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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,
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PART V.

Average and Extreme Values drawn from Small Numbers of Facts. Such Averages are in use by persons entitled to consideration. Averages from Small Groups of Facts may coincide with the True Average or differ but little from it. Examples. Extreme Values. Necessity of Collecting very Large Numbers of Facts in order to ascertain them. Average Values derived from Small Numbers of Facts are sufficient to refute rash Assertions, careless suggestions, Tests based upon scanty Observations, and Assertions springing out of wild and fanciful Hypotheses. Examples. The Lung Test of Ploucquet, and the Measurements by Orfila of the Long Bones. Hahnemann's recommendation of Belladonna as a Preventive of Scarlatina. Experiments of Dr. Balfour. Averages from Small Groups of Facts may also be used with advantage when the results obtained are uniform in character, or the differences between contrasted series strongly marked. Examples from a Series of Inquiries into the Health of Letter-press Printers, London Bakers, and Intemperate Persons.

I RESERVED for this lecture the very important practical question of the use (if any) which we may be allowed to make of average and extreme values derived from small numbers of facts.

Those persons who are in the habit of accepting universal usage as an answer to all doubts and difficulties, would perhaps be satisfied if they were told that averages drawn from small numbers of facts are largely used, and much relied upon, by observers whose sagacity and sound sense are never called in question. But even persons of a more sceptical turn of mind, who insist on thinking for themselves, and who repudiate mere usage and authority, will see that good reasons may be assigned in favour of the use of averages deduced from small numbers of facts. The facts brought forward in the early part of my last lecture will, I think, predispose such persons to this view of the case.

It is obviously possible that in making use of an average derived from so small a number as twenty-five facts, for instance, we may chance to encounter one or other of the widely-separated extremes, or some average bordering upon those extremes. But just as, in the common affairs of life, we are not deterred from action by the thought of what is possible, but prefer to look to what is probable, so in matters of science we are justified in making use of probabilities, instead of being compelled to absolute inaction by the dread of what is possible but not likely. Perhaps, therefore, if we make a different use of the facts which seem to raise so formidable an obstacle to our dealing with small numbers of observations, we may find reason for taking a more favourable view of such a procedure. Let us see what will happen if, instead of confining our attention to the extreme average results of different numbers of facts, or in other words to the possible, we take into account the average results which approach the true average, no less than those which are removed from it, or, in other words, the probable.

I will suppose that we have collected twenty-five facts, and calculated an average;—it is clear that this average, small as is the number of facts on which it is based, might coincide with the true average, or be so little removed from it, as not to affect our reasonings in any way, or to lead to any error worth mentioning when comparing that average with the result of some other similar calculation.

The facts relating to the ages at death of the English aristocracy will supply us with a very good illustration. The average age at death of this class, as deduced from 1,600 recorded ages, is 60½ years, or, in round numbers, 60 years. Now out of 72 groups consisting each of 25 facts, no less than 9 groups, or one-eighth of the whole, correspond with the true average; while 26 groups, or more than one-third of the whole, either correspond with the average, or exceed or fall short of it by a single year; and 35 groups, or almost exactly one half of all the groups, either coincide with the average, or are removed from it by only two years in excess or defect. If we group this same order of facts by fifties, instead of twenty-fives, no less than 25 in 36 groups are found either to coincide with the true average, or to approximate so near as two years in excess or defect.

The same kind of result happens when we adopt the same plan with the 6,400 facts relating to the mean age at death of the mixed upper and middle classes of English society. The true average in this case is 66 years; and there are no less than 20 instances out of 128, or about 1 in 6, in which the average of 50 facts coincides with the true average. The instances in which the average of 50 facts coincides with the true one, or exceeds or falls short of it by one year only, are 42 in number, or about one-third; while the instances in which the averages either coincide with the true one, or do not depart from it by more than two years, are 71 in number, or considerably more than one-half of the entire collection of groups of 50 facts.

The same result, again, happens with the averages showing the attendance of male patients at the hospital. There are just 15 instances in which the averages of 50 facts coincide with the true average of 40 per cent. This is at the rate of 1 coincidence in every 8 averages; while no less than 44 averages, or more than one-third, either coincide with the true average or do not depart from it more widely than 2 in excess or defect.

