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Exonian 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 III.

The Numerical Method, its Principles and Rules. Our Individual Facts should be Counterparts of each other. Instances of Neglect of this Rule. Different Occupations described by the same Name. The same persons differently designated through Errors of Judgment, with which the Numerical Method is not chargeable. Examples of Errors of "Non-observation". Bryan Robinson's Experiments on the Pulse. Units should be grouped in Hundreds, or Portions of a Hundred. Average and Extreme Values as Data for Reasoning. Examples of the Use of Extreme Values. The Lung-Tests. Orfila's Calculation of the Stature of the Body from the Length of the Long Bones. Fallacy of Applying Average Results to Individual Instances. Source of the Fallacy. True Uses of Average Results. The Principle of Assurance of Life, Health, and Property. The True Claims of the Numerical Method.

THE first and most obvious principle of the numerical method is, that the individual facts which we bring together, and from which we obtain our averages, should be, in everything but the inseparable incident of variable numerical values, counterparts of each other. If, for instance, we wish to ascertain the average age at death of a given class of men, we should take care to comprise in our tables only men belonging to that class. And we should not be content with excluding from our tables all persons belonging to any other class: we ought to go further, and make ourselves sure that, under one common name, several classes of persons, differing from each other in very important particulars, are not confounded. Take, for example, that useful body of men, the letter-press printers. We should commit a very great error if we brought together in one group, from the Reports of the Registrar-General, the deaths, and ages at death, of all persons designated as printers, believing printers and letter-press printers to mean one and the same class of men, and forgetting that there are copper-plate printers, and lithographic printers, and calico-printers, and silk-printers. But, even supposing that we admitted into our collection of ages at death none but letter-press printers, we might still fall into very serious error, if we overlooked the fact that in printing offices there are several classes of men, such as compositors, pressmen, readers, engine-men, and warehousemen, any one of whom would certainly be returned to the Registrar-General simply as a printer.

Similar precautions, I need scarcely remind you, will have to be observed in making use of the Registrar-General's returns of the *causes of death*, especially in respect of all those diseases which are either difficult of description or difficult of diagnosis. Of these returns, I need not observe, there is scarcely a single item which can be relied upon as a simple isolated statement of the number of deaths from a given cause. But it ought not to be forgotten that, in comparing one year with another year, separated from it only by a short interval, we may obtain very valuable information. The returns of years

separated by longer intervals of time are subject to serious modifications from the improvements which are being constantly effected in medical education, and in medical nomenclature and diagnosis.

This subject of the reports of the causes of death in the tables of the Registrar-General is a very fruitful one, and might of itself be made the means of illustrating one whole class of fallacies clinging to the use of numbers; but the limited time at my command prevents me from entering into it more at length. We must take care, therefore, not to mistake names for things. We must, at the very outset of any inquiry embracing a considerable body of facts, make ourselves acquainted with the nature of the individual facts or units from which our average results are to be deduced. If we do not observe this precaution, we must fall into very serious errors.

There is a source of error very similar to the one to which I have just adverted; it consists in losing sight of certain constituent members of a class of persons, in consequence of their having changed their names. The early history of the sanitary movement affords an instructive example of this kind. One of our leading sanitary reformers caused to be abstracted from the Returns of the Registrar-General the ages at death of all adult males dying in the metropolis in one year; and he arranged the results in three classes—namely, gentry (including professional men); tradesmen; and operatives (or labouring men); and he obtained results which, if real, would have shown a frightful excess of mortality among the working class. But the results were unreal. They did not display the true state of the case. The labouring class was made to appear very short lived, because the persons dying in workhouses, who belong chiefly to that class, were placed apart in a class by themselves; and we all know that great numbers of men, who have been working men, die in our workhouses at very advanced ages. Had these old men been comprised in the tables, instead of being excluded from them, we should have had, as I ascertained by making the necessary correction, a very different result. The working class, though still somewhat shorter lived than the class of gentry, were found to differ very little from the class of tradesmen.

