number of approximate but interesting data by the method just described, measuring various faculties of the natives, such as their delicacy of eyesight and hearing, their swiftness in running, their accuracy of aim with spear, arrow, boomerang, sling, gun, and so forth, laterally from the object aimed at, or else vertically; distance of throw, the stature, and much else. But he should certainly use three test objects, and not two only.

It should be remarked that, if the distribution of deviation proved to be constant throughout any large class of faculties, though the Q might differ in different sub-classes of it, then, even though the distribution of that faculty was very far indeed from being normal, an appropriate table of distribution could still be compiled in order to solve such problems as those mentioned above. I have as yet no accurate data to put this idea to a practical test.

There are three convenient stages of approximation in expressing the variety of the various measures in a series, each of which reaches considerably nearer to precision than the one before. The first is to give only Q and M; the second is to record the measures at the grades 10°, 25°, 50°, 75°, and 90°; the third is the more minute method, adopted in the tables of percentiles—viz., to give the measures at 5°, 10°, 20°, &c., 80°, 90°, and 95°. It may in some cases be found worth while to go further, say to 1° and 99°, or even also to 0°·1, and 99°·9. So much for the method of expressing variety.

The use of Q is by no means limited to the objects just named. It is a necessary datum wherever the law of frequency of error has to be applied, whose properties are applicable to a very large number of anthropological problems with more accuracy of result than might have been anticipated, as the series are only approximately normal. This has been practically shown by the agreement among themselves of several inquiries to which I will shortly allude. It is theoretically defensible by two considerations. The one is that the law of frequency supposes the amount of error or of deviation to be the same in symmetrically disposed grades on either side of 50°, their signs being alone different, *minus* on the one side of 50° and *plus* on the other. Now, in an observed series there may be, and often is, a want of symmetry, but if the deviate, say at 70°, is as much greater than the normal value as the deviate at 30° is less than the normal, then the effects of these two upon the final result will be much the same as if there had been exact symmetry at those points. The other consideration is that any nonconformity between the observed deviates and the theoretical ones mostly affects the extremities of the series, and consequently has but a small and perhaps insensible effect on the broad general conclusions. We need care little for any vagaries outside of the grades 5° and 95°, if the intervening portion gives fairly good results. As the latter forms nine-tenths of the whole series, the irregularities in the remaining tenth are of small relative importance.

One great use of Q or of any of its equivalents, as the mean error, &c., is to enable us to estimate the trustworthiness of our average results. We require to know both Q and the number of the observations, before it is possible to estimate the degree of dependence to be placed on M. If only one observation was accessible, then the degree of its trustworthiness (its "probable error") would of course be equal to Q; in other words, its error would be just as likely as not to exceed Q. If there were two, two hundred, two thousand, or any other number of observations, the probable errors of their respective values M would be reduced, but not in simple proportion. They would be equal to Q divided by the square roots of those numbers.

When we desire to ascertain the trustworthiness of the difference between the M values of two series, as between the mean statures of the professional and artisan class as derived from certain observations, the properties of the law of frequency of error must again be appealed to. Anthropologists are much engaged in studying such differences as these; but from their disregard of the simple datum Q or of some one of its equivalents, and from not being familiar with the way of employing them, there is usually a lamentable and quite unnecessary vagueness in the value that can be assigned to their results. This is especially the case in comparisons between the average dimensions of the skulls of various races, when the average values are alone given, and when they have been derived, as is often the case, from the measurement of only a few specimens. An almost solitary exception to this needless laxity in statistical treatment will be found in a brief but admirably-expressed memoir by Dr. Venn, the well-known author of the "Logic of Chance." It is upon Cambridge anthropometry, and was published in the last number of the Journal of this Institute. It deserves to be a model to those who are engaged in similar inquiries.

