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Eroonian 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 VI.

The Recurrence, year by year, of similar Aggregate Numbers, as Representatives of the Number of Births, Deaths, and Marriages, Diseases, and Crimes, and other Social Phenomena. Observations of M. Quetelet of Brussels on Marriages and Deaths in Belgium. Marriage Statistics in England. Statistics of Suicides in England. Striking Uniformity in Results. Serious Inferences drawn by Quetelet from this Order of Facts. His Views adopted by Mr. Buckle in his "History of Civilisation in England". The Question examined by means of a larger and more varied Collection of Facts than has hitherto been used for this purpose. Events dependent on Volition compared with Events from which the Human Will is excluded. The Weather, the Prices of Cereals, Diseases, etc., compared with Crimes and the Disposal of Criminals. Results conflicting; but deliberate Acts shown to issue in Figures marked by greater Steadiness than Acts prompted by Passion. Examples of Low Annual Fluctuation. Students registered at the College of Surgeons. The Consumption of Tobacco and Wine. Our Commercial Shipping. It is not possible to determine the Nature of Forces by the Figures which represent their Action in the Aggregate. Summary of the Principles established. Unconscious Application of the Numerical Method by Ambrose Paré in the Treatment of Gun-shot Wounds.

ONE subject connected with the numerical method, or the logic of large numbers, still remains to be considered. It is one of great interest, and, in some points of view, of great importance. I mean the reproduction, year by year, of the same, or nearly the same, numerical results in respect of births, deaths, and marriages; diseases, and crimes; and, indeed, of most other social phenomena recorded in our blue books and other periodical reports. The first statist who drew attention to this striking phenomenon was, I believe, M. Quetelet of Brussels. An able English statist, Mr. Samuel Brown, has treated of it in an essay published in the eighth number of the *Assurance Magazine*, under the title of "The Uniform Action of the Human Will, as exhibited by its Mean Results on Social Statistics;" and I have myself contributed a considerable body of facts bearing upon the same phenomenon.

I will first place before you some of the facts to which M. Quetelet and Mr. Samuel Brown call attention, and then proceed to examine certain deductions which Quetelet and an English disciple of his have seen fit to draw from them.

The first fact I shall cite is given on the authority of Quetelet. He tells us that from 1825 to 1845, the number of marriages in Belgium was from 28,000 to 29,000 annually, increasing with the population. These numbers happen to correspond very closely with the number of deaths occurring annually in the towns. Now, in the case of the marriages, the extreme numbers, the highest and the lowest, in these twenty years, were 32,680 and 26,117; and, in the case of the deaths, 35,606 and 24,539. So that, in the case of the marriages, there was a difference between the highest and lowest numbers

of 6,563, or about 20 per cent.; and in the case of the deaths, a difference of 11,067, or upwards of 30 per cent.

In the case of these Belgian returns, it does not appear that any correction was made for increase of population in the twenty years embraced in the calculation; and, as Quetelet's object was rather to compare the deaths with the marriages, than to view the one or the other as isolated facts, such correction was not needed. But, in the facts which I am now about to adduce, this correction is made. The facts are taken from the eighth, ninth, and tenth Reports of the Registrar-General.

I will first give you the numbers of men and women annually married in England and Wales, out of 50,000 men and 50,000 women living in each of the six years from 1839 to 1844 inclusive. They were as follows:—1589, 1561, 1539, 1472, 1515, 1597. The greatest number in any year was 1597; the least, 1472; and the difference between them is only 125, or less than 8 per cent. of the greatest number.

If, again, we take the number of marriages occurring in 100,000 females living, in averages of 10 years from 1796 to 1836, we have the following general results:—Greatest average in any 10 years of the five decimal periods, 1716; least average, 1533; difference, 183, or about 10 per cent. of the greatest average.

Or lastly (for these three leading facts relating to marriages must suffice), let us take the proportion of females married under 21, in the 6 years 1841 to 1846 inclusive, reduced also to a scale of 100,000; and we find that the greatest proportion married under age in any of the 6 years is 13,730, and the least proportion 13,160, between which two numbers we have a difference of only 570, or about 4 per cent. of the greatest of the two numbers.

Thus we have here three orders of facts relating to the same subject—marriages as they occur in England; and in no one of them (they all embrace five or six years, or averages of years) does the per centage variation, from extreme to extreme, exceed 10 per cent., and in one of them it is as low as 4 per cent.