Ignorant and prejudiced persons are apt to point to such mistakes as these as proof of the fallaciousness of the numerical method, or, as they prefer to call it, *statistics*; and they are fond of saying that anything may be proved by statistics. But they are greatly in error. The numerical method is in no respect in fault. The error committed in this case was an error of judgment—an error (to use an expressive phrase of John Stuart Mill's) of non-observation, which the observer fell into before he began to collect his figures, and which he would probably have committed in any investigation whatever, even though not a single figure was used in it. Bryan Robinson, one of our earliest cultivators of a numerical method, made a very similar mistake, and fell into a like error of non-observation, when overlooking, or being ignorant of, the remarkable influence of age on the pulse, he compared the pulses of persons of different statures, beginning with young children, and ending with full-grown adults; and so succeeded in expressing the relation of the pulse and the stature by an empirical mathematical formula. He found "that the mean pulses of regular proportioned bodies are to one another as the biquadrate roots of the cubes of the lengths of the bodies."

Now, it is obvious that Bryan Robinson's error did not consist in careless counting of pulses, in an imperfect record of his figures, or in erroneous calculation. He committed a very common mistake in planning his observations. He did not contrive, as all sound experimenters, whether their results are to be expressed in words or in figures, do,—he did not contrive to make all other circumstances equal except the one of which the variation was the phenomenon he desired to discover and measure.

These two illustrations of a class of errors very commonly attributed to what are termed *statistics* will serve not only to indicate the nature of a special fallacy, but to render it probable that other fallacies also are really traceable not to the

method itself, but to offences against the recognised canons of inductive logic, which all experimenters and observers are as prone to commit as the statist himself.

Those who accuse the statist of unusual liability to error, or still worse, of dealing with materials by the clever manipulation of which anything may be proved, would perhaps be equally disposed to depreciate his discoveries though made, as they are and must be, by the skilful application of the same logical methods that lead men to discovery in other branches of science. But scientific men, who make use of numbers as their data for reasoning, are justified in claiming for their discoveries the same credit which attaches to those who arrive at the same original results by the use of the same logical methods.

Assuming that in any inquiry which we are about to institute with the aid of numbers, we take the necessary precautions to insure the exact similarity of our individual facts, so that they may be counterparts of each other, in every respect, if they are simple units, or differ only in numerical value, if units of variable magnitude; and that we observe all the obvious logical canons applicable to experimental inquiries in general, the rules for arranging our facts with a view to the summing up of their results are simple and obvious. Whether we are dealing with simple units or units of variable magnitude, we should always endeavour to make our facts, if small in number, some simple fraction of 100; such as one-fourth, one-half, or one-fifth, so that we may easily express the results in centesimal proportions.

Having taken care that our individual facts are comparable facts, and having formed them into groups of convenient size, it remains that we take those simple precautions in adding the units together, in dividing the sum total so as to obtain average or mean results, and in calculating fractional or centesimal proportions, which the rules of arithmetic prescribe.

These average results are for us what the more simple results of observation or experiment are to others. It is by comparing one average with another obtained under different circumstances that we discover new truths. But it is sometimes forgotten that, in using masses of figures, we possess ourselves not merely of average results, but also of extreme values, and that these extreme values are to questions of *possibility* what averages are to questions of *probability*. As the skilful use of these extreme values often leads to very important practical results, I will illustrate it by examples.

In courts of law, as you are well aware, witnesses are much more frequently interrogated respecting the possible than the probable. They are required to state whether a certain event, or certain condition of things, is possible, not whether it is merely probable; and it is obvious that, when the matters under consideration assume a numerical form, the answer is tantamount to a statement of extreme values.