From the facts and considerations which I have just adduced, I think that I am justified in inferring that the objections to the use of comparatively small bodies of facts which arise out of the divergence of the extreme values when facts are few, and their convergence when facts are many, if not removed, are at least robbed of much of their force. Seeing that there are so many instances of coincidence between the averages derived from small numbers of facts and the true average, and so many other instances in which the averages of small numbers of facts differ but little, whether in excess or defect, from the true mean, we shall certainly be justified in making use of these averages from small groups of facts, provided that we speak of the evidence they afford with due reserve, and lose no fitting opportunity of confirming or invalidating the inferences which we draw from them. To reject such averages altogether would be to throw away very valuable opportunities of enlarging the boundaries of science.

The difficulty of collecting a considerable number of facts in illustration of any one subject, especially when it requires for its elucidation a series of averages, would form a very serious obstacle to the promotion of science. If, for instance, instead of collecting, as I did several years since, 25 observations at each of 13 intervals of age, in order to ascertain approximately the number of the pulse at different periods of life, and in the two sexes (a task which involved, as it was, no less than 650 separate observations), I had felt that no average could be of any service unless it was based upon hundreds of facts, I should have shrunk from the undertaking in despair. As it is, I have obtained results which, though not absolutely true, may be looked upon as very useful approximations—near enough

to the truth for the purposes of the physiologist and practitioner.

Be it recollected that I am speaking of *averages*, not of *extremes*. Now, it is obvious that in order to arrive at real *extremes*, we must accumulate vast numbers of facts. Millions of facts, for instance, would have had to be collected in order to find that single extreme of old age presented by that famous personage old Parr; and millions of facts would be exhausted in arriving at a Daniel Lambert, or such other miracle of obesity. So of giants and dwarfs.

Up to this time, I have examined the question of the number of facts which will suffice to supply a true average as an abstract question, with little or no reference to the special questions for the solution of which our facts were collected and our averages struck. But it is evident that these special questions differ very widely from each other, and that some will require at our hands a very laborious, skilful, and subtle treatment, while others may be decided quickly, as well as satisfactorily, by a very sparing use of figures, and a short, quick, and decisive manipulation. To the first class belong the greater part of our hygienic and therapeutic inquiries—inquiries into the true influence of locality, occupation, and habits of life, on health and longevity—and inquiries into the alleged efficacy of particular remedies or modes of treatment. To the second class belong hasty and rash assertions, careless and superficial suggestions, and tests based upon scanty observations or ill-devised experiments. Also a widely different and very important class of inquiries in which conviction is forced upon the mind by the uniform character of the few facts observed, and the marked contrast existing between the average and extreme results of two groups of observations modelled on a sound plan and conducted with true logical consistency. Sometimes we are called upon (as in the attempt of Orfila to which I referred in my last lecture, to calculate the stature of the body by the length of a cylindrical bone) to make a better use than it occurred to the observer himself to do of his own facts, and, by applying some simple and sound logical method, to show the error into which he had fallen. Sometimes we have to meet some plausible suggestion based upon a very small body of facts by so manipulating another small collection of facts as to demonstrate the futility of the suggestion; such was the case of the static lung tests which I also brought forward in my last lecture. One test rested from the first on a mere assumption, the other was proposed on the strength of three facts, one of which ought not to have been used at all; and about thirty really comparable facts, logically arranged and compared, were obviously sufficient to condemn the first of these tests. The second stood self-condemned.