One more fact relating to suicides will suffice for the present. The suicides ascertained to have happened in London in the five years 1846 to 1850 inclusive were as follows:—Among females, 97, 84, 72, 69, 70; among males, 169, 172, 175, 144, 159. Greatest number of females, 97; least number, 69; difference, 28; greatest number of males, 175; least number, 144; difference, 31; being about 29 per cent. in the case of females, and about 18 per cent. in the case of males. For three successive years, in both sexes, the difference is very slight: in females, in the last three of the series, 72, 69, and 70; in males, in the first three, 169, 172, 175.

From this instance, then, it appears that, both for males and females, there is a peculiar but similar suicide-rate year by year. But as, in this case, the facts are far from numerous, the fluctuations are, as we should expect, proportionably high:—Instead of 10 to 4 per cent. in the case of marriages counting by the 10,000, from 29 to 18 per cent. in the case of suicides counting by the 100; but in both cases there is the same curious recurrence of similar figures year by year.

I may add, that no fact is better ascertained than the similarity of the numbers which year by year represent any well defined fact in social history. For example, in this very case of suicide, not only is there a great similarity in the number of suicides year by year, in the same population, and in the two sexes, but also a very curious uniformity in the instruments which are used to effect them.

So much, then, for the facts. Let me now call your attention very earnestly and seriously to the inferences which have been drawn from them.

Quetelet, who, as I have said, was the first to draw attention to the broad fact of the recurrence year by year of similar figures, as representing facts in social history or science, was also the first to take notice of the still more curious fact that events, such as marriages, brought about to a large extent by human volition, shared with events such as natural deaths, brought about by unconscious forces, this same property of reproduction in nearly equal aggregate numbers year by year.

Thus he observes, in reference to the first facts which I quoted (those relating to marriages and deaths in Belgium), "Cependant, on ne se consulte pas pour mourir, comme on le fait pour se marier." In other words, death is involuntary, and marriage a voluntary act; and yet there is greater fluctuation in the number of deaths than in the number of marriages.

Now, this same steadiness observable in the aggregate of events brought to pass by the agency of the human will, is also to be observed in the same order of facts when compared with such events as are wholly independent of human volition; and Quetelet invites attention particularly to the close resemblance of the curves which represent the development of the human stature and weight to those which display the number of crimes committed at the corresponding ages. Taking a large and broad view of all his facts, he gives vent to his feelings about them in these words:—"It must be confessed that, distressing as the truth at first appears, if we submit to a well followed out series of observations the physical world and the social system, it would be difficult to decide in respect to which of the two the acting causes produce their effects with most regularity."

After thus stating that the truth which he was propounding appeared at first sight distressing (*affligeant* is the word he uses), we might naturally have expected that some attempt would have been made to give comfort to such of his many readers as might chance to take the painful fact to heart; but so far is this from being the case, that he elsewhere expresses himself in terms calculated to make most men shudder. He actually tells us that experience demonstrates, "avec toute l'evidence possible", that "it is society which prepares the crime, and that the criminal is only the instrument that carries it into execution".

These dangerous sentiments have, I am sorry to say, been recently adopted, and even amplified, by an English author of learning and repute, in his *History of Civilisation in England*. Mr. Buckle, after raising a very useless question as to whether consciousness is a *faculty*, or merely a *state or condition* of the mind, and asserting that, if it be a *faculty*, it is an extremely fallible one—an assertion which would be equally true of any of our external senses,—proceeds to treat *freedom of the will* as a mere *opinion*; human actions as determined by motives which are the necessary results of certain antecedents; and the fluctuating number of good and bad actions as "the result of large and general causes, which, working upon the aggregate of society, must produce certain consequences, without regard to the volition of those particular men of whom the society is composed". After these preliminary statements of opinion, the full and complete adoption of M. Quetelet's dangerous views by Mr. Buckle cannot surprise us. That I may not be supposed to misrepresent him, I will quote his very words, from pp. 25 and 27 of his Introduction. Speaking of *suicide*, he says that "all the evidence we possess respecting it points to one great conclusion"—"that suicide is merely the product of the general condition of society, and that the individual felon only carries into effect what is a necessary consequence of preceding circumstances"; and, again, speaking of crimes generally, "that the offences of men are the result not so much of the vices of the individual offender, as of the state of society into which that individual is thrown".

