MEDICAL ERRORS.

BY THE SAME AUTHOR.

A MANUAL OF MEDICAL DIAGNOSIS,

BEING AN ANALYSIS OF THE SIGNS AND SYMPTOMS OF DISEASE.

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MEDICAL ERRORS.

FALLACIES

CONNECTED WITH THE APPLICATION

OF THE

INDUCTIVE METHOD OF REASONING

TO THE

Science of Medicine.

BY

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PREFACE.

When I was requested by the esteemed President of the College of Physicians to deliver the Lumleian Lectures for 1864, it was not without some misgiving that I ventured to call the attention of the medical profession to the great want of logical precision in the mode of reasoning, which forms the basis of so many of our theories. The subject had long occupied my thoughts, and daily experience had only strengthened my conviction of its importance. In presence of such an audience, and with so little time at my disposal, it would have been unprofitable to enter upon the elements of logic; to give anything like a full and consistent account of the Inductive Philosophy alone, would have required much more time than is allotted for this course of lectures; and my plan would have failed in its object, had it not been possible to illustrate my meaning by current examples.

The lectures are now published just as they were delivered, with the addition only of certain portions which were suppressed at the time, so as to occupy no more than the three hours allowed me. But it would require a volume of much larger dimensions to do full justice to the two subjects which I have endeavoured to place side by side. The true meaning of induction, the tests of its accuracy, and the value of its results, have been contrasted with the mistaken notion of it which is generally entertained, the fallacies which are accepted, and the erroneous conclusions which have consequently been adopted. And, though it be vain to expect that true principles of reasoning could be thus made intelligible to persons who have not previously studied the subject, I may yet be permitted to hope that the examples of false reasoning, selected from recent medical literature, may prompt a desire to become better acquainted with what I conceive to be the only true means for the advancement of science. I trust, too, that on their perusal, my lectures may continue to receive the approbation of those, my seniors in the profession, whose too partial judgment of the fragmentary portion presented to my audience at the college, has tempted me to publish them in their complete form.

In criticising the writings of others, I trust that I have not said anything calculated to give offence. The critic very generally shields himself behind the incognito of an anonymous article in a review, but when he lays aside all disguise, as I have done, he cannot be supposed to be prompted by the insane desire to make himself enemies. Many of the writers who have been referred to in these pages are personal friends, and I should be sorry if they could possibly feel aggrieved by selections from their writings having been taken to illustrate my meaning. Most of them occupy a higher position in public estimation than myself; and any criticism of mine will consequently leave them unscathed. Be this as it may, the following pages are but the effort of one who longs to see the science of Medicine placed in its proper position, alike beyond the reach of hostile depreciation and of base and fraudulent imitation.

BRUTON STREET,

November, 1864.

MEDICAL ERRORS.

SINCE the days of Bacon the mind of scientific observers in England has conformed so much to the general system of reasoning laid down by the great author of the Inductive Philosophy, that we may be said to have adopted it more or less as our necessary guide in legitimate medicine, without almost a thought of the principles on which it rests. No more complete condemnation could, in the opinion of most of us, be pronounced upon any views of pathology and practice, than that they were opposed to the teaching of observation and experiment; in other words, that they were opposed to the principles of inductive reasoning. And yet we are so little conversant with the rules by which these processes ought to be conducted, that we are necessarily to a great extent incompetent to detect the fallacies which are so often introduced into arguments assuming to be based on sound principles. It is not alone among the men whose time is engrossed by the current calls of daily practice that this want of logical knowledge is perceived, but also among those who, from their higher attainments and greater amount of leisure, have had ample opportunity to make themselves acquainted with various branches of study, and even among those who profess to teach others the principles on which the science of medicine ought to be cultivated. Few, indeed, have attempted to apply the rules of inductive reasoning to medicine, though many of the collateral branches of knowledge which are embraced in the education of medical men, are cited by logical writers as instances of the progress made, in consequence of the employment of the methods of research which this department of philosophy has suggested. The learned author of the "History of the Inductive Sciences" probably felt that he was too little conversant with the principles of pathology and therapeutics, to include any part of these subjects in the outline he has given. And yet, I think, I shall be able to show, that although much behind what are called the exact sciences, still the principles of medicine are largely based upon true and legitimate inductions. My task I feel to be one of much difficulty, but to none could I with more confidence submit the considerations which spring out of such a theme, than to the Fellows and Members of the Royal College of Physicians, among whom are to be found all who are most distinguished in the medical section of our profession, all who have most contributed to the advances already made, and who are, consequently, best able to judge of the correctness of the principles of reasoning on which our future progress so much depends. But I must crave your indulgence if, in endeavouring to place in a clear and intelligible light my own views on the subject, I perchance fail, as is too probable, to awaken such a degree of interest as may overcome the ordinary feeling of repulsion to anything of the nature of dry argumentation.

Some years ago a late Fellow of this College presented to the world what he designated a "Legacy to his Younger Brethren."* Himself advanced in years, and more conversant than any other, from his literary position, with the doubts and difficulties that presented themselves to the minds of the thinkers and writers of his own day, and their longings and aspirations after a certainty which is, in all human probability, unattainable, he only gave utterance to a very general scepticism, which, probably, from his mental organization, met with a very ready response in his own mind. To him the outcry which was somewhat unreasonably raised against his little book was of small moment. He only did that publicly, which so many had been doing in secret for a long antecedent period; and telling his fellow-labourers that Art was but the handmaid of Nature, asserted with more boldness than wisdom, that many of the remedies on which our forefathers, if not our own generation, had relied, were unworthy of the confidence so implicitly placed in them. Had he gone on from this proposition, to show what manner of service Art could render, my task need hardly have been undertaken. But while that part of his writings which calls in question the power of medicine, properly so called, to cure disease, will be approved by most of his readers, he utterly fails, as it seems to me, to point out either the causes of error in practice which he denounces, or the grounds for adopting those plans of

* "Nature and Art in Disease," by Sir John Forbes, M.D.

treatment which he sanctions. The main proposition of the book, that recovery is, when it takes place, a natural process which may be aided or hindered by the interference of Art, scarcely required to be enunciated in such a formal manner. At the same time, doubts as to the value or powers of remedies, have received undue attention, as might, indeed, be anticipated in a sceptical inquiry, from which the reasoning of an inductive logic has been so completely excluded.

My object in pursuing the investigation which the title of this course of lectures indicates, is of a totally different nature. The errors in practice which have sprung up at various times, the false views which have been promulgated, and which have, perhaps, attained a certain currency, must all more or less be dependent on false reasoning. And now that we have discarded the old method of assuming premises, of the truth of which nothing could be known, the error must lie in the application of that other mode of reasoning, to which the name inductive has been given. We cannot now return to the *à priori* arguments which satisfied the inquiries of an earlier age, when perfectly gratuitous assumptions were accepted as explaining the phenomena of life; and remedies were prescribed in compliance with theories which, though wholly unsupported by fact, were yet supposed to have their basis in the very nature of things. Such, for example, was the doctrine of signatures, which taught that every natural object possessing any remedial powers, bore on itself some mark by which its uses in the economy might be recognised. Turmeric, from its yellow colour, must be a remedy for jaundice; the petals of the red rose ought to cure a hæmorrhage; poppies should act on the brain from the peculiar form of their growth; and the proper covering for a person suffering from scarlet fever or measles must necessarily be scarlet cloth. Among such assertions we can recognise some which probably rested on experience, and to which the doctrine of signatures was applied as an explanation, while others are wholly *à priori* deductions, and are utterly without foundation in practical experience. It is quite possible that in such an example as the poppy, the erroneous theory may have first led to its employment, and a true discovery may thus have been made; but the result does not prove the correctness of the reasoning. The deductive arguments which form the basis of the teaching of the present day, are all more or less founded on conclusions which are supposed to be derived from an accumulation of facts; were it otherwise, the firm faith in the truth of inductive reasoning which so characterises the English mind, would at once set such teaching aside. Ours is not the same form of error as that of a bygone age, but if error there be, the fallacy must be sought in the application of the inductive method of reasoning to the science of medicine.

In the discussion of the fallacies into which, as medical men, we are prone to fall, it will be necessary to consider some of the errors which the recent literature of medicine discloses, because it is among those theories which have already been the snare to some, that we must look for such as are likely in various forms to mislead others; and it is only by the evidence of past mistakes that we learn the dangers we must in future avoid. It is impossible to pass in review all the difficulties which present themselves in establishing a legitimate induction; but my subject would be very imperfectly handled, and would indeed be wholly unintelligible, were I not to state something of what we are taught by the ablest writers on this subject. I do not, however, propose to give a dissertation on logic, and therefore shall not bind myself to any system in the way in which these references to methods of argumentation are introduced.

I need hardly say that there are two great divisions in which all minor distinctions finally merge, viz., the deductive and the inductive methods of reasoning. Their relative advantages and disadvantages have been very frequently made a subject of contention by their respective partizans. Contrasted together under the names of the Aristotelian and Baconian systems of philosophy, it has been too much the fashion to dissever them from each other, and to regard them as antagonistic in their nature and results. In truth they are but parts of the same system, to one or other of which at particular times undue prominence may have been given, but which are really mutually dependent on each other for all their value. In the hands of the sophists and the schoolmen the Aristotelian logic had degenerated into mere verbal strife, in which scarcely any principle was involved beyond an adherence to rule in the form which the argument assumed; and this circumstance very much tended to conceal the erroneous nature of the hypothesis on which it rested. It became necessary that a revolution should be effected in the manner in which scientific questions were discussed; and to Bacon especially belongs the merit of pointing out that in a majority of cases the premises were assumed by the schoolmen without any sufficient warrant; and that the laws of nature could not be guessed at *à priori*, but could only be established by careful examination of a sufficient number of individual instances. Possibly Bacon's own mind may have scorned a system of reasoning which had so degenerated in his day; and his ardent followers doubtless overlooked the importance of the correct application of principles once ascertained: but inductive reasoning cannot proceed one step without the aid derived from deductive argument; and unconsciously those who speak most disparagingly of its importance, are themselves in the constant habit of employing it. My own belief in this matter is that, as any deduction which is not based on induction cannot be trustworthy, so any induction which is not linked with some deductive inference is utterly worthless. In simple language, any conclusion based on a mere hypothesis which is not proved by a careful examination of facts, is not to be relied upon, because some future observation may prove the hypothesis to be entirely gratuitous, and all the reasoning from it false. And on the other hand, the simple enumeration of facts is a very profitless addition to our knowledge, unless the conclusion proved by them be of such a character as to be capable of future application, either in the way of practical usefulness, or as leading on to the discovery of fresh truth.

I shall have occasion afterwards to show that the guiding principles in treatment are chiefly deductive in their character, and that in this department of knowledge we seldom meet with a simple induction; but all the force and value of such deductions, is dependent on their being grounded on inductions of more or less absolute correctness. In many of the most important inductions which have marked the progress of science, the hypothesis which has served as the link to bind together the various elements of which they are composed, has been in some sort a deduction, but it has always rested upon previously ascertained facts. I am aware that in making the assertion that all deductions ought to be based upon principles first arrived at by the inductive method, I am not quite in accord with some of the highest authorities in this matter, who allege that a class of perfectly trustworthy inferences exists, which are deduced wholly from intuition, and are hence called necessary truths: such for example as the axioms in mathematics, of which some at least are said to be self-evident to every one capable of understanding their terms: *e.g.*, that two straight lines cannot enclose a space, or that parallel lines can never meet. The question is one of pure metaphysics, whether such axioms are accepted because it is impossible for the man to whom they are propounded to believe anything else, or whether they are accepted simply because he can at once recall to mind hundreds of instances, which immediately confirm the truth of the proposition made to him. For my own part I must place myself on the side of those who regard it as an instance of the very simplest form of induction, where the facts are already known, and an hypothesis is suggested for their explanation; but the examples being so simple, the mind at once calls up some one or more of them to recollection, and proceeds to try experimentally the truth of the hypothesis.

That this is the correct view, seems to me to be borne out by the circumstance that the knowledge that day follows night has just as much the character of necessary truth; but yet no one would say that they depend upon or are produced by each other: and the explanation of their necessary connexion by our knowledge of the revolution of the earth on its axis is most clearly a result of inductive reasoning. So also, to educated minds, many points of philosophical truth are equally self-evident and necessary, such as that an acid must neutralize an alkali, but no one can doubt that such a conclusion is wholly the result of education.

As commonly stated, the first part of the process in the construction of an argument based on the inductive method, consists in the collection of particular facts which agree in some one or more points; their harmony forming the groundwork of the inference which is afterwards drawn. We shall see by and by what rules can be laid down as to the number of instances required to prove the existence of such a harmony; but it may be here stated that while it is on the harmony that the inference rests, cases which differ or disagree on the point under consideration must not be excluded from the enumeration, as they form the most useful tests of its truth.

The next step is to frame an hypothesis to link together or to explain the phenomena; and this must at least possess the character of embodying the general fact observed in each of the particular instances. Such an assumption very often goes before the induction altogether: at least it goes before our consciousness of collecting facts; though in all probability the facts have come before the mind, and have impressed it unconsciously, and so have suggested the hypothesis around which we subsequently begin to collect instances bearing upon the point. These are analysed and compared until it is found that in all of them two facts bear a certain relation to each other, and will always be found in the same connexion, unless interfered with by some other circumstance. The form under which this relation is perceived is the hypothesis which, whether assumed antecedently or subsequently to the accumulation of facts, gives the special character of induction to the observation: to this the name of law is applied when the mutual harmony of the several facts is explained by it, and it has stood the test of full experimentation. There is no real difference between an assumption made without any sufficient warrant, and one based on an observed harmony, except that one will stand the test of experiment, and the other will not. For while it is quite true that the hypothesis is very often suggested to the mind of the accurate observer by some harmony which arrests his attention, it is nevertheless true that in many cases it is a mere deduction; and in most of those which are classed as the highest inductions, there is a combination of both forms of reasoning, often united with an idea which is not the fruit of any reasoning process whatever, but is simply the bright offspring of genius.

In whichsoever way the hypothesis is assumed in the first instance, it is of the essence of inductive reasoning that it should be put to the test of experiment, either by selecting from the cases already collected such as may serve for the purpose of testing its truth, or by collecting new instances specially selected and prepared with this view; and this constitutes the third part of the inductive process properly so called. If any one circumstance may be pointed out more than another, as characteristic of the process, it is this; and the term *à priori* argument has been chiefly used to designate those methods in which an assumption has been made, and a train of reasoning based upon it, when it was either quite impossible to bring it to the test of experiment at all, or if possible at least it had not been attempted. The hypothesis may fairly explain all the known facts, but it has none of the characters of a law until it has been tested.

The final portion of the reasoning process is to apply to other cases, by way of inference, a law which has been already established by induction. This is wholly independent of the inductive method. Deduction may just as correctly be applied to a false theory as to a true one; the premises being granted, certain conclusions may be legitimately drawn from them, whether they be true or false, or simply unproved; the conclusions participating in the very same character of truth, falsehood, or uncertainty. The correctness of the manner in which this process is performed is of the highest importance in the science of medicine; for inference and analogy must be our guides, when laws are wanting, and indeed they suggest the principal rules which are followed in practice. Inferential or deductive argument is often the only means we have of contradicting a false hypothesis which cannot be brought to the test of experiment; it is also the mode in which all dogmatic teaching is conveyed, and so becomes the groundwork of future inductions of more general character.

Perhaps I may best illustrate my meaning by a very simple example. It is by no means an instance of the highest class of induction, and to some logicians, it may seem almost undeserving of the name; but it commends itself to my own mind by its extreme simplicity. It occurred to me when considering the relations which the squares of successive numbers in arithmetic bear to each other. If they are written down in order we obtain—1, 4, 9, 16, 25, &c., which differ from each other by the several quantities, 3, 5, 7, 9, &c., as $1+3=4$; $4+5=9$; $9+7=16$; $16+9=25$, &c. These numbers consist of successive odd numerals, and may be represented thus—

$$
\begin{array}{llllllll}
1^2 & = & 1 & & & & \ldots\ldots\ldots\ldots\ldots\ldots & \text{1st odd No.} \\
2^2 & = & 4 & = & 1^2 & + & 3 \ \ldots\ldots & \text{2nd odd No.} \\
3^2 & = & 9 & = & 2^2 & + & 5 \ \ldots\ldots & \text{3rd odd No.} \\
4^2 & = & 16 & = & 3^2 & + & 7 \ \ldots\ldots & \text{4th odd No.} \\
5^2 & = & 25 & = & 4^2 & + & 9 \ \ldots\ldots & \text{5th odd No.}
\end{array}
$$

Whence it appears that the square of any number is equal to the square of the numeral immediately preceding it, together with its own corresponding odd number. I might be satisfied with the number of instances taken, and these are quite sufficient to fulfil the requirements of a strict induction, and give me the empirical law which has been enunciated. But let us next proceed, by experiment, to try whether this law will hold good of any other two numbers standing in succession at a long interval.

$$\begin{aligned} \text{Is } 18^2 &= 17^2 + \text{the 18th odd Number?} \\ \text{now } 17^2 &= 289 \\ \text{and } 18^2 &= 324 = 17^2 + 35. \end{aligned}$$

And I find, on calculation, that this number 35 is the 18th odd numeral.

We have now satisfied the requirements of the inductive process so far, that we may be certain that our law will hold good for any successive numbers whatsoever. But in its present form, the law is a very imperfect one; we cannot say why it is so; we can make no use of it. Let us try whether we can resolve it into some more general law of numbers, and for this purpose, employ the deductive method. I conceive that in a large majority of cases, when a special induction is brought under some general law, the mode of reasoning will be found to be mainly deductive. Of this relation, I shall have afterwards to speak more fully; but it may be here remarked, that all mathematical reasoning proceeds on this method, certain premises being granted under the name of definitions, axioms, facts, or necessary truths, the whole science consists of logical deductions from them. My object at present is simply to give an example of its application, and though the illustration is not quite so simple as that just given, yet I trust it will be intelligible. The more general law under which this case falls, is that known to algebraists as the Binomial Theorem; but we need not go into the meaning and origin of this principle in order to understand the formula by which the law is applied. Let us call any numeral "a," the number next above it must of course be "$a + 1$" and $(a + 1)^2 = a^2 + 2a + 1$.