There is another very peculiar class of cases in which we feel instinctively that it is more waste of time to collect large masses of facts. I mean cases in which we distrust from the first either the honesty or the sanity of the person who proposes a remedy or a preventive. We do not trouble ourselves to combat the dishonest assertions of advertising quacks by putting on the heavy armour of statistics, or wielding the *pièces de précision* of logic; and when we have to do with the apostles or disciples of wild medical heresies, we feel that a very moderate array of figures will suffice to dispose of their most plausible assertions. I will take a case in point. Hahnemann, the fanciful founder of the homœopathic hypothesis, in the course of that amusing series of experiments on his own person which supplied him with many of the data for the alleged virtues of his pharmacopœia, administered to himself small doses of belladonna; and either having a slight cold at the time, or bringing his *attention* and *imagination* to bear on every slight sensation which he happened to experience, really felt, or thought that he felt, some soreness of throat, and saw, or thought that he saw, some redness of skin. These real or fancied sensations, supported by quotations from authors, afforded to him quite ground enough for the assertion that belladonna was a prophylactic against scarlet fever. Two drops of a solution of three grains of the extract in an ounce of water, given twice a day to a child a year old, were alleged to be an almost unfailing preservative from the contagion of the fever. And, strange to say, many practitioners who have no faith in Hahnemann's experiments or hypotheses, have given belladonna, and have deemed it efficacious in preventing scarlet fever. They have probably been induced to make a trial of the alleged preventive by the very natural and excusable desire to give confidence to that class of patients who will insist on interference and activity of treatment at the hands of their medical attendant; and they have fallen into the very common error, on which the success of the charlatan, equally with that of the more regular practi-

tioner, so largely depends, of confounding the *post hoc* with the *propter hoc*. They happened, in their practice, to encounter patients to whom they gave the belladonna, and who enjoyed also an immunity from scarlatina, and they jumped to the conclusion that the belladonna had been the cause of the freedom from attack.

Now, I need not say that an alleged preventive, recommended on the strength of Hahnemann's experiments, or put forward in conformity with his hypothesis, does not require a heavy battery of facts and figures to demolish it. An experiment on a very small scale is all that the supporters of Hahnemann's hypothesis, or the believers in his alleged preventive, have a right to expect at our hands. Such an experiment, devised with true logical acumen, and carried out with corresponding care, was made by my friend, Dr. Balfour, at the Royal Military Asylum at Chelsea. Scarlet fever having broken out in the institution, Dr. Balfour took 151 boys of whom he had tolerably satisfactory evidence that they had not had scarlatina, and divided them into two sections, taking them alternately from the list, to prevent the imputation of selection. To the first section (76) he gave belladonna; to the second (75) he gave none; the result was that two in each section were attacked by the disease. Of this experiment, Dr. Balfour modestly says, "The numbers are too small to justify deductions as to the prophylactic power of belladonna, but the observation is good, because it shows how apt we are to be misled by imperfect observation. Had I given the remedy to all the boys, I should probably have attributed to it the cessation of the epidemic." (See *West On the Diseases of Infancy and Childhood*, 3rd edit., 1854.) I think you will agree with me, that a perfectly gratuitous assertion, advanced in support of a very fanciful hypothesis, has met at Dr. Balfour's hands with much more respectful treatment than it deserves, and that the facts adduced are amply sufficient under the circumstances of the case.

Of that class of inquiries in which conviction is forced upon the mind by the uniform character of the facts observed, or by the marked contrast of the average and extreme results of two groups of observations, I have yet to give some examples. I have no difficulty in finding illustrations of both kinds in my own inquiries into the influence of occupations upon health. I will take the series of inquiries relating to letter-press printers, referring you for minute information concerning them to the first volume of the *First Report of the Health of Towns' Commission*, published in 1844. I will merely remark that I took every precaution to conduct my inquiries and make my comparisons with logical accuracy and precision. In the course of these inquiries, I collected information respecting two groups of 40 compositors. One group of 40 worked in 5 rooms, which yielded to each man, on an average, 303 cubic feet of air; the other group also worked in 5 rooms, which gave to each man the more liberal supply of 789 cubic feet of air. The only other difference between the two sets of rooms was in the number of gas lights. The one set were lighted by 60, the other by 75 gas jets. I made the circumstances of the two groups as equal as I could, by dividing the quantity of air per man by the number of lights burning in the evening, and thus obtained a quotient of 5 against a quotient of 10½. The figures 5 and 10 represented fairly enough, for purposes of comparison, the condition in respect of the supply of air of the two groups. The first group, after correction made for the number of lights, had less than half the quantity of air which the second group had; and out of the 40 men working in the smaller rooms, 5 had spit blood, 6 were subject to severe colds, 6 complained of indigestion, 2 of great debility, and 1 of rheumatism. On the other hand, of the 40 men working in the more spacious rooms, not one had spit blood, 1 only was subject to colds, 1 to pain in the chest, 2 to indigestion, 1 to nervous symptoms, 1 to headache, and 1 had varicose veins.