I will take an illustration of the use of extreme values from the subject of infanticide. Two of the tests proposed for determining the question whether an infant had or had not breathed are numerical tests. The one consists in comparing the weight of the lungs, in a case under investigation, with the ascertained weights of the lungs of infants who have and who have not breathed. The other consists in comparing the ratio which the weight of the lungs bears to the weight of the body with similarly ascertained ratios. Of these two static lung tests, I will take the first—the absolute weight of the lungs,—only observing, in passing, of the other, or Ploucquet's test, that it was actually proposed as a test on the strength of three experiments, and those not strictly comparable ones. At first, the standard weight of the lungs of a mature infant who had not breathed was assumed to be *one ounce*, and that of an infant who had breathed *two ounces*. But actual experiments on the weight of the lungs before and after respiration soon showed that these assumed standard weights were much too wide asunder, and by no means corresponded with the averages obtained. Even after this correction, however, the average weight of the lungs continued to be used; and to be spoken of in medico-legal treatises with considerable respect, as one of the tests of respiration. In making use of it for this purpose, the important consideration was overlooked that, in a trial for infanticide, the question is not whether it is more probable that the child breathed after its birth than that it was still-born, but whether it was *possible* that the child supposed to have breathed after its birth might not, after all, have breathed at all. Now, it is quite clear that the question which is apt to be raised in a court of law is also the question which we ought to aim at solving in the closet. Instead, therefore, of comparing the average weights before and after respiration with each

other, we ought to contrast the extreme weights. This is done in a table which will be found at page 127 of my work on the *Principles of Forensic Medicine*, from which it appears that in thirty-four weighings of the lungs, the minimum weights before and after respiration exactly corresponded; while two of the weights of lungs in still-born children greatly exceeded the highest weights in children born alive—a fact in direct contradiction of the prevailing and accepted hypothesis, which makes the weight of the lungs after respiration greater than the weight in still-born infants. This table affords a good and sufficient illustration of the use that may be made of extreme values, and of the errors which may be committed by those who employ average values for purposes to which they are inapplicable.

The science of forensic medicine affords, at least, one other good example of the undue and improper value sometimes attached to average results. No less an authority than Orfila fell into this error, in attempting to calculate the stature of the body from the length of some long bone, such as the humerus or femur. He made several measurements of all the long bones of the extremities, and of the statures of the persons to whom they belonged; and then, without even accurately casting up the sums of the several measurements, and dividing by their number, so as to arrive at average results, rashly expressed the opinion that the use of his measurements would enable a man to estimate very closely the real stature of a skeleton by means of the ascertained length of one or other of these bones. In order to satisfy myself whether this opinion of Orfila's was well or ill-founded, I resorted to his original measurements, and, bringing together all those measurements of any one bone (say the femur) which exactly corresponded, I placed opposite to them, in another column of the table, the highest and lowest stature to which he found them to correspond. The results are shown in another table, for which I must refer you to page 26 of the same publication. That table shows clearly that, in making use of Orfila's facts, it would be possible to exaggerate or undervalue the stature by no less than $8\frac{1}{4}$ inches.

I may, perhaps, be allowed to refer to this table as an illustration of the value of that universal logical method which consists in making equal every element of a scientific comparison except the one of which we wish to determine the variation. Bones of equal length are compared with statures which prove to be variable, and to vary within very wide limits.

This example from Orfila may also be used as an apt illustration of a very common mistake into which otherwise able men are apt to fall, of making practical application of the average results of a number of instances to a single instance of the same kind. Let me say a few words with a view of tracing this fallacy to its source, and guarding against the danger of our falling into it ourselves.