Or, more fully:

$$\begin{array}{rrrrr} & & a & + & 1 \\ & & a & + & 1 \\ \hline & & a & + & 1 \\ a^2 & + & a & & \\ \hline a^2 + 2 & & a & + & 1 \\ \hline \end{array}$$

Now let 17 stand for "a."

$$\begin{array}{lcl} \text{then } (a + 1) & = & 18 \\ \text{and } 18^2 & = & 17^2 + 2\ (17) + 1 \\ \text{or } 324 & = & 289 + 34 + 1 \end{array}$$

From this general law, it appears that we can predict what number must be added to the square of one numeral to obtain the square of the next above it; and it also explains why the corresponding odd number is that sought for, because it is one more than twice the lower numeral,—in other words, one more than the lower numeral counted by twos,—one more than the even numbers counted up to the lower figure, and consequently the odd numbers counted up to the higher one.

In this illustration may be seen the meaning and object of the various steps of the reasoning process already described; the observation of facts which exhibit some harmony or relation among themselves; the assumption of some formula which expresses that relation; the process of testing its validity by experiment; and the employment of laws already established for the purpose of solving fresh problems when presented to our minds. The examples are, I confess, rather meagre, and are not quite unexceptionable, but will serve this purpose, I trust, even better than if they had been more brilliant instances of inductive and deductive reasoning.

One of the latest writers on this subject—perhaps I ought to say one of the very few who have attempted to point out the place and the uses of induction in the study of medicine*—has unintentionally obscured what he designates "the purely inductive method of research," by importing into his consideration of it much of the deductive or *à priori* system of reasoning. The learned Professor of Medicine of Edinburgh teaches his students that their theories are to be grounded on analogy, and that these analogies must all have reference to the one fundamental principle, which, in "technical language," is stated to be "the unity of structure and function of organisms in time and space." Now, supposing this principle to be all that he claims for it, the reasoning by analogy from such a principle must be regarded as deductive. It is quite true that afterwards he refers to the comparison and tabulation of facts as a necessary part of the reasoning process, but he explicitly states that the first step is the assumption of a theory, whereas the first real step is the ascertaining that we have facts to deal with, and a knowledge of their general bearing. In the subsequent investigation, those facts only are to be taken account of, according to this author, which are shown to be analogous by their relation to the antecedent "principle," while those are rejected which are merely similar. The same obscurity

* "Medical Observation and Research," by Thomas Laycock, M.D., &c. Lecture vii.

pervades the whole of his exposition of the subject. The "theoretical" determination of Adams and Leverrier, "that a large planet was revolving on the furthest bounds of our system," is cited as an example of the first step in the inductive method; whereas they discovered no law, but merely employed the known laws of gravitation to solve the problem of the disturbances in the orbit of Saturn. They were truly discoverers, but discovery is not necessarily induction. A certain hypothesis had been suggested, and a very intricate proposition had to be worked out for its solution, but the whole operation proceeded on the assumption of ascertained laws, and their part was only applying the deductive method of reasoning to a particular instance, and predicting the results. Similarly, the "fundamental principle" itself seems to me to be a theory rather of the deductive than the inductive kind. It is based on the recognised laws of certain homologies hitherto ascertained, but itself has no pretensions to the character of a law. It is an hypothesis assumed *à priori* to explain a few limited examples, which has not been put to the test of experiment. Future investigations may prove or disprove its truth, and every argument based on it must necessarily partake of its hypothetical character; it therefore does not satisfy the requirements of inductive reasoning.

Induction in the sense to which the term is restricted by logicians, is the means by which we arrive at certain conclusions, especially with regard to the relations of cause and effect, which, from their certainty, have received the name of laws. It proceeds by collecting particulars together, but it does not rest there, neither is it satisfied with the mere tracing out of resemblances among the instances enumerated; its object is only attained when a law is established which can afterwards be applied to other cases. If the law fail in its subsequent application, unless some sufficient reason can be assigned for its failure, we must believe that the induction has been faulty, and, in all probability, the number of instances collected has not been sufficient. This is especially the case with reference to those less general and more complicated laws, to which the name of "empirical laws" has been given. The more universal the law, the more simple it generally is, and the fewer instances are needed to establish it; and when exceptions occur, the explanation of their cause always serves to prove its truth. Laws of nature have never been traced by a simple accumulation of facts; they are by no means manifest as the necessary result of the induction, however carefully framed. They have generally been first of all suggested, perhaps by accident, to some master-mind which has caught a glimpse of something behind the induction; and then a theory is proposed, which he proceeds to test by the laws ascertained in previous inductions, or by a set of fresh experiments. What may be called the necessary result of a simple induction is very generally, like the example just given, of comparatively little practical value.

I cannot here refrain from alluding to the most perfect example of a law of nature—the Newtonian theory of gravitation,—because its development, as detailed in the "History of the Inductive Sciences,"* so fully bears out what has just been said. Very correct calculations of the places of the planets had long been in existence before Kepler arranged them in such a form as to be able to try upon them a succession of hypotheses, one of which led to the true law of their orbit, viz., that it presents the form of an ellipse. In his hands the argument assumed the form of an induction, associated with an empirical law, which of necessity failed to account for the deviations from their true path; it was, consequently, comparatively barren in results. Men's minds had been subsequently drawn to the speculation that something or other must bind the planets to the sun, and the subordinate satellites to their respective planets; but the theory which presented itself to Newton was not traceable in any previous induction. The idea that the circumstances, whatever they might be, which caused a stone to fall to the ground from any known height, might also reach to the moon and keep her in her orbit; and that, if they reached to the moon, they might, in like manner, extend to the sun, and so on throughout space; and that the law of these circumstances must be the same wherever they existed: —this idea finds no place in any of the previous inductions which had been made in astronomy and mechanics. But as soon as it was applied to them, it was found to be the very law to which they all pointed, and which satisfied all their conditions. Not only so, but it accounted for disturbances which the previous empirical rules left untouched; and each apparent exception only confirmed

* "History of the Inductive Sciences," by W. Whewell, D.D. Vol. II. Book vii.

its truth, until it has received what might be called its crowning confirmation, in the employment of these exceptions for the purpose of pointing out the exact spot in the heavens, where an unknown planet was to be found.

When an ascertained law is absolute, and the other conditions are known, the result of its application may be calculated with certainty; and if the facts upon trial do not correspond with the anticipations, we conclude that we have reasoned incorrectly from it. The special province of logic is to show whether the reasoning be correct; it therefore serves to indicate where the error lies, when our anticipations are not realized, and even may point out that an argument is fallacious when its results seem to be true. Time does not permit me to enter further into this subject, which is more closely connected with deductive than inductive reasoning. It is only necessary to say that the syllogism, which is often regarded as the great hindrance to its study, is after all merely the form, as the late Archbishop Whately expressed it, into which every argument may be thrown if sufficiently curtailed, and by means of which its conclusiveness may be tested. A perfect logician who thoroughly understands the nature of the syllogism, will sooner than any other, point out a fallacy, in an argument, but it by no means guards against false assumptions; and of this the Archbishop himself was, in his faith in quackery, a notorious example.

The various steps of the reasoning process, as already referred to, are very frequently described under the names of induction, generalization, experimentation, and deduction. It is with the two first of these steps that we are chiefly concerned, because in them fallacies are most liable to occur. If it be true, as I have alleged, that no induction is of any value without an hypothesis, which leads on to generalization, it is easy to understand how important it is that the assumption be not hastily made, without sufficient warrant from the facts collected. Many errors have arisen from this one cause. For example, some years ago it was believed that there were certain conditions of disease, associated with what is called inflammatory action, in which blood-letting aided in the recovery of the patient. Whether this conclusion were right or wrong is not now the question; but the assumptions made to give the character of a generalization to this supposed induction, were of such a nature as to render any conclusions drawn from them, even if true, perfectly untrustworthy. To take only one instance: the blood drawn in such circumstances was observed to present a peculiar character called the buffy coat, and the theory propounded was that blood-letting was useful, because it withdrew from the circulating medium the excess of fibrin which gave it this appearance. But in order to do this, evidently the greater part of the blood must be abstracted, and something else must take its place; in other words, the patient must probably die, in order that the disease might be cured. This false assumption not only led to most reckless and injudicious treatment, but in the reaction following on the discovery of the fallacy, such a state of feeling has arisen that a physician hardly dares now to use the lancet, whatever his views of its employment may be.

Necessary as some assumption is to the very essence of induction, yet nothing can be more destructive of right reasoning than a foregone conclusion. No sooner has it been arrived at, than an attempt is made to prove its truth, by collecting affirmative instances, and we encounter all the difficulties met with in the arrangement of statistics. It is well known that almost any proposition may be proved by them, and it is scarcely possible in dealing with them, to avoid arranging the figures in such a manner that they shall seem to affirm the conclusion already arrived at. I conceive that in their nature and object statistics or averages differ very considerably from true induction. A perfectly reliable induction might be drawn from one single instance, where the scope of a law is distinctly seen, and in its subsequent application the law so propounded might never fail. On the other hand, thousands of cases arranged in a statistical form might give a perfectly true answer to any particular question in the aggregate; and yet the answer might fail, and indeed very probably would fail, if an attempt were made to apply it to a particular example, because it involves no law of causation, and what is true of the whole as a mass, never is or can be, absolutely true of the individuals composing it. For instance, in the example already cited, the discovery of the laws of gravitation is assigned by common report to Newton's attention being arrested by the fall of an apple; and it might very well have been so, even if the story be a myth—the one example might have suggested the law, even though the general facts of weight or gravity were previously well known to him; the other cases serving merely as experimental tests of the truth of the induction; but when once framed, the laws deduced from it were applicable to every two solid bodies which were within an appreciable distance of each other. On the other hand, the statists of the Registrar General's office have been calculating averages of deaths for every metropolitan district weekly, quarterly, annually for many years; and could tell with a wonderful approach to accuracy what would be the relative proportion of each district for the next ten years;—what allowance must be made on the whole for increase of population—what for improvement in the general health and longevity of the population; but they cannot at all tell how many persons will die next week. The number may be a tenth above or below the average; and this may go on for weeks or months in succession, with scarcely an assignable reason. In the end a counterbalancing period will occur, and the general result, if calculated for ten or twenty years, will be found to be true; but as there is no law so there can be no certainty in the application to any particular time or place. So again, even though the causes which temporarily increase the mortality are known, the limit of the variations must be quite uncertain, because the degree of their influence and the power of counteracting tendencies never have been defined, or brought under the restrictions of even an empirical law.

The great distinction between the result of a statistical calculation, and of an inductive argument, seems to consist in this, that an induction properly so called, by establishing a law, points out a relation of cause and effect, which can only be superseded by the interference of some more powerful or more universal cause. Whereas an average, even when derived from a sufficiently large number of instances, is only a calculation of chances, which gives us no insight into the true relation subsisting between them. No doubt hidden causes exist, and at some future day their laws too may be traced; but so long as a result is obtained only from statistical calculation, and has not been brought into the domain of laws of causation, it must be classed simply as a fortuitous event; and although its relative frequency may be fairly estimated, its cause can only be guessed at.

This seems to me to be one of the most common fallacies in the supposed employment of the inductive method of reasoning. It is assumed that by the collection of a number of particular instances having some one or more points in common, the requirements of this method have been fulfilled, without any indication of a direct relation of cause and effect; when indeed no law has been sought for, and no explanation of occasional failures has been attempted. A medical treatise is too often estimated merely according to the number of cases, and especially successful cases occurring in the author's practice.

In pointing out the difference between these two methods, it is not my wish to depreciate statistical inquiries, when rightly employed. My object is to show that they cannot be put in the place of inductions. They very often serve to test the value of a deductive argument, in cases where the antecedent induction is imperfect, and the argument consequently uncertain; or they may, to a certain extent, be employed to test the truth of an hypothesis, when direct experiment is impossible, and a correct induction cannot be framed. They might also be of immense importance, if rightly used, in attempting to estimate such slight degrees of influence as many remedies do possess, in contributing to the recovery of a patient from an attack of illness, even when they cannot control the disease under which he is labouring. It seems to me, that the statistics of our large hospitals collected under ever-varying circumstances, as to the antecedents of the patient, the nature of the attack, and the pet prejudices and customs of the physician by whom he is treated, amounting as they soon would do to a large number, would afford a basis of calculation on such points of very considerable value; because the chances of error would, from the nature of the inquiry, be comparatively small. In a large number of diseases the physician can only treat certain prominent symptoms, leaving the ultimate recovery of the patient to nature. No known remedy has ever been shown to exert over them a specific influence; but they are invariably attended by definite symptoms, which individually are more or less under the control of art. The habit of prescribing has empirically determined that certain of these shall be dealt with in a definite manner; but we look in vain for any evidence of the correctness of the rule, inasmuch as no law of cause and effect is involved, and no trustworthy averages have been obtained. When, therefore, any new suggestion is made, either in the way of recommending that some other symptom should receive more attention, or proposing to employ different means for attaining the end contemplated by existing modes of treatment, the means of testing the value of the suggestion are wanting, and we are only guided by vague impressions, in adopting or rejecting it. The experience of one man is generally too much restricted by the smallness of the number of cases, and by the general tone, if I may so call it, of his practice, to arrive at any correct solution of the problem; but hospital statistics, if properly used, would afford a ready answer.

As an illustration of what is here alleged, let us take the case of rheumatic fever. Most persons ultimately recover from this disease, except when the heart becomes affected, and even of those with cardiac complications, the deaths are comparatively few. It therefore teaches almost nothing when a man records a dozen, a score, or even a hundred cases of recovery from this disease, whether his treatment happened to consist chiefly in the administration of nitre, of lemon juice, of calomel and opium, of alkalies, or even of brandy. For all that the selected cases show, the result might have been the same if nothing but cold water had been taken. In fact, the deaths are so few, that even large hospital statistics would probably not give practically useful averages of mortality. But there are two circumstances which are constantly observed in rheumatic fever, viz., an average duration of a considerable period, and a tendency to heart affections of very great frequency. If, therefore, an observer were able to show that all his cases recovered more quickly than the average, and that all, or nearly all of them, escaped any heart affection, the result would be definite in its character, and would demand an inquiry whether treatment had anything to do with bringing about the result. In this further investigation we must bear in mind that there are several points to which treatment may be directed in this disease,—the diathesis itself, the febrile state, the tendency to inflammatory exudation, or the acidity of the system. Each of these has in its turn claimed the attention of medical men; and although never probably regarded quite as specific, the treatment directed to meet these several symptoms has at one time or other been considered the best, on the whole, for patients suffering from acute rheumatism; in fact, as positively influencing the result, and producing beneficial effects which could not be obtained under what is called the expectant method, or treatment with cold water. The very fact that the recommendations have been so various, and that each method has had its ardent supporters, and has enjoyed a fleeting reputation, proves that the argument by which it has been supported must have been faulty; and I think it is not difficult to trace that the error has consisted in a misapplication of statistics. Had it been possible to establish any law of causation, the cases collected by each writer on the subject were sufficiently numerous for the purpose; but for the employment of the numerical method, the number has always been far too small, as will be shown by and by, to enable him to estimate the effect of those ordinary influences which tend to falsify the results of all averages of limited extent.

Recently an attempt has been made, by taking the statistics of one large hospital, to show what is the relative average of heart-disease, when the one symptom of acidity is especially treated. By fixing the attention on two points only, the liability to error is considerably reduced, but the cases collected are not yet nearly sufficient to establish the point. It is alleged in this report,* that by the administration of large doses of alkalies, the cardiac inflammations are reduced to a minimum, and that they are not so affected by any other treatment. Other modes of treatment are not however, adequately represented, and it is very much to be wished that similar statistics should be given for all the large hospitals. Were this done, we could say with some degree of certainty, what ought to be the treatment, at which we now only guess. The alkaline treatment is a deduction. It is rational to suppose that it would be advantageous for the patient to neutralize the excess of acid; and hospital statistics might determine whether it were so or not, but even if they all led to the same conclusion, we should still be as far as ever from a true induction, because there is evidently no relation of cause and effect, in so far as the modification of the disease is concerned. We know nothing of the means of checking even its outward manifestations, which seem to go on quite unaffected by the excess of alkali hourly passing off through the kidney, and no doubt carrying with it a large amount of the acid generated in the system by the disease. In fact, all that hospital statistics can teach in this case is, whether by directing treatment mainly to neutralizing acid, we do better for our patients than if we fix our attention on any other symptom.

It does not appear to me that any hypothesis satisfactorily adapts itself to the facts hitherto recorded. We may not assume even that the acid is wholly and con-

* "Medico-Chirurgical Transactions," vol. xlv., p. 343.

stantly neutralized by the alkalies administered, because we find fresh joints attacked after the urine has become fully alkaline, and has been maintained in that state by their frequent repetition. We might suggest that the acidity of the blood was the cause of the fibrinous exudation, when we find it prevented by the alkaline treatment; but when we ask if this be a law, we are immediately met by the answer that there is great excess of acid in gout without any similar tendency, and that there may be acid dyspepsia with constant acidity of urine, and yet no fibrin deposited upon the cardiac membranes. Were we disposed to take as our theory that the excess of alkali held the fibrin in solution, and prevented its deposition, we are met with the difficulty that before this can rank as an induction, we must be able to show that it will do so in pleurisy, in pneumonia and peritonitis.

Let me take another example of a similar kind—the treatment of continued fever by quinine. Cinchona bark is known to cure ague: this is an instance of an induction of an inferior degree, to which I shall have to return hereafter; it is enough for our present purpose that it is so. It is also very generally acknowledged that quinine is the best remedy both for the prevention and cure of the malignant intermittents of the tropics; it is further alleged that it has been given with benefit more or less marked in the tropical remittent fevers—fevers as it is supposed, of a wholly different origin. Assuming these facts to be proved, and believing that the tropical remittent fevers bear the same relation to continued fever in this country, which the intermittents of those regions bear to ague, Dr. Dundas was led to the conclusion that sulphate of quina was the best remedy to administer in continued fever. It needed no large accumulation of facts to show that there was no such evidence of causation as could give to it the force of an induction, even if all were admitted which its proposer alleged. But just as there is in rheumatic fever one prominent symptom—acidity—so continued fever is constantly marked by quickness of pulse; and it happens that when the remedy is given in such doses as to produce the condition called cinchonism, it causes a remarkable reduction in the frequency of the pulse. No statistics can ever prove that this reduction of pulse is associated with diminution of fever, because we know that it is not so, and that it may return with unabated violence if the physiological effect of the remedy pass off; but by large hospital statistics, and by them alone can we determine, whether, by the depressant action of the quinine, combined with its tonic powers, fever is on the whole better treated by the employment of this than of any other remedy; whether in fact fever patients die in smaller numbers and convalescence is more rapid and attended with fewer sequelæ of a serious kind, when cinchonism has been established, than under any other circumstances. Generally, our attention is mainly directed to the heat of skin and to the general depression, and with this view, we give salines with ammonia and wine; but there are some who think that in doing so, we do not best meet the emergency. This can only be determined by a very large enumeration of cases, because, to say the least, the difference in result is not apparently very greatly in favour of the cinchona treatment.* The question is, in its present condition, one only to be dealt with by statistics, or the numerical method, and the difficulties in the way of arriving at trustworthy results by mere numbers are more formidable than is generally supposed.