Now, I should not have asked you to entertain a question of morals in this place, if it were not that these opinions, striking, as I think they do, at the root of all morality and of social order and security, are professedly built up upon one of the most curious and interesting phenomena of the numerical method. I am happy to say that, in examining the validity of these opinions, I shall incidentally be dealing with facts in which, as medical men, we are all interested.

I will begin by asking your attention to the facts upon which these opinions are based; and then to the opinions themselves.

And first of the facts. These are, in the main, correctly stated, though they have been generally treated, both at home and abroad, in a superficial and somewhat unsatisfactory manner. I will endeavour to give you a larger and more complete view of them.

Let me first explain a term which I am about to use—the term *average or mean fluctuation*. It is "the quotient obtained by dividing the sum of all the successive differences between year and year by the number of those differences, and then reducing that quotient to a percentage proportion of the average of all the years." Now, the first general principle which I will lay down (by aid of this mean fluctuation) is this—that the rate of fluctuation diminishes as the number of facts increases.

This principle is, as it were, clearly displayed in the results of a comparison which I have made between the same orders of facts in England and Wales and in London respectively.

In England and Wales, with the larger number of facts, the births fluctuate as 2.01; and in London, with the smaller number of facts, as 1.95. The difference is here insignificant, and the fluctuation is largest where the facts are most numerous. But the marriages, which fluctuate as 3.29 in England, fluctuate as 3.75 in London; and the deaths, which fluctuate as 5.57 in England, fluctuate as 9.51 in London. So with special causes of death. As a general rule, the fluctuation is less for England than for London. Out of ten leading causes of death, there is only one (measles) in which the fluctuation is at a higher rate for England than for the metropolis. I will give the facts in the nine other cases in a tabular form and in round numbers.

Fluctuations.		London.	England.
Scarlatina		46	20
Whooping-cough		32	18
Typhus		20	10
Phthisis		6	6
Bronchitis and Asthma		15	11
Cancer		5	3
Apoplexy		6	3
Delirium Tremens		12	5
Violence, Privation, etc.		8	3

Ceteris paribus, then, the annual fluctuation is greater or less, as the number of facts is less or greater. The more the facts, the less the fluctuation.

Having established this principle as a necessary preliminary, I proceed to develop the broad facts bearing on the question we are considering, by the aid of equal, or nearly equal, groups of facts placed in juxtaposition. I hope, by comparing in this manner events wholly independent of human volition, or only remotely influenced by it, with events brought about by the operation of the will, to ascertain which of the two betrays most uniformity and steadiness.

As examples of events wholly independent of the will, or only remotely influenced by acts of volition, I will take the number of deaths from special diseases; and, as examples of voluntary acts, crimes and the disposal of criminals. Let it be understood that, in making this comparison, I am taking equal numbers and equal periods of time, and the same limits of space. I will not complicate matters by stating the numbers of facts, or the terms of years, or the place. I find, then, that when involuntary and voluntary acts are thus rigidly contrasted, we get such results as the following:—

Deaths from all causes present an annual fluctuation expressed by the figure 10; and cases summarily disposed of, or held to bail, by the figure 5.

Deaths in the northern districts of London, an annual fluctuation of 6; persons committed to trial, a fluctuation of 8.

Deaths in the west districts, fluctuation 9; convictions and sentences, 8.

Deaths from diarrhoea fluctuate as 30; persons acquitted, as 10. Deaths from inflammation of the brain have the low annual fluctuation of 5; while the police cases in which bills were not found, or the accused were not prosecuted, fluctuated as 13.

I will add from the same table a comparison of births, on which the influence of human volition is but remote and slight, with the aggregate of cases discharged by the magistrate. The births fluctuate as 2, and the discharges as 4.

Taking these six comparisons together, it appears that *three* of them display a higher rate of fluctuation in events beyond the influence of the will; while three of them yield exactly the opposite result.