Another class of investigations in which a knowledge of Q is essential, was spoken of some time back—namely, into the questions of Correlation in the widest sense of the word. These problems have nothing to do with the relations of the M values, but are solely concerned with variations, that is with the deviations from M at the various grades. It is true that a knowledge of M is requisite. We have to subtract it from the measures in order to get at the deviations. But after this is done, M is put aside. It has no part in the work of the problem; it is only after the results have been arrived at without its use that it is again brought forward and added to them. Numerous properties of the law of frequency of error in which Q is the datum, were utilized in my inquiries into family likeness in stature, and in all cases they brought out consistent results. An excellent example of their consistency was seen in the results of the methods employed to determine the variety of individual statures in families of brothers. Four different properties of the law had to be applied to partly different samples of the same group in order to determine the value of the Q of stature in fraternities, and they respectively gave 1·07, 0·98, 1·10, and 1·10 inch, which, statistically speaking, are much alike. Certain properties of the law of frequency of error were also applied to family likeness in eye colour, with results that gave by calculation the total number of light-eyed children in families differently grouped according to their parentage and grand-parentage, and according to three different sets of data. The resulting figures were 623, 601, and 614 respectively, the observed number being 629 (Proc. Roy. Soc., 1886, p. 415). Other properties of the same law have been applied by myself in the book already mentioned to determine the ratio of artistic to non-artistic children in families whose parentages were known to be either both artistic, one artistic and one not, or neither artistic. They gave the ratios of 64, 39, and 21 respectively, as against the observed values in 1507 children, of 60, 39, and 17.

Lastly, as regards the correlation of lengths of the different limbs. It has already been shown that the correlation connects the deviations, and has nothing to do with the mean or average values. Now, to express this relation truly, so that it shall be reciprocal, the scale of deviation of the correlated limbs, say, for example, of the cubit and of the stature of adult males, must be reduced to a common standard. We therefore reduce them severally to scales in each of which their own Q is the unit. The Q of the cubit is 0·56 inch, therefore we divide each of its deviations by 0·56. The Q of the stature is 1·75 inch, so we divide each of its deviations by 1·75. When this is done the correlation is perfect. The value of regression is found to be 0·8, whether the cubit be taken as the "subject" and the mean of the corresponding statures as the "relative," or *vice versâ*.

The value of the regression was ascertained for each of many pairs of the following elements, and a comparison made in each case between the correlated values as observed and those calculated from the ratio of regression. The coincidence was close throughout, quite as much so as the small number of cases under examination, 350 in all, could lead us to hope. The elements were nine in number,[1] viz., head length, breadth of head, length of right leg below the knee, of left cubit, of left middle finger, of the height sitting above the chair, of stature, of the differences between the two foregoing (which indicate the total

[1] The head length is here the maximum length measured from the notch below the brow. The cubit is measured with the hand prone, from the flexed elbow to the tip of the middle finger. The height of knee is taken from a stool, on which the foot rests with the knee flexed at right angles; from this the measured thickness of the heel of the boot is subtracted. All measures had to be made in the ordinary clothing. The M and Q values of these elements among adult males were found to be as follows: left cubit, 18·05 and 0·56; stature, 67·2 and 1·75; head length, 7·62 and 0·19; head breadth, 6·00 and 0·18; left middle finger, 4·54 and 0·15; height of right knee, 20·50 and 0·80; all the measures being in inches. The values of *r* in the following pairs of variables were found to be: head length and stature, 0·35; left middle finger and stature, 0·70; head breadth and head length, 0·45; height of knee and stature, 0·9; left cubit and height of right knee, 0·8. The comparison of the observed results with those calculated from the above data showed a very close agreement (Proc. Roy. Soc., Dec. 20, 1888). The measures were of 350 male adults, containing a large proportion of students barely above twenty-one years of age and several artizans, made at the laboratory at South Kensington, belonging to the author. The smallness of the number of measures, viz., 350, is of little importance, as the results run with fair smoothness. Neither does the fact of most of the persons measured being hardly full grown, and of others being of the generally short class of artizans, affect the main results. It somewhat diminishes the values of M, and very slightly increases that of Q, but it cannot be expected to have any considerable influence on the value of *r*.

length of the lower limbs), and of the span. Anthropologists seem to have little idea of the wide fields of inquiry open to them as soon as they are prepared to deal with the variety of individuals, and cease to narrow their view to the consideration of the average value of all of them.