The numerical method as it is called, is intended in some measure to supplement the inductive, and to indicate whether, on the whole, any relation may be believed to exist between two phenomena, where no law of causation has been yet proved. In this way it has occasionally served to point out unsuspected relations, and even to open the way for framing inductions. In order to do this, however, the number of cases required is very much greater than that which is needed to establish a law of causation. The great difficulty in tracing out the relation of cause and effect consists in the presence of an immense number of circumstances, many of which it is impossible to exclude, while all may contribute more or less to the production, or at least the modification, of the result. Most of these circumstances are also variable, and one or other may be absent in any given number of cases; but an induction with reference to any one is only possible if the whole of the remainder can be excluded, or can be shown to be powerless except in conjunction with the efficient cause. The numerical method does not require any such exclusion, but endeavours to eliminate the variable circumstances by bringing together a large number of instances. These must be arranged in two groups, in one of which the phenomenon which we seek to study is

* "Medical Times and Gazette," Jan. 8, 1853.

present, while in the other it is absent. And in each group there ought to be an equal number of examples in which the other variables co-exist, so that they, in fact, neutralize each other, by producing an equal effect in each series.

The more simple and the fewer these concomitant circumstances are, the smaller may be the number of cases selected to form the average; the more complicated and the more numerous they are, the larger must be the basis of any reliable statistics.

If, again, it happens that the particular circumstance we are studying, exercises much more influence than any other in bringing about a given result, a smaller number of cases will serve to establish such an inference, than when its action is comparatively weak. Let us further attempt to illustrate the difference between a law of causation and the calculation of an average, by the progress of any form of disorder, and its treatment by a particular remedy. It is quite clear that if any disease which is known to be invariably or most commonly fatal when left to itself, and to be very little, if at all modified, by ordinary treatment, were, under the influence of some special remedy to cease to be generally fatal, and terminated in death only when some other cause combined to produce the fatal result—if such were the case, a law of causation of a certain kind, not the very highest, but an empirical law, would clearly have been established. A comparatively small number of real recoveries would be sufficient for its verification, and one or two distinct failures for which no valid reason can be assigned would serve to disprove, or at least to cause us to doubt its existence. Such a law will not be found absolutely true in every case, but it is true for all ordinary purposes. An example of this kind was offered by the investigation undertaken some years ago at the Middlesex Hospital, of the curative powers of certain supposed remedies in cancer. It was known that by the method employed, the cancerous tumour was removed, and it was alleged that the remedy had the power to prevent its recurrence. The issue is here very simple; the cancer did return, and therefore the law which was alleged to have been discovered was proved to have no existence. Any further questions of temporary benefit in freedom from pain, prolonging of life, &c., were such as could only be determined by experience, and therefore need not occupy our attention now.

Again, supposing the disorder one that is not necessarily fatal, perhaps never so; but that when left to itself, it generally goes on for weeks, or months, or years; and though more rapid recoveries without medicine do occasionally take place, they are still so few that they do not materially interfere with the general fact. If now on the employment of some specific remedy, rapid recovery were the almost invariable result, we should again have clear indications of the existence of an empirical law. It would only be necessary to show that other modes of treatment did not exercise the same specific influence, and also to explain, if possible, the causes of its occasional failure. Thus, when we find in the history of ague, that in former days it generally lasted for many months, whereas now, by a few doses of cinchona bark, or quinine, we can generally arrest it in two or three days, I think there can be no question that an empirical law of causation has been discovered. Similarly, if a case of lepra or psoriasis has persisted for a long period in spite of various remedies, which seemed to produce no influence over its progress; and if after the administration of a few doses of Fowler's solution, the scales begin to be modified in character, and the modification goes on till recovery is established, this also seems to me to prove the existence of an empirical law.

The number of instances required to frame an induction in each of these examples is of course different, just in proportion to the certainty of the progress of the disease without the administration of the special remedy, and the possibility of other causes exerting any influence over it; and also, in proportion to the clearness of the evidence by which the causation is established, the quickness or the invariableness with which the effect follows. If, on the other hand, in a few well-marked examples free from complication, the remedy is administered to the full extent of producing its constitutional effects, without arresting the progress of the disease, we may be certain that the supposed law is a false assumption.

Our next consideration is whether, by the method of averages, we can ascertain the relative power of the remedy, as one of many circumstances which conduce to the result. The question is not whether there be evidence that we can promote the recovery of a patient by the judicious employment of remedies, the actions of which are known, and seem likely to place him in a more favourable position for the operation of those natural processes by which the cure of disease is brought about; it is not whether we do right or wrong in giving occasional purgatives or opiates, or in using any other means which fulfil a single end invariably, and are only employed as adjuncts to systematic treatment. The question is, whether we can prove that, in the course of any given disorder, some one remedy is to be used in preference to all others, and is to be considered as essential and indispensable, if we would treat the disease aright; while at the same time there is no indication why such means ought to be used, because no law of causation can be established. We want to estimate what influence, on the whole, any agent exerts in promoting or retarding recovery. For the solution of this problem, a few cases, however well selected, are perfectly worthless, and can only have any value if they can be made to form part of some very extended statistical inquiry. If, for example, we attempt to solve the question whether salines are best in fevers, bleeding or calomel and opium in inflammations, &c., it is manifest that the progress of the case is so indistinctly connected with the remedies used, that we cannot assert that they stand to each other in the relation of cause and effect. In former days, many doctrines were taught dogmatically of the curative powers of certain remedies in particular diseases, which are questioned now; and the reason of this state of doubt in the medical world is simply that, while there has been no attempt to prove their causation by inductive reasoning, the number of instances by which their powers were supposed to have been indicated, have been too few to satisfy the requirements of the method of averages. Neither has the inference of their beneficial action been a legitimate application of deductive argument, as it has rested on no known truth of physiology, pathology, or therapeutics, but has been a mere blind empiricism, with no better foundation than an *à priori* conclusion of what ought to be, not of what actually is.

Undoubtedly, much of our treatment is based on deductive reasoning, and our confidence in its correctness depends on our conviction of the truth of the laws by which we are guided, and from which the deduction is drawn. In this way we use such remedies as purgatives, opiates, &c., as knowing the law of their action, and applying that agency to the relief of a certain symptom. Empirical practice, on the other hand, appeals to no laws, is guided by no analogies, rests on no principles, but simply asserts that experience teaches the benefit of the plan proposed. On no better grounds rests the ordinary saline treatment of fevers; and I believe that the new treatment by quinine, as well as nearly all the remedies used in rheumatic fever are equally empirical. Blood-letting in inflammation, and calomel and opium, though each in their turn supposed to be explained by some law of causation, have in my apprehension no claim to anything beyond mere empiricism, and so have lost in the present day of sceptical inquiry, the prominent position they once held. Let us then next examine the means we possess of testing the powers of remedies in such cases by the method of averages as contrasted with correct induction.

There are two principal facts to which such averages may be applied, for the purpose of ascertaining how far a remedy influences the progress of disease; viz., the proportion of deaths, and the duration of sickness in cases of recovery; to which may, in some instances, be added the character and severity of the sequelæ. The principle involved in such a statistical inquiry may be stated briefly as a process of elimination of chances, or variable causes; the object of the average being to neutralize the effect of every variable cause which cannot be excluded from the calculation, so as to leave as the residual phenomenon, only the effect produced by the remedy administered. By this it is meant that such a number of cases of a given disease should be brought together, as may be necessary to give every variety of example under one particular form of treatment, and an equal number under any other form, and then a comparison of the mortality and the duration should be made under the two systems. Very few persons have, I suspect, the very least idea what the number of instances required may be. It depends to a certain extent, indeed, upon the power of the remedy: if that be great in proportion to the influence of the other variable causes, it will soon make itself felt; if the average of its influence be small, it will be more slowly perceived.

To make myself understood, I must shortly refer to what may be called the variable causes influencing the result of an attack of disease.

First, before the attack: the sex, age, and social position of the individual; his previous state of health, including early constitution, acquired habit, and the effect of the relative amount and purity of food and air; his actual condition, whether suffering from any minor ailment, (to say nothing of major complications, which may be excluded), from actual privation or cold, or from any recent excess. Secondly, as regards the seizure itself: its immediate cause, its intensity, the rapidity of its development and progress, and the extent to which the special organ attacked is affected by it. Thirdly, the circumstances external to the patient influencing the progress of the disorder: such as his home, the means at his command, the friends that surround him, ignorant or well-informed, his nurse and his food, including stimulants, as well as other nourishment; the skill of his medical attendant, and the judgment with which other subsidiary remedies are employed; if necessary, the influence which the conditions calling for their employment, exercise over the disease, no less than the remedies themselves; and, perhaps more than anything else, the discretion with which the amount of stimulants is strictly limited to the exigencies of the particular case. Lastly: the wonderful and inexplicable influence of mind over body, the condition of hope or fear, of quiet confidence or restless anxiety. This list is far within the limits of all possible circumstances affecting the result, because it is intended to be general, and to include those only which are undoubtedly of sufficient power to lead to a fatal result, or a lingering convalescence: I need not, therefore, go into a detailed examination of them individually. The list is a long one, and each circumstance mentioned presents several varieties; so that if it be required to neutralize their influence completely, the number of cases selected must be such as shall fairly represent all possible conditions in these respects, and afford a true comparison between the two series. For whatever the number needed, it must be borne in mind that it is essentially a comparison, and that a series of hundreds of cases which seemed to do well under a particular mode of treatment is valueless, because perchance a similar series in which the remedy was not administered might have done better.

It may be interesting to consider for a few moments the algebraical formula by which the number of cases may be estimated which are required to give a fair average. The result, I confess, is somewhat startling. Let us merely assume that out of every variable circumstance which can unfavourably influence the result, one or more may be absent or present without regarding their intensity. In fact, let us assume that the series of cases shall include the very worst, in which all the causes are present and acting unfavourably; the most hopeful, where nothing stands in the way of the patient's recovery; as well as every intermediate degree in which one, two, three, or more of these unfavourable circumstances are absent, the remainder acting conjointly until one only is left. Speaking mathematically, such a series would come under the class of "combinations without repetition." Let us first select, for example, a very small number, say four. The variations obtained when only one of the four circumstances is present on each occasion, the rest being absent, is evidently 4; a, b, c, d: when two are present together, we have ab, ac, ad, bc, bd, $cd=6$: when three are present together, and one only absent each time, we get abc, abd, acd, $bcd=4$: when all are present together, we can have only one, $abcd$; the whole variations together being 15. Or, let us take five circumstances, which may be present or absent singly or conjointly.

Here, *a*, *b*, *c*, *d*, *e*=5.
ab, *ac*, *ad*, *ae*, *bc*, *bd*, *be*, *cd*, *ce*, *de*=10.
abc, *abd*, *abe*, *acd*, *ace*, *ade*, *bcd*, *bce*, *bde*, *cde* =10.
abcd, *abce*, *abde*, *acde*, *bcde*=5.
abcde=1. Total, 31.

The formula given by algebraists for this calculation is very simple. It consists of a series of fractions, of which the highest number of the series multiplied in succession by each lower number forms the numerators; and the numeral 1, multiplied by each higher number in succession, forms the denominators.

Thus, the first number is . . . . $\frac{5}{1} = 5$

The second . . . $\frac{5\times4}{1\times2} = \frac{20}{2} = 10$

The third . . $\frac{5\times4\times3}{1\times2\times3} = \frac{60}{6} = 10$

The fourth . $\frac{5\times4\times3\times2}{1\times2\times3\times4} = \frac{120}{24} = 5$

The fifth . $\frac{5\times4\times3\times2\times1}{1\times2\times3\times4\times5} = 1$

Total 31

The totals as ascertained by experiment for these two cases are 15 and 31, which may be represented as 16—1 and $32-1$, or the fourth and fifth powers of 2 minus 1; 2^4-1 and 2^5-1. Calculation has shown that this is true of all numbers whatsoever; that is to say, that the combinations without repetition of any number of varying circumstances is equal to the corresponding power of 2 minus 1. Consequently, if there be 10 such circumstances which may each be present or absent, the number of cases which will not be exactly alike is over 1,000; if there be 15 such, the number will be over 32,000, and each additional circumstance will double the previous number. It seems to me that this gives an explanation of what must have been ever present to the minds of most of us in the whole course of our practice, that no two cases of disease are exactly alike. In the short enumeration of variable circumstances I have given, with reference to all forms of disease, the number greatly exceeds 15; and consequently the number of cases observed before we may expect to meet two similar instances, must be quite beyond the bounds of any one man's experience, however extensive.

It is quite true that some of these may be excluded by classification; others may be eliminated by proving that, with reference to the whole series, they have no effect, or the effect is so constant that an allowance can be made for it. For instance, it may happen that sex does not influence the mortality in some given diseases, and this reduces by one-half the number of cases required to give an average. If, on the other hand, we try to eliminate the effect of age, by arranging the cases in groups of years, although practically useful in many instances, it does not at all diminish the number required to form the average, except in so far as there may chance to be a large proportion of cases belonging to one group of years, when the rest, of course, ought to be excluded. So also it may happen that all the cases, or nearly all, occurred in hospital, when in like manner the remainder should be excluded from the calculation, as introducing new variable elements. But whatever may be done to reduce their number, and render the series a manageable one, it must always be to a certain extent doubtful, whether the numbers taken, just include, or just exclude the very circumstance which, of all others, most conduces to a fatal termination, or to a speedy convalescence.

Differences of time and place offer, perhaps, the greatest obstacles to instituting a just comparison between any two modes of practice. It is quite unfair to contrast the mortality of thirty years ago with that of to-day, and assert that the difference must be due to improved methods of diagnosis and treatment, because the disease may have been a much more necessarily fatal one then than now: just as the mortality differs in any two epidemics of fever. Similarly, we cannot compare the relative mortality of fever in Paris, Edinburgh, and London, and assert that in that city in which it is lowest, the knowledge of treatment is most advanced; because, not only may the intensity of the fever be different in each locality, but the homes and habits of the people may in each case either predispose them to an unfavourable result, or give them unusual powers of resistance. And yet it happens that the tone and general custom of practice in one country is very different from that in another, and at the same time it more or less regulates the whole of the treatment of cases occurring in each; so that, while on the one hand the difference of treatment is that circumstance the effect of which we are desirous of studying, and it is nowhere so distinctly marked as in distant localities, we must remember on the other, that by selecting our series of cases from places so different, we introduce a fresh varying circumstance, which may be really the influential one. In this way the classification of cases very often quite vitiates the result, although it is really often of absolute necessity in making our calculations.

In the class of cases to which this mode of inquiry is applicable, we place those only in which we cannot trace any such relation of cause and effect as clearly to establish an empirical law of the action of the remedy; that is to say, that patients do not all recover who are brought under its influence, or do not all begin to recover so soon as its constitutional effects are displayed. What we seek to determine is, whether it has a more marked influence on the ultimate result than any other circumstance. For example, the use of one particular drug, such as an alkali in acute rheumatism, may have more power to prevent the occurrence of heart disease in that disorder, than the amount of food or stimulants taken, or the frequency with which purgatives are given, and yet the system may be thoroughly impregnated with alkali, without its appearing to modify in the very least the other symptoms of the disease. It may indeed be of such power, with reference to heart disease, that it may exceed the combined influence of age, intensity of attack, and all the other varying circumstances already referred to. If this be so, it is clear that the statistics required to establish the fact need not be nearly so extensive as when the influence which we seek to estimate is not more marked than that of a great many others. Still, to give anything like certainty to the observation, the statistics ought to be full, comprehensive, obtained from different localities, and made by different observers. Perhaps there is no better test of the sufficiency of such data than that proposed by Dr. Guy.* He suggests, as a test of the sufficiency of the data, that the enumerated cases should be divided into four equal parts—honestly—not distributing good and bad cases in equal shares, but taking them, for instance, in alphabetical order. If the averages obtained from each fourth part agree with one another, and with those derived from the whole together, the probability is very strong that they are correct, and that the number has been sufficient to distribute pretty equally the remaining influences; whereas if they differ, they must be viewed with great suspicion. A division into two equal parts is less trustworthy than into four, but may be resorted to when the numbers are small.

I shall endeavour to show presently that the statistical method is not by any means the best on which medical practice can be made to rest; but setting aside the impracticability of its employment, when the influence of the remedy is almost overborne by that of other circumstances, and the calculation of its power is almost impossible,—even in those more ordinary cases in which the number of examples need not be so great, it has not been hitherto employed with any approach to correctness. This is a circumstance to be deplored, as

* "Journal of Statistical Society," anno 1839, vol. ii., p 33.

it would add so much to our confidence in treatment, if the results of various methods of dealing with the ordinary diseases met with in hospitals, were fairly tabulated.

I have been led into the consideration of this subject by an attempt which has been recently made, on the part of the British Medical Association, to obtain from among its members, records of the results of treatment in certain specified diseases; and I propose now to occupy a short time in endeavouring to ascertain how far these therapeutical inquiries are suited for the application of the numerical method. The first that was issued* had reference to acute pneumonia; and in the form of return on which the particulars are to be stated, several of the variable circumstances already mentioned are placed in successive columns, so that it becomes possible to exclude them in calculating the results; for wherever one is found to have any very marked influence, the cases may be arranged in groups, in accordance with its presence or absence. Thus, for example; while it is very probable that sex has no relation to the mortality from pneumonia, age is likely to exercise a very marked influence on the progress of the disorder, independently of all remedies, and still more to modify the effect of certain modes of treatment, as I think is admitted to have been the case with reference to blood-letting. If, on an analysis of the returns, this should be found to be so, the cases must be divided into cycles of years: next comes the previous condition of health, followed by the period when treatment was begun; these are also very likely to affect the result,

* "British Medical Journal," 25th Oct., 1862.

and a further subdivision of each cycle must be made. Then again, the extent of the disease must be admitted by every one who has had to treat such cases, to be a circumstance of immense importance, and the number of cases in each subdivision must be again still further reduced. Complicated cases, of course, must be excluded; and, lastly, the cases remaining under each of these minute subdivisions, must be arranged according to the varieties of treatment. This will probably be found in scarcely any two cases exactly to correspond; because, even supposing that some prominent remedy is employed in each case, and the list is restricted to calomel, antimony, and salines, the dose will vary in some, the combination in others—one man gives opium more freely, another always gives purgatives—one relies very much on early stimulation, another on early depletion and starvation. If all these circumstances be carefully attended to, the numbers will, of necessity, be reduced to such very small dimensions in each series, that they can afford no ground whatever for calculating averages or estimating the relative value of treatment. It is just possible that one remedy might turn out to be attended with unusual fatality, or another followed by remarkable success; but if this be so, it is a result which the general capacity for observation among educated medical men of the present day renders highly improbable. All the remedies proposed have been tried over and over again, and have been proved to have no distinct power over inflammation of the lungs: each may be the means of producing a certain amount of beneficial action in particular cases, and will, according to the judgment of the practitioner, be employed when he thinks necessary. Indeed, the feeling of responsibility in treating such cases is so strong as to render it highly probable that, if it should be found on analysis of the cases, that any one drug had been given more constantly than another, in those which were unsuccessful, the real explanation consisted rather in the circumstance that those cases which seemed to call for its administration were of such a character as would probably terminate fatally, than that the remedy worked any ill to the patient.

