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Croonian Lectures

ON

THE NUMERICAL METHOD, AND ITS APPLICATION TO THE SCIENCE AND ART OF MEDICINE.

DELIVERED AT THE ROYAL COLLEGE OF PHYSICIANS, 1860.

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PART IV.

Though large numbers of Facts are necessary for Scientific Discovery, and for Practical Application to Individual Cases, it is allowable to use Smaller Numbers of Facts for certain Purposes. Our Facts should be numerous, because they differ widely from each other. The Population of England and Wales taken as an Example; and the Houses and Ships which we insure against Fire and Wreck. The Analogy of Games of Chance. Parallel Groups of Averages compared. The Ages at Death of the English Aristocracy, and of the Upper and Middle Classes in England, and the Attendances at King's College Hospital, used to prove the Divergence of the Groups when the Facts are few, and their Convergence when the Facts are numerous. The Mode of Approximation shown in the Case of the Temperatures of different Years. The Births of Male and Female Children in England and Wales, in Populations of different Sizes, used to illustrate the same Truth. What Number of Facts sufficient in any given Inquiry? Mathematical Formulæ in Use for this Purpose.

IN all the observations which I have hitherto made on the numerical method—on the logic, that is to say, of average and extreme values—I have assumed that our averages and extremes are obtained from a considerable body of facts. When our object is to ascertain any great scientific truth, or to make safe application of average results to a large number of similar individual instances, we must work with large bodies of facts. But there are many cases in which the use of smaller numbers of facts is allowable and expedient; and it is important to ascertain what these exceptional cases are. To this subject, then, I now address myself. I shall first endeavour to assign some sufficient reasons why our facts should be numerous: I shall then prove that, when they are numerous, the average results of two or more large groups of the same facts so nearly coincide as to confirm each other; but that, on the contrary, when they are not numerous, the average results diverge widely, approximating gradually as we increase the number of our facts. I shall then proceed to prove that, in dealing with smaller numbers of facts, there is always a presumption in favour of our averages approaching so close to the mean results of a larger body of the same facts, as to be useful for many scientific and practical purposes. And, lastly, I shall endeavour to show what is the real value to be attached to the great fact now so well known and so generally acknowledged, of the recurrence year by year of similar numerical values, as the representatives of the collective acts of a number of individuals exercising their own free wills, and making their own more or less deliberate choice; and yet in the mass supplying the statistic with materials for average results, almost as uniform as those which characterise the operations of unintelligent forces on equally unintelligent matter.

And first I will speak of the reasons why our facts from which we deduce average and extreme results should be numerous. The first and most obvious reason is, that the facts themselves,

whether they be of the order of *simple units* or of units of *variable value*, differ widely one from another. The people of England and Wales, for instance, among whom, one with another, there prevails a rate of mortality of 1 in 45, are made up of persons of every age from infancy to decrepitude; of the two sexes, male and female; of several distinct ranks or classes, from the royal family down to the pauper; and of occupations, and branches of occupation, counting by hundreds and thousands. Among persons thus widely differing at any given moment of time, the seasons and weather, the amount of employment, the abundance or scarcity of food, the existence or non-existence of epidemic maladies, are causing each year, each month, each week, each day, a special and peculiar mortality. A fall in the temperature of a few degrees in winter will immediately affect the number of deaths among young children and aged persons. A similar rise of temperature in summer will add considerably to the deaths from English cholera. Hence, to obtain this proportion of 1 in 45, it is necessary not merely to collect several facts, but to continue collecting them for a considerable period of time. The deaths in no one place, and at no one time of the year, will suffice to furnish forth the average of 1 in 45. It requires a whole kingdom for a whole year.

Nor is the necessity for numerous facts, and the reason for collecting them in large numbers, different in the case of inanimate objects. Take, for instance, the property we insure against fire or shipwreck. Houses and ships differ from each other almost as much as men and women. A building is exposed to risk from fire, in consequence of the materials of which it is built, the way in which those materials are put together, the business which is carried on upon the premises, the careful or careless habits, and, in some cases, the honesty or dishonesty, of the inmates. Hence, to arrive at the average risk of fire, you must bring together cases enough of fires and exemptions from them to exhaust all the combinations and permutations of which these elements of safety or danger are susceptible; or, if not all, in any case a very considerable number.