I now proceed to a similar comparison of deaths with crimes, and their treatment by the magistrate, for a longer term of years. The facts are as follows:—

Deaths attributed to asthma.	Fluctuation	16	1
Offences against the person	"	14	
Sudden deaths	"	18	2
Offences against property with violence	"	13	
Deaths from diseases of the liver	"	5	3
Forgery and offences against the currency	"	22	
Deaths in west districts of London	"	14	4
Offences against property without violence	"	6	
Deaths from diseases of the organs of respiration	"	11	5
Convictions and sentences	"	7	
Deaths from all causes	"	16	6
Summarily disposed of, or held to bail	"	3	

Deaths in northern districts of London	5	7
Commitments	7	
Deaths from hydrocephalus	6	8
Acquitted	9	
Deaths from erysipelas	17	9
Bills not found or not prosecuted	13	

Putting all these nine comparisons into one category, and comparing the rates of fluctuation, we arrive at this result: that in six instances, the events not under the influence of volition, display greater fluctuations than those mainly brought about by volition and deliberation; while in three instances, the reverse obtains.

I will ask your attention to the results of only one more tabular comparison of the same class.

Deaths from purpura and scurvy.	Fluctuated as	6	1
Malicious offences against property	"	23	
Deaths from mortification .	"	7	2
Offences against the Mutiny Act ,	"	20	
Deaths from diseases of the spleen	"	14	3
Sentenced to death .	"	18	
Deaths through malformations .	"	5	4
Miscellaneous offences .	"	18	
Deaths from erysipelas .	"	9	5
Offences against the person .	"	10	
Deaths from scarlatina .	"	20	6
Committed to the same prison once	"	8	
Deaths from dropsy .	"	4	7
Remanded and discharged .	"	7	
Deaths from hooping-cough .	"	18	8
Debtors and civil process .	"	6	
Deaths from age .	"	5	9
Commitments .	"	5	
Deaths from measles .	"	34	10
Committed to same prison twice .	"	5	
Deaths from pneumonia .	"	5	11
Offences against property without violence .	"	3	

There are here eleven comparisons, of which one presents an equal fluctuation in the events compared, leaving ten comparisons in which the rates of fluctuation differ. Well! of these ten instances, four are cases in which the event independent of volition shows greater fluctuation than the event brought about by volition and deliberation—the volition of the accused and the deliberation of the judge; while six cases yield an opposite result.

If we take the three orders of facts together, we arrive at this general result: that in thirteen cases the events uninfluenced directly by the human will show more fluctuation from year to year than those which are the result of volition and deliberation, while twelve cases show an opposite result.

The balance being thus evenly struck when we compare disease with crime, I must ask you to bear with me while I extend the comparison of the two orders of events by substituting for disease events or circumstances still less influenced by human volition. I will take the three elements of the weather—the mean annual temperature of the air in tenths of degrees, the annual rain-fall in tenths of inches, and the horizontal movement of the air in miles; and I will contrast these facts, wholly uninfluenced as they are by the human will, with events relating to crime and criminals, summed up in an equal number of units.

The mean annual temperature of the air in fifteen years.	Fluctuated as	2	1
The number of acquittals	10	29	
The annual rain-fall	13	13	2
Bills not found or not prosecuted	14	14	
Horizontal movement of the air	9	9	3
Acquittals	9	9	

Of these three instances, one displays a higher rate of fluctuation for the voluntary acts; the other two a higher rate for the wholly involuntary phenomenon.

My next comparison will be between the prices of the cereals—wheat, barley, and oats, as being greatly dependent on causes beyond human volition, and crimes with their treatment. The prices are given in pence, the unit of a penny being compared with the voluntary or deliberative act as a unit. The following are the results:—

Price of wheat	Fluctuation	13	1
Miscellaneous offences	"	18	
Forgery, etc.	"	8	
Price of barley	"	14½	2
Offences against the person	"	13½	
Price of oats	"	20	3
Malicious offences against property	"	23	

These three comparisons yield a negative result; for in the case of the price of wheat, the two events compared with it neutralise each other; and of the remaining two comparisons, one shows a higher rate of fluctuation for the involuntary event, the other for the voluntary.

If now I range on the one side the three orders of facts least influenced, if influenced at all, by human volition, and on the other, the several voluntary and deliberate acts in which criminals or accused persons, on the one hand, and the magistracy on the other, play their part, I arrive at this result, that in sixteen instances the involuntary events show a greater fluctuation than the voluntary acts; and in fourteen cases the reverse obtains.

As far then as these careful and exact comparisons go, they justify us in stating that events brought about by human volition recur year by year with at least as much regularity as those in which human volition plays no part at all, or is only remotely influential in bringing the event to pass.