The apparent intention of the statistics proposed was to ascertain if cases in which some special plan of treatment was constantly adopted, did, on the whole, rather better or rather worse than the remainder. But considering the number and variety of the circumstances influencing the progress of pneumonia, such a result could scarcely be obtained from a series counted by tens of thousands. It does not appear to me that trustworthy information of any kind is likely to be obtained from the returns on this subject. The reporter could not prescribe rules which were to be absolutely followed, in all cases, according to the mode of treatment selected; because in bad cases, in other words, in those which chiefly determine the relative mortality, he could not call on his brethren to make such an experiment, and abstain from employing the means which they might think necessary to save life, merely for the purpose of carrying it out fairly. Acute pneumonia is just one of those diseases in which a certain number of individuals attacked will die, in spite of any treatment yet known, while a certain number will recover, if entirely left to themselves. Between these extremes lie a number of cases, some of which will recover, if properly treated, who would otherwise have died; and some will recover perfectly who would have been left with damaged lungs, if no treatment had been employed. But these facts cannot, as far as I can judge, be made the subjects of a statistical inquiry; it will still be a matter on which the medical attendant must exercise his judgment, with special reference to the case before him. I shall have occasion to point out hereafter that, in treatment of diseases of this class, we have less regard to the name by which the disease is known, than to the condition of the patient suffering under it: our treatment is directed to the living individual, with all his peculiar tendencies and infirmities, and with all his vital functions proceeding, who happens for the time to have a special form of abnormal action going on, it may be in his lungs or in some other organ.

I have said that it is not at all probable that Dr. Bennett's statistics will bring out any startling results, because the various methods in the treatment of pneumonia have been fairly tried, and one has not seemed to claim our confidence much more decidedly than another. But it has often happened in the history of medicine, that a mode of treatment has been spoken of as extremely successful which has afterwards fallen into disrepute, as inefficacious, or been condemned as prejudicial. In the former case, the collection of statistics would soon prove that there was little or no difference between cases in which it was and cases in which it was not administered; in the latter, we ought not to need to wait for any large number of instances being brought together.

However variable and however complicated the other circumstances might be, yet if this relation be assumed, of treatment actually prejudicial to the recovery of the patient, it will be found that the general average of even a small number of cases bears unmistakeable evidence of the operation of such a cause. The result is necessarily more distinctly influenced by it than by other unfavourable circumstances; because every effort is made to remove pernicious influences as soon as recognised; but mistaken treatment is persevered in till the theory is proved to be untenable. A very remarkable example of this is seen in Bouillaud's treatment of rheumatic fever, by the copious and frequently repeated abstraction of blood. For though the practice of blood-letting was nearly as universal in those days as it is now uncommon, yet it required no more than the evidence of his own cases to show that his application of it was most pernicious. The average mortality among them was evidently much higher, and the average duration of the illness much longer, than under any other treatment whatever. For this reason, if for no other, it is most desirable that good hospital statistics should be constantly published, to form a basis of comparison, by which new plans of treatment might be tested, and progress in scientific knowledge and methods of cure might be marked.

The Reporter on Acute Pneumonia is well known to have expressed very decided opinions on the treatment of this disease, and to have brought forward the statistics of his own cases as conclusively proving the truth of the doctrines he has advanced. He even goes so far as to say that it is reasonable to conclude that the difference in the mortality between his own cases observed in Edinburgh, and those of M. Louis in Paris, "was owing to the treatment, and that such is a legitimate application of statistics." We may well ask what the numbers were which gave such remarkable evidence; they are 65 on one side of the Channel, and 75 on the other! In reply to this argument, it is only necessary to cite the experience of other observers who have had a very much smaller mortality than Dr. Bennett; for surely he is in all fairness bound to admit, that a man who only loses one patient in 60, or another who has actually only one death among 90 recorded cases, must have discovered a mode of treatment better than his own. Statements to this effect are made by a writer who has collected the largest number of statistics which I have met with on the subject;* and it is very remarkable, that though the cases just referred to are included among those in which venesection was not practised, yet the statistics fail in showing the advantage of abstaining from bleeding in pneumonia. All that can be shown from them, in reality, amounts to this, that excessive bleedings, under any circumstances, and even moderate bleedings, practised on elderly persons, are decidedly injurious; but this was not the issue raised by Dr. Bennett. They are collected from very various sources, and their importance in the author's own estimation is not great; for although he draws various conclusions from a strict analysis of all that admit of it, he does not even sum up the figures which he gives as a whole. The result, however, is

* "Brit. and Foreign Med.-Chir. Review," vol. xxii., July 1858.

this:—Of 1750 patients treated by repeated or large bleedings, the mortality was 18·5 per cent. Of about 1000 treated by few and small bleedings, it was 13·5 per cent. Taking both these together, the cases in which blood-letting formed one part of the treatment gave a death-rate of 164 in the thousand; while 10,000 cases treated almost entirely without venesection, gave a death-rate of 203 in the thousand. This does not include the Army statistics given in another page, where 16,000 cases gave a death-rate of only 39 per thousand during twenty years, when moderate bleeding was the rule of practice. Dr. Bennett's death-rate, as compared with this, would be 43. He very properly, as it appears to me, takes exception to the "jumbling together the different experiences and cases of different practitioners," as only leading to erroneous results; and the indication which I have given of the enormous number of cases required to eliminate fallacies, in estimating the value of one influence out of so many, is sufficient to show that counting will not serve our purpose. It was not without some surprise that I have found Dr. Bennett's name connected with this Therapeutic Inquiry.

In one of his earlier papers, a smaller number of cases is appealed to as leading to an induction which, stated in his own words, is, "that the treatment of inflammation to be successful, must be in harmony with the laws which govern the formation, development, and disintegration of cells." The number of cases is perfectly sufficient for the purpose of framing an induction, no doubt, if any relation of cause and effect could be clearly traced, and if any law even of an empirical kind could have been laid down. I confess that, in the so-called induction just recited, I can trace no law. That there are laws governing the formation, development, and disintegration of cells, I fully admit, though I conceive we are scarcely at all acquainted with them as yet; and the truism, that treatment to be successful must be in harmony with them, does not teach me whether more or less blood, richer or poorer blood, will cause the formation of more or fewer cells, will promote or retard their development, will hasten or postpone their disintegration; and therefore teaches me nothing about the abstraction of blood. In his later papers, this idea of induction seems to be abandoned, and the cases are treated as an example of the legitimate application of statistics. To this there is the manifest objection that the numbers must be far too few to be relied on as giving any indication of the practical value of omitting one mode of treatment, or introducing another. The difficulty of eliminating the influence of various causes is extremely great, when the small number of cases occurring in one man's practice forms the basis of calculation: in addition to this, the locality and atmospheric conditions of the town in which his cases occur, the hygiene of the hospital where most of them are treated, the diet and amount of stimulants which he prescribes, and the minor influences of remedies necessarily employed as adjuvants to the chief plan of treatment, render it impossible to draw any inference from the ratio of mortality, as to the power of any given remedial means.

It is not my purpose to offer any opinion on the plans of treatment which I may have occasion to discuss; my object is simply to show where the arguments employed fail in complying with the rules of the inductive method of reasoning. Dr. Bennett may be perfectly right in his plan of treatment; but he has not in my opinion succeeded either in establishing an induction, or proving by statistics that it is better to abstain from blood-letting altogether in pneumonia, than to employ it in moderation. So far as I am acquainted with the facts of the case, I believe that Chomel's practice may be classed with Bouillaud's, as an instance where even limited statistics are sufficient to prove the pernicious character of the treatment.

I will pass on to the third Therapeutical Inquiry issued by the British Medical Association, on the subject of the treatment of Tape-worm by the oil of Male Fern.* This question stands on a wholly different ground from that which has just been discussed. Here we have to do with an empirical law which has been already established to the satisfaction of most medical men. No inductive argument is, in my opinion, more conclusive in the matter of therapeutics than this, viz., that when a tape-worm is present in the alimentary canal, certain remedies, of which oil of male fern is one, affect the parasite so as to cause it to let go its hold, and this is followed in due time by its expulsion, and the cessation of the symptoms dependent on its presence. Half a dozen instances in which the effect distinctly and rapidly follows the cause, are just as good as a thousand for the purposes of an induction, which serves to establish a definite law of this kind. Neither does the occurrence of exceptions much affect

* "British Medical Journal," Nov. 22, 1862.

the truthfulness of the induction, because there are so many circumstances which we can readily appreciate as interfering with the operation of such a law. Statistics will not, in my opinion, throw any light on the law of causation involved in this action: no possible collection of cases can prove a negative, or overturn a law, if properly established. It is of very little use to know, that it fails once in 5, or 10, or 20 times, because we already know that it does fail, and therefore that the very next case may be one of failure. The proportion of successful and unsuccessful cases is all that the method of averages can teach.

Statistics might, however, have been made to tell us not merely how often, on the whole, male fern fails, but whether it fails more or less frequently than Kousso, for example. They might have helped to throw some light, too, on the causes of failure. To this, Dr. Fleming has cursorily called attention, his main object being to establish the fact that the oil has anthelmintic properties with reference to this parasite. Perhaps it may be possible, by reviewing a large number of cases, to reach some higher and more universal law of causation which may be applied to the treatment of other animals which infest the human body. But the question to which, as it appears to me, the Inquiry ought to have been mainly directed, was to determine which anthelmintic was most constantly successful, and why any one of them ever fails, if it be, as is supposed, a poison to the worm. Every observant practitioner is convinced of the efficacy of certain drugs, as anthelmintics, in the case of tapeworm, but at the same time there can be no doubt that they all occasionally fail.

Let us now return to the second Therapeutical Inquiry, the treatment of non-syphilitic Psoriasis.* It holds a place somewhat intermediate between the other two: for while it does not present the same degree of vagueness as that which has reference to the treatment of acute pneumonia, it has less of certainty than the treatment of tape-worm, and less pretension to be ranked as an induction. The subject is also a fair one for experimental investigation, because, first, the disease itself never causes the death of the patient; and secondly, it tends to go on for an indefinite period if no means be employed to check it. I have ventured to call the action of male fern by which the parasite is expelled, an empirical law. The relation of cause and effect is too plainly marked to escape observation in all save the exceptional cases where its action is somehow interfered with; and it is hardly possible to deny the existence of a similar relation in the use of certain remedies for lepra and psoriasis. Arsenic, for example, modifies the squamous eruption in these disorders. This is recognised as a law of a lower order, not only from the promptness and certainty of the action being less marked, but also from the observation of the action of arsenic under other circumstances, which seems to suggest that there may be some other explanation than that it is directly curative in the squamous disorders. I mean that the hypothesis would rather take the form of asserting that arsenic acted as a stimulant to the skin in a physiological sense, than that pathologically it was curative in non-syphilitic psoriasis. Still, I think, it must for the present be placed in the class of empirical laws of limited significance.

* "British Medical Journal," Nov. 8, 1862.

In this view of the case, it is a very fair subject for investigation, whether in cases entirely free from all syphilitic taint, the combination of mercury, as in Donovan's solution, does materially aid the action of the arsenic; and if a certain number of persons had agreed to give Fowler's and Donovan's solution to each alternate patient that presented himself affected with the disorder, following no preconceived idea of their efficacy in particular cases, but merely noting the symptoms in each, a satisfactory answer might probably have been obtained, provided the examples were sufficiently numerous. But, in addition to these two, six other remedies are recommended for trial, and among them there is not one that has ever been proved to have any direct action, such as the arsenic exhibits. They have, indeed, been chiefly employed as adjuvants to the arsenical treatment, but for them no empirical law has as yet been established. They can only be regarded as circumstances which may possibly have some remote influence over the progress of the case: to prove their efficacy at all, would require a very large collection of instances for the purpose of excluding the various sources of fallacy. Perhaps, when the Returns come in, the whole of these last taken together, may serve as a basis of comparison to indicate the actual value of the treatment by arsenic. Beyond that, I cannot conceive that they can be worth the trouble of collecting and comparing.

With reference to the fourth Therapeutical Inquiry—the treatment of Scarlatina,*—it is only necessary to observe that it stands on very much the same ground

* "British Medical Journal," Nov. 29, 1862.

as the first, except that it is made only in regard to a small number of remedies, and therefore the series need not be quite so long in order to obtain trustworthy results. Unlike the two last subjects which have been considered, there is no remedy for scarlatina of which the direct influence is in the least degree proved; unlike them too, the mortality is often great, and the fatal issue may turn upon some single circumstance which can only be seen by the practitioner at the moment. He often acts upon a sort of intuitive perception, without being able to state its importance in so many words; and no experimental system of treatment can be fully carried out as in the chronic maladies which never cause death. Such influences, like those of good nursing and the proper use of stimulants, cannot be appreciated by statistical returns, and must be eliminated by the method of averages, each requiring the series to be very greatly extended to obtain correct results. One great source of fallacy is introduced into this inquiry which has been avoided in the others, viz., the subdivision of the disease into different species. Who can say where one division terminates and the next begins, when it is really the same disease in all? And yet, perhaps, it was unavoidable, because while that form to which the name of "simplex" has been assigned is of comparatively minor importance, and is attended by a low mortality, that form which has received the name of "maligna" is exceedingly fatal and destructive. On the whole, I think it not improbable that the report upon the treatment of scarlatina may produce results from the method of averages of much greater value with reference to treatment than any of the others, provided only that the number of cases be anything like sufficient for the purpose.

I have gone, perhaps, more fully into the question of statistical investigations and the method of averages, than its direct bearing on my principal subject seemed to justify. The excuse must be that the fundamental error into which medical writers are prone to fall, is the idea that induction is almost synonymous with the enumeration of a large number of instances, and that any reasoning from such a collection of cases is properly called inductive reasoning. I have endeavoured to show that, from a very large number of cases, trustworthy averages may be obtained, and that from these, under proper restrictions, correct inferences may occasionally be drawn; but that this does not give such collections of cases the character of inductions. A legitimate induction does not demand any lengthy series to prove its truthfulness; but while it assumes a certain familiarity on the part of the observer with the facts bearing on the subject, its distinctive character, as has been already explained, consists in the discovery of some law which will stand the test of experimental inquiry, and is found true for every case which comes under its operation. If the law fail in any case where the special circumstances to which it relates are present, we ought to be able to show that some higher law interfered with its operation, or else we must abandon the law, or at least hold it in suspense. This is never the case with an average; for, supposing we have ascertained that the basis of calculation is sufficiently large, and that the results are perfectly correct, we can still only say that, in another series of a similar kind, a like result will be obtained. It does not apply to individual instances. It is of the very nature of averages that certain circumstances are acknowledged to vary in most individuals in the series, and consequently that the majority of them are exceptions to the average result, and what is true of the whole is not absolutely true of any individual except by accident.

This point seems to me to be one of the greatest importance in the consideration of what is a correct induction.

Let us then endeavour, before we proceed further, to ascertain what is meant by the term "law." In few words, it is an expression of the mode in which any given cause operates to produce a certain effect; and it is manifest that the accuracy of the law, and the definiteness of its character, are in proportion to our acquaintance with the operation of the cause. The cause may be a very complex one, and the influence of each part very difficult to estimate, or the effect may be very difficult of analysis, and in either case the law of its action will be very obscure. It is not at all of the essence of a law to explain the production of any phenomenon, although it may greatly contribute towards such a result. The laws of gravitation are constantly cited as a remarkable instance of the discovery of the cause of some of the most important phenomena connected with physical science; but in truth we know nothing of the cause of gravitation, although there is no subject of which the laws are better understood. By the discovery of these laws, a great step was made towards an explanation of the causes which govern the motions of the heavenly bodies, when they were thus seen to be under the control of a force producing on the surface of the earth that condition of matter which conveys to our feelings the sense of weight. In this we have an instance of the most elementary law with which we are conversant, and yet one which offers not the very least explanation of the cause which produces the effect. We simply say, the law of gravitation is a law of nature; nothing in the form of matter is exempt from its operation.

It might be said that if a law be only a formula for expressing the manner in which any cause operates, and induction be characterised as the process by which a law is discovered, it would be better at once to say that induction consists in discovering, from a collection of instances having a certain effect in common, the cause or causes by which that effect is produced. This is a perfectly true statement of what induction is when the cause can be discovered. On closer examination, however, we find that all that we know of a cause is that a certain effect necessarily follows or co-exists with it; and this is the law of its action; and further, that before we have established the law of the sequence or co-existence, we have no right to assert that there is any relation of cause and effect, however invariably the two phenomena may be found together. Day invariably follows night, but is not caused by it: day is caused by the sun-rise. There can be no such law as that out of darkness light should come; but there is a universal law that from every luminous body rays of light proceed. It is usually so easy to assume that the *post hoc* is also the *propter hoc*, that were it even possible to arrive at a knowledge of causes in place of mere laws of causation, the necessity for the enunciation of a law is such a valuable check, that it could not be dispensed with in determining what is a true and what a false induction.

Laws differ very much in the degree of exactness with which each specifies the mode of action whereby the effect is produced. This is in great measure dependent on the complex character of most occurrences, as well as of their producing causes. In medical science, we hardly find any such thing as a single effect produced by a simple cause. Some can, no doubt, be analysed, and it may be possible to show that the compound effect was due in certain proportions, to each one of the causes which combined for its production; but as a general rule this is impossible, and we are obliged to regard some at least of the elements of causation as composite, and be content with laws applicable to such knowledge. This gives rise to what are called empirical laws—laws which are to be regarded as mere temporary substitutes for higher knowledge, but which in the meantime express, so far as we know it, the relation of cause and effect in this complex form. Laws of the highest class are found to govern in the most absolute manner the action of all forms of matter under any circumstances whatsoever, while those of the lowest class are only observable on special occasions, apply to a few individuals, and are liable to constant interruption from the interference of some law relating to a more universal and more potent cause. The higher law is also simple and clear, and may be stated in very precise terms; the lower one is much more vague and indefinite in its expression. The one has apparently reached the greatest possible precision in expressing the mode of causation; the other is still comparatively obscure: it is of partial extent, and will, no doubt, ultimately be resolved into some more general law, with which we are yet unacquainted. Such is the difference between what are called laws of nature and empirical laws.