Enough has now been said to justify the claims with which I started, and which take this final form. First, wherever it is likely to be of use, that, in those series of which the M is calculated, the measures at a certain number of selected grades should also be calculated and given, sufficient to enable the rest of the marshaled series to be found with adequate accuracy by interpolation. Secondly, that the value of Q or of one of its equivalents should always be given as well, and for two reasons. The one is, that M and Q suffice between them to give an approximate determination of the whole series, which is the more closely approximate as the series is more closely of the normal type; and, secondly, because Q or one of its equivalents is an essential datum before any application can be made of the law of frequency of error. The properties of this law are, as we have seen, largely available in anthropological inquiry. They enable us to define the trustworthiness of our results, and to deal with such interesting problems as those of correlation and family resemblance, which cannot be solved without their help.

Table of ordinates to the normal Curve of Distribution, in which the unit = the probable error, and the grades, which are the abscissæ, run from 0° *to* 100°.

Grades.	0	1	2	3	4	5	6	7	8	9
0	∞	−3·45	−3·05	−2·79	−2·60	−2·44	−2·31	−2·19	−2·08	−1·99
10	−1·90	−1·82	−1·74	−1·67	−1·60	−1·54	−1·47	−1·42	−1·36	−1·30
20	−1·25	−1·20	−1·15	−1·10	−1·05	−1·00	−0·95	−0·91	−0·86	−0·82
30	−0·78	−0·74	−0·69	−0·65	−0·61	−0·57	−0·53	−0·49	−0·45	−0·41
40	−0·38	−0·34	−0·30	−0·26	−0·22	−0·19	−0·15	−0·11	−0·07	−0·04
50	+0·00	+0·04	+0·07	+0·11	+0·15	+0·19	+0·22	+0·26	+0·30	+0·34
60	+0·38	+0·41	+0·45	+0·49	+0·53	+0·57	+0·61	+0·65	+0·69	+0·74
70	+0·78	+0·82	+0 86	+0·91	+0·95	+1·00	+1·05	+1·10	+1·15	+1·20
80	+1·25	+1·30	+1·36	+1·42	+1·47	+1·54	+1·60	+1·67	+1·74	+1·82
90	+1·99	+1·99	+2·08	+2·19	+2·31	+2·44	+2·60	+2·79	+3·05	+3·45

This table is an inverse rendering of the values derived by interpolation from the ordinary table of the probability integral, but its unit is changed from that of the modulus to that of the probable error, or what is almost exactly the same thing, to Q; and the (centesimal) grades are reckoned from 0° to 100°. In the usual way of reckoning, the 50th grade should have been reckoned as 0°, and the deviations should have run on the one side down to − 50°, and on the other up to + 50°.

Referring to what was said some way back, that if 30 per cent. of the natives whose strength was being tested fail to pull 60 pounds, then 60 pounds must be taken as the measure corresponding to the grade of 30°; the reason for this is as follows: The 30th grade separates the man who ranks 30th in a class of 100 men from his neighbour who ranks 31st. It does so for the same reason that grade 1° separates the man who ranks 1st from the man who ranks 2nd. Now, the 30th man failed in the test, and the 31st succeeded. Therefore the grade corresponding to bare success lies between them, and is the same as grade 30°.

It was moved by Professor FLOWER, seconded by Mr. HYDE CLARKE, and unanimonsly resolved :—

"That the thanks of the meeting be given to the President for his address, and that it be printed in the Journal of the Institute."

The Scrutineers gave in their Report and the following gentlemen were declared to be duly elected to serve as Officers and Council for the year 1889 :—

President.—John Beddoe, Esq., M.D., F.R.S.

Vice-Presidents.—Hyde Clarke, Esq.; J. G. Garson, Esq., M.D.; Prof. A. H. Keane, B.A.

Secretary.—F. W. Rudler, Esq., F.G.S.

Treasurer.—A. L. Lewis, Esq., F.C.A.

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A vote of thanks to the retiring Vice-President, the retiring Councillors, the Auditors, the Scrutineers, the Secretary and the Treasurer was moved by Sir BEAUCHAMP WALKER, seconded by Mr. T. V. HOLMES, and carried unanimously.