Up to this point, then, the statements of Quetelet, as adopted by Buckle, are fully borne out by the most rigid and exact comparison of which the facts in question are susceptible; nor are those statements subject to any material modification, if, disregarding the number of the facts, we compare the limits of fluctuation of different groups of facts.

The wholly involuntary elements of the weather show a range of fluctuation from $2\frac{1}{2}$ for the temperature to 28; for the rain-fall. The prices of cereals, depending in part on the weather, and in part on a multitude of acts of human volition, have a range of fluctuation from 13 to 20. The deaths by 15 classes, and 8 special causes of disease, in which human volition is only remotely influential, and not exercised with the intention of bringing about the result, have ranges of fluctuation, respectively, of from 3 to 34, and $3\frac{1}{2}$ to 31. Six deliberate acts of magisterial discretion, in disposing of offenders, yield a range of fluctuation of 3 to 12; while 6 classes of offences, in which the human will acts with less deliberation, range from 5 to 22. The voluntary act of marriage occupies an intermediate place, with a fluctuation of $3\frac{1}{2}$ per cent., between births with a fluctuation of 2 and deaths of $5\frac{1}{2}$ per cent. These figures are for England. Lastly, it is worthy of remark, that the maximum and minimum prices of consols, and of the leading public securities, determined, like births, deaths, and marriages, by an infinite number of causes, and, like marriages, by an immense number of individual acts of volition, have the narrow range of fluctuation of 3 to 6.

The facts which I have just laid before you form but a small portion of those which I submitted to the Statistical Society two years ago. I have again studied those facts very carefully; and I think that I find in them a warrant for an opinion which, if really well founded, goes far to place the question raised by Quetelet and Mr. Buckle in a new light.

In the first place, as a general rule, I find the fluctuations in deaths, from special diseases, in conformity with our knowledge as medical men. I find every degree of fluctuation, from the minimum of $3\frac{1}{2}$ per cent. in the most steady of all the causes of death (diseases of the brain and nervous system) up to the maximum of 154 per cent. in the case of the least steady of all (cholera). The highest rates of fluctuation belong to epidemic and contagious maladies; an intermediate rate to diseases of the organs of respiration; and the lowest rates to local diseases, especially to those which depend, like consumption and cancer, on structural degeneracy. A low place, but not the lowest, is occupied by deaths from violence and privation. One other remarkable fact in reference to death by disease is, that the rate of fluctuation in the class of zymotic maladies not only exceeds the rate in all other classes of disease, but also that which prevails in any other order of facts whatever. So that it is no longer true that the weather must be looked upon as the very type of changeableness. The real type is to be found in the diseases which arise out of the influence of this variable, the weather, on a second variable, the human body.

Let me again draw attention to the fact that the causes of death most dependent on the exercise of volition—death by violence, privation, etc.—show great steadiness, though they are surpassed in this respect by several local diseases. This fact tends to raise the question whether the introduction of human volition among the causes which determine a given event does not always tend to steadiness and freedom from fluctuation?

If we compare disease and crime, we certainly receive one affirmative answer; for there is a much wider range of fluctuation in deaths from special diseases than in crimes of different

kinds. The physical world, in this important aspect of it, is certainly less steady than the moral world.

If, again, we consult the miscellaneous events and facts brought together in one of the tables submitted to the Statistical Society, December 1857, we find this attribute of steadiness to belong to a great variety of events and facts determined by human volition.

The introduction, then, of the human will as a factor among the many causes which combine to bring about events in social science, tends to bring about steadiness—to limit annual fluctuations.

But I think that I can go a step farther than this. I think I can show that where the human will is set in motion as the result of thought and deliberation, and not of mere passionate impulse, steadiness is the result. Thus if we compare crimes, in which the human will acts under the instigation of passion, with the disposal of criminals in the police court or at sessions, which is the result of deliberation, we find the crimes fluctuating between the numbers $5\frac{1}{2}$ and 22, while the discretionary acts of the magistrate range between the figures $3\frac{1}{2}$ and 12 $\frac{1}{2}$. That is to say that deliberate acts issue in figures showing greater steadiness than acts prompted by passion.