Or take the ship which we insure against wreck. What a multitude of causes combine to affect the risk! There is the construction of the ship itself; its age; the skill, experience, and care of the captain and crew; the quality of the compass and chronometer; the accuracy of charts; the nature of the cargo; the length of the voyage; and the destination of the vessel. Each possible combination of these and other like elements of safety or danger must enter into our tables, as observed results, in order that a true and safe average may be obtained. And here, as in the case of the rate of mortality, it will not do to take the experience of a limited period. Our facts must be gathered from the records of several years, because successive years differ widely from each other in the prevalence of stormy weather.

Again, the same or similar reasons obviously exist for multiplying our observations, when, instead of simple units, such as a death, a fire, a wreck, we have to deal with units of variable magnitude, such as the ages at death, the pulse, or the respiration. I will take, as an example, one of the simplest possible cases—the pulse at some specified age. I will suppose that every known cause of difference between one person and another is eliminated; that we count the pulse at the same time of the day, in the same posture, at rest, and fasting; and that we eliminate the element of sex, by restricting our observations to males, and to healthy males—to such, at least, as have all the outward appearances of health. Well; even in this simple and well defined case, it is not a few facts that will serve our purpose, and yield us a true average. I will take the age from 49 to 56 inclusive—a period of seven years; and I find that, in twenty-five observations, I encountered pulses ranging from 46 to 92 beats in the minute. If I had a pulse for every possible number between 46 and 92, I must have had at least forty-six instead of twenty-five observations. Hence it is apparent that, though twenty-five observations may be useful for rough purposes of comparison, they are very far from sufficient to ensure accuracy.

Another reason that might be assigned for multiplying our observations, is the proved necessity for so doing in the analogous case of experiments on what is vulgarly called *chance*. Every one sees that, with a well formed unloaded die, there is an equal chance of throwing an ace, a deuce, a tres, a quatre, a cinq, a six. There is no reason in the nature of things why one should turn up rather than another; and the chances obviously are, that any one of the six, say an ace, will be thrown every sixth time. And yet, in order to verify this suggestion of common sense experimentally, it would certainly be necessary to throw the die a great number of times. Again, a little consideration and a very simple calculation will show that, in the case of two dice, there is a chance of throwing doublets, say aces, once in every thirty-six throws; and yet I have a very distinct recollection of an experiment in which the dice were thrown for hours together without even approaching the true mathematical proportion of 1 in 36.

When I say that these experiments with the dice furnish a case analogous to the mortality in England and Wales, to losses by fire and shipwreck, and to the average number of the pulse, I mean that each fall of the dice is determined by a combination of forces, trifling in degree, but very variable, just as a death, a fire, a shipwreck, a pulse of a given number, is determined by the combined action of a number of forces, of a very different kind it is true, but equally beyond our power to measure or appreciate. Between events attributed to chance and events brought about by other causes, the only difference is that we can ascertain by calculation how often an event governed by chance should take place, while we do not know how often events attributable to causes with which we are more familiar ought to happen. In neither case, however, can we say with any approach to certainty how many experiments will suffice to realise a true result—how many throws of the dice to obtain the true mathematical proportion; how many experiments or observations to achieve the true average.

I shall have more to say on this subject bye and bye; at present, I pass on to the proof that when we deal with large numbers of facts, the average results of two or more considerable groups nearly correspond with each other, while on the other hand, there is great divergence in the case of averages obtained from small numbers of facts.

I will prove these two positions by means of a very simple order of facts—the average ages at death of the English aristocracy, and of the united upper and middle classes of English society. 1600 ages at death, of the members of the English peerage and baronetage, taken from the pages of the *Annual Register*, yield an average duration of life for adult male members of those two classes of 60½ years. Now, if we distribute these into two groups of 800 each, these two considerable groups differ from each other in average result by little more than one year. But when we split up these 1600 facts into smaller groups of 400, 200, 100, 50, and 25 facts respectively, the small difference of little more than one year becomes (if we take the difference between the highest and lowest averages) 3, 5, 7, 11, and 19 years respectively.