In all the cases in which we deduce average, or mean, numerical results from a collection of individual facts, we adopt precisely the same mode of procedure. It matters not whether we are dealing, as the astronomer sometimes does, with observations which owe their variation in numerical value to the variation in the quality of the observations themselves; or, as the engineer sometimes does, with experiments which owe their variable numerical value to differences in the quality of the material upon which he is making his experiments; or, as the physician is constantly doing, with results which owe their variation in numerical value to the variable proportion in which the numerous concurrent causes of the individual results are combined. All these cases, however they may differ in other respects, agree in this, that columns of numerical facts, of which probably no two are exactly alike, are added together, and the sum divided by the number of facts. So, also, with the average age of a living population, or the average age at death, or the average frequency of the pulse or respiration. The respective ages of the living, or of the dead, and the respective numbers of the pulse or breathing, are written down, added together, and the sum of the numbers divided by the number of the facts. And so again with the simplest order of facts, which do not differ in numerical value,—mere units, such as births and deaths—we sum them up, and compare them with the number of the population out of which those births and deaths happened; and, as the population is much more numerous than the births or deaths occurring in it in such short spaces of time as a year, the result is represented by a small fraction. The deaths, one year with another, in England and Wales, for instance, amount to one in every forty-five of the living population. Now, it is very important that we should form a clear conception of the way in which these average

results are obtained. They are deduced from the whole of the figures employed, and not from a part of them only. It is not out of every forty-five persons living in England and Wales in one year that one death happens. We could not undertake to say that, out of forty-five persons occupying some large country mansion, or some school, or some small group of houses, throughout a year, one would die. We should prove very bad prophets if we did. Our assertion that one in forty-five of the living population dies annually applies to the whole population of England and Wales, of both sexes, and all ages, but not to every group of five and forty persons, and especially not to every group of forty-five infants or forty-five aged men or women. It must be obvious to the meanest capacity that this large truth, expressed in numbers, has no practical application whatever to any individual man, woman, or child; and that it could not even be safely applied to any single group of forty-five persons, though closely resembling, in the sexes and ages of the persons of whom it consists, the population of England and Wales of which it forms a part.

The same observation applies to facts of variable numerical value. If I count the pulses of fifty persons, add the numbers together, and obtain an average result, my average is that of the whole fifty facts, and not of any smaller number of facts. I could neither undertake to state the average of five pulses, or of ten pulses, nor the number of the pulse of any person whom I had not seen before. All that it would be at all reasonable to expect is, that the pulse of this person would have the same frequency as that of some one person, or possibly two persons, out of the fifty from which I obtained my average. But, even in this not unreasonable expectation, I might be deceived; for this pulse might chance to be of that rare exceptional order which does not occur once in fifty, or even in one hundred or one thousand times—a pulse of forty, for instance.

The true nature of the case is, perhaps, better seen when we deal with instances in which the fractional result obtained is larger than in the case of the mortality of England and Wales to which I have just referred. Take, as a good and instructive example, the mortality in hospitals, or the deaths from pulmonary consumption occurring among the adult male population of England and Wales. Say that the mortality of a general hospital is *one in ten* of its inmates; and the deaths from pulmonary consumption *one in five* of the adult male population. Every man with a grain of sense in his composition will see that it is not one out of every ten patients in the hospital, or one out of every five of the male population, that perishes, but simply at the rate of one in ten, and one in five. The proportions—one in ten, and one in five—were the results of a large calculation, founded upon a large number of instances, and do not admit of application to small groups of persons.

These considerations are so obvious that you may, perhaps, think I ought not to have detained you with them. But the fact is that those persons who object to the use of the numerical method, or habitually underrate its value, really so far misunderstand those who practise the method as to accuse them of the very misconceptions which I am now endeavouring to remove. These remarks, therefore, are not uncalled for; nor is it unnecessary that I should tell you at once what kind and degree of importance the men who know most of the numerical method, theoretically and practically, really attach to it.