I need not further discuss this question here, but merely add that the two principal marks of an empirical law are, that it either relates to a complex cause, where it is impossible to point out the influence of each particular circumstance in producing the effect; or that there is evidently some link wanting in our knowledge of the relation of cause and effect which unites the two phenomena.* It seems scarcely possible therefore to conceive that in the study of medicine we shall ever arrive at anything more definite than empirical laws; and it must be remembered that to prove an empirical law is not necessarily to assert a law of causation. The two things may be invariably associated together, simply in consequence of some connection between their several causes, while they have no power mutually to produce each other. The imperfect character of the lowest forms of empirical laws not only limits their application to certain individuals under special circumstances, but occasionally produces exceptions, which we are wholly unable to explain, seeing we do not know precisely how the cause operates. For example: the laws of gravitation have been shown to be co-extensive with the existence of matter, so far as our means of observation go, whether in the direction

* See Mill's "System of Logic," Book III., chap. xvi.

of immeasurable distance and masses of enormous magnitude, or in the opposite extreme of almost inconceivable minuteness of size and closeness of contact. When we say that a balloon rises from the earth contrary to gravity, we do not mean that this is an exception to the law, but only that it is an instance in which its action is not at once perceived, as it is conjoined with other laws of expansion and movement among gases. These so modify the result that the power of gravitation itself raises the car and its passengers apparently in opposition to its own laws; the same cause which chains them to the earth, under ordinary circumstances, is still that which makes the inflated balloon lift them up from it. We may very well apply to such a case the proverb that the exception proves the rule. When we examine an empirical law, such as that cinchona bark cures ague, how limited is its application;—to the human species only of all creation, and but to those among them suffering from ague. It is also subject to the more general laws of the development of certain principles in the growth of plants, as well as to those of absorption in the individual to whom it is administered. Besides all this, cinchona bark is not the only substance which cures ague; and cases occasionally occur in which it entirely fails, when we are unable to explain the cause of its failure.

No doubt since its first promulgation the law of gravity has seemed to meet with exceptions quite as definite as those met with in the treatment of ague, but they have been all completely resolved: the law was so definite and simple, that observers knew where to look for the explanation of the exception.

Before its promulgation many of the facts were grouped together under certain assumed laws of weight, which asserted that every solid body had a tendency to fall towards the centre of the earth ; even these, though purely empirical laws, were very much simpler and more definite than the law of treatment by cinchona. In each of these instances there is a relation of causation, and therefore they are all instances of true induction, although one only reaches to the highest point at which induction aims, and the others present varying degrees of imperfection. The certainty with which the law was established in each case did not depend upon the number of instances which formed the basis for the induction, so much as on the precision with which the law could be applied to any subsequent case, by way of experiment. All minds are so naturally conversant with a consciousness of weight, that but few examples needed to be brought together to establish empirical laws of the tendency towards the centre of the earth ; and laws so clear and simple as those of gravitation needed little beyond experimental observations to establish their universality. With cinchona bark the case was very different. Brought to this country first as the Jesuits' bark, prejudice was at once enlisted against it ; and although in many cases there seemed a clear relation of cause and effect, yet, on the other hand, there were many failures ; so that it required a much larger accumulation of experience to form the groundwork of induction than was needed in the other example. We can scarcely imagine a physician unbiassed by prejudice prescribing bark in half a dozen successful cases of ague, without coming to the conclusion that there was some direct relation of cause and effect between the administration and the cure. Given, a patient labouring under any disease which has rebelled against all other means of treatment, and no change whatever being made in any of the variable circumstances surrounding him except in the remedies used, is it not exceedingly probable that, if recovery commences at once and goes on rapidly, the treatment is the active agent in the recovery? When this occurs not once or twice, but nearly as often as it is tried—and not in the hands only of one, but of many observers—we may unquestionably assert that a law of causation has been proved to exist. But what shall we say of the failures?

When the bark was given in substance, the failures were many, because the absorption of the active ingredient was interfered with. In more recent times, when the salts can be separated from the woody fibre, and their form and mode of administration can be varied, one cause of failure is removed, and the experiment is now a much more certain one. At an earlier period consequently a correct induction could not be so confidently framed, and any logical inquiry would take more or less the statistical form. To such an observer as Sydenham it did so present itself, and he acknowledged the Jesuits' bark to be the most powerful of the circumstances conducing to the recovery of the patient. Soon, however, the multitude of observations led to the suggestion of an hypothesis that bark did more than this, that it checked the paroxysms of ague, and prevented their recurrence. With improved chemical skill this has now been proved to be an empirical law, the remaining exceptions being so few that they may be disregarded; but till it becomes somewhat more definite, and till all the exceptions can be explained, the law must be regarded as a very imperfect one.

We have already found that a similar empirical law has been established in the treatment of tape-worm by male fern. Its employment for this purpose is as old as the time of Galen; but in the earlier period of its use the failures must have been so numerous, when the powder of the root itself was used, that it could only be regarded as one circumstance among many, tending in a remarkable manner to the recovery of the patient. Now that chemistry has presented us with the oil extracted by ether, the failures have become so few that it does not require any large number of instances to establish the law, that this drug causes the tape-worm to be dislodged. The correct hypothesis, too, having been suggested, that it acts as a poison to the parasite, it required only the verification of experiment by different observers to be admitted into the list of true inductions indicating laws of causation, although there remain a few occasional failures still to be accounted for.

It may be said of failures generally that, when fully explained, they always confirm the law to which they seemed to form an exception. If a full explanation cannot be given, yet if they may be reasonably resolved into a modification caused by some other law, they then leave the law quite unaffected by their recurrence. If they seem wholly inexplicable, and cannot be referred to accident, they tend to throw doubt over the truth of the induction, in direct proportion to the frequency of their repetition.

The existence of perfect uniformity, and the absence of exception, although giving great force to a law once established, cannot alone be made the basis of an induction, without the addition of some theory which brings the uniformity into the relation of cause and effect, and asserts the law of its action. For, however extended our present knowledge is, the very next observation may record an exception of such a character as to show that there could be no law of causation in the matter. Such uniformities observed before the hypothesis is suggested, at once take their place in the induction when framed, and serve, to a certain extent, in lieu of experiments, to prove the truth of the law. For example, it had been very long observed that all mammals had seven cervical vertebræ—the same number make up the lengthy neck of the giraffe, and the almost rudimentary neck of the mole; but it could not be called a law of causation that all mammals have exactly that number, as at any moment a specimen might have been found in which the number was more or fewer. When, however, the law of the homologies of the skeleton was propounded, this fact fell at once into its place as an additional evidence of its truth. On the other hand, if before the discovery of Australia a zoologist, in comparing the offspring of mammals with those of reptiles, had propounded as a law of nature what had hitherto been found invariable,—viz., that all mammalian offspring remained in the uterus of the mother until they were fully developed,—his apparent induction would have been disproved by the subsequent discovery of the marsupials.

An induction is only complete when it establishes a law of causation. In some instances this is proved by the simple observation of the fact, that the one circumstance is invariably attended by the other, in such a way that it is impossible to produce the one without the presence of the other. If the examination of the subject can go no further, and no explanation be offered,—that is to say, if no higher law be indicated by the relation, and no causes of wider range be involved in it,—we must be content with an empirical law, which merely asserts the relation. But it very often happens, that we trace in one of these subordinate laws the influence of some higher agency; or that some part of the effect is due to causes with which we are already familiar. This experience not only makes the law much more definite, but greatly strengthens our confidence in its truth, and reduces very much the chances of error, and the need for numerous observations.

When Jenner first investigated the subject of cow-pox, the idea was presented to his mind, by hearing it spoken of as a familiar and well-known fact among the dairy servants in the neighbourhood where he was residing, that persons whose hands were infected by milking cows suffering from the vaccine disease never took the small-pox. Very shortly before that time, the practice of inoculation had been introduced as a preventive measure, in consequence of the observation, that persons who once had suffered from small-pox were not liable to a second attack. Both questions had been fully discussed, and were found to be only part of a more general law, that some diseases had the power of conferring upon the persons who had been once subjected to them a certain degree of immunity from subsequent attacks of the same disorder. When, therefore, the hypothesis suggested itself to his mind that some other analogous disease might have the power of conferring the same sort of immunity, a comparatively small number of well-authenticated cases was sufficient to form the basis of an induction, that vaccination was a preventive of small-pox. That the cases were simple evidences of causation was very much confirmed by the law coinciding with that which had been already established, with regard to inoculation. When, therefore, he proceeded to make the experiments of first inserting the vaccine virus, and producing the disorder known as cow-pox, and after a short interval inoculating the same individual with small-pox matter; and when he had observed that in such circumstances no effect was produced by the inoculation, his induction at once established a law by which succeeding generations have been so greatly benefited. Occasional failures cannot overturn the force of his argument, though they may prove that in some individuals the protective influence is speedily exhausted, or that in others it only serves to modify the disease when it occurs.

The inoculation of the syphilitic virus, which has been introduced of late years, on the other hand, has none of the elements of a true induction. The hypothesis, that such a proceeding might act as a preventive measure, was not based on any previous enumeration of cases; the general impression being that patients might suffer over and over again from this disease. The hypothesis, that a fresh infection might be made the means of eradicating an old-standing taint, is opposed to all experience, either in that or in any other constitutional affection; and required no long series of experiments to prove its utter groundlessness. But it was alleged that, by renewing the infection again and again at shorter and shorter intervals, the susceptibility of the constitution might at length wear itself out, and no fresh infection would make any impression. Numerous and long continued experiments have been made to test this hypothesis, without any satisfactory result, which has probably arisen partly from imperfect observation, as the various forms of the disorder have not been properly discriminated. Some varieties of the disease are rarely followed by a second attack, and as a general rule patients who have once had secondary symptoms are not liable to a similar attack from a new infection, though the latent poison may at any time be roused into fresh activity. Indeed, we may well ask, *Cui bono?* if all were proved which was urged in favour of the prevention of the susceptibility to this disease. Is it that a man may afterwards go and expose himself voluntarily to the risk of infection without danger? Surely, this is not one of the highest aims of medical science. But it is with the logical, and not the moral aspect of the experiment, that we are now concerned; and I maintain, that when there is no law of causation involved in the hypothesis, when the general facts of diseased action and the special laws of this affection are alike opposed to the suggestion on which the experiments have been based, the failures are sufficient to refute it altogether.

The two cases just cited have special reference to the second part of the inductive process, viz., the suggestion of an hypothesis which may form the basis of a generalization or law of causation. In the one, the induction may be said to have rested on the popular belief in the district, of which, no doubt, numerous instances were present to the mind of Jenner when he framed his hypothesis, and made the experiments which proved its correctness, and established it as a law; in the other, the hypothesis was started without any sufficient number of instances to warrant its suggestion, and the experiments which followed only served to show that it was untenable. In this respect it can scarcely be said to be a fallacy of induction, because one element of the process was left out. At the same time, no doubt the author of the theory believed that he was resting on other previously ascertained truths, and that he was, in fact, completing what had been left unfinished. This is the form of error which, in the application of the deductive process, is the most common in the present day. We are not apt to set out from a fresh hypothesis which has been arrived at wholly *à priori;* from some imaginary relation of primary causes, which so obscured the science of medicine, in common with all other sciences, in a bygone age: but we are very prone to assume that a principle which has been already established with reference to one set of cases may be applied to another, and to employ a deduction which was perfectly true in a restricted sense, in some more general manner, when it becomes absolutely false. There seems to me to be nothing in the history of syphilisation, as it has been called, beyond an utterly baseless application of the laws of small-pox and cow-pox inoculation to a disease in which the main fact of non-recurrence was from the first unascertained.

It may not be out of place here to allude to another false induction, if I may so call it, which had especial relation to the prevention of disease. Dr. Christison, in his "Dispensatory," states, under the article Belladonna, that it had been recommended as a prophylactic for scarlatina, and that, though generally doubted in this country, he thought it yet merited a trial. The idea came from the fertile brain of Hahnemann, who cleverly introduced any fragment of legitimate argument to support his own extravagant hypothesis. His mode of reasoning was shortly this—Belladonna produces sore throat and red eruption on the skin, and therefore is the appropriate remedy for scarlatina. Quinine, which is the true agent for the cure of ague, and lemon-juice, which equally eradicates scurvy, are each of them also prophylactic; hence belladonna is an agent which has the power of preventing an attack of scarlatina. If the premises be granted, the conclusion is highly probable, and required only the evidence of facts to substantiate it; such facts his followers were supposed to supply. It happened, however, that some who disbelieved the hypothesis were convinced by the facts, and stated their convictions in such distinct terms that even Dr. Christison* felt himself bound to recommend a trial of the agent, though coming from such a questionable source.

Various observations were made, and statistics collected on the subject, both in Germany and England.

* Christison's "Dispensatory," p. 216. Edin. 1848.

Some of the reporters, in detailing their observations, even stated their conviction that belladonna was, in its preservative powers, nearly on a par with vaccination, and differed only in the circumstance that the one was temporary, while the other was permanent in its action. One fallacy, however, pervades all the earlier observations on the subject. During the prevalence of the epidemic, belladonna was given to all or nearly all the children who had not previously had scarlatina; and consequently there is no possibility of instituting a comparison between those who took, and those who did not take, the alleged preservative. The comparison is only instituted hypothetically between what would or might have been the result if no belladonna had been given, and the result actually obtained. It was assumed, that the disease would have spread, after the experiment began, at the same rate that it had done before; or a certain general estimate was taken of the number of cases likely to occur when the epidemic appeared; and if the number fell short of the probability thus calculated, it was assumed to be due to the preservative influence of belladonna. In this there is a most palpable fallacy. It is quite impossible, under any circumstances, to know what number of children will catch scarlet fever in a given locality, any more than it is possible for the statists of the General Register Office, to say what will be the mortality of London or any of its districts next week. More than this, the fact of the epidemic having already prevailed some little time, was in itself a sufficient reason for its cessation after the experiment was begun.

The only fair experiment on the subject with which I am acquainted is that performed by Dr. Graham Balfour, at the Royal Military Asylum, at Chelsea, and recorded by Dr. West.* Among the boys who had not had scarlatina a comparison is made, which seems to possess all the required elements of exactness, between seventy-six to whom belladonna was given and seventy-five to whom it was not administered, but who in all other respects were placed in circumstances which were exactly alike in the two series. The result was, that two in each section subsequently took the disease. From this observation two important lessons are drawn: first, that the cessation of the epidemic was not due to the employment of the prophylactic, as was assumed in former cases when it was given to all alike; second, that the disease attacked equally those who had and those who had not taken it. The numbers are necessarily too small to establish the conclusion irreversibly, as it is still possible that these examples were of the nature of exceptions; they are, however, by no means the first that have been recorded. While the preventive power of belladonna could be regarded as at all presenting the character of a fair induction, it was perfectly natural and right that causes explaining these exceptions should be sought for; and the circumstance that the explanations offered were wholly unsatisfactory, forms now a very strong argument in favour of the belief, that in Dr. Balfour's experiment the only legitimate conclusion was, that the remedy was wholly powerless to produce the effect alleged.

A large proportion of the fallacies in medicine, arising

* West's "Diseases of Infancy," 3rd Edition, p. 600.

out of a supposed application of the inductive method of reasoning, are of a similar kind. They consist in errors of observation and experiment. It would lead me too far were I to enter into the logical examination of the processes by which a correct result may be arrived at and errors avoided. They are represented by Mill under the form of canons,* to which I must refer those who wish to study the subject more fully. I may just explain here, that the purpose of any such rules is to determine what evidence is sufficient to establish the relation of cause and effect. It has been already stated explicitly, that a true induction is one which establishes a law of causation, and, therefore, any canons by which it may be tried must have reference to this relation. To persons who have not studied the subject, nothing at first sight is more simple. No effect can exist without a sufficient cause; no cause can come into operation without its effect following, if it be not interfered with by some other circumstance. What, then, can be more simple than to trace the causes of phenomena? To such persons, the observation that a certain effect has been produced in a given number of instances when a certain supposed cause has been in operation, assumes the character of a legitimate induction; and the conclusion is regarded as perfectly unassailable, if some instances can be collected in which the antecedent being absent, the consequent also failed. But considering the very complex nature of causation in medical science, it must be evident that such proof is of very little value. Some other cause might have

* Mill's "System of Logic," Book III., chap. viii.

been present in the whole number of cases in which the effect followed, and might have been absent, or might have been overruled by some more powerful one in all the cases of failure; so that the supposed cause may, in reality, have had no part in producing the effect. What is needed for the proof of such a proposition is, that the instances collected, which agree in presenting examples of the co-existence of the one antecedent and consequent, shall agree in nothing else whatsoever; and similarly, that those in which they are respectively present and absent shall be identical in every respect, save that of the presence or absence of the particular phenomenon under investigation. Manifestly, this is quite unattainable in medical science; but an approach may be made to it in the collection of a number of cases which shall present every possible variety: so that, on the one hand, all the causes which might singly or together produce the effect, shall be absent in one or other of the cases which agree in the co-existence of the phenomena; while, among those in which the effect is absent, some one case or other will exhibit the presence of every possible cause, except that to which the true law of causation alone applies.

Let it be understood that we are not now considering the numerical method, but the laws of causation. It may or may not be true, that any given remedy contributes more or less to the recovery of a patient from a certain disease,—by stimulating some secretion, for example, and so placing him in a better position for the natural process of cure, or by any other indirect mode of action. In such a case, the remedy only stands on the same ground as any other of the circumstances which contribute to recovery, and no induction can be proved regarding it. If, for example, we endeavour to ascertain whether mercury be a remedy for pneumonia, we find first, that cases in which it has been given do not recover, and that cases do recover in which it has not been employed; and, consequently, that there are other influencing circumstances present, which of necessity "disguise the effect of the mercury, and almost preclude us from knowing whether it has any effect or not;" so that the utmost its advocates can hope for, is the knowledge acquired from hospital statistics, "that there are rather more recoveries, and rather fewer failures, when mercury is administered, than when it is not; a result," in the language of Mill, "of very secondary value even as a guide to practice, and almost worthless as a contribution to the theory of the subject."* To prove that a certain remedy contributes directly to a given result, it is not necessary that the instances collected be very numerous, provided they supply examples sufficient to prove the comparative inefficacy of every other cause which cannot be excluded from the enumeration, and such unfailing success when the remedy is employed, that no doubt of its inherent power remains.