Now I find this very important view confirmed by many cases of low fluctuation. The lowest rate of fluctuation, for instance, which I find in any of my tables is that which takes place in the number of students annually registered at the College of Surgeons. The next lowest is the fluctuation in the quantity of manufactured tobacco taken for home consumption—a quantity determined by an infinite number of single acts of volition, themselves the result of habits repeating themselves day by day without excitement. The annual consumption of wine, and the vessels which carry on our commerce at the deliberate bidding of the merchant, show similar steadiness. So also do marriages, which, though in part the result of passionate attachments, do not take place without grave acts of deliberation on the part of parents. The chief elements of fluctuation in them, when we take into account the whole mass of the people, is the price of food; but the deliberations which precede the marriages are the elements of steadiness.

The antidote to Quetelet's dangerous opinions, as adopted and amplified by Mr. Buckle, seems then to be supplied to us in the simple statement of the fact that the human will, when introduced as a factor among the many causes which contribute to bring about any social phenomenon, is a cause of steadiness; and that this is especially the case when the human will acts after due deliberation. Those who believe in the existence of a free will in man, could not desire a better illustration of its workings, on the large scale, than the fact I now state. If, as a general rule, unconscious forces, taken one with another, and in variable combinations, bring about events characterised by large annual fluctuations; and the introduction among such forces of the human will acting under the instigation of passion, and ending in detected crime, gives rise to a certain degree of steadiness; but the human will acting with deliberation, and in obedience to law, to a greater degree of steadiness; then, I submit, that there is no justification whatever for the misgivings and openly-expressed scepticism of Quetelet, or of those whom he has been the means of leading into error.

The views of Quetelet, and of his admirers, might have been met by the simple allegation that numerical results do not, and cannot, give us any clue to the nature or intensity of the causes by which they are brought about. The only salient point which any annual returns present is the greater or less steadiness—the greater or less freedom from fluctuation to which I have just adverted. That our figures do not of themselves give us any clue to the nature or intensity of the causes or forces by which the facts or events they represent were brought about, I think that I can prove to you by a very simple experiment.

I abstracted from the out-patient books of King's College Hospital the consumptive and non-consumptive patients, in groups of 50, up to the number of 5,000. Of this aggregate of 5,000, 172 proved to be cases of consumption, and 4,828 of other diseases. I represented the cases of consumption by white peas, and other diseases by black ones; these I put in a bag, and, after mixing them well together, proceeded to draw them out, blindfold, by fifties; and noted down the result. I repeated this experiment, and I found that between the attendances of these two classes of patients at the hospital, determined as they must have been by a great variety of motives, and the drawings of the white and black balls, effected by variable combinations of very trivial forces, there were several very remarkable coincidences. The figures were nearly the

same, but the forces totally different in kind. Now, if these concurrent forces, so contrasted in character, and so different in intensity, may find their ultimate expression in very nearly the same figures, what ground can there be for inferring from near equality in numbers, or similarity in annual recurrence of figures, identity of character? and yet such an inference as this must have lain at the root of Quetelet's painful misgivings and dangerous assertions.

I trust that I have said enough upon this subject. I will now briefly apply to the science and art of medicine what I said in my last lecture of sciences and arts generally, and finish this lecture and course by a short history of a great discovery in surgery, from which I will draw one or two brief inferences.

1. The numerical method admits of most useful application whenever we can reapply the results obtained from a considerable body of facts to a similar large body of facts of the same order.

2. The average results which we obtain from large numbers of facts, as well as the extreme values, are as useful, for all purposes of comparison and reasoning, as any other ascertained truths, whatever the shape which they assume.

3. The numerical method admits of very limited use in actual practice. Average results cannot be safely applied to individual cases, and extreme results rarely come into play.

4. The numerical method is to be commended as contributing to precision, accuracy, and truthfulness.

5. Average results obtained from small bodies of facts may be employed with advantage to test and refute opinions carelessly adopted and rashly propounded; to establish new truths when the figures exhibit in themselves great uniformity, or when compared with similar small collections of facts, if the numerical results are widely divergent; and lastly, to indicate probabilities to be afterwards confirmed or invalidated by more extensive induction.

I see that there is just time enough, within the hour, to give you my promised surgical history, and to add a very few words of comment.