Similar results are obtained when the larger mixed class of the English upper and middle classes, consisting chiefly of the peerage and baronetage, and members of the learned and other professions, is substituted for the English aristocracy. The average ages at death of the whole 6,400 of whom the group consists is, in round numbers, 66 years. When distributed into two groups of 3,200 each, the difference between the averages of those large groups but little exceeds half a year. But on distributing the whole number into groups of 1,600, 800, 400, 200, 100, and 50 respectively, the difference between the extreme averages yielded by these smaller groups becomes 3, 3½, 6½, 12, 18, and 27½ respectively.

I will give one more illustration from a quite different order of facts. Instead of units of variable magnitude—the ages at death of different classes of persons—I will take the simple units of attendances at an hospital. In the place of an event brought to pass by a large combination of causes acting during long terms of years, I take an occurrence determined by a comparatively small number of causes originating in the human will acting for a short space of time. I extracted from the out-patient books of King's College Hospital, in the order in which they were entered, 6,400 attendances of men and women; and from this large collection of facts I obtained an average attendance of men, as compared with women, of 40 in the hundred. This being the true proportion, the actual proportions per cent. of male attendances varied, when the facts were grouped by fifties, from 64 to 24, being a range of 40; when by hundreds, the range was reduced from 40 to 24; when in groups

of 200, the range became 11; when in groups of 400, 8; in groups of 800, 3; in groups of 1,600, about 1½; and in groups of 3,200, about the fifth of an unit.

These three orders of facts, then, fully confirm each other, and prove that while small numbers of facts yield averages distant from the true average, large numbers of facts yield averages more to be relied upon; and that the approximation to the true average increases rapidly as the number of our facts increases.

If I were to express these averages in curves, you would observe that, while the successive averages derived from 25 facts show great irregularities, those irregularities become much less as the number of facts increases, until at length the line corresponding to successive averages drawn from very considerable groups of facts becomes very nearly straight.

The comparisons which I have just made are perhaps open to an objection which I must endeavour to remove. The number of groups of facts is not the same; it diminishes as the number of facts increases. For instance, the wide range of 19 years in the first order of facts which I brought under your notice is the difference between the highest and lowest averages of no less than 64 groups of 25 facts, while the limited range of one year is obtained from only two groups of 800 facts each. I will endeavour to meet this objection by taking a very different order of facts, and comparing in every instance two groups with each other. In order, also, to show more clearly than I have yet done the process by which the wide differences between averages deduced from small bodies of facts is gradually narrowed, I will adopt a plan of elimination by which equal and similar facts shall be set off against each other, leaving on each side those facts only which are without a parallel in the opposed column.

The facts to which I allude are the average temperatures of the several *weeks* of successive years, and the average temperatures of the several *days* of the years 1857-8. I compared in both cases corresponding periods (weeks or days) with each other. Having arranged the weeks or days of the two years in parallel columns, I drew a line with red ink through those average temperatures which happened to correspond, leaving untouched those which had no counterpart. Now the result of this work of elimination was very interesting and instructive.

In the table of weeks I had three contrasted double columns of 25, 50, and 100 weeks; and I found that while the two groups of 25 mean temperatures displayed only seven coincidences, or 28 per cent., the two groups of 50 facts showed 26 coincidences, or 52 per cent., and the two groups of 100 facts (being the weeks of two years compared with the corresponding weeks of two other years) exhibited 62 per cent. of coincidences. The table of daily temperatures showed for 25, 50, 75, 100, 125, 150, and 175 facts, respectively, the ascending scale of coincidences—32, 38, 50, 62, 69, 70, and 72 per cent.