When we speak of such a complex subject as the recovery of a patient, it must be evident that proof of this kind is in general a matter of great difficulty. The number of influencing circumstances, as I have already explained, is so great, that even if the remedy do possess the supposed power, it is very difficult to estimate it; while, if it do not, a number of successful

* Mill's "System of Logic," Book III., chap. x.

cases following each other, while the remedy is being tried, almost unavoidably leads to the impression, that the recoveries are due to its administration. Neither can we at present penetrate far into the mystery of morbid action, so as to simplify the problem, by limiting ourselves to the inquiry, what influence is possessed by such and such remedies over each separate part of that action. In some few cases, we are able to pass over all the intermediate links, and assert that the medicine does cure the disease. In a few more instances, we can trace a relation of causation between the modification of some one of the morbid processes, and the treatment adopted; and in a still larger number we can say positively of any given drug, that in sufficient quantity it will produce a certain definite action on the body; but we can only theoretically combine this action with the process of cure, and are unable to assert positively that it will be beneficial.

I must again refer a little more in detail to Bouillaud's observations on the effects of blood-letting in rheumatic fever, although the conclusions he draws are so palpably false that they can impose upon no one who takes the trouble to investigate them. Without doubt, they had in their day a certain influence on medical practice (pernicious, as I believe), though they exhibit the most complete ignorance of all correct principles of reasoning. The author claims for himself the merit of trying to arrive at the greatest exactitude; venturing even to designate some of his conclusions by the name of laws—a term which he especially applies to the relation which he seeks to establish between acute rheumatism and disease of the heart, but also in some degree implying that it is applicable to his new method, as he terms it, of bleeding "coup sur coup."* He also points to his own statistics, as a model to be followed by those who aspire to be the teachers of a future age. He was, no doubt, the first to draw special attention to the association of disease of the heart with acute rheumatism; but his conclusions regarding that association have proved to be very far from correct. His statistics † give a total of 64 clear and unmistakeable cases of cardiac inflammation, as he terms them, out of 74 cases of acute rheumatism. In the present day, the statistics on this subject give a perfectly different result. The coincidence of cardiac inflammation, even including cases of previous disease of the heart, does not probably exceed 50 per cent.‡ Two errors serve wholly to vitiate his conclusion: first, all cardiac murmurs occurring during an attack of acute rheumatism are set down as evidence of disease of the heart; secondly, no allowance is made for the effect of treatment, which must have greatly increased the number of murmurs heard when they led to such statistical results as those given by the author. Errors such as these serve as illustrations of the necessity for a large number of examples obtained from various localities, and observed by different individuals, when the question does not

* Bouillaud, "Traité Clinique du Rhumatisme Articulaire."

† Ibid., p. 143.

‡ Fuller on "Rheumatism," p. 274; Barclay, "Cases of Disease of the Heart," "Med.-Chir. Trans.," vol. xxxv., p. 18. Dr. Dickinson's "Statistics" are much more favourable, "Med.-Chir. Trans.," vol. xlv, p. 274. See also "British Med. Journal," Aug. 29th, 1863; "Statistics of the Treatment of Rheumatic Fever," by T. K. Chambers, M.D.

assume the character of a true induction. While M. Bouillaud confined his inquiry to the existence of cardiac inflammation as a result of acute rheumatism, he established an undeniable principle in medicine. When he passed beyond this question, and endeavoured by limited statistics to indicate the frequency of the complication, he obtained a ratio which the experience even of his own contemporaries showed to be much over-estimated.

The error into which he fell in giving the results of his new method of treatment was somewhat different. He tells us in his preface, that it is necessary, in order to estimate the results of statistics, to classify well the facts recorded. No proposition can be more undeniable. But in order that these results may be trustworthy, the principles of classification must themselves be true. Some general average is required to afford a basis of comparison with the averages derived from each of the several classes. All the cases having one feature in common must be compared with all those in which it is absent. The same process must be again repeated with other prominent circumstances, and again the one set of averages contrasted with the others. Nothing of this kind seems to have been present to the mind of M. Bouillaud. In the early part of the volume,* mention is made of several fatal cases, from which proofs are drawn of the pathological states accompanying rheumatic fever. Several of these evidently occurred under his own charge, and were treated by the new method; but when the statistics of treatment are given in the latter part of the volume, not one fatal case is included

* "Traité Clinique du Rhumatisme Articulaire," p. 147, et seq.

in the series. On what principle the selection is made does not appear, but the examination of the cases is entered upon in a most philosophical spirit, and the classification of the selected cases is unobjectionable. That one of the results arrived at in the investigation is the entire absence of a fatal termination is, under the circumstances, not surprising; but it is surely one of the most extraordinary instances of self-deception that could be found in the history of medicine, that an observer should claim for himself extreme carefulness in tabulating his cases and forming his conclusions, and should honestly record a number of fatal cases under his own treatment, and yet should delude himself into the belief that he was giving a correct estimate of its effects, when he states* that no death occurred among the worst cases. This statement, which, by implication, contains so false an estimate of the value of treatment, is all the more remarkable as he previously cites the death-rate observed by other writers on rheumatic fever.

But Bouillaud was only an exaggeration of the prevailing views of his own time, and it would lead us further than my present limits will permit to enter fully into the question of the grounds on which those opinions rested. I will only say that they were never proved by inductive reasoning, and presented none of the characters of empirical laws; and that they never were even made the subject of any proper statistical inquiry, proving the influence for good or harm which venesection possessed, as one of the many circumstances which combine to produce the result. In modern practice we have no chance of arriving at any knowledge

* "Traité Clinique du Rhumatisme Articulaire," p. 370.

of its influence by statistics, because the prejudice against bleeding is now so strong and so universal, that in any case of real danger a physician cannot employ it without running the risk of being charged with the death of his patient, if the case should afterwards terminate fatally. Only a quarter of a century ago the prejudice ran so entirely in the opposite direction, that medical men were themselves led away by the universal impression, that in serious disease of any kind, the first step in treatment consisted in the use of the lancet. We can hardly then compare the practice of a bygone age with that of the present day, for the purpose of estimating the value of blood-letting, except in so far as the comparison proves one extreme to have been more hurtful than the other. We can only express our astonishment at the unbounded faith with which it was once regarded by men of the highest eminence, as indispensable in the treatment of inflammatory diseases, when we now see that the same affections can be successfully treated without it. Had any inductive argument remained to us which proved its efficacy in those days, we must have adopted the suggestion, that such a change had taken place, in the type of disease as rendered the practice inapplicable to inflammations in the present day. In the absence of any such proof, we may pretty safely conclude from the evidence we possess, that some among our predecessors, especially those of the French school, did actual harm by the reckless use of the lancet; and that in our own practice of the present day, there is some degree of prejudice, and some submission to fashion, in abstaining so entirely as we do from the abstraction of blood.

I need not, however, examine a question which has been so ably discussed* on a recent occasion, when we were taught that there is no change of type in disease, though cholera was unknown in Europe forty years ago, and though the plague, once so constant a visitant in this city, has, for the present, entirely ceased to spread beyond the regions of the Levant, where it still seems to have its constant habitat.

The history of medicine presents so many fallacies of observation, that I need only cite a few instances from the most recent medical literature. They may be divided into two classes; of which one has reference to remedies proposed specially for the cure of certain diseases, while the other contains those which have been supposed to have a more general action, and to be applicable by inference or deduction to various disorders, in which that action may be reasonably anticipated to be beneficial.

Not very long ago, a very favourable report was brought from Novia Scotia, of the curative power of sarracenia in small-pox. A very small number of cases in which recovery followed its administration, were considered sufficient to justify a letter to *The Times*, from a surgeon, asserting its efficacy, and even recommending that every household should be provided with sarracenia during the prevalence of small-pox in London, to be taken at once, when the symptoms of the disease were seen. To arrive at such a conclusion regarding the influence of this plant, required the clearest and most conclusive induction, inasmuch as it sought to establish a direct relation of cause and effect. So far as can be gathered from the published

* "Blood-Letting in Disease," by Dr. Markham. Brit. Med. Journal, June 1864.

opinions of the writer in question, he was satisfied with the general report of its powers transmitted from America, without any analysis of the cases on which this conclusion rested, as the basis of his induction; while his experiment consisted simply in giving it to a few persons who ultimately recovered, without his attempting to compare these with analogous instances in which sarracenia was not given, or to show that the mortality was in any way influenced by its administration. From such premises no trustworthy conclusion could be drawn. Even if it had been afterwards proved that sarracenia was a specific for small-pox, the letter in *The Times* could not have advanced the argument in the remotest degree, except in so far as its wide publicity might lead to numerous trials.

The remedy was subsequently tried at the Small-pox Hospital;* and to determine the question of its specific influence as soon as possible, it was given only in cases which experience had shown were almost certainly fatal. It was in each case administered from the earliest possible period in their history; they all died, and not one of the fifteen on whom it was tried, gave the least indication that it influenced the progress of the disease. The Reporter deemed it unnecessary to try it in milder cases, as he could not trace its action in any of the functions during its administration. The experiment proved unquestionably, that the sarracenia did not possess such a specific power as had been alleged; and there seemed to be no reason to anticipate, that a remedy which was wholly inert in the treatment of the severer cases, would be found, by numerical

* "Lancet," July 4, 1863.

calculation, to modify the mortality or the duration of the less fatal forms of the disease. Indeed, it seems to be a most inexplicable matter how the first favourable impression of the pitcher-plant arose; and still more, how the writer in *The Times* could persuade himself that, by such an experiment as he detailed, he had fairly tested the remedy, or that such a trial of its virtue proved the correctness of the opinions previously entertained. I presume that most medical men who paid any attention to the letter in question, arrived at this conclusion before the experiments were made.

Many examples might be cited of the prevalence of the same sort of fallacy from a subject already spoken of, viz., the treatment of acute rheumatism. That we know no specific for the disease, is proved by the various recommendations of writers on this subject; each succeeding author believing, that he has detailed that mode of treatment, which most conduces to the recovery of the patient. Some of them are based on deductions from the manner in which certain remedies are likely to influence particular symptoms; but others have no argument in their favour beyond their alleged success. It has not been asserted in any case that the remedy was specific; they have only been regarded by their advocates as means conducing in a very prominent degree to the recovery of the patient. I need hardly say, that the number of instances given has never been sufficient to prove this assumption. In fact, though such a result can only be proved numerically, the argument has generally been treated as if it were a legitimate induction, and the number of cases given has been only such as would have been barely sufficient for proof if the law of their action had been plain and unmistakable. In some instances, indeed, an attempt at the numerical or statistical method has been made; and I am led to recur to this disease, in order to mention a very recent instance, in which, as it seems to me, the proof of the allegation made is extremely illogical. The author* attempts to institute a comparison between the results of various forms of internal medication and the external application of wool to the surface of the skin, by bedding the patients in blankets. The cases are given only in the statistical form, and my remarks are limited to the published report, as I have had no means of forming an opinion on the efficacy of the practice in particular cases. The analysis includes 243 cases of rheumatic fever, and the conclusions regarding the "blanketing of patients" profess to be based on a comparison of 180 with the remainder; but a very little examination of the tables given shows, that the comparison ought to have been limited to the eleven cases in which internal remedies were not administered, since it is nowhere shown that such means were either inert or injurious, in so far as they were employed. In fact, it is stated, that a very large proportion of the patients bedded in blankets were placed under the alkaline treatment; and, as in the series examined by Dr. Dickinson, the proportion of cardiac inflammations was still lower than in those under consideration, it is quite possible that the blanketing system may have actually raised the proportion of inflammations of the heart from that observed at St. George's Hospital, under the

* "British Medical Journal," August 1863.

full alkaline treatment, to that obtained at St. Mary's. I need say nothing of one manifest source of fallacy which requires no comment, viz., that patients who "wilfully threw off their blankets" are included in the class of cases bedded in sheets. How very easy is it to persuade oneself when one's pet theory fails, that it fails through the fault of some one else, and not because we have erected it on a foundation of sand! I presume that the author of this paper conceived that he was fulfilling the requirements of the numerical method in giving these statistics on acute rheumatism; for he nowhere professes to enunciate any law of causation. They offer a valuable corroboration of those previously published on the employment of alkalies; though even now the series is insufficient to establish the point, or to show the relative value of the remedy, until they can be compared with the statistics of other hospitals, where the expectant or the lemon-juice system of treatment is adopted.

My object in citing this example is to point out how inadequate the facts are to establish the assumption by the numerical method, and that in their present form they do not throw the least light on the question proposed, whether bedding in blankets, in place of bedding in sheets, modifies the course and progress of rheumatic fever, though it is quite possible that it may do so to a very marked degree. It has been already shown, that a numerical series must be of great length to eliminate the various sources of fallacy, and to neutralize the influence of circumstances, which it is impossible to exclude altogether. But to include in so short a series as 180 "nearly all" of 174 instances—in each of which one circumstance at all events was present, which has been already affirmed by competent observers to exert a very marked influence over the result—is surely sufficient to render the conclusions perfectly valueless.

Pulmonary consumption is another of those diseases which have, at all periods in the history of medicine, occupied the attention of speculative minds, seducing them into false theories and erroneous conclusions, and not unfrequently luring them on till they have fallen into the snare of downright quackery; deceiving others, themselves the victims of a complete delusion. The error must in this case consist in faulty observation, as the disease is one which, in a great majority of instances, proves fatal. If the treatment proposed have any virtue, the proportion of deaths must be strikingly reduced, or the duration of life manifestly prolonged. In fact, there are few diseases in which it would be easier to establish a law of causation with reference to the curative powers of a remedy, if any such relation really existed. I need not go into the particulars of the various propositions which have been made by men of all degrees of eminence on this fertile subject; but I would point out one remarkable fact, that whereas some have taken a permanent place in our estimation as remedial means in the treatment of consumption, others have been entirely consigned to oblivion. It would not be difficult to show, that the former chiefly consist of those remedies which have been recommended as modifying some particular symptom, while among the latter will be found all that have been introduced as specifics for the disease. As an example of the one class, we may take cod-liver oil, which especially counteracts the emaciating tendency of the tubercular diathesis, and now holds the first place as a remedy in the treatment of all those varieties, of which emaciation is a prominent symptom. On the other hand, the proposal to administer the hypophosphites has met with no response from any who have given them a fair trial, and may, perhaps, profitably occupy our attention for a few minutes as an example of faulty induction.

This plan of treatment formed the subject of a paper which was presented to the Academy of Medicine in Paris seven or eight years ago; and we may therefore fairly conclude that the author claimed for himself a place among the scientific men of our own day. Writing again on the same subject, two years later, he affirms his conviction that these salts "will prove as sure a remedy in consumption as quinine is in intermittent fever, and as effectual a preservative as vaccination in small-pox." During the few years that have followed this announcement, I find no record of any but failures in their employment. Already, therefore, we may assume that it has taken its place beside naphtha and other ineffectual specifics. So far as the author's own statements throw any light upon the fallacy, it would seem that it affects both parts of the inductive reasoning, the correct observation of facts, and the suggestion of an hypothesis by which their occurrence might be explained. When the subject first presented itself to his mind, it was as a deduction from the speculations of certain physiologists, who have alleged that there is an excess of oxygenation in pulmonary consumption, along with a deficiency of the phosphorus ingredients in the body. The theory based upon this speculation appealed to no known facts, offered no explanation of occasional recoveries from phthisis in its various stages, and did not harmonize, to all appearance, with the beneficial agency of fresh air and a warm climate. It therefore demanded very careful observation and experimentation to test its truthfulness; and, though the theory was suggested, as indeed very many of the laws of induction are, by deductive argument, it yet might have been interwoven into a perfect induction, had the observations been sufficiently numerous and correct. It would still have wanted some modification before it could have been taken even as an empirical law, because it takes no account of the large quantity of the phosphorus ingredients, which are hourly passing out of the body through the kidneys; and this seems to imply that if the loss of phosphorus is so important, the fault must lie in its elimination rather than in any deficiency of the supply. The great error, however, must have been in the observation of facts. The remedy is at one time spoken of as a specific, which, of course, means nothing less than that it cures the disease; while, at another, it is only said to have "produced even more benefit than could have been expected from it, if the degree of injury already sustained by the lungs, previous to the use of the treatment, be taken into account." It does not appear that any allowance is made for spontaneous recovery from a condition presenting symptoms which were only suspicious, and at the very most, indicating the earliest stage of the disease; nor would this have been necessary, had the specific power of the remedy been proved in severer cases. I need not say that as a contribution to the numerical method, the recital of 34 cases, which form the basis of his first report, or 150, as alluded to in his subsequent memoir, is utterly worthless for the purpose of establishing a claim to any degree of superiority, on the whole, for this particular mode of practice. That no empirical law has been established, I think is made clear by the fact, that the disease is not arrested by the use of the hypophosphites. It seems quite unnecessary to point out how utterly futile it must be under such circumstances to argue regarding their prophylactic powers. Had it been proved that their tendency was distinctly curative when the disease had already made some progress, there might have been ground for an inference, that they also possessed the power of preventing its development. But, as this argument in their favour has been found to fail, nothing remains but full and complete statistics to prove the assertion. The difficulties in the way of obtaining this evidence would be almost overwhelming. It is undoubtedly true, that hereditary tendency is one of the most powerful causes in giving a predisposition to pulmonary consumption; but there are others not less important in its development, and certain circumstances which must be considered as counteracting agents. The two series, as already explained, must contain instances, as nearly similar as possible on all these points, of sufficient number to eliminate and neutralize the effects of other agencies—one set having for a number of years employed the prophylactic, the other set not having done so. The number of deaths in each series, if the ratio admitted the application of Dr. Guy's method, would probably give a correct account of the merit of Dr. Churchill's recommendation; but no experience short of this could be regarded as satisfactory.