In the year of our Lord 1536, Ambrose Paré served as surgeon to a French army relieving Turin. In one of the battles in which this army was engaged, there were many wounded with all sorts of weapons, but chiefly with bullets. Now Ambrose Paré, after confessing that he was at that time a novice in surgery, tells us that he had read in *John de Vigo* (who seems to have been the great surgical authority in those days) that gun-shot wounds being poisoned by the gunpowder, it "was expedient to burn or cauterise them with oil of elders, scalding hot, with a little treacle mixed therewith." Knowing that caustics could not be poured into wounds without causing excessive pain, and having his doubts about the proper treatment, Ambrose Paré determined to see whether other army surgeons dressed these wounds in any other manner. But he found that they all used "that method of dressing which Vigo prescribes; and that they filled, as full as they could, the wounds made by gun-shot with tents and pledgets dipped in this scalding oil, at the first dressing." This determined Paré to adopt the same treatment. But "it chanced on a time," as he tells us, "that by reason of the multitude that were hurt, he wanted this oil; and because there were some few left to be dressed, he was forced, that he might seem to want nothing, and that he might not leave them undressed, to apply a digestive made of the yolk of an egg, oil of roses, and turpentine." I will give you the result in his own words:—"I could not sleep all that night, for I was troubled in mind, and the dressing of the preceding day (which I judged unfit) troubled my thoughts; and I feared that the next day I should find them dead, or at the point of death, by the poison of the wounds, which I had not dressed with the scalding oil. Therefore, I rose early in the morning; I visited my patients, and, beyond expectation, I found such as I had dressed with the digestive only, free from vehemency of pain, to have had good rest, and that their wounds were not inflamed nor tumefied; but, on the contrary, the others that were burnt with the scalding oil were feverish, tormented with much pain, and the parts about their wounds were swollen. When I had many times tried this in divers others, I thought thus much, that neither I nor any other should ever cauterise any wounded with gun-shot."

In this striking passage of the life of the great French surgeon, we have, as I think, an apt illustration of the unconscious application of the numerical method, and of the logic of observation, a quarter of a century before Bacon was born, and more than two centuries before a numerical method was thought of. Here were two sets of facts differing only in the

one point of the method of treatment accidentally adopted—on the one side only *some few* that were left to be dressed, on the other the multitude of wounded, less this small number; but the small number, in virtue of the uniformity of result satisfying the mind of this acute and truly logical observer, and leading him to the safe conclusion which his subsequent experience amply confirmed.

This, I repeat, is an unconscious illustration of the value of that numerical method which I have endeavoured to set before you, to explain, and to vindicate, in these three lectures.

Lettsomian Lectures

ON

THE OPHTHALMOSCOPE AND ITS APPLICATION.

DELIVERED BEFORE THE MEDICAL SOCIETY OF LONDON.

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LECTURE II.

MR. PRESIDENT AND GENTLEMEN,—Allow me a very brief recapitulation of a few of the subjects that occupied my last lecture. I shewed that Mr. Cumming had made no practical use of his discovery of illuminating the eye, not having even succeeded in complete illumination; that an instrument was needed to throw the light into the eye, and at the same time to admit of our seeing distinctly, which, under circumstances, might be used alone, especially when a near sighted eye was examined, but that for the most part a lens was required in addition; that, as soon as the interior of the eye receives the light, a red glare is visible, and that the visual focus of the observer is not got till the individual parts of the fundus of the eye, the optic disc, and the retinal vessels, come into view; that the optic disc is the termination of the optic nerve; that the retina is recognised by its vessels, being ordinarily unseen from its transparency; that the choroid chiefly gives the glare to the lightened eye, its individual vessels not being discernible in health, but the larger vascular plexuses recognised by markings, which are seen in contrast to their interspaces, which are darker, because containing more choroidal pigment. I shewed too that the retinal vessels, uncertain in number, enter the eye through the optic disc, sometimes centrally, sometimes at the side; and that there is great variety in the natural, or physiological, or healthy eye, with which it is necessary to be acquainted.

I proceed now to diseased states; and commence with *Cataract*. Few surgeons, I suspect, have long been engaged in active duty without being aware of the difficulty attendant on the diagnosis of incipient cataract. Doubtless, the majority of my hearers know this. Nor is it the general surgeon only who may be so puzzled. From my own personal knowledge, such men as Tyrrell and Dalrymple were deceived, as the progress of cases shewed. It would be easy for me to adduce parallel examples occurring among living practitioners. Indeed, till the ophthalmoscope appeared, we were necessarily often in difficulty; and no one felt this more than myself, as the means at our disposal—the catoptric test and the concentrated light—were not enough. It was especially so in old age, when the natural coloration of the lens, the amber hue existed, and particularly when it was unusually developed, for then streaks of opacity at the back of the crystalline lens, or general haziness of this body, were easily overlooked. The coloration itself was liable to be mistaken for disease, when there coexisted impaired vision from any cause.