The tables themselves (especially the table of daily averages) have a somewhat confused appearance, I therefore content myself with giving you the results. This form of table is certainly useful, by showing the easy process, so to speak, by which averages approximate gradually to the true values as the facts increase. The first small group of facts shows few coincidences, and many eccentric and unmatched figures. Enlarge the groups by equal additions to both, and the new facts on either side not only have their own counterparts, but find for themselves other counterparts in the first group of facts; and this process goes on with each new addition of figures, until at length only a few isolated and exceptional values remain in each of the contrasted groups, which isolated and exceptional values, being set off against each other, leave a very small balance to be distributed over the aggregate of all the figures, and making the ultimate difference in the case of considerable masses of facts to fall short of a single unit.

I will give one more illustration of the relation existing between numbers of facts and soundness of average results. I will take the births of male and female children in England for the six years 1839-44. I thus obtain six corresponding columns of figures. I must premise that, as a general rule, male births everywhere preponderate over female births. There is very generally a slight excess of males. From the recorded facts for England and Wales, I have prepared a table which comprises 29 districts, towns, and counties, as well as England itself, beginning with the city of Salisbury, having on an average 271 births, and passing through progressively increasing populations and births, till the figures culminate in England itself with its 515,478 births. With the numerical returns before me, I calculated the greatest and least number of male births, on the uniform scale of one million, for all the places

and districts specified, and I added a column showing the range or difference between the greatest and least annual averages.

The table will be found in an essay of mine on the Relative Value of Averages from Different Numbers of Observations, published in the *Journal of the Statistical Society*, vol. xiii, p. 30. It results from this table that, as a general rule, subject, as are all general rules, to exceptions, the range or difference between the greatest and least number of male births increases as the number of facts decreases. Thus (I exclude exceptions) the range for all England, with upwards of half a million of facts, is 3,028; for the north-west counties, with more than 76,000 facts, it is 5,428; for the metropolis, with more than 59,000 facts, 4,321; for the eastern counties, with upwards of 32,000 facts, 4,855; for the northern counties, with nearly 28,000 facts, 6,405; for Staffordshire, with nearly 16,000 facts, 8,672; for Derbyshire, with nearly 8,000 facts, 9,582; for Cambridgeshire, with close upon 6,000 facts, 15,502; for Huntingdonshire, with about 2,000 facts, 34,794; for St. George's in the East, with 1,500 facts, 37,848; for Rutlandshire, with about 700 facts, 60,304; for Canterbury, with 368 facts, 73,680; for Salisbury, with 271 facts, 107,016.

Having now established by an appeal to facts the first of the main positions which I undertook to prove—having shown that when our facts are numerous our average results are trustworthy, and *vice versa*, and further, that for the same order of facts, we approximate to a true average as we gradually substitute larger for smaller groups of facts, I now proceed to inquire what number of facts in any given inquiry will suffice to yield a true average; and what use are we allowed to make of averages derived from comparatively small numbers of facts.

It is not easy to answer this question. It is probable, and even certain, that the number will vary with the nature of the facts themselves, and that, as a general rule, more facts will be necessary in the case of units of variable magnitude than in the case of simple units; more facts in the case of events brought about by the combined action of many forces than in the case of events brought to pass by a small number of forces acting together; more facts when, in such a question as the duration of human life, we admit into our tables several classes than when we restrict our inquiry to one class. Take, as cases in point, the attendance of male patients at an hospital, and the duration of life of the aristocracy and of the upper and middle classes of English society respectively. The causes which determine the attendance of a patient at a given hospital on a given day are both few in number, and for the most part trivial in kind; but the causes which determine the death of a man at a certain age are very numerous, and have been in continuous operation for a long period of time. Now the parallel columns of figures which represent the range or difference between the highest and lowest averages in these two classes of facts certainly approximate more rapidly in the case of the attendances at hospital than in the case of the ages at death of the upper and middle classes of society, though the total number of facts and groups of facts is the same in both instances. Again, if we compare the single class of the aristocracy with the mixed upper and middle class, the first displays the most rapid rate of approximation of the parallel columns of averages. For the aristocracy the ranges, or differences between the highest and lowest averages, for 50, 100, 200, 400, and 800 facts, were as the numbers 11, 7, 5, 3, 1; but for the mixed upper and middle class, for the same numbers of facts, 28, 18, 12, 6, and 4. Though the averages are more numerous in the case of the mixed class, being 64 as against 16, and the comparison therefore wanting in exactness, the difference in the rate of approximation is too considerable, I think, to be accounted for otherwise than by the different character of the facts themselves. Be this as it may, I think that the true average age at death of the English aristocracy would be ascertained—certainly for every practical purpose—by less than 1,000 facts; and the average age of the upper and middle classes by considerably less than 3,000 facts.