Now, though forty facts is but a small number, and would be felt to be wholly insufficient to determine any disputed question, if the results yielded by the two groups had differed little in character or number, I was satisfied at the time of the inquiry, and am still more convinced now, that the figures in this case were the numerical expressions of a great truth, which would be confirmed by all future inquiries. I was confirmed in this belief, at the time, by the striking coincidence of the results in the case of the two groups of forty collected from different printing offices with analogous results of comparisons somewhat different, but scarcely less exact, made under the same roof. You are probably aware that a very vicious principle of construction prevails in many of our great printing establishments. The building often consists of one space, divided horizontally by floors perforated by a common staircase. The lower

floor is occupied by the steam-engine, the heat from which, together with the foul air of the intermediate rooms, rises into the upper floor. In most cases of this kind the two upper floors are thickly occupied by compositors. Now two printing offices having this very faulty construction supplied me with two comparisons, which I was not surprised to find issued in a decided confirmation of the results to which I have just referred. In one of these offices, 15 men were employed on the second floor, and 17 men on the third and uppermost floor. Of the 15 men on the second floor, 1 had a cough, 1 was subject to indigestion, 1 had ulcers of the legs, and 1 might be termed a valetudinarian. But of the 17 men employed in the upper room, with a smaller quantity of a more impure air to breathe, 3 had spit blood, 2 were subject to affections of the lungs, and 5 to constant and severe colds. In the other printing office, out of 15 men employed in the lower of the two rooms, 1 only had spit blood, 1 complained of weakness, and 1 of dyspepsia, making 3 in all; while out of 20 men in the upper room, having less and worse air to breathe, 2 had spit blood, 2 were dyspeptics, 2 had rheumatism, 2 were hypochondriacs, 1 was subject to headache, and 1 had varicose veins. In the lower room, the invalids were as 4 in 20; in the upper room, as 10 in 20.

The general results of the inquiry were in strict accordance with those drawn from the smaller numbers of facts; and though the whole body of facts scarcely exceeds 300, I place as firm a confidence in the broad general principle which the figures express as I do in the most rigorous and exact demonstration of the mathematics. Had the percentage proportions differed but little from each other, I should have spoken with hesitation, and waited either for more facts of the same order, or for confirmation, or the reverse, at the hands of others.

To this illustration of the sufficiency of comparatively small collections of facts, I will add a second, drawn from my inquiries into the health of a class of men whose hard case is now deservedly attracting much public attention and sympathy. I may state briefly that these men (the journeyman bakers) are exposed to the combined operation of overwork and nightwork, of a very confined, heated, and impure atmosphere, and of frequent mechanical and chemical irritation of the lungs—the mechanical irritation of the flour, and the chemical irritation of the sulphurous acid and smoky gusts from the oven's mouth. Well, I compared these men with other classes, and particularly with the compositors. I purposely selected about 100 compositors working in the smallest, closest, and hottest rooms, and, taking the occurrence of hæmoptysis among them as a good test and measure of their liability to consumption, found it at the rate of 12½ per cent. This proportion, be it recollected, represents the extreme of disease among compositors. This result I proceeded to compare with the percentage of spitting of blood in 111 journeymen bakers. It turned out to be 31 per cent. But when I compared this worst group of compositors with the poor bakers who work in the worst places for the worst class of masters—I mean the under-priced bakers—the proportion rose to 1 in 2; and of those whom I examined, no less than 6 in succession had spit blood. Now, here again, if I had not sought and found additional facts of the same class, I should have certainly placed the most perfect reliance on the results of this comparison of one century of facts with another. I should have felt confident that my percentages were the numerical expression of a great truth, though not of the precise ratio of the liability to consumption of the two classes of men.