A wine-merchant was assured by a gentleman engaged in ophthalmic practice in this metropolis, that he had cataract. An operation was proposed, and consented to. The chloroformist was even named. I was consulted, in order to satisfy some friends. I found the lenses very highly coloured, but not opaque, for I could see distinctly all the fundus of the eye.

Much hepatic derangement with a very debilitated system seemed to be the cause of defective sight, and a few weeks treatment confirmed the diagnosis I had given.

Whenever, therefore, there is a suspicion of cataract, an ophthalmic investigation should be instituted. A less intense light than ordinarily employed, is preferable, because it is possible here to over illuminate the eye, and the striæ may not be seen. With very high illumination, opacities may be less apparent than without the ophthalmoscope. It is on this account too that the eye should be examined at first without the lens. A source of fallacy must here be guarded against. The dots of pigment that are so often seen on the capsule of the lens as the result of inflammatory attacks, by which the iris and the capsule have been brought into apposition, as also cicatrices or spots on the cornea, must not be mistaken for cataract opacities. It is enough to direct attention to this point to prevent that mistake. Positively, cataract may now be detected before a person is himself aware of it.

A lady applied to me for defective sight in the left eye. I had little doubt that I saw radiating striæ in her lens; but to be quite sure, I used the ophthalmoscope, and had the fullest evidence of cataract. I looked at the other eye, and discovered a few small and irregular linear opacities, which I had not seen in my naked eye examination. I was told, in answer to my questions, that that eye was quite well, and vision was not impaired; yet I found by some delicate visual tests, that there was not perfect sight, but the derangement was so trifling, that it had not been noticed. I thought this a great triumph of modern science.

It is of no small diagnostic value to have a correct idea of the condition of the interior of the eye in cataract, to ascertain whether the loss of sight is in proportion to the degree of the opacity, and, in fact, depends on it, or on a more extended diseased state. We can get this knowledge by no other means. Do not considerations like these warrant an often repeated assertion of mine, that for what we have gained in this respect alone, the ophthalmoscope is of immense value; and if we went no further, might be truthfully considered amongst the greatest additions to recent ophthalmology.

Another condition of the crystalline lens can be learned only in the recent state by illumination; I mean, *Partial Dislocation of this body backwards, or twisting out of its place by direct violence*. This may be palpable later when the opacity has pervaded the lens, but certainly not by any ordinary means till then.

Disease of the *Vitreous Humour* seems to cause more astonishment to an inexperienced observer than any other morbid changes; partly, I suppose, because any physical alteration apart from greater fluidity was never supposed, and because the moving about of shreds and granules, and flakes and threads, are such palpable and material things. We now know that this body, which certainly is of low organisation, since it is often very rudely treated in certain operations, and portions of it is lost with apparent impunity, does become materially altered, partaking of disease that invades the eyeball generally. Apparently, at times, it is the sole seat of morbid phenomena, which are great impediments to vision. This is quite new to us; and no speculation, guessing, conjecture, or indication, brought it within the range of probability. In inflammations of the eyeball in general, it mostly becomes opaque, and looks muddy, and more or less hides, as in a fog, the fundus of the eye. Careful examination generally discovers minute bodies in constant motion. I have seen opacity of it, in the greatest degree, without any other symptom that I could detect, or any cause; and some cases that seemed quite hopeless, from the degree of turbidity, and the corresponding loss of sight, and the absence of all indication for treatment, have got quite well. So frequently have I seen the cloudiness pass away entirely in acute specific inflammations of syphilis and rheumatism, even where there were floating patches and filaments, which we call exudations, that I have little prognostic dread of it, as compared with cloudiness unaccompanied by acute disease.

Floating objects are not uncommon in the vitreous humour while its transparency remains; and they may exist alone, or in connection with diseases of the retina or the choroid coats. These are generally seen by the individuals themselves, and are often the circumstances that induce them to apply for relief. This brings me to speak of *muscæ volitantes*, in connection with the internal ocular inspection, and that only. I think that any one who has studied the writings of Sir D. Brewster, and the masterly essay of Dr. Mackenzie, himself a sufferer from *muscæ*, or investigated the subject with pains and care, cannot fail to conclude that they are minute floating