This question is, from the very nature of the case, insusceptible of mathematical treatment; but you are probably aware that there are mathematical formulæ for calculating the limits of possible error attaching to any given number of facts, large or small, irrespective altogether of the nature of the facts. Of these mathematical formulæ, Gavarret has made much use in questioning the sufficiency in point of number of some collections of facts made by Louis. These calculations may, of course, be applied in cases of unusual difficulty, where the validity of an argument based upon average results is called in question. But such applications of the pure mathe-

matics must be very rare; and they are certainly not free from objections.

But, whatever the proper answer to this question may be, it must be quite obvious that, if we are obliged to collect one thousand, two thousand, or three thousand facts, in order to arrive at a true average, and to content ourselves with nothing short of these high numbers; and if we must then submit our averages to the ordeal of a mathematical formula, we shall be driven, if not for state purposes where we can command almost unlimited supplies of facts, at least for scientific purposes, to forego the use of the numerical method altogether. For it does not often happen that, for ordinary scientific purposes (I am thinking especially of medical science), we can bring together any great body of facts. This being the case, the very important question arises, Are we debarred altogether from making use of small numbers of facts? Is there not, in the absence of certainty, at least a fair probability, that the average results of even a small number of facts may be entitled to confidence? Admitting the possibility of such averages being remote from the truth (and this admission is forced upon us by the facts brought forward in this lecture), is there not some escape from the very disagreeable dilemma of being obliged to reject all average results except those derived from one thousand, two thousand, or three thousand facts? I reserve the answer to this question for my next lecture.

A Course of Lectures ON THE NATURE AND TREATMENT OF DEFORMITIES.

DELIVERED AT THE ROYAL ORTHOPÆDIC HOSPITAL,
MAY 1860.

BY
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LECTURE I (concluded from p. 451).

Causes of Non-Congenital Deformity. In the non-congenital deformities, the causes are numerous. First, we have contraction of the toes from mechanical causes—tight boots, for example; rheumatism—a common cause—when all the toes suffer; as also in contraction of the heel, when the toes also suffer, as will be explained hereafter.

Disease in the ankle-joint or its neighbourhood, rheumatism, general debility, curvature of the bones, irritation or disease of the spinal marrow or its membrane from numerous causes, inducing an irregular nervous supply or complete paralysis of one or more muscles,—produce deformity of the ankle-joint, in the shape of talipes equinus or equino-varus, which is, in fact, precisely analogous to talipes varus; talipes valgus, talipes calcaneus, in which the gastrocnemius is paralysed; and lastly, talipes calcaneo-valgus.

In the knee-joint, the same causes will be found to have existed, with the addition of idiopathic inflammation, producing, however, more displacement than is ever met with in deformities of the ankle-joint, inasmuch as the knee is perhaps of all joints most dependent upon its passive attachments and the integrity of the muscles, from the nature of the articulation, and from its being situated midway between the trunk and the sole of the foot. Hence, when once a deviation occurs, the mechanical effect of the weight of the body acts as a constant secondary cause in increasing the deformity. We then meet with the following varieties: contraction in the flexed or extended position, as in the congenital deformity; in the inverted or everted, producing outward or inward inclination of the knee-joint—the latter in combination with curvature of the bones. There is also combined with contraction in the flexed position more or less displacement of the tibia (to which I have previously alluded), when the contraction arises from inflammation of the joint, either primarily or secondarily. This last condition is met with in cases where disease of the bone has existed in the immediate neighbourhood of the joint. Paralysis of the extensors of the leg is also a common cause of contraction of the knee-joint.

In the hip-joint, there are the same causes of deformity, with the exception of paralysis producing contraction in the