I will ask your attention to only one more inquiry of this kind. Having, several years since, obtained from the Registrar-General nearly 10,000 ages at death of adult male inhabitants of London dying in 1839, I arranged them in groups, with a view of illustrating the influence of occupation and habits of life upon health. One habit, the influence of which upon health it seemed desirable to ascertain, was that of intemperance; and I thought that if I compared the average age at death of certain classes known to be peculiarly exposed to the temptation of drinking with the classes most resembling them, but not so exposed to temptation, I should obtain some instructive results. I accordingly compared the brewers' draymen with the mass of labourers, potboys with footmen, and licensed victuallers with other tradesmen. In each case I found the percentage proportion of deaths between the ages of 30 and 50, and particularly between the ages of 30 and 40, in excess in the class of men most exposed to the temptation. In the case of two out of the three comparisons, the excess was such as to show itself most strikingly when the percentages were converted into curves. Now, this was a case of what would be generally thought a very insufficient collection of facts. The deaths of draymen were only 51 in number; and those of potboys or pot-

men only 61. The other classes were much more considerable, and perhaps adequate in number. Now, I should have attached no importance to those figures, or to the curves which represent them, if the percentages and curves had approximated at all closely to each other. But as it was, I deemed the figures and curves good, partly for illustration, and partly for use in conjunction with other facts leading to the same results.

And here let me observe, that as a broad general rule (at least in my own experience), the numerical results obtained by dealing even with moderate collections of facts have very generally borne out the views of the sounder class of thinkers, who have formed their opinions by means of that unrecorded collection of observations which we call experience. Perhaps I ought to add that they have as often run counter to the hypotheses and theories of men who have shaped their views upon their wishes and hopes, rather than upon their real experience.

In my next and last lecture, I shall consider the most interesting general fact which statistical inquiries have made known to us—the recurrence, year by year, of aggregate figures of nearly equal value, as representatives of the great leading facts of social and medical science.

A Course of Lectures ON URINE, URINARY DEPOSITS, AND CALCULI.

DELIVERED AT THE PATHOLOGICAL LABORATORY,
DURING THE SESSION 1857-58.

BY

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LECTURE VI.

THE ANATOMY OF THE KIDNEY. ITS ACTION IN HEALTH AND DISEASE. *Cortical and Medullary Portions of the Kidney. Pelvis. Mammilla. Infundibula and Calyces. Artery. Vein. Nerves. Lymphatics. Secreting Apparatus. The Uriniferous Tube. Of the Circulation in the Kidney. Epithelium. Of the Basement Membrane of the Tubes and of the so-called Matrix. On some Points connected with the Physiology of the Kidney. ON THE FORMATION OF CASTS OF THE URINIFEROUS TUBES. Of the Cast. Circumstances under which the Renal Secretion may be altered in Quantity or Quality. On the Absorption of Substances from the Stomach, and their Excretion in the Urine. MORBID CHANGES AFFECTING THE STRUCTURE OF THE KIDNEY. Of Bright's Disease. Dr. Johnson's Investigations.*

[Continued from page 490.]

On some Points connected with the Physiology of the Kidney. The course of the blood has been fully described, and it has been shown how eminently adapted the Malpighian tuft is to facilitate the free escape of the watery parts of the blood. The influence of the nervous system upon the secretion of urine is well known. Nerves may be traced upon the large arteries, and followed on the small vessels for a considerable distance. I have seen nerves upon the Malpighian arteries which may be traced up to the tuft. I have not succeeded in demonstrating nerves in connexion with the capillaries of the tuft; but a careful examination of several specimens has led me to conclude that these capillaries are supplied with nerves, as appears also to be the case with the capillaries of the ciliary processes of the eye, and some other capillary vessels. I am now investigating the mode of distribution of the nerves to vascular structures, and I hope soon to be able to express myself more definitely on these very interesting questions.

Virchow lays considerable stress on the existence of a collateral circulation through the vasa recta. He considers that the circulation in the medullary portion of the kidney is more free than it is in the cortex, because the blood in the latter region does not pass through Malpighian bodies. There can be no doubt of the correctness of Virchow's views upon this question, and I can confirm many of his statements from personal observation. Some time since, in examining some specimens of diseased kidney, I was much struck with the thickness of the walls of some of the vasa recta in the pyramids. Upon