It has been already stated, that in the process of framing an induction, the theory is very often not prompted by observation of the facts accumulated; but either arises out of some previously ascertained law, assuming the form of a deduction or analogy; or springs up spontaneously in the mind of the observer, who is unable to give any account of the process by which it was arrived at. In almost all, however, I think it will be found, that if facts have not been previously collated and tabulated, they are more or less familiar to the mind; and a very competent general knowledge of the relation has generally preceded the suggestion of the hypothesis, which is intended to explain them. It is not at all impossible that a new theory and a true one may occur to a man who is not familiar with his subject; or it may be suggested by the observation of only one or two instances; but in the very complex form in which causation is presented in the science of medicine, a small number of facts are very unsafe as the basis of a theory. When the relation of cause and effect comes into very remarkable prominence, and other influences seem to conduce very slightly, if at all, to the same end, we may, perhaps, venture to disregard them; but it must be remembered that errors very frequently spring from this source, especially in advocating plans of treatment. The whole history of epidemic diseases presents a continuous series of similar mistakes, and has left us at the present time very little in advance of the knowledge possessed by the last generation. We, perhaps, know a little more of the general management of such cases; but we are no nearer to anything like a specific treatment in any one of them, notwithstanding all the suggestions which have been made. I might occupy much time were I to attempt an analysis of those which have reference to one of these diseases alone—the most formidable, the most deadly, and the least understood of them all, the epidemic cholera. I need hardly say that in many there was no theory propounded at all—no attempt at an induction of the very lowest kind; men were at their wits' end; and every possible remedy in all imaginable forms and doses was tried. It very commonly happened that when the disease was on the decline, cases which, at the commencement of the epidemic, would have been certainly fatal, began to recover; and if, among the various trials, some new remedy was hit upon just at this crisis in its history, the practitioner was too ready to assign to it a power which, in reality, it did not possess. Such was the common history of the plans of treatment proposed. The next outburst gave an opportunity for testing the power of the remedy, and it was invariably proved to have no specific action in the arrest of the disease. It is to be regretted that sufficient statistics have not been collected to enable us to say whether, on the whole, more lives were saved under one plan than another. The information was sought for by this college, as well as by the Board of Health, but the results obtained by careful investigation of all the facts can only be regarded as negative. The number of instances collected was far too small for the purpose of obtaining any averages. It was quite clear from those inquiries—as, indeed, must have been self-evident to any one who read the various proposals made in the journals of the day—that no induction had been established, indicating that any remedy possessed curative powers in the treatment of cholera.

One proposition, however, I must allude to a little more at length, as it was more distinctly associated with a theory, and to some extent, at least, assumed the character of inductive reasoning. I allude to the treatment by purgatives. One or two remarkable recoveries took place after the administration of castor oil, and it was consequently assumed that the action of the oil was directly curative by aiding in the elimination of the poison. Here, the first error committed was in the selection of so small a number of cases as the basis of the reasoning; then, the theory proposed was not in harmony with other laws of causation; and lastly, it was not brought to the test of strict experiment. It was not alleged that, in these cases, any circumstance pointed directly to the castor oil as the curative agent; the disease was not arrested immediately on its administration; death did sometimes occur; and many other means were employed besides the purgative. Such conditions demanded a large accumulation of facts to frame a legitimate induction; and, perhaps, could only have been proved, if at all susceptible of proof, by statistics.

The hypothesis, again, drawn from the analogy of mineral or vegetable poisons, asserted in general terms that the action of purgatives aided the elimination of the poison of cholera. It was not a mere empirical law that was suggested, such as that of the action of cinchona bark in ague, but one of much higher importance, viz., that there was some material thing present in the body of a cholera patient which had to pass out of his system, and that medicine might aid in its elimination. If such a law could be proved of cholera, it ought to be applicable to the whole series of epidemic diseases; and the only basis on which such a law could rest would be a series of inductions of a similar kind for each individual of the class. For instance, it should have been shown that to rub croton oil liniment on the skin was the best treatment for small-pox, in order to eliminate the poison by the natural channel through which it finds its exit from the body.* But, even if it were conceded, that it was not necessary that the law should apply to other epidemic diseases, and that the inductions which have grouped them together were all in error, it was still necessary that the law should rest upon known facts connected with the disease itself; that there was a material thing in the body to which the name poison could be applied; and that this material thing did pass out by the bowels; and that purgatives could aid its expulsion. Not one of these was proved. A mineral or vegetable poison is perfectly different from a condition of body produced by the introduction of a something which has never yet been traced—so excessively minute is it—derived more or less directly from another individual whose tissues have been similarly diseased. There is no poison, in the ordinary sense of the word, pre-

* This practice, I am informed, has actually been proposed and adopted by some persons since the above was written; but the suggestion is so manifestly irrational that the text has not been altered.

sent in cholera. Some part of the body is in a state of change; it may be one organ like the blood, or it may be many; and emanations from the body in which that change is going on, somehow or other communicate the same abnormal state to another person, whose tissues in course of time pass through the same changes.

No one would propose to eliminate the diseased tissue, or a tissue in a state of change; but beyond that, we know nothing of cholera poison. Indeed, the idea of increasing the discharges from the bowels is opposed to the fact, that the thickened condition of the blood following on the abstraction of serum is prejudicial to life: this I think has been proved by the wonderful efficacy of fluid injected into the veins in bringing back, temporarily, to life and consciousness, patients who were in the last stage of collapse.

The theory, however, was one which was very easily put to the test of experiment; and it seems to us now, in looking back, strange that the author should not have so tested it, before venturing to bring it publicly forward. Whenever it was tried by other competent observers, as was done at most of the London hospitals in the course of a few weeks, invariable failure attended the trial. It seems probable that the theory was based on a false analogy, which associated under the common term "poison" two conditions, which are really quite dissimilar; and that the author applied to the one, deductions drawn from facts connected with the other. On this assumption he rested mainly for the acceptance of his inferences; he does not seem to have attached much importance to the cases adduced, although he was aware that without such supposed evidence of its having been tested, he could not have gained a hearing.

It is perhaps premature to speak of a remedy introduced to the notice of the profession only about three years ago; but while pronouncing no opinion whatever on its merits, we may at least be permitted to criticise the arguments by which it was supported. The process of reasoning which led to its employment, as described by the author, is very analogous to that which was followed with regard to the hypophosphites; with this difference only, that the one was suggested as a specific for a particular disorder, the other as likely to produce certain effects on the blood, which, in the author's opinion, must lead to beneficial results in a variety of diseases. This primary difference materially affects the whole of the argument subsequently. There are a few instances suggested in which something of the character of an induction might have been framed, had the remedy responded to the anticipations. In diabetes, for example, the peroxide of hydrogen having the property of decomposing grape-sugar out of the body, might have promoted the change of this substance in the blood, and prevented its elimination by the kidney; but five cases only showed that it was valueless. It does not arrest the progress of cancer or of phthisis. But if it do not form the basis of an induction, may not 223 instances be taken as a contribution to the statistics of therapeutical observations? Let us examine the conclusions a little more particularly. It is stated that "in chronic and subacute rheumatism it is of very great value;" we turn to the statistics, and find "chronic rheumatism, one case," and "subacute rheumatism, two cases." Again, "In valvular disease of the heart, attended with pulmonary congestion, it largely relieves the attendant apnœa"—seven cases form the basis of this inference. Next comes struma, where "it removes glandular swellings, like iodine:" this assertion rests on two cases. I need not further go into details, for it must be evident that the examples given are not of such a character as to authorize statements made, which may be perfectly true in themselves, but do not certainly rest either on the inductive or the numerical method. That I may not give an unfair impression of the author, I must add that he cites forty-four cases of anæmia and one hundred of phthisis: but with very nearly negative results. He concludes that the peroxide aids the assimilation of iron, of which he offers no proof; and also that it soothes the dying bed of the consumptive patient, relieving his breathlessness like an opiate, without its narcotic effect. This, of course, must be mere matter of opinion, which can neither be proved or disproved by any known "method," but seems a comparatively small result from an agent which theoretically was one of such promise.

Another remedy, perhaps, hardly deserves mention, and yet it serves to illustrate this part of my subject. Podophyllin has been known but a short time in England, and no great amount of experience has been obtained of its value as a therapeutical agent. In America, however, it has been used nearly twenty years. Its properties are manifestly cathartic, and in large doses it acts as an irritant poison, producing vomiting and hypercatharsis. But it has been observed that the stools are dark coloured, and are supposed to contain a large proportion of bile; hence by most writers it is spoken of as a cholagogue. They are not all agreed in what way this effect is brought about, as it has been recently taught that it only unloads the gall-bladder: earlier experiments suggested the idea of increased secretion, and at once an analogy was supposed to exist between its action and that of mercury. There are, indeed, persons who question this supposed action of mercury in stimulating the function of the liver; but no sooner was the analogy conceived than an hypothesis was framed, that in its other actions on the economy the same analogy would also be found to exist. It was consequently put to the test of experiment, as was supposed; and the conclusion arrived at seems to be that, as in some instances recovery took place after its employment, the correspondence between the two agents was quite established, and that all the benefits of a mercurial course, without its attendant evils, might be attained by the long-continued use of podophyllin. Here, the original imperfect observation which led to the belief that the two agents possessed similar powers, with reference to the liver, suggested the assumption of the hypothesis; this, again, was neither proved nor disproved by the subsequent experiments, which were not of such a character as to be capable of testing its truth. Whatever be the virtues of podophyllin, it is not in consequence of such a parody on inductive reasoning that they will be accepted by the profession in England.

Let me cite one more example of a false theory associated with a generalization from insufficient data. The supporting plan of treatment, as it was called by its chief advocate, assumes that in all acute diseases the natural tendency of the process is to the restoration of health, and that the great aim of the physician must be to keep his patient alive until the period of recovery arrives; it further assumes, that for this purpose the chief instrument is alcohol, in some form or other. No one, I should think, is prepared to question that in a certain number of cases, of all except incurable diseases, recovery may take place without the administration of any remedy whatever. We might even go further; and admit that in a majority of instances this result might occur. But unless our whole past experience is worthless, this is not the case in all: there are very many occasions when the disease actually kills, and the life of the patient depends on its being arrested in its progress. Here, therefore, the first fallacy is introduced, in the assumption that what is true of a certain number of cases is true of all, and that what is true in the majority of one form of acute disorder is true universally of all acute diseases.

The second error has reference to the means of maintaining life; a mere hypothesis being asserted which is nowhere brought to the real test of experiment. Undoubtedly it is the business of the physician to sustain the life of the patient by all means in his power—if life fail, recovery is impossible: but the man who bleeds equally intends to preserve life with the man who stimulates. On this point the false reasoning is of a deductive kind. The experience of every practitioner must supply him with instances in which the administration of large quantities of wine and brandy to patients suffering under severe forms of typhus, has apparently rescued them from impending death. Dr. Todd hence argued, that a circumstance which seemed of such value in the maintenance of life in typhus, ought to be similarly efficacious when death was imminent in other acute diseases; and that the same means which were powerful to save, when life was fast ebbing away, would be still more efficacious if administered at an earlier period of the disorder, and in larger quantity than that usually adopted. No experiments are given to show whether the alcoholic fluid acted as a stimulus to the nervous system, or as a general sustainer of life: and the author does not allege that no one died to whom it was properly administered, because his own cases contradict this inference. The argument does not prove an induction; it is wholly *à priori,* and the number of cases collected is quite valueless, as any indication by the numerical method of the success of the practice. Indeed, so far as can be gathered from the perusal of his lectures, it would seem that, though the cases reported number ninety-three, they were not intended to be a contribution to statistics, as they are evidently selected for the illustration of particular points. We must, therefore, conclude that they are given with some idea of proving experimentally the truth of the hypothesis. A study of them seems, on the contrary, to show that they contain in themselves a complete refutation of it, if they be regarded as fair samples of Dr. Todd's practice; if they be not, they are most unfortunately selected. The eighteen cases of rheumatic fever reported give fifteen in which there was cardiac complication, and in some of these the stimulating treatment was fully carried out. In fever, again, eleven deaths occurred among the twenty-four cases recorded.*

This subject is one on which, as it seems to me, it is impossible to lay down positive rules; and medical men, as a body, however well educated, can scarcely escape from the influences of prejudice or fashion, in their adoption of the current doctrines of the day. That this one has been injurious I do not hesitate to affirm, though believing most firmly that in some forms of fever free stimulation is absolutely necessary to save life. The impression, however, rests on no better argument than that of experience: it is no induction, properly so called; for there is no manifest relation of cause and effect. Experience simply asserts that in a number of desperate cases wine has been given, and some, which seemed hopeless without its aid, have recovered after its administration; and therefore, probably, in consequence of it. As a deduction, it so entirely commends itself to reason that we dare not treat a patient in great danger without stimulants, and are, therefore, debarred from the possibility of getting proof of their importance, of such a kind as to establish an induction or a numerical ratio. The contrary has occasionally been asserted, and it would be a great addition to our knowledge if fair statistics could be obtained which might point out how far we are justified in dispensing with the use of stimulants in fever,—how much we may give, how much we must give. Even in regard to typhus, the teaching of the schools is at present very

* "Clinical Lectures," by R. B. Todd, M.D., "On Acute Diseases."

vague; and with reference to other acute diseases we know almost nothing, beyond the circumstance that, when they present what are called typhoid symptoms, we believe wine is needed just as in low typhoid itself.

The theory of stimulation in acute disease may be regarded not unjustly as a sort of reaction and protest against the previously existing one of blood-letting. Many of us are old enough to remember persons whose practice was perhaps regarded as somewhat antiquated, but who still bled and leeched in what was called congestive typhus; and few can take up a book of practical medicine without being struck with the apparent contradiction between the theory of blood-letting propounded by the author, and his own practice, as he so rarely meets with a case coming under that category which in his published work he has indicated as requiring the use of the lancet.

It is especially in the treatment of disease that fallacies in the application of the inductive method of reasoning have been traced. If, on the other hand, we turn to pathology, we find that the names of those whom we are in the habit of reverencing as the great teachers of our own or past days are all of them associated with the discovery of some guiding principle in which the laws of causation are more or less distinctly discernible. It is unnecessary here to refer to more than one or two instances in which correct inductions have led to most important advances in the science of medicine. I have already referred to the well-known discovery of Jenner, as one of the most perfect instances of correct reasoning which its annals record. About thirty years earlier, another very remarkable instance is found in the observations of Sir George Baker, on the effects of lead-poisoning. For nearly fifty years it had been known that the fraudulent adulteration of wine with lead had produced symptoms similar to those which were so common in Devonshire. Other observers had already pointed out that the endemic colic of that district was due to the use of cider as an article of diet; but it remained for him to propose the hypothesis which brought those two observations into harmony, and gave to his reasoning the character of a true induction.

Coming down to a period much nearer our own day, we have Laennec's discovery of the laws by which the sounds elicited by percussion and auscultation were associated with diseases of the chest. Taking up the laws, such as they are, of acoustics, it was the merit of this observer to apply them, with a certain approximation to truth, to those sounds which were audible within the chest walls. No doubt many of the inferences which he drew were not borne out by subsequent observations, and many additions have been since made to those which he left imperfect. It was of the nature of such observations that they could not attain to the simplicity of general laws, such as those of gravitation, of optics, or even of pure acoustics, which might be enunciated in a few simple propositions. Such laws leave little for others to discover beyond the working out of their legitimate conclusions, and elucidating the exceptions, by explaining the circumstances which interfere with their operation. So far as one man could be expected to trace out the newly-discovered laws of complex phenomena, it must be conceded that Laennec was highly successful. It was chiefly in the observation of heart disease, a subject which at the time when he wrote was but little known, that this great master failed. His failure is a good illustration of the need for correct observation of facts, as the basis of sound induction. The diseases of the heart had not then been studied to a sufficient extent for his mind to embrace the subject in all its details. Yet even here true principles were evolved; and to the distinguished President of the College and others who have worked in the same field, we owe our present advance in the knowledge of the diseases of that organ. They have rather corrected the deductions which were made from the principles laid down, and added fresh truths to those already acquired, than overturned anything which had been previously established.

It is one of the great achievements of inductive reasoning, that when a principle is once laid down, when a theory has been applied to the accumulation of facts, and they have been associated under some generalization, it immediately becomes of itself a basis for fresh inductions. A very good instance of this process may be found in the development of the laws of tubercular deposit by Louis, based on the facts proved by Laennec. This observer seems to me to have been himself blind to the true method of inductive reasoning, and has been content with expressing his views as the results of the numerical method. Any one who reads his treatise will at once discover what seems to have entirely escaped his notice, that some of his conclusions have a very different significance from others: that some of them are mere averages of the cases he happened to examine, while others point directly to some law of causation which we may not yet have been able to solve, but which is nevertheless one of general application. Take, for example, the one inference, that a local deposit of tubercle is formed at the apex, and not at the base of the lung; and the other, that in so many cases the right lung alone was affected, and in so many the left. Possibly the preponderance of one side over the other may some day be established as an universal principle, which may be explained by a more extensive generalization of the cause of the deposit, wheresoever it takes place; but at present it cannot be said to be even an empirical law. Indeed, I do not know that the same average exactly, has been obtained by any other observer, and it is utterly without value in our knowledge of the disease. Whereas the positive induction, that the apex is essentially the site of local deposit, is one to which every day bears its testimony, and at every examination of the chest forms one of the elements of the deductive argument by which we arrive at a true diagnosis of disease.

Were it necessary to give further instances of the value of such empirical laws, the whole range of semeiology would supply them on almost any subject which we chose to select. This it is which gives diagnosis its peculiar charm. Taking for its groundwork all the facts which have been clearly established, all the theories which have been wrought out into empirical laws, it groups together the particulars of each case; and proceeds to inquire how these several laws serve to explain the phenomena present. Having ascertained which of them are applicable to the case under consideration, deductive reasoning proceeds to show that the results of their combination would be the very existence of such a series of facts as it presents. One of the most beautiful illustrations of this subject is found in the relation of varying conditions of the urine to disease of the kidney, so associated with the name of the late Dr. Bright. To him belongs the merit of first tracing out the connection between albuminous urine and degeneration of the kidney. But even now, carefully as the subject has been studied by others since his day, the association does not go beyond an empirical law. We do not know why it is that in their diseased condition albumen mingles with the secretion, but we know that it does so always; we do not quite know by what process degeneration is brought about, but we can say, with positive certainty, in a definite number of cases, that the disease has begun, and that its progress, alas! can scarcely be retarded. So far the relation of cause and effect can be stated in the terms of an empirical law; but it is not possible, as yet, to bring under any more general law those exceptional cases in which temporary and transitory albuminuria exists, or to associate the degeneration of the kidney with degenerations of other tissues of the body. If this great observer erred in linking together the phenomena of two different forms of disease, as different stages of the same condition, this circumstance does not detract from the merit of that most important generalization which first brought them both into relation with the phenomenon of albuminous urine.

As I have already cited one example of a false generalization connected with cholera, I cannot here refrain from referring to the labours of the late Dr. Baly as an example of a definite, though limited, induction on the subject of its propagation. The problem that was presented to him for solution was to analyse the facts accumulated by various observers, and to develope from them a correct statement of the mode of its transmission. In making this review, it does not appear that any theory presented itself to his own mind as that which would sufficiently account for the phenomena observed; and as I previously remarked, all the most important and valuable laws of causation seem to have been discovered by the assumption of an hypothesis which has presented itself to the observer's own mind, as if by happy accident, in considering the subject of the induction he proposed to frame. Dr. Baly states at the outset the various theories already started, most of them based on a very limited enumeration of particulars, and applies them experimentally to the facts which he arranges together for this purpose. The first part of the inductive argument is thus taken from the observation of others, defective as it was at that point of time, and he proceeds to try, by way of experiment, how the argument adapts itself to special instances. He finds that most of them fail on some particular occasion, and ends by stating that the only legitimate inference is limited to the statement which, so far as it goes, may be taken as an empirical law: viz., that the disease is propagated by human intercourse, and that certain unknown causes aid or limit its transmission to particular persons. In short, the main result of his labour was this—to establish that the disease in question was one of the epidemic and transmissible species, and was not produced by atmospheric or telluric influences.