In the first place, they affirm that the numerical method admits of most useful application whenever we can reapply the results obtained from a considerable body of facts to a similar collection of facts of the same order. Such is the insurance of life and of health, and the insurance of property against fire or shipwreck. A large body of facts has been collected showing the mortality and liability to sickness at different ages, and the relative number of vessels destroyed or injured by shipwreck, and of houses injured or consumed by fire. We construct tables presenting the average results of these facts; we add a certain fraction for office expenses, and a certain fraction for possible inaccuracies in our calculations, and proceed to risk millions of money in insuring individual lives, ships, and houses; and our procedure is justified by experience. Though one man shall be knocked down by a carriage, and killed on the spot, as he leaves the insurance office, fresh from the payment of his first premium; though a vessel founder at sea, or a house be burnt to the ground, within a week of the act of insurance, the underwriters are not troubled. Such cases were comprised in the columns of the tables on which their calculations of risk were based; and it is reasonable to expect that these, as well as every possible variety of more favourable occurrence, should sooner or later take place.

In the second place, the advocates of the numerical method affirm that, subject to certain simple provisos (as that the individual facts shall be strictly comparable facts; that they shall be carefully and skilfully handled; that the calculations of average results shall be correctly made; and that the facts themselves shall be numerous), the averages obtained (and the same remark applies to the extreme values) are as useful, for all purposes of comparison and reasoning, as any other ascertained truths, whatever the shape which they may assume.

In the third place, the advocates of the numerical method do not claim for it any great amount of utility when directly applied in the service of the arts. They allege only on its behalf, that average values express tersely and concisely many of the facts with which the practitioner ought to be acquainted; but they admit that they cannot be safely applied to individual instances. It is otherwise with extreme values, which do admit of important practical applications. The engineer, for instance, having ascertained the maximum strength of some material which he is about to use in some important public structure, as a bridge or a ship, satisfies himself and the public by largely exceeding that maximum in his calculations, and providing the strength of his materials accordingly.

These, then, are the limits of the claims set up on behalf of the numerical method; and these are the limits within which its applications to the science and art of medicine are restricted. I shall consider further the rules and principles of the numerical method in my next.

Lectures

ON

THE HISTORY OF MEDICINE.

DELIVERED AT

THE GROSVENOR PLACE SCHOOL OF MEDICINE.

BY

ALEXANDER HENRY, M.D.

LECTURE III.

Circumstances favouring the Development of Philosophy among the Greeks. Character of early Grecian Philosophy. Pythagoras. Alcmaeon. Empedocles. Anaxagoras. Democritus. Heraclitus. Public Practice of Medicine: its Origin. Dispersion of the Crotona School. Democedes. Acron. Public Gymnasia: their influence on Medicine. Herodiscus. Iccus. Summary.

[Concluded from page 390.]

HAVING given a fragmentary notice of the opinions of the philosophers who preceded Hippocrates or were contemporary with him, I will direct your attention to some facts in the history of the commencement of the practice of medicine in public.

The philosophic schools, especially that of Italy, at an early period produced men who began to deprive the Æsculapian priests of their monopoly of medical practice. For the purpose, however, of gaining and retaining the confidence of the people, they at first employed, like the priests, incantations, expiatory sacrifices, and other superstitious practices. Gradually, however, they threw off the mask of mystery, and openly avowed that disease was treated by natural means.

An event which contributed in no small degree to the emancipation of medicine from priestcraft and superstition, was the dispersion of the Pythagorean Society of Crotona. This, which was the result of a popular outbreak, broke the firm bonds which had hitherto held the members together, and diminished the religious feeling with which they had hitherto held inviolable the secrets of their order. Travelling into foreign states, they associated with the uninitiated, and revealed to them, with little or no scruple, their mysterious knowledge; and thus this, no longer a secret, became the common property of the public. Of these practitioners who thus emanated from the school of Crotona, several had the title of *Periodeuta*, or travellers; because they travelled from country to country, practising their art. By this proceeding they gained the enmity of the Æsculapian priests, who still desired to conceal their secrets under the veil of mystery; but, in the end, there is no doubt that they were mainly instrumental in bringing about, by the force of public opinion, a salutary revolution in the manner in which medicine was practised.