A perusal of his monograph brings to one's mind very forcibly the description given by Dr. Whewell of Kepler's inquiry, which terminated in the discovery of the law that the orbit of the planets was in the form of an ellipse. The chief difference between them appears to consist in the fact, that the various hypotheses tried by the astronomer were not only those suggested by others, but also included some which occurred to his own mind; and perhaps, for this reason, he came nearer to framing a true induction. The successful one was not more directly suggested by the facts observed than the others. He set himself to guess the true form of the orbit, and he tried many possible forms before that of the ellipse. That it happened to be the right one did not therefore remove it from the list of guesses, all of which were previously wrong. Their failure must have had an influence on his mind; and perhaps, if more were known of the actual train of reasoning, and he had been less honest or less particular in relating his failures, more credit would now be given to him as an inductive philosopher.

One curious fact with reference to the labours of Baly ought to be adverted to. The hypothesis of the actual transmission by drinking water had already been asserted on the evidence of a small number of cases collected chiefly by the late Dr. Snow, to whom the assumption is due; but the facts adduced in its support did not seem to warrant its adoption; and cases presented themselves, which, if they did not wholly negative the proposition, at least proved it to be one of very limited application. More recently, however, the hypothesis has been submitted anew to the test of experiment. The numerical method, or method of averages, was used for the purpose of determining the effect of foul drinking water as one of the causes of the spread of cholera. Here let it be remarked, that the question regarded from this point of view is at once taken out of the category of inductive reasoning. Dr. Snow had assumed as his theory, that the fæcal discharges from persons suffering under the disease contained that form of animal matter in a state of change which could reproduce the disease in another person, and that this so-called poison was conveyed in the drinking water, as well as probably through other channels. Had this been established on a sufficient number of well-authenticated cases, and been proved by subsequent experiments, a most important induction would have been framed. Dr. Baly's arguments seem to have thrown very great doubt upon the validity of the conclusion, and to have left it simply as a statement of a general fact, that foul drinking water is one of the causes which contributes especially to the mortality from this disease. The statistical inquiry takes it up at this point, and proceeds to investigate whether, when a population is taken in which the other circumstances which could modify the effect were as nearly as possible analogous, this one alone had any marked influence. The answer, so far as it goes, is most distinct, and appears to be free from any source of fallacy. A number of streets were selected, in which houses standing side by side were supplied, as far as could be known, indiscriminately, with water from two distinct sources; a comparatively pure, and a very impure water, being distributed to different houses in the same street. The local influences must, therefore, have been the same in all; and there could only remain the individual susceptibilities and habits of the persons who suffered, and those who escaped, and their exposure to causes apart from those affecting the whole neighbourhood in which they lived. These it was necessary to eliminate by collecting a sufficient number of instances to afford a fair average. The varying circumstance which it was proposed to investigate being the drinking water, the houses in each street were placed in two groups, according as they were supplied from each source. These groups were collected into districts, and the districts summed up into two grand totals, giving a population of 161,000 and 156,000 persons respectively, and the cholera deaths were found to be 37 per 10,000 in the one total, 127 per 10,000 in the other. Now, to apply Dr. Guy's test, let us see whether a similar result is obtained by subdividing the totals. The districts gave, as the relative mortality in the two classes, per 10,000 persons, 73 to 209, 41 to 170, 39 to 122, 30 to 99, and 10 to 131. These numbers do not give quite the same ratio as the grand totals, but each one indicates an enormous preponderance in the one class over the other, and we may simply conclude, that the evidence is complete that foul drinking water is one of the causes of death by cholera. It will be observed that deaths do occur among persons using the comparatively pure water, in considerable numbers; and hence we can only conclude, that its relative impurity is one of the causes, not *the* cause, of the disease. The knowledge of this average influence may be of use some day in aiding to solve the problems which would determine its law of causation; but it is very clear, from the results themselves, that the statistical inquiry has not produced any distinct generalization or induction.

I have dwelt upon these facts in some detail, because they afford a very good illustration of the class of truths obtained from averages. Were a smaller number of instances to be taken, we might easily select a considerable number of streets in which, while in the houses supplied with impure water the deaths were very numerous, not a single death took place in those which had the purer supply. And, on the other hand, it might be shown that in half a dozen streets, containing over 2,000 inhabitants, the ratio of mortality with the comparatively pure water supply was 112, while that with impure water was only 32. False inferences such as these may always be made, when an insufficient number of cases is taken to obtain a fair average. Again, it is to be remarked, that the correctness of this average very much depends upon the circumstance that the houses were contiguous and similar in every respect, except that of the water supply; so that occupations and trades, wealth and poverty, ventilation, drainage, and atmospheric influences, ceased to become disturbing causes in obtaining the results. Were this not so, the influence of the water supply could not have been traced, because we find, from a table published by the Registrar-General for a period of 14 weeks,* that while a district supplied by the Southwark Company, who drew their

* "Seventeenth Annual Report," p. 92.

water from the Thames at Battersea, had a cholera death-rate of 102; another district supplied by the Chelsea Company, drawing their water from the very same part of the river, had a death-rate of only 18 per 10,000 inhabitants,—a number which was very considerably exceeded by those which received their water from the Thames at Barnes and at Thames Ditton. In these cases other variable circumstances came into operation, which very considerably modified the results; and if they had not been eliminated, the number of cases which it would have been necessary to bring together to elucidate the influence of the water supply would have been almost incalculable. Such results prove that the water supply can exert only a very limited influence, when the variations of other circumstances alter the result so materially while the water supply remains the same: and they confirm the previous conclusions of Baly, that there is no such direct relation of causation as had been previously asserted from partial observations.

There is, perhaps, no subject on which speculation has been more freely indulged, than in the causation of disease. Yet, if there be any truth in the inductive reasoning, this question especially falls under its domain, and no suggestion or hypothesis ought to be admitted which is not proved in the manner required by its rules. To establish the relation of cause and effect, and to indicate its laws, are the peculiar province of induction; and if the evidence adduced in favour of any hypothetical cause fail to answer to the tests which are imposed by its canons, we must be content to regard the causation as at least unproved, if not wholly false.

The subject of etiology is, indeed, in some sense inseparable from the science of medicine; it enters more or less into all our speculations, and very often modifies our treatment. But it must be admitted that at present it rests on a very insecure basis, since very few of our theories have been at all proved by inductive reasoning. To take only the example of the specific fevers. The very names by which the class is known are all more or less objectionable, because of the theories which they involve, and it is by no means easy to give them a place in classification which does not imply certain elements in causation. The present generation would perhaps be pretty unanimous in assuming that they are caused by some pernicious emanation generated in the bodies of the sick, and propagated to the healthy. Such an empirical law is pretty clearly established with reference to some of those belonging to the class, and probably none ought to be admitted into it to which the law is inapplicable. But we do not need to go back fifty years to find very accomplished physicians who questioned this statement when applied to some disorders to which they would not have hesitated to apply the name of specific fever, and it is not very uncommon even now to have it said that cholera is not infectious in the ordinary sense of the term, and therefore it ought not to be placed in the same class as small-pox and scarlatina. Others, again, will ask whether erysipelas ought to belong to the class; and if it do not, whether there be any reason for placing there such an affection as hospital gangrene.

These points are not mentioned with any idea of attempting to show how far induction has been the basis of such generalizations as are accepted in this difficult subject, a question into which we have not time to enter, but merely to indicate that we are still very far from having arrived at any law of causation in some of the instances referred to; and that the law which has been established with reference to the class as a whole, is still entirely empirical. Attempts have been made to enunciate much more precise laws. It has been asserted that some of these diseases have depended upon overcrowding of dwellings, others upon imperfect drainage and sewer emanations; some persons maintain that the poison is generated *de novo* under such circumstances, while others assume that the waste material thrown off from the body of the patient is ripened into an infectious miasm, in the situation thus prepared for its reception. We are indebted to Dr. Christison* for a very able analysis of the facts on which these supposed relations depend, and he has satisfactorily proved to my mind, that they have no claim to be ranked as laws of causation. In his admirable account of the present state of our knowledge on the subject, he points out the difficulty of reconciling "sporadic" cases of typhus with the general law of epidemic diseases. But it does not seem to me that the difficulty is such as to throw any doubt on the correctness of the induction by which it has been established. The very same difficulties are experienced every day in tracing the origin of cases of small-pox, and yet no one would venture to assume that any combination of circumstances could generate this disease without the introduction of some portion of the specific poison.

* President's Address in the Public Health Department of the Social Science Association, delivered Oct. 13, 1864.

The same suggestive address offers a very good example of the small value attaching to the enumeration of particulars without an hypothesis, around which they may be associated. Dr. Christison records some very remarkable facts about the prevalence of ague in certain districts of Scotland nearly a century ago, and its gradual decrease and entire disappearance during the latter half of that period; so that now the disorder never arises in the country itself, and is only seen in imported cases. The one hypothesis which presents itself to his mind is discarded as apparently inadequate to explain the phenomena recorded; and though a very interesting contribution to our knowledge, the accumulation of facts remains, in its present state, quite barren of results.

In its practical application, medicine must be in great measure deductive. This is no disparagement to the science, which, so far as it is true, is a science of observation and experiment. The exact sciences are themselves all deductive, and rest on a comparatively small number of inductions of the very highest order. Their laws are co-extensive with the realms of nature and the furthest reaches of thought; and in their application to individual cases, they are unerring if the deductive process of reasoning be logically carried out. In medicine the most absurd systems, as well as the most scientific, have been equally deductive; but the former have rested on hypotheses which were perfectly gratuitous, or inductions which were utterly false. Scientific medicine endeavours to appropriate the laws of physiology, pathology, and therapeutics, and to apply them to the management of any case under consideration. The process of reasoning, it must be confessed, is a very complex one, even when no doubt attaches to any of the premises, and the conclusion is very liable to be erroneous. First, the exact state of the patient has to be made out, and in this how constantly does every one fail, how obscure are the symptoms, how uncertain the pathological state! Then from physiology we learn what the effect of this condition must be,—how far the various functions must be deranged by it; but how much of this is guess-work! Lastly, our knowledge of therapeutics suggests something which may modify the condition known to us pathologically, or the function which physiology teaches us is disturbed, and we endeavour to fulfil one or both of these intentions,—unfortunately, very often with but little success. Such as it is, however, it seems to me that this deductive system of treatment holds a much higher place than that which rests only on certain empirical laws; and is very much more philosophical and trustworthy than the lower form of empiricism which claims no higher guide than mere experience.

The numerical method has not yet been applied to any great extent in therapeutical inquiries. The difficulties attending its employment are so great, and the method itself so open to fallacy, that the results are not likely to be very available for scientific purposes. There are, however, two ways in which it may contribute to the advancement of knowledge. First, as preparing the way for induction, by the collection of facts, which exhibit such a marked preponderance in the influence of one particular circumstance, that there seems every probability of the existence of a relation of causation which we may successfuly explore. Secondly, in showing the relative power of two or more agents which have been regarded as alike influencing one particular organ. I think it will also be admitted, that we are better able to judge of the fruits of experience when they are tabulated in a statistical form than when merely stored up in the memory, even if we receive with a certain amount of caution the inferences derived from them.

Experience will always hold a high place in the estimation both of the practitioner and the public. Scientific knowledge is of the first importance in the process of education, and without it the information that certain remedies are proper to be used in certain diseases will be valueless; but yet the nice adaptation of means to ends can only be gained by experience; and the tact with which remedies are administered in analogous instances will often make up, or even more than make up, for great ignorance of the reason why they are employed. Didactic teaching describes the symptoms of disease, and lays down rules how they should be met by treatment; but we must not forget, that while these are symptoms of disease, they are also actions or functions of living organs, and that remedies are not measured by chemical equivalents, but by the actual condition of the patient on whose organs they are to produce their effects. Most men as they advance in years learn to rely more upon their experience, and less upon their previously acquired knowledge, because of the infinite variety of forms which the same disease presents in different individuals. Shades of difference which can scarcely be expressed in words, are at once recognised by the eye, the ear, the hand, educated by long experience and observation; and scientific principles seem almost to merge in the application of the rules of art. When thus employed, an enlightened experience really marks out the accomplished physician, and serves as the best guide in the practice of our profession.

If, on the other hand, experience be taken as the method by which the value of any form of treatment is to be determined, it will be found most uncertain and fallacious. We have indeed certain rules of practice, based on the accumulated experience of past ages, which have been handed down to us in an empirical form, though the theory with which they were first associated has been either entirely lost and forgotten, or has been abandoned as incompatible with advancing knowledge. Such customs may be regarded as a rough substitute for the numerical method; but I trust the day is not far distant when all of them will be submitted to more exact scrutiny, and reasons more definite may be discovered, if they are really worth retaining in practice.

Experience may also be employed as a test of the truth of practical rules derived by deduction from ascertained facts in the science of medicine; and when many individuals coincide in testifying to the value of any such rule of art, their experience must have some weight in confirming the *à priori* argument. But if we are bound to acknowledge the uncertainty of the numerical method, as a test in therapeutical inquiries, it must be evident that experience, unaided by numbers, is still less to be relied upon, when its results have not been recorded, since memory is so little to be trusted in scientific investigation. For example, the treatment of acute inflammations by calomel and opium, which was deduced from the supposed action of mercury as a solvent of fibrine, has not been very long introduced into practice. A few years ago experience would have been said to be universally in its favour, especially in the treatment of inflammations of serous membranes. Now, not a few of the most intelligent members of the profession discard it altogether, and a certain vague feeling of doubt as to its efficacy more or less pervades all classes. If the facts had been tabulated, perhaps we might have been nearer to a solution of this question: at all events, the answer of experience is not a very certain one. A similar state of feeling partially exists with reference to the administration of saline draughts in fever, as well as to many other customary rules of treatment, which, whether good or bad in themselves, are doubted in this age of scepticism, for no other reason than that they belong to a bygone period in the history of medicine.

Whatever estimate may be taken of the value of such experience, the mere fact of its having been, to a certain extent, universal, has given it a character very different from that which so often assumes the name among us. There seems to me to be no argument more fallacious or more opposed to sound inductive reasoning than that which asserts the curative power of a remedy, because in ten, twenty, or even a hundred cases, recovery followed its administration; and yet this is what is commonly meant when experience is appealed to. It is much to be hoped that scientific medicine may ere long be delivered from this, the oldest, the most obstinate, the most universal fallacy which has in all ages hindered more than any other the progress of knowledge, and has been the constant theme of logicians of all times,—the *post hoc ergo propter hoc* ;—the belief that a sequence necessarily implies a relation of cause and effect; and this not only in cases where the constancy of the association is so great as to strike the least observant, but where it has happened only in a few cases. Three or four rapid recoveries after the employment of a certain drug, are, I might almost say, universally cited by the correspondents of medical journals, as distinct evidence of its beneficial agency.

In therapeutics, fallacies, from the misapplication of the inductive method of reasoning, are very numerous; few rules of practice rest upon direct induction, and in them the process has never gone further than the establishment of empirical laws. Valuable as such laws are in practice, their discovery has perhaps rather retarded than aided the progress of science. We know less about ague than we do about continued fever, though we can cure the one in a couple of days, and we scarcely know whether our treatment tends to help or to hinder the natural process of recovery in the other.

There is another aspect, however, in which the inductive method may be viewed with reference to treatment; and this is perhaps one of the most important questions in therapeutics, and one which, I believe, will occupy the attention of medical thinkers, more than it has hitherto done. The question of the actions of remedies, not with reference to particular forms of disease, but to the special parts of the organism affected by their presence; their influence in stimulating certain actions or functions which go on alike in health and disease, and the difference of their action on the same tissue, when in its natural state, and when altered in some given manner:—this, I think, is the direction which our inquiries must take, if we wish to establish anything like laws which shall guide our practice. It is matter of great regret that the British Medical Association had not so framed their therapeutical inquiries as to embrace this object, rather than the very vague and ill-defined one of the therapeutical action of remedies in curing diseases. There is already a considerable number of well-ascertained facts regarding certain drugs which have all the characters of correct induction, and enter into many of our deductions or inferential arguments with reference to the proper management of our patients. The action of purgatives, considered in this light, stands on a very different footing from that of diuretic remedies. The one class of drugs never deceives us,—their action is defined by an empirical law; and although it may be greater or less, according to circumstances, although the agency of the weaker sorts may be overruled by some other influence which for the time overpowers it, yet we can be certain of producing alvine evacuation, by employing more and more powerful remedies, unless the effect be interfered with by some mechanical obstacle. Diuretics, on the other hand, constantly fail; and though we may trace the remedy in the urine alike in cases of success and of failure, we know not why it seems at one time to stimulate the organ by its presence, and at another to be wholly inert. So little indeed is known of this action, that the number of individuals composing the group is differently stated by different authors. The power of depressant remedies, again, is one with which we are all familiar,—antimony, ipecacuanha, digitalis, in some respects analogous, yet how dissimilar! No less decided is the spasmodic muscular action and nervous excitability produced by strychnia. These and such like, standing on the ground of inductive argument, place in our hands means to attain a certain end, of which there can be no doubt: whether we shall use them aright for the cure of disease, does not lie within the province of inductive reasoning.

The limits assigned to this course of lectures has rendered it impossible to take such an extended survey of the present state of medical practice and medical reasoning, as my subject seems to demand. Neither has it been possible to give more than a mere outline of the methods which ought to be employed by us in the search after truth. With these restrictions, I have been unable to give so full a statement as I could have wished of the fallacies connected with the application of the inductive method of reasoning to the science of medicine. If I have at all succeeded in calling attention to the necessity for a sure foundation being laid in correct induction, before we proceed to erect a system of therapeutics; if the attention of the workers and the teachers of the day may chance to have been awakened to the importance of the knowledge of sound principles of reasoning to themselves and their pupils, my most ardent wish will have been gratified. I could not hope to make so short an exposition of medical errors more than suggestive, and the wide field of observation from which my sketch has been taken could not fail to render its details somewhat confused. Yet I venture to hope, that the more striking features have been sufficiently marked to leave an impress behind of their importance and